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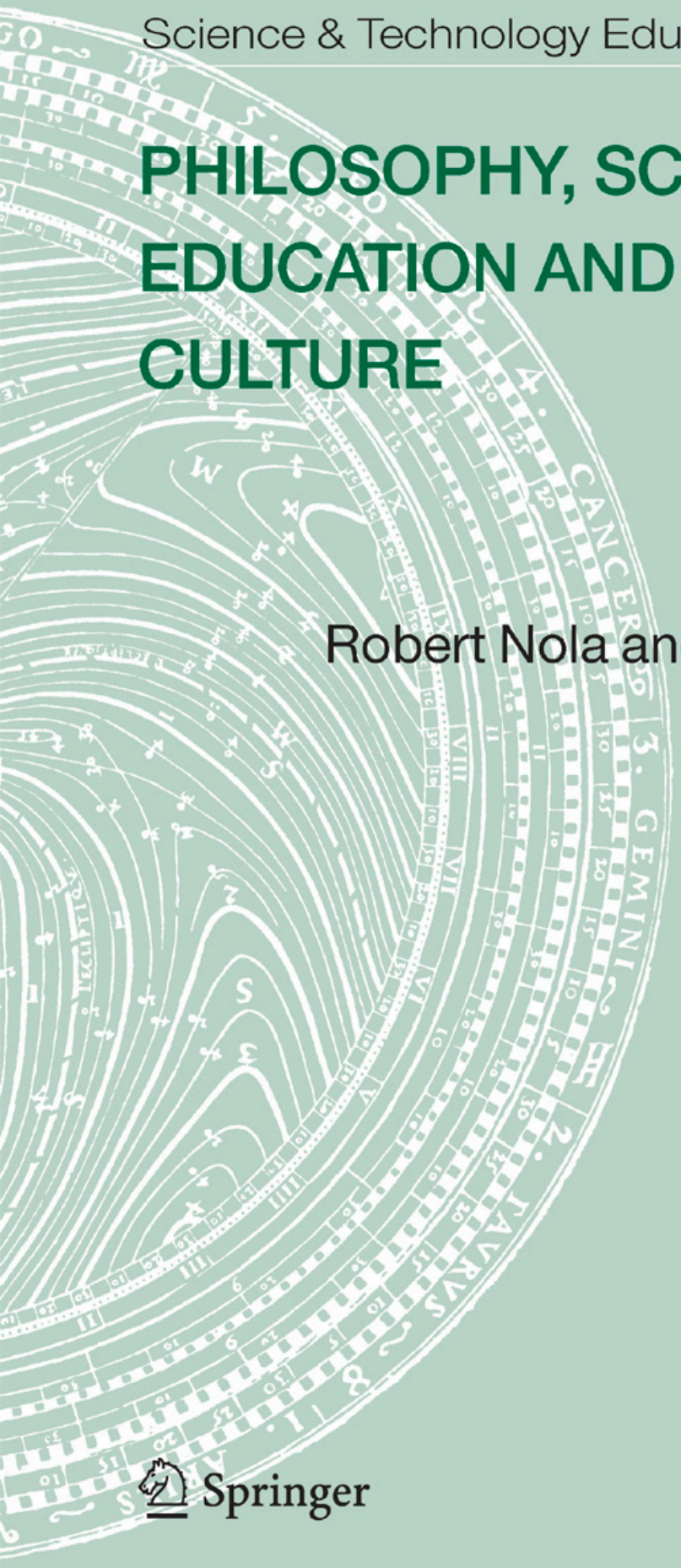
Robert Nola and Gürol Irzik



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by

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Dedication

To Jan and Sibel

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INTRODUCTON

1 THE THEMES OF THIS BOOK

In the last two decades various forms of constructivism, multiculturalism and postmodernism, have dominated the literature of both education in general and science education in particular. As a result of this influence, a number of theoreticians of education and science educators gave up the ideals of universalism, transcultural rationality, scientific method, objective truth and knowledge; instead they adopted localist, relativist, and anti-realist perspectives in science as well as in philosophy. Above all, in doing so, they abandoned the ideal of critical inquiry, despite the intentions of many to the contrary.

In this book we argue that the influence of these fashionable currents of thought were largely negative in many respects. First, they portrayed a wrong image of science by conflating the internal content of science (laws, theories, data and the like) with external factors impinging upon science such as its institutional arrangements, its funding, its technological applications, etc. Science is then easily depicted as an activity not guided by well-established, transculturally applicable norms of rationality and method, but by subjective constructions, local concerns, social factors, and power relations. The role of reality as a check over beliefs is constantly downplayed; that science can give us objective truth about a mind-independent world is often denied in favour of either an idealist or a phenomenalist or a sceptical position.

Second, and following from the above, local belief systems and cultural practices are celebrated uncritically as "knowledge" to be respected. It is claimed that there are as many sciences as there are cultures, that science is just a narrative, a language game that is on a par with other language games such as fables and gossip. Difference, incommensurability, dissensus, and paralogy are all embraced and encouraged without giving much thought to their implications.

Third, the three fashions of thought – constructivism, postmodernism, and epistemic multiculturalism – adopt very poor and discredited epistemologies, often under the guise of a revolutionary or novel approach. We are thus told that not only the claims we make about what we know but also the very definition of knowledge itself, are either social constructions or individual constructions out of experiences. It is denied that there is something in the real world in virtue of which our beliefs are made true. It is boldly argued that we cannot compare reality with our beliefs about it because we have no independent access to any such reality. Hence, it is claimed, we cannot know reality as it is, but only know our experiences or what we construct out of our

experiences. That this traps us in a "world of experiences" cut off from the real world does not seem to bother their advocates.

Finally, after all these views are adopted in various combinations and forms, they are applied to teaching and learning of science. Educators with radical constructivist leanings, for example, invite pupils to construct their own concepts and theories about nature, and then to "negotiate" them with other pupils and teachers. Radical constructivists do not attempt to make their students see that they are wrong when they go astray, since, after all, there is no objective knowledge and truth to be acquired, but only "viable constructs". Of course, in this way, the difference between a right answer and a wrong one disappears – but what reason is there to bother about truth and falsity?

Epistemic multiculturalist teachers encourage their pupils to regard their cultural or indigenous belief systems as being on a par with science, without worrying at all about the discrepancies between them and scientific theories. On the contrary, this is celebrated as "border crossing" rather than seen as a source of anxiety that can cause confusion in students. To alleviate the anxiety, some epistemic multiculturalists resort to an effective strategy: tell students, or if that is too didacticist give them the impression, that all belief systems are equally valid. After all, what matters is not truth, but whether belief systems serve the social purposes of cultures embodying them!

As for postmodernist teachers, they preach that students and non-students alike ought to follow Lyotard and advocate incredulity towards all meta-narratives, including those that attempt to justify scientific beliefs and methods. Furthermore, established canons – in science as well as in literature and philosophy – should be given no privileged place in education because they often disguise hegemonic and Eurocentric ideas and ideals as universal, and serve nothing but the *status quo*. On the contrary, it is said that since the ultimate aim of education is empowerment, all claims to universality and all searches for a rational consensus should be undermined by paralogical activity and criticized in the name of an allegedly emancipatory and more democratic politics.

The main purpose of this book is to show that none of these views stand critical scrutiny and they are not to be taken seriously. This is not, however, a merely critical book. We have also taken pains to construct a positive account of education and draw its implications for teaching science. This begins in the next section by arguing that critical inquiry is the core aim of education. Parts I and II spell out the philosophical underpinnings of this view. Part I sets out our philosophical position about the nature of knowledge (that is, epistemology) and its relation to education. Although radical constructivism is presented as a novel epistemology, we place it in the context of a number of rival theories of knowledge and critically evaluate it.

Part II sets out some theories of method as examples of critical inquiry in science.

Parts I and II deal exclusively with philosophical theories that enable us to develop a platform for the rationalist and realist approach to science that we advocate from which we can launch our criticism of radical constructivism. In Part III we develop of our criticism of sociological and postmodernist approaches to knowledge and science. In Part IV we distinguish between various versions of multiculturalism and then criticize what we characterize as epistemic multiculturalism. More detailed descriptions of the content of each chapter can be found in the synopses at the beginning of each Part.

This book is about *philosophical* theories of knowledge and science that impinge on science education. Emphatically, it is not about techniques for the classroom teaching of science; nor is it an empirical study concerning science teaching and learning. Although it is not directly about pedagogy, it does have pedagogical implications. In claiming that critical inquiry is the core aim of all education, including science education, we are not advancing this as an empirical claim about what in fact are the aims of science educators, governments, or communities with an interest in science education. Rather, ours is a philosophical theory of a strongly *normative* character in which critical inquiry *ought* to be the aim of education, regardless of whether *in fact* it is an aim of educators. Quite often, science educators confuse normative issues with empirical ones, based on sociological or psychological research. The normative-empirical distinction is crucial in Parts I and II since the ideas of critical inquiry, knowledge and scientific method are intrinsically normative, and not empirical or factual, matters.

That this is a critical book mainly about epistemology and science perhaps needs some justification. The doctrines of constructivism, postmodernism and epistemic multiculturalism that have become popular in the literature on not just science education but all education during the last several decades are essentially theories of knowledge and science. These currents of thought became fashionable by attacking rationalist and realist conception of knowledge and science. One of our major claims is that these currents of thought embody poor theories, and we take pains to show this by carefully examining them. This matters for an education in science because it involves, among other things, learning about the nature of science. In science courses students should learn not only the content of science, but also have a good idea about what sort of a thing science is and how it differs from other disciplines. Notions such as *theory*, *law*, *model*, *idealization*, *method*, *evidence*, *confirmation*, *explanation*, *knowledge*, *truth*, and *reality* show up naturally and inescapably in almost every science textbook and science classroom. It is therefore essential that both science students and teachers alike should have a solid grasp of them.

This is not, therefore, a purely critical book on the nature of knowledge and science. In being more constructive we have attempted to spell out in detail our own positive account, which is a form of rationalism and realism. Concerning education, we defend the view that its core aim is critical inquiry. Ironically, despite their rhetoric to the contrary, constructivist, postmodernist and epistemic multiculturalist theories of knowledge, science and pedagogy are far from developing a healthy, informed and constructive critical attitude in students. But in our view what is so unique about science is that it is precisely the embodiment of a critical attitude *par excellence*, even toward itself. Normatively speaking, a science education that does not concern itself with critical inquiry is a lame conception of education, whether in science or any other subject matter. In the following section we elaborate on this point.

2 THE AIM OF EDUCATION IS CRITICAL INQUIRY

2.1 Some Conceptions of Education

How do we understand the notion of education itself? This is more fully discussed in Chapter 1; but first, some preliminary remarks before we develop our thesis that the aim of education is critical inquiry. Some linguistic ruminations are initially helpful, but they do not take us very far. As common usage reveals, there is the important normative ideal of being an educated person, rather than an ignoramus, a dogmatist, or simply being "half-educated". These are noun forms indicating, broadly, the mental development and outlook of a person. Another noun form is that in which 'education' refers to a systematic course of instruction. In contrast verb forms indicate an activity of educating. And these take us back to the Latin etymological roots of the term in the verbs *educare*, to bring up, rear or foster especially in relation to children, and in *educere*. This last comes from a shortened form of *ex*-, 'out of', and *ducere*, 'to lead' (this latter also gives rise to the English 'duct', as a channel or canal).

'So what!' you might say, 'if etymology tells us that the English 'education' is a noun derived from the Latin for 'to lead out' and related terms such as '*eductio'* and '*eduction*-'. Isn't this a somewhat loose metaphor? Is all education a leading out? Is there always something to be got out of pupils? Is it not also getting something put in, if the something is not there in the pupil in the first place?' Clearly getting something into a pupil, as well as getting something out, seems to be a large part of education.

Though there are clear limitations to the linguistic and etymological approach, it is not without some value. In his discussion of the etymology of 'education', Partridge (1983) gives us a long list of the various ways in which the original Latin root *'-duct-'* appears in English. This shows that

the word ‘education’ is not unrelated to other words in English from Latin such as ‘conducive’, ‘induction’, ‘seduction’, ‘subdue’, ‘traduce’ and ‘product’. Many of these have had some role on the seamier side of education, as when teaching is likened to an induction process, or teachers seduce students into believing something, or treat them as a product, the final “outcome” of an educational “process”. This serves to remind us of ways in which ideal processes of education can be subverted. This is also the case for the Latin verb *docare*, ‘to teach’, which gives in English ‘doctrine’ and ‘indoctrination’. On the more positive side the Latin verb *erudire*, ‘to instruct’ or ‘to train’, has given us the English ‘erudite’ said of a person who is quite learned.

The term ‘education’ can also be highly ambiguous, as Passmore indicates:

> A systematic book on the philosophy of education would need, in fact, to distinguish between education$_1$ (upbringing), education$_2$ (schooling), education$_3$ (producing educated [people]) – to say nothing of education$_4$, the study of these processes. (Passmore 1980, p. 22)

Even though further ambiguities can be distinguished within it, we will consider largely Passmore’s education$_3$, limited to the context of educating people in science. In fact we will focus, as is common in science education, on the natural and physical sciences, including some of the biological sciences; we will say little of the social sciences or mathematics (though many of the same considerations apply in teaching these subjects).

There are many aims that people might have for education. A person might have quite particular aims such as attending Adult Education classes to learn the rudiments of a foreign language before embarking on an overseas trip, or to catch up on the latest development in the use of some new computer software. And we do want our young children to acquire the rudiments of reading, writing and arithmetic, amongst other things, when they first attend a school.

But the aims can be much less particular when we have in mind broad purposes for education such as: acquiring skills that empower one in life; acquiring skills in some field so that one can become a productive member of the community; enabling a person to enter fully into their community life; enhancing one’s artistic abilities; and so on. In his *Education and the Social Order* Bertrand Russell suggests the following three broad aims of education (though others can also be envisaged, as we will indicate shortly):

> … the first considers that the sole purpose of education is to provide opportunities for growth and to remove hampering influences. The second holds that the purpose of education is to give culture to the individual and to develop their capacities to the utmost. The third holds that education is to be considered rather in relation to the community than in relation to the individual, and that its business is to train useful citizens. (Russell 1977, p. 21)

Russell also comments that the first of these is a relatively recent aim for education while the other two have been with us for many centuries. Often it

is the third of these that informs science education while the second and first have little emphasis. It is often said that there is a crisis in education in the sciences because fewer and fewer people are taking up science as a vocation thereby becoming useful citizens in a "knowledge" or a technological society which requires trained scientists for various purposes, the main one being a force for economic growth. In contrast to this social aim, it is important to realise that there is a role for science education in imparting an understanding of science as an important aspect of our culture; and it can have an important role in developing our individual capacities. Further, it is also important to consider the ways in which learning science can enhance our intellectual growth and free us from unrealised hampering influences. Such is the case for some in the non-Western world who regard science as a source of liberation from their own confining "local knowledges" and practices (Nanda 2002, Chapter 1). All of these aims can be adopted without regarding science as a means to being a citizen with a certain kind of training who can then enter into the productive work force to enhance, say, business profitability. However, all three purposes for education commonly co-exist with no single purpose exclusively holding sway; but one may well dominate the others at a given time.

Russell also has some comments on how teaching and learning are to be carried out. There are two broad theories of this. One has its origins in Plato (see chapter 3); it is also advocated by many others including Rousseau. This is the view that all of us come equipped with an innate nature and the task of education is largely to use its resources to bring out that nature. A different view, advocated by Locke, behaviourists and many others, rejects such a role for our innate nature (assuming we have one). Rather, to use the phrase Locke helped get wide currency in respect of our minds, we are more akin to a "blank slate" (or *tabula rasa*) which has no prior writing on it and is ready for whatever writing is to be put there. Educators can then write on it according to the requirements of an education programme designed by the state, educational authorities, or whoever else, so that pupils come to "fit in" with their requirements (e.g., to be good, productive and obedient citizens). These two innate/blank slate approaches to education are made graphic in Pinker's book (2003, p. 222) appropriately called *The Blank Slate*. Though the contrast is in terms of the "Noble Savage" (in which pupils are credited with some innate capacities) versus the "Blank Slate", both lead to views about education ultimately founded in underlying theories of human nature. Such a contrast should not be taken to rule out a third position that is some uneasy combination of both the "innate" and "blank slate" approaches (not a matter broached here).

2.2 Critical Inquiry as the Core Aim of Education

The above lists some important, broad aims for education that can be adopted in one manner or another, or with suitable qualifications as to what the aim is. Adopting any or all of these aims suggests that there is no one single aim for education but a plurality of aims that can vary over time with the circumstance and the needs of those who want education, or those who require an educated class. While not wishing to undercut this plurality, we wish to claim that there is still an important core aim for education that must also be realised and without which the aims mentioned may not be realised, or only fitfully be realised. This aim is a "core" aim in the sense that it is *presupposed* by the other aims, or is *necessary* for the full realisation of any other aims. This core aim of education is: *to produce people who can be rational and critical inquirers into whatever subject matter or discipline in which education is being acquired*.

This is a broad aim that allows any subject matter to be the object of critical inquiry. It also includes being a critical inquirer into the other various aims of education listed above, thereby subverting faulty or inadequate or tendentious aims, or recognising what are the most worthy aims, and what are good means for achieving them. Thus once a person becomes a critical inquirer they are then able to see, if one's aim is self-development, both what this really means and what are the best means to achieve it. In this sense critical inquiry is a core, presuppositional aim that can be directed upon the further aim of self-development, or any of the other several aims. Even critical inquiry itself can be the object of critical scrutiny. In fact the main thrust of this book is such an exercise. Though we do not address the matter directly here, critical inquiry can be, and has been, directed upon itself in a way that is not question begging.[1] This self-reflective task is one that has been undertaken by many philosophers interested in the justification of our methods, including even the methods of critical inquiry in science.[2]

Our view of a core aim of education in critical inquiry follows that of John Anderson.[3] He argues, in writings from the 1930s to the 1960s, that a core aim of education is not something ulterior or extrinsic, such as: satisfying the "needs" of society (for higher productivity, skilled employees in certain areas, vocational training, etc); or enabling someone to fit into society so that they accept its mores and traditions; or enabling "communication"; or leading to greater "self-expression", or "self-development" or promoting "mental health"; and so on. Some of these are important aims and can be a consequence, or an accompaniment, of some more fundamental underlying aim. In a crisp phrase, Anderson's claim is that the core aim of education is to produce people who engage in *critical inquiry* (alternatively criticism, or critical evaluation.) For Anderson "true education" is not initiation into one's tradition, or to acquisition of a

vocation, or any of the above extrinsic goals people have had in advocating education. On this he says:

> Tradition itself [and one may add any of the above extrinsic aims] invites criticism, because it represents certain things as worthwhile but is unable to give an account of their value. Thus the aim of education is to give an account of things, to find out *the reason why*, and thus to put knowledge in the place of opinion. (Anderson 1980, p. 70)

The important and deeply Socratic and Platonic roots of Anderson's position will be much more fully explored throughout Part I. Anderson puts crucial emphasis on education being the acquisition and application of the means of critical inquiry in order to acquire knowledge rather than simply the means whereby one acquires beliefs or opinion, or skills or abilities, in the absence of any critical assessment.

In saying that critical inquiry is the aim of education, we are definitely not making an empirical claim about what in fact some society has as its educational aims. More often than not the declared educational aims of governments are to provide employment for its citizens, or to make them fit in with the government's preferred way of life. What aims societies actually have, and how well they are realised, is a matter for sociology to investigate. In contrast our aim is not one which is to be empirically discovered. It is to be treated as an overriding *normative* goal for education that may not be realised or only fitfully realised; and where it is realised it may well be subversive of other purposes to which governments and others may wish to put education.

For Anderson, the educated, whatever else they may be, *ought* to have a highly developed ability to critically evaluate any beliefs, any assumptions and presuppositions, any attitudes, judgements and evaluations (including those of critical inquiry), and any traditions and customs of one's society and culture. The use of the world 'ought' here underlines the normative character of the kind of education that has as its core aim critical inquiry. The educated do not necessarily know a lot; so the mere collection of items of knowledge is not the goal intended, though the activity of critical inquiry can produce such knowledge and understanding. We will see in Chapter 3 that Socrates did not claim to know much at all, including the answers to his 'What is X?' questions; but he did claim to be able to carry on a well-conducted critical inquiry. Nor are the educated always right; critical inquiry will also help one recognise the fallibility of the application of the canons of critical inquiry and its products.

Critical inquiry makes a positive contribution in the way that it underpins knowledge; but it also has a subversive aspect as it may show that our former opinions, beliefs and practices are untenable. Critical inquiry can undermine some of the goals and traditions of society as well as revealing what is worthy of acceptance. As John Mackie puts Anderson's position, such an

aim enables one not only to acquire fresh knowledge but also to engage with more problematic aspects of life:

> Education means the seeing through of pretensions of all kinds, of all doctrines which serve as a smokescreen to protect and further undisclosed demands and policies, whether these are social or 'practical' or religious or arise within the organised pursuit of the sciences themselves. (*ibid*. 1980, p. 14)

Anderson clearly recognises that critical inquiry may not add support to accepted belief or tradition; instead it may bring one into conflict with a society's accepted belief, or tradition. This is something that needs to be noted in talk of 'multicultural science' with its neglect of critical inquiry.

This core aim for education directly concerns the process of criticism itself, and then derivatively the products of that critical evaluation viz., knowledge. People will be educated in Anderson's sense just when they are able to engage in critical inquiry with respect to the beliefs and opinions of themselves and others so that they attain knowledge by coming to have a grasp of the reasons or evidence as to why some beliefs/opinions are true and others false. Once so educated, a person is then able to find their way in life, to communicate, to express and develop themselves, to find what vocation suits them best, to be an informed participant within one's society – just to mention a few of the items that might be misleadingly elevated to the status of a core aim which informs all the other aims for education.

2.3 The Aim of Critical Inquiry in Relation to Other Aims

How does Anderson's core aim compare with the proposals of others? Already it can be seen that education as critical inquiry underlies the several other aims already listed. We will mention only two further luminaries from the past who propose aims for education. John Henry Newman spells out his view of the aim of a university education, saying of the pursuit of knowledge in universities 'that knowledge is, not merely a means to something beyond it, or the preliminary of certain arts into which it naturally resolves, but an end sufficient to rest in and pursue for its own sake' (Newman 1915, p. 84). We may agree with Newman's idea that knowledge is to be pursued for its own sake; but it lacks a rationale because he does not dig deep enough into the epistemic conditions that create knowledge. For Anderson this is the province of critical inquiry in which we provide evidence that something is so, or can give good reasons for its being so. Knowledge, whether pursued for its own sake or not, can only arise when our beliefs have gone through the process of a properly conducted critical inquiry.

Again A. N. Whitehead tells us: 'Education is the art of the utilisation of knowledge. This is an art very difficult to impart.' (Whitehead 1932, p. 6) Whitehead only mentions the use of knowledge, but does not tell us what knowledge is. Perhaps part of the difficulty he envisages arises from his omission of the role of critical inquiry not only in establishing something as

knowledge, but also in telling us how we should apply knowledge in various circumstances, the inquiry here being one of some means best suited to some end. Once again Anderson's notion of critical inquiry seems more fundamental. It tells us that critical inquiry is to be applied to discovering not just what the best means to some end are, but also what are worthy or valuable ends to pursue.

The role that Newman gives to knowledge is also a role that Anderson assigns to critical inquiry. Like Newman, Anderson claims that the aim of critical inquiry needs no further justification, particularly a utilitarian justification in terms of that to which it gives rise. Of course the products of critical inquiry will have many uses in society, especially in a fully participatory democracy. In fact it is the ability each of us can acquire to conduct the right kind of critical inquiry into any matter that makes a person a worthy citizen; any derogation from this can damage the life we all ought to lead. But these uses do not provide a further justification for the aim of critical inquiry. They are merely its applications in the course of human existence, which can be useful. Critical inquiry can be said to be the *intrinsic* goal of education in the sense that it is not regarded as a means for yet some further end. It is properly conducted critical inquiry that makes the consequences of its application useful to us; justification does not flow the other way from the usefulness of the applications of critical inquiry back up to critical inquiry itself. This is what it means to say that critical inquiry is of value in itself, or is intrinsically valuable, and is not merely valuable because of some end it to which gives rise.

If any extrinsic justification were needed, then it would be to show that critical inquiry is what leads to the truth about any matter whatever, and what exposes error and falsity. Here the ultimate aim of education would be to uncover truths, or expose error, in some field; and the correct application of critical inquiry is directed at those ends. What needs to be shown is that the methods of critical inquiry do take us to the truth, and not falsehood or error. To show that the methods of inquiry do reliably yield truth, and not error, is an important matter in epistemology that is touched upon in Parts I and II. It is this connection that is so graphically illustrated in Plato's account of Socrates' method of inquiry, discussed in Chapter 3.

If critical inquiry is the aim of education, then what is it? This is a matter that we will leave to Parts I and II where we spell out some of the core features of what counts as critical inquiry. We think that science provides good examples of the application of critical inquiry. In fact for many philosophers, science is the most organised exemplification of critical inquiry that we humans have devised when compared with other systems of belief and the way they are established and maintained. But note that science is not the only home for critical inquiry; it occurs in other spheres from the law to life more generally. Nor does science itself escape critical inquiry,

especially in respect of the uses to which it can be put. Science, like any other mode of investigation, should thrive on impartial, but not necessarily passionless, critical inquiry. The emphasis on critical inquiry is of a piece with remarks such as those made by Socrates who said that 'the unexamined life is not worth living' (Plato, 1993, p. 63, *Apology* 38A,) the process of examination itself involving the use of critical inquiry. It is also of a piece with J. S. Mill's remark: 'It is better to be a human being dissatisfied than a pig satisfied; better to be a Socrates dissatisfied than a fool satisfied'.[4] In effect Mill invites us to compare a life of ignorance with that in which the activity of critical inquiry has a wide role in life.

Could one have education without critical inquiry being part of it? Alas, this is all too common. It readily occurs when education is simply a means whereby beliefs are inculcated and held in ways that have nothing to do with critical inquiry. But this would hardly be an education worthy of the name, as one might say. In putting the matter in terms of worth, we can introduce a normative notion of education that has added on to it the very idea of the aim, or goal, or primary value, viz., education through engagement with critical inquiry. Is this merely a piece of linguistic legislation rather than a report on how the term 'education' is used? Not entirely. There is a meaning of the term 'education' that has norms built into it. It is this notion that in Chapter 1 we will carefully distinguish from the non-normative notion of education.

In the process of being educated one learns something; but also one can unlearn other things when it is shown that they are erroneous. As will be set out in Section 1.3, it is evident that what we learn need not be true or rational; people can learn the false and the irrational. This is a surprising fact not always fully noticed, or given its due, in accounts of the nature of learning. We will make much of this in Chapter 1. This is graphically illustrated in the case of cult beliefs, pseudo-scientific beliefs, weird (or ordinary) religions, and the like. George Orwell's *Nineteen Eight-Four* provides a fictional illustration that has been very influential. Winston Smith during his incarceration and at the behest of his jailers, not only came to believe of his former freedom that it was slavery, but he also came to terms with, and believed, the absurd claim that 2+2=5. He also came to believe that five fingers, and not four, can be seen when looking at the back of another person's hand held up with the thumb hidden. In the chilling final two sentences of the book Smith manages to overturn all his previous inclinations when he finally learns, as a result of the struggle with himself: 'He had won the victory over himself. He loved Big Brother'. He does not merely *say* that he believes these things when he does not; his captors are aware of that. What his captors want is that what he learns become beliefs that he can call his own and that he can give his full commitment to them like any other belief. This is learning and education – but of a peculiar sort akin to brainwashing.

A theme we will develop in Chapter 1 is that learning, and thus some kinds of education, are not logically or conceptually linked with truth and rationality, two of the prime notions that help flesh out our notion of critical inquiry.

Not all education is immediately involved with, or has as its direct object, critical inquiry. But critical inquiry can impinge in various ways, directly or indirectly. We receive an education when learning a foreign language, something that requires mastery of much information about grammar and vocabulary. Acquiring information also arises in history, science, geography, and so on. In an illuminating chapter entitled 'Teaching to Acquire Information' Passmore (1980, p. 57) makes the correct point that the information imparted can be true, or it can be false. Since this is the case, then critical inquiry with its central role for truth plays a part in enabling teacher and pupil to sort information from misinformation in the education process. Moreover, critical inquiry is not a contentless process; it is always engaged with the beliefs we hold. In order to evaluate them we often need a store of correct information at hand to enter into the process of critical inquiry. This will become evident in the role critical inquiry plays in the various kinds of scientific method described in Part II. Finally, we get information not just by observing, or through experience, or picking it up through reading and study. We also get it by reasoning about other information we already have in order to make explicit new information that would otherwise remain implicit and unnoticed in the store of information we already have. Being able to reason correctly is a central feature of critical inquiry. In ways such as these, critical inquiry plays a role in education in areas that might appear to involve only acquisition of information.

2.4 The Varieties of Critical Inquiry

Critical inquiry is a many-splendoured thing. By critical inquiry we mean at least the informal modes of reasoning that humans employ, or the more formal modes of deductive and inductive reasoning as found in elementary logic texts. Clearly these modes can be taught in specifically designed courses. But to some extent they are also innate in all humans through our common biological inheritance. Just how good we are as "natural" reasoners is a matter that has been well investigated recently by cognitive psychologists. It turns out that we humans are good at some kinds of reasoning and not so good at others. We do better at concrete cases of reasoning than with more formal reasoning. We do better with forms of reasoning using the rule *Modus Ponens* than a closely related rule *Modus Tollens* (as the Wason "selection card" test shows); unfortunately we do quite badly at probabilistic forms of reasoning, and those involving risk assessment.[5] These matters concern just how well some of the principles of critical inquiry are embodied in individual learners. When we speak of

critical inquiry we will mean the existence of principles governing such kinds of correct reasoning, independently of how well or badly they might be embodied in individual inquirers. Our focus is on the norms that can be embodied in any inquiry that a careful reasoner can follow.

Science is a prime area in which the principles of critical inquiry play a crucial role. As result we devote all of Part II to a discussion of principles of scientific method. It is our view that the principles employed in actual science can also be employed in the learning of science. As we will argue, it is learning in accordance with such principles that give the pupil knowledge; without it pupils merely pick up information or true beliefs. In our view this is not what learning and education in science should be. In fact we endorse the idea that the learning of science can be greatly enhanced by the study of the history of science and the way scientists themselves have arrived at their results.[6] By this we do not mean that pupils should necessarily follow all the "ins and outs" of the actual reasoning processes of scientists. Rather actual episodes in the history of science can provide a model for the teaching of scientific theories and concepts. Such models provide an intellectual context in which scientific problem arises, and they highlight the ways in which scientists addressed and solved the problems. Displaying the historical and problem context can give life to scientific matters that might otherwise remain bloodless and uninteresting. In fact the history of science is a vast repository of the very principles of method that lie at the core of all critical inquiry, and which can be deployed in science teaching.

In Part II we consider the aims of science as an important aspect of scientific method. We also look at inductive forms of reasoning. We also make an attempt to introduce some of the main ideas behind probabilistic reasoning. This is something that has been given little attention in science education. Yet science is full of probabilistic reasoning. It is as if science education is lost in a time wrap of the 1970s in which the main issues of methodology to be discussed are those of Feyerabend and Kuhn. The discussion of method has moved on in philosophy of science to reflect better what is current methodological practice in science.[7] Part II also discusses the hypothetico-deductive method and also the methodology of model building in the sciences. All of these matters, and others we do not discuss, are part of what we mean by the broad term 'critical inquiry'.

There are many other aspects to critical inquiry. One crucial aspect lies at the heart of the matters discussed in Part I. This concerns the big difference that philosophers always draw between belief and knowledge. This is often ignored or not understood in science education, or education more generally. Belief, we will argue, is a *naturalistic* state of the human mind since we have, unlike most animals, the cognitive equipment to form beliefs. In contrast knowledge is a *normative* notion that indicates that we have an *entitlement* to believe on the basis of evidence or reason; or we have a

warrant; or we have a *justification* or we have a good *reason* for the belief. The notions of entitlement, warrant, reason, and justification are all normative in character. As will be argued in Chapter 2 we upgrade our beliefs when we give evidence for them, and thereby display the entitlement, warrant, reason or justification. To make the upgrade we must employ the principles of critical inquiry. As we will argue, if a person comes to a belief in ways which do not employ the principles of critical inquiry then they do not have knowledge. At best they have a belief (which might be true or false), or they have got a piece of (true or false) information, or they have formed an opinion, or whatever. But they do not have knowledge.

As we will argue, students only have knowledge when they acquire their beliefs in a quite particular way; no other way will suffice. And this way is *via* critical inquiry. This will become clear when we consider in Chapter 2 the special requirements on knowledge, viz., that what is known be true, and that the knower have sufficient evidence to establish its truth. Giving sufficient evidence to establish some claim is a central aspect of critical inquiry. It is also a central aspect of scientific method.

In Part I we consider Plato's account of knowledge which essentially involves the idea of giving reasons. Plato makes the giving of reasons central to his theory of learning, as will be illustrate. More broadly, the whole gamut of aspects of critical inquiry enters into the dialectical structure of all of Plato's Socratic dialogues. This is a matter we can only briefly touch upon. But the Socratic method of inquiry is something that is central to the rational tradition of critical inquiry.

NOTES

[1] See Siegel 1997, chapter 5 'Why be Rational?' where there is a discussion of how one might justify a commitment to rationality, and that this is not a self-defeating inquiry to undertake or an irrational commitment to make.

[2] For a survey of attempts to justify the methods of science see Nola and Sankey 'A Selective Survey of Theories of Scientific Method' in Nola and Sankey (eds.) 2000, pp. 1-65.

[3] Anderson's writings on education are collected in Anderson 1980, and are prefaced by three useful essays, of which the paper by Mackie is the most useful for the points being made above.

[4] See any edition of Mill, *Utilitarianism*, Chapter 2, end of 6th paragraph.

[5] On the Wason selection task see the following. Books all of which have a discussion of it. One of the classic texts about empirical aspects of probabilistic reasoning is Kahneman, Slovic and Tversky (eds.) (1982). An overview can be found in Manktelow and Over (1990). There are often useful accounts of this research in books on cognitive psychology such as Sternberg 1999, chapters 11 and 12. The findings they report have proven, on the whole, to be robust results about our natural reasoning abilities and are now set out and discussed in most books on cognitive psychology.

[6] This is argued in Matthews 1994; the subtitle of the book is 'The Role of History and Philosophy of Science' while the main title is 'Science Teaching'.

[7] One exception is Gauch 2003, who has an interest in science education and who discusses probabilistic forms of reasoning in a way science educationalists ought to take on board as part of their theoretical background.

CHAPTER 1

BELIEF, LEARNING AND EDUCATION

This chapter sketches some features of our notions of belief and its connection to learning and education. It argues that, on the whole, there is no logical or conceptual connection between, on the one hand, belief, learning and education and, on the other, critical inquiry. Critical inquiry is something we have to "add on" as an aim to the processes of learning and education; there is no logical or conceptual guarantee that these will produce critical inquiry. Also, we need to find methods of teaching and learning that are best conducive to the realization of this end. What is critical inquiry? In the *Introduction* Section 2 this was argued to be a core aim of education. Our account emerges more fully throughout chapters 2 to 5, and most of Part II is devoted to it. Our overall argument is that knowledge is intimately connected with critical inquiry as one of its products, while mere belief is not. To mark this crucial difference, in this chapter we will confine ourselves to the connection between belief and learning. Our crucial point is that acquiring beliefs is not necessarily tied to critical inquiry and can proceed without it. In contrast critical inquiry is essential to knowledge and cannot be obtained with out it. We discuss the nature of knowledge and how it is learned through critical inquiry in Chapters 2 and 3. This may seem strange to those in education, in large part due to the misunderstanding of the nature of knowledge – an issue we also address.

We begin this chapter by stating something very obvious, viz., that we humans are unique in that we can form beliefs, and we can use language to express them. Given the centrality of beliefs, the second Section 1.2 focuses on the surprisingly difficult question: what is a belief? The discussion sketches some logical and conceptual points sufficient to spell out central aspects of what counts as a belief. Does all learning involve belief? Section 1.3 draws a distinction between learning *how*, learning *why*, learning *that*, and so on, and investigates the extent to which learning *that* (which does involve belief) is involved in the other kinds of learning. It also makes clear that truth and rationality are not intrinsically linked to any of these kinds of learning.

Reasons for the emphasis on belief emerge in Section 1.4 where a minimal conception of learning is set out. Often learning involves taking up new beliefs, as well as discarding old beliefs. Then we argue our central thesis. We can learn not only what is true or rational, but also what is false and

irrational. Since truth and rationality are bound up with the notion of critical inquiry, then what we show is that learning is quite independent of truth and rationality and in no way entails any aspect of it. At best truth, rationality and all the other features of critical inquiry are an external "add-on". Though they are often employed in learning, they are not an intrinsic feature of learning. This is an important logical point that is often obscured in the voluminous literature on learning. Section 1.5 explores a few theories of learning to discover to what extent learning is, or is not, linked to critical inquiry. We show that there is no intrinsic connection to behaviourist theories of learning, or Piaget's theory. This supports our view and is not problematic.

Section 1.6 explores the link between education and the notions of learning that have been explored and education. We argue that there is a descriptive notion of education that is not conceptually linked to critical inquiry. However we claim that there is also a special normative notion of education that is so linked, and it is this notion that we explore and employ in the remainder of the book. Though the notion of critical inquiry is invoked, fuller discussion of it is left to subsequent chapters.

1.1 BELIEFS AND LANGUAGE AS (NEARLY) UNIQUELY HUMAN

One main feature that distinguish humans from all the other creatures of the animal kingdom (of which we are part), is the *extent* to which we form beliefs about any matter whatever, and the language(s) we use to express these beliefs. Language has a wide range of functions other than stating beliefs. We use it to express feelings and emotions, to issue commands, ask questions, give warnings, conduct ceremonies (as in naming a ship, or in a marriage ceremony, etc.), and so on. We will not enter into discussions as to whether other animals have beliefs, or even have a language (if only a language of gestures) to express their beliefs. (We think they do, but this is not germane to our purpose.) What distinguishes us humans from all the other animals is the extent to which we can use a finite vocabulary, and the finite rules of grammar, to make a potentially infinite number of grammatically meaningful sentences, and to use these to express a potentially infinite number of beliefs (understood as belief contents).

Some might wish to claim that what distinguishes us from the animals is our ability to possess concepts rather than beliefs; yet others say that it is our ability to use the sentences of some language rather than our possession of beliefs. Thus they would say that we English speakers master the use of the words 'red' and 'square', or that we have the concepts of *red* and *square* in preference to saying we have the belief *this is red*, or *square here*. In philosophy from Frege to Quine and beyond the tendency has been in the theory of meaning to take the notion of a belief, proposition or sentence as the unit of analysis from which the notion of a concept can be distilled. We

will follow the Frege tradition and take seriously his dictum 'never ask for the meaning of a word in isolation but only in the context of a proposition' (Frege 1960, p. xxii). This is tantamount to claiming that we can only specify concepts in the context of our propositional beliefs. For many purposes this difference in emphasis will not be important, but for some it is. So from the beginning we will adopt the notion of a belief as more fundamental (thus the belief *this is red*) and distil from it when necessary the notion of a concept (thus the concept *red*). A further reason for focusing on beliefs rather than concepts is that we do have a clearer idea of when someone has a belief, and when they change it, than we do when someone has a concept, and when they change it. But these raise deep issues in philosophy and cognitive psychology that we will not touch upon.[1]

Two conditions are necessary for us to form beliefs and to acquire and use a language. The first is that each of us needs a sufficiently well-developed brain that enables us to entertain and express beliefs in language. The complex neuro-physiological functioning of our brains that enable belief formation and language use is something that has evolved over time for humans, and hardly, or not at all, for other species. Discoveries about our unique brain structure with its cognitive architecture is a rapidly developing science that will tell us much about what are the special features that make it possible for us to learn and be educated in ways quite unlike our cat, dog or any other animal.

Since our purposes are largely philosophical and conceptual, we need not delve into this science in any detail. But we do acknowledge that the developing science of evolutionary psychology is beginning to have important things to say about learning. We should not forget that it is the very features of our brains that have evolved for specific purposes that make learning and education possible for us. As Pinker (2003) forcibly argues, the brain is not merely a "blank slate" upon which any experience can write. The very mechanisms whereby we learn are themselves evolved adaptations.[2]

The second important condition is our sociality. Humans and animals alike learn a wide range of things from others in their group (and must have the brain capacity to so learn, whatever this be). In particular, we adult humans have already acquired, by means of some learning process, a natural language (English, Turkish, etc.) and a large number of beliefs that we can express in language. In fact the world's natural languages are culturally transmitted objects passed on from generation to generation. But note also that our ability to have beliefs, or to acquire any natural language, depends on our evolved brains with their Chomskian deep structures and the like, that make this possible. This takes us back to the first necessary feature in which our brains have evolved in such a way that all people on Earth regardless of culture have the capacity to learn a language, to form beliefs, and so on. Our

common human evolutionary inheritance of such capacities to learn language and have beliefs is overlain with differing cultural influences that lead us to learn a particular natural language or acquire the beliefs and values of a particular cultural tradition. The complexities of how culture and biology interact is something that we need not go into; but we do reject any simple picture of genetic determinism, or cultural determinism, or fixed percentage interaction of one with the other.[3]

There are different kinds of belief that we can acquire, some necessary for the acquisition of other beliefs. Amongst the central beliefs we must acquire are *semantic beliefs* about connections between words or sentences and the world; these are crucial to the natural language we learn. Thus we learn that names such as 'Mama' refer to this, and not that, person; 'Papa' refers to this other, and not that, person; and so on for other proper names. We also learn names for kinds such as 'food', 'cat', 'gold', and then learn about the kinds to which we can refer. We also need to learn *how to* combine these words grammatically. Once we master the meanings of quite basic words and sentences, we exhibit, when young, an amazing capacity to launch off into the full natural language of our respective communities, and so can express any number of other beliefs. Here we state the obvious, viz., that we do learn such things; but as philosophers, we leave to the plethora of empirical theories the matter of *how* we learn to do this, and *how* we may best be done.

We can entertain a very wide variety of beliefs about a host of subject matters once we have acquired the basic semantic beliefs and syntactical competence. Thus we can readily acquire beliefs about our immediately observable environment such as 'the coffee cup is empty' or 'the radio is too loud' and so on. But quickly we go beyond beliefs about the observable and form beliefs about unobservable things such as 'electrons are negatively charged' or mathematical beliefs, say, '17 is a prime number', or mythical beliefs such as 'evil spirits have taken charge of his soul', and so on. We also form moral beliefs, aesthetic beliefs, theological beliefs, beliefs that comprise a whole range of different world-views, beliefs about what we desire, what we hope for, what we aim to avoid and/or wish for others, and what we value.

Each adult has already had its initiation into the language and beliefs of their community through some processes of learning and education. Young children entering into their community also acquire, semantic beliefs, beliefs about their experience and immediate environment, and beliefs that go beyond this. How do they acquire such beliefs? Their evolved brain capacities play a big role here. But so does teaching, the teachers often being their parents, their immediate family or community; or the teaching and learning takes place through special social institutional arrangements such as schools, universities and the like. With no teachers at all, it is unlikely that the young would acquire beliefs beyond that of the legendary 'wild child'

who allegedly grew up provided with only food by animals in the absence of any teachers of language. Both the social context of at least one other teacher, and the existence of a language to be taught are, alongside our commonly evolved brain capacities, the necessary prerequisites for language learning and belief acquisition.

1.2 WHAT IS BELIEF?

The above simple truths about language acquisition crucially use the notion of *belief*. But what is belief? As noted, beliefs can be about any subject matter whatever. We have beliefs about purported factual matters with belief contents such as *that the Earth orbits the Sun*, or *that Rome is the capital city of Italy*. But importantly note that what we believe need not be true. We can just as well hold beliefs that contradict these, viz., *that the Sun orbits the Earth*, or *that New York is the capital city of Italy*, and so on. In what follows we will use the notation common in philosophy and logic in which the letter 'p' can stand for any propositional content of a belief, such as that expressed by 'the Earth orbits the Sun', that '2+2=4', 'murder is deeply repugnant and wrong', and so on.

Beliefs are, most likely, states of the brain, though what sort of brain state is currently not well understood; and in any case this need not concern us here. An interesting account is being developed in evolutionary psychology about the different "modules" in the brain that take sensory input and provide, as output, visual, auditory and other perceptual beliefs. There is also a "central processor" which acts on these "output" beliefs making inferences, thereby producing yet other beliefs. According to evolutionary psychologists, the evolution of these and other "modules" in the brain do play an important role in belief formation and in learning or acquiring beliefs that we need to keep in mind as an important background consideration.[4] Also such beliefs when true, we will assume, are *representations* in the mind of how the world is. Just how they represent is not a matter we need to consider. But contrary to some who might be over-influenced by postmodernists views and argue that our beliefs do not represent, we will maintain that they do represent. This is consistent with something else we do deny, viz., that the only function of language is to express such representing beliefs. There is much talk in philosophy and education of *representation*, a matter that can be quite obscure. Here we will use it to refer to the relation between our beliefs and what is purported to be the case in the world. Thus the belief *that the Sun orbits the Earth* is a representation of how the world is (no matter whether right or wrong).

Setting aside these matters, the following sketches some important points about our concept of belief that are relevant to learning and education.

First, beliefs are akin to dispositions in that not all the beliefs a person holds are currently in their conscious mind. For example, if asked, say, whether there are warm seas in Antarctica, each person has a disposition to answer 'Yes' or 'No', these answers being an indication that either one believes this, or does not. In this sense the beliefs we hold are dispositional states of our brains and/or minds that can be triggered causing us to act or speak in particular ways. Again how beliefs have this dispositional character need not concern us, through an interesting story will be told about this in our developing neuro-physiology of the brain.

The second point about belief is the quite crucial claim about the independence of belief and truth. The contents of our beliefs can be either true or false. Using the notion suggested above in which 'p' stands for any propositional content, we can express the following important logical thesis about belief and truth:

if a person believes that p then it does not logically follow that p is true; nor does it follow that p is false.

In other words, the belief that p is logically independent of the truth or falsity of p. (As will be seen later, this is an important difference between belief and knowledge; in contrast, knowledge that p entails the truth that p.)

Third, there is an important distinction between the *act* and the *content* of belief. On the one hand there is the *act* of a person believing; but on the other hand there is a specific *content* about what is believed. On the act side, people come to form beliefs, maintain beliefs, alter beliefs, and so on. There are causes of these believing activities, just as there are causes of other human activities; the act of believing is just one kind of human activity, though a particularly mental or cognitive activity, and not just a bodily activity. The activity of believing also causes further activities of ours, especially when our beliefs are, in part, the cause of our actions. For example, suppose a person has the (false) belief that there are skiing fields in Fiji. Then this belief can be part of the cause of their travelling to Fiji for a skiing holiday. Of course, they also have to desire that they ski as well, this being another cause of their action. The main joint causes of actions are beliefs and desires. Acts of believing are part of the causal network in which we exist; so they can affect and be affected.

However on the content side, the contents of our acts of believing do not stand in causal relations. Rather belief contents stand in logical relations to one another such as implication, contradiction, logical independence, inductive support, and so on. Using the notation just introduced, the act/content distinction can be expressed as follows. For any person A (Alice or Arthur) we indicate their act of believing by the phrase: *A's believing that p*. It is these acts that stand in causal relations. In contrast the *propositional content* of A's belief can be indicated by the contained phrase: *that p*. It is

these contents that stand in logical relations. They are inert and do not cause anything; but they still have distinctive logical connections with one another. The subject logic is the study of such logical relations.

Fourth, it is important to note that the very same propositional content can be common to many different speech acts of ours. Thus we may *state* a belief, say, that nuclear radiation is harmful; or we may *ask,* or *question,* whether nuclear radiation is harmful, or *warn* someone that it is harmful, or express a *hope* that nuclear radiation is harmful, and so on. The propositional *content* is the same in all these different speech acts, viz,. *activities* of stating, warning, asking, hoping, etc, viz., *that nuclear radiation is harmful*.

The fifth important claim about belief is summed up by the philosopher Frank Ramsey's apt remark that our 'beliefs are … a map by which we steer' (Ramsey 1990, p.146). Beliefs are the kinds of thing that we take on board as part of the "furniture" of our minds. But they are also like maps in that they purport to represent the world in certain ways (and like maps they can correctly or incorrectly represent – see the second point above). Once having taken them up into our minds we can act on them, and conduct our lives on the basis of them. That is, we "steer" our way through life on the basis of our beliefs. Thus consider the belief that, say, big doses of Vitamin C will cure a cold. Now it does not matter whether this is true or false. But if a person really believes this, then when they have a cold they will act on this, viz., take larges does of Vitamin C (providing, of course, that they realize that they do believe this, they are not suffering from weakness of will and fail to do what they believe they should do, and so on). Again, consider a person who holds the quite false belief that there skiing fields in Fiji. As weird as their belief might strike us, their willingness to act on it is a big part of the evidence that they hold this odd belief and "steer" by it (e.g., they take skis to Fiji).

The sixth point is that beliefs are also a particular kind of mental attitude. They differ from other mental attitudes, which can have the same propositional content. Thus I may *wonder* whether there are skiing fields in Fiji, or *entertain* the thought that, or *hypothesize* that, or *conjecture* that, or *suppose* that, or *fear* that, *hope* that, *propose* that, etc, there are skiing fields in Fiji. All of these, indicate different mental attitudes, directed upon the same propositional content, viz*., that there are skiing fields in Fiji*. None of these mental attitudes entail the *belief* that there are skiing fields in Fiji. Perhaps the closest mental attitude which might do this is that of *accepts that* …. However A *accepts* that p is strictly not equivalent to A *believes* that p. Thus one might say: 'I accept for the sake of getting on with you that there are skiing fields in Fiji; but do not expect me to believe this'. Thus one may accept that p for various reasons but not believe that p in the sense that it is something to steer our lives by.

This completes our discussion of some of the central features of belief. Belief is not the same as knowledge; the important difference between knowledge and belief is the topic in Chapters 2, 3 and 4.

1.3 LEARNING AND ITS INDEPENDENCE FROM CRITICAL INQUIRY

With the notion of belief just outlined, it is now possible to say something about a *minimal* concept of learning. There are a large number of things we can learn, such as beliefs, skills, behaviours, qualities, dispositions, capacities, and so on. We also learn concepts; but as indicated in Section 1.1, this will be subsumed under learning beliefs. Here the primary focus will be on the learning of beliefs and skills. Importantly the word 'learn' also enters into a number of different grammatical constructions such as: a person learns *how to* …, learns *to* …, or learns *why* …, or learns *that* …. These are quite different locutions and something needs to be said about each.

1.3.1 Learning How To ....

Consider learning as skill acquisition. The expression 'A person learns *how to* …' can be completed in various ways by phrases that refer to skills or abilities such as 'speak Turkish' or 'play the piano'; or 'prepare slides of stained cellular material for viewing under a microscope', 'solve quadratic equations' or 'apply Newton's laws of motion' (to give some examples from science education). Here a person is, in learning, attempting to acquire a skill and performing some activity in order to acquire it (the process may or may not involve teachers). Such learning may or may not be successful. So the locution 'learn how to …' is not a success or an achievement verb, as Gilbert Ryle would say;[5] rather it indicates an on-going process. (Note that the slightly different locution 'a person *has learned* how to….' does carry with it the implication of success.)

Not all learning to acquire skills need directly involve matters of rationality, such as learning how to poach an egg; but some do. Consider the case of learning to make inferences. A person can learn how to use the rule of inference called *Modus Ponens*: given (premise) p and given that p entails (conclusion) q, then infer q. And let us suppose that they apply it correctly in concrete cases of reasoning. Now it is important to note that, from the claim that a person learns the rule, nothing follows about the rule being correct. But it is, as a matter of fact, a correct rule, as is shown in logic. This now raises a more delicate matter. Suppose a person learns, and applies successfully, a rule that says: given q and given that p entails q, then infer p. This is not an acceptable rule of logic; it is a fallacy known as 'the fallacy of affirming the consequent', the consequent being 'q' as it occurs in the premise 'p entails q'.

Now a person can successfully learn this fallacious rule. And they can learn to successfully apply it.

The point being made here is that a person can acquire a skill and learn *how to*, where what they learn is how to use a particular rule of inference; but from this nothing follows about the rule being a correct rule. People can just as well learn fallacious rules. Again, consider what is known as the Gambler's Fallacy.[6] This is an inference a person makes at a gaming table, or in making bets, when they say: 'I have lost up until now; so I infer that my luck must change very soon, for the next bet I am about to make!' One can easily learn this fallacy and how to apply it from one's gambling companions. One can even be a good learner, and have excellent, persuasive teachers of the fallacy! But having excellent teachers that one even admires is no guarantee that what one learns from them is true. What these examples illustrate is that truth, correctness or rationality is not guaranteed to be a feature of *learning how to*; one can just as well learn the false the incorrect and the irrational.

The above account of learning *how to* is individualistic, but it need not always be so. We acquire skills in groups, as when we learn to play games in teams, play in a musical group or orchestra, or perform in a play. Clearly the presence of teachers adds a collective element when an instructor teaches us to learn *how to* do something (drive a car, etc); but they are excluded when one learns for one's self *how to* do something. Also one can learn from one's fellow pupils, if there are some. In the sciences learning *how to* may also be either individually or collectively. And some may be intrinsically so, as when one has to cooperate with others in learning how to perform some complex experiment or operation in areas as diverse as acquiring skills in a chemistry laboratory or out in the bush in learning conservation techniques. In not emphasising the collective character of much learning we do not intend to ignore its importance. Rather it goes without saying that while an individual can learn by themselves, some kinds of learning may have to be essentially collective (as in playing in an orchestra).

Learning *how to solve problems* is a crucial part of any education in science. Here we do not propose any empirical theories about how this might be done by pupils or imparted by teachers; this is outside the scope of our book. But what is said in Bransford *et. al.* (1999, chapter 2) on the *learning how to* of experts as opposed to novices is quite consistent with our approach here. We extend what they say to the idea of *know how to* (see Section 2.1.1) and the kind of organisation of knowledge by pupils that expert problem solving involves in our discussion of *know why* and *explanation* and *understanding* in Section 2.5.

1.3.2 Learning To ...

The locution 'learn *to* …' is used in ways which differ from 'learn *how to* …' in that what one learns, or acquires is not a skill, but a disposition or a character trait, which, of course, may well reveal a skill. Thus a child can be taught, and thus learn, *to* be tidy; they learn a certain disposition to behave, and they learn the "how to" that leads to success in being tidy. A person can be taught, and thus learn, *to* be polite; they learn a certain disposition and the "know how" pertaining to the different sorts of politeness that are to be exhibited in different circumstances. In Shaw's play, *Pygmalion*, Professor Higgins teaches Eliza Doolittle so that she *learns to* speak proper English, thereby acquiring the disposition to do so without any prompting. In all these cases learns *to* … connotes success. Finally a person can *learn to* appreciate European Renaissance painting. However such *learning to* is not strictly acquiring a disposition but is more an attainment.[7] It may also involve a lot of learning *about* the European Renaissance where the leaning about involves much learning *that* …, a matter to be addressed shortly.

1.3.3 Learning Why ....

Consider now a very different context in which the word 'learn' can occur. The phrase 'A person learns *why* …' is to be completed differently, since what fills the blank is not an expression referring to an ability or skill. Rather what fills the blank is a sentence describing some state of affairs for which there is some explanation that the person is trying to master. Thus a person can variously learn *why* water exhibits the anomalous behaviour of expanding when heated between 0° to 4°, or *why* the planets move in ellipses when under a central inverse square law, or *why* an ozone hole has appeared annually in the atmosphere above Antarctica, and so on. In learning *why*, a person is attempting to master the generally accepted, or the *purportedly* correct, explanation of some happening.

Need the explanation learned be the correct explanation? Not necessarily. All that is required in learning *why* (or learning *how*) is mastery of the *supposedly* correct explanation, or the explanation generally accepted, say, by one's teachers. Importantly it need not be the correct explanation. In eighteenth century chemistry, the phlogiston theory of combustion prevailed as an explanation of why metals, like tin, were reduced to a powder when burned. This was said to occur because, when heated, a substance called phlogiston left the tin. It was not because the tin was oxidized since oxygen was unknown until Lavoisier discovered it at the end of the eighteenth century and proposed theories about the activity of oxygen. Yet all scientists before Lavoisier's time, and even the young Lavoisier himself as a student of chemistry, *learned why* metals combusted; and the purported explanation they learned was in terms of the quite false phlogiston theory. Thus one can

learn *why*, where what is learned is not the correct explanation, or the explanation accepted now. And one can also unlearn *why*, as did Lavoisier himself and many of the chemists who came to accept the new oxygen theory. The important point here is the independence of *what is learned* from the *correctness* of what is learned. We will see that there are several other important independence claims to note.

There are some relations between the kinds of learning so far distinguished, or lack of them, to note. One can learn certain skills but not explain why these skills are successful. Thus a person may learn *how to* ride a bicycle, and especially learn *how to* lean while cornering. But they do not necessarily learn *why* leaning to the extent they do enables cornering; but the do get the "feel' for what they do in learning *how to* ride successfully. Learning *why* leaning (to the extent one does) while cornering is an issue that calls for an explanation in theories of the dynamics of motion that most bicycle riders need know little about. Again, one may learn *that* the sky is dark at night, merely by looking. Most find this such a natural phenomenon they would not think of asking, or of attempting to learn, *why* the night sky is dark. But learning *why* this is so takes one into an interesting problem known as Olber's paradox. What this illustrates is the logical independence of learning *that* from learning *why*.

1.3.4 Learning That ....

The locution 'a person learns *that* …' has just been introduced. The blank is to be filled by a complete sentence which expresses a propositional content, or as we will say, a belief content, or just a belief. Thus a person can be said to learn *that*, say, the Earth orbits the Sun (but in learning *that* they need not learn *how*, or learn *why*, this happens). Note that a person can also be said to learn the contrary belief, viz., a person learns *that* the Sun orbits the Earth. This we now take to be false, but it was what every educated person *learned* up until the time of Copernicus and Galileo (and some might even now still learn it). From Ancient times until the seventeenth century Europeans learned *that* the Sun orbits the Earth, since that was the prevailing belief over that period. In addition the educated learned *why* this occurred, where the explanation could not possibly be anything like the one we now adopt. And they learned it just as effectively as we now learn the opposite, viz., *that*, or *why*, the Earth orbits the Sun. It does not follow that when one learns *that* …, one has learned a truth; one can just as well learn what is false. Learning that p is independent of whether p is true or false. This is another example of the important principle of the independence of learning from truth or correctness.

Learning *that* does *not* involve success in the sense that one what learns must be true. The success in learning is the "taking on board" of the belief being taught, this belief being, as was said in Section 1.2, a belief by which

one "steers" through life. The success is in getting the belief that p into one's mind; it is not the success of getting a belief that is true. Such *learning that p* is a stronger notion than merely *accepting that p*, where the pupil's acceptance is due not to learning but, say, the need to pass a test on the subject. Students may well "fake" the learning process in this way in order to succeed; they accept that p, but do not believe that p.

This can be further illustrated in a more complex case. A person may learn *that* the Bible says that the World was created in six days; that is, A successfully acquires mastery of what the Bible says. But in acquiring this belief A does get a true belief. The logical point is that A can learn *that* some authority says that p. And the proposition *that the authority says that p* may be true (or false). However it does not logically follow that if A correctly learns *that the authority says that p*, that A also learns *that p*. It is logically fallacious to infer that because A acquired a true belief about what the Bible says, that they also learn that what the Bible says is true. Non-believers, particularly atheists, can correctly learn what the Bible says; but they can deny the content of what it says, viz., *that* the World was created in six days

What relations are there between skill learning *how to* and cognitive learning *that*? In some cases the connection seems weak or non-existent. Thus in learning *how to* ride a bicycle there is no obvious kind of learning *that* which has to take place which pertains to the riding. Of course a person has to learn *that* these are handlebars, these others are pedals, and the like, in order to be receptive to any instruction. But none of this pertains to learning *that* with respect to riding. In contrast, other cases show a strong interconnection. Suppose a person sets about to learn *that* (say) the theory of relativity has strong evidential support. Then one has to, at least, learn about the theory of relativity, what its hypotheses are, and so on, and finally learn what the relevant evidence is. All of these involve much learning *that*, perhaps along with learning *how to* make certain kinds of calculations. But in order to assess the evidential support, one has to learn *how to* do this, by considering details of theories of evidential support (again involving further learning *how*, and *that*). Learning *that*, *how*, *why*, exhibit differing degrees of dependence and/or independence from case to case.

1.4 A MINIMAL WORKING DEFINITION OF *LEARNING THAT*

Given the above four different constructions in which the word 'learn' can occur (there may be more), we now investigate whether there can be a minimal working definition of learning that encompasses them all. In philosophy of education there is a voluminous literature on empirical theories of learning such as behaviourism, gestalt psychology, cognitive psychology, Piaget's theory, constructivism, and so on. These are important; but our concerns in this book are not directly empirical, though something will be

briefly said of a few of these theories in later sections. Much harder to find in the literature is a broad working definition of what learning is. In what follows we consider a few suggestions and make one of our own in the form of a 'minimal working definition', which will be followed by a number of qualifications and explanatory remarks.

A clear conception of learning does not always emerge from educational literature. Thus we find that 'learning is a process of enculturating that is supported in part through social interaction and the circulation of narrative' (Brown et. al. 1998, p. 27). If learning is a process of enculturation, then it will not necessarily, or always, involve critical inquiry in the sense we use that notion. But the definition is too narrow in ruling out matters other than "narrative" and ruling out self-learning. Another definition comes from an influential text:

> Learning is a relatively permanent change in behaviour or in behavioural potentiality that results from experience and cannot be attributed to temporary body states such as those induced by illness, fatigue, or drugs (Hergenhahn and Olson 1993, p. 7)

There are good and bad features of this rather behaviourist definition. The emphasis here is on learning through experience as the cause of behavioural change. But not all experience is to be admitted. The authors, correctly, wish to resist the idea that all learning is due to reinforcement, though some may well be. Also the last clause correctly rules out behaviour caused in the wrong way by experience, such as that due to illness or fatigue. (Though it is unclear, perhaps it rules out the sort experiences had by Winston Smith caused under duress by his interrogator O'Brien in *Nineteen Eighty-Four* that lead him to learn to love Big Brother). The behavioural change is meant to be relatively permanent, the use of the world 'potentiality' allowing that the behavioural change due to experience may not be immediate and only bring about its effect after a while.

On the bad side, the definition is restricted to behaviour and changes in it. It is too narrow in that nothing is said about the learning *how*, *learning why* or *learning that*. It is also important to note that the beliefs we learn *that* can also be causes of our behaviour; beliefs are the things we use to "steer" through life, and so they are often the determinants of our behaviour, along with the desires that we either have innately or have learned. This reveals that the definition is too narrow in another way: it says nothing of the learning of desires and wants, or about the learning of values (though the learning of moral values, as opposed to epistemic values, is outside the scope of this book). It is also too narrow in omitting cognitive factors such as reasons and reasoning as a cause of behaviour.

A positive feature of the definition is that learning is at least a change in some aspect of a person.[8] This idea is at the core of a different definition:

> whatever is learned in the course of education or related enterprises could hardly be other than a matter of the *acquisition* of skills, capacities, dispositions or qualities not

> previously possessed – although it may also be a matter of the development of already given (innate) qualities or potentialities. (Carr 2003, p. 4)

This definition of learning is broad enough to encompass learning subjects other than humans, as it should. Thus other biological species such as rats, mice or octopi, can be subjects of learning in that they acquire new characteristics through learning processes (often by operant conditioning). And it also encompasses learning *how*, *why* and *that*, especially in the case of humans as learners. But it says nothing about the manner whereby the changes in characteristics are acquired. Would any mode of acquisition do? We will return to this important matter later.

We propose a minimal working definition of *learning that* which is a species of the previous, generic definition. If 'p' stands for any propositional content such as that the Earth orbits the Sun, or that 2+3=8, etc, then:

> *A person learns that p* = the person formerly did not believe that p, but *via* some process of acquisition, comes to have the belief that p.

In the case in which learning something also involves unlearning something else, and assuming that people generally do not consciously entertain contradictions we need to add: if, in the course of coming to believe that p, A discovers that they hold a contrary belief, that q, then they also unlearn in abandoning their belief that q.

We say that this is a working definition since it will be expanded upon or qualified in five ways indicated below. We do not intend the definition to be *stipulative*. Rather its is intended to be at least minimally *descriptively* adequate of what counts as learning in that it indicates a criterion for most (if not all) cases of learning *that*. Later we will consider a definition of learning that is not merely descriptive but normative in that it lays down what learning *ought* to be.[9] Five comments qualify the minimal definition.

(1) The first comment on the definition is it applies to any proposition whatever, whether it be from everyday life, science, mathematics, logic, theology, morality, politics, etc. (2) One may ask if the notion of acceptance could replace that of belief in the definition, so that learning is coming to accept what one did not formerly accept. As an illustration, a pupil might come to accept the theory of evolution (or the Bible story of creation) for the purposes of passing an examination on the topic; but they do not believe it. The difference between acceptance and belief was indicated in Section 1.2. However it is not necessary to modify the working definition of learning to accommodate this case, as long as it is recognised that there can be different, but related, "propositional objects" that are learned. At the end of the previous section we introduced a distinction between learning *that an authority says that p*, and learning *that p*. Here there are different "objects" learned, viz., *learning that an authority says that p* (they get this right as an examination indicates), versus *learning that p*. Now both kinds of learning

can take place, and both equally well fit the definition since, as far as the outcomes in the examination are concerned, they are much the same. But if what a person wants in leaning is mastery of a subject matter, and not merely the display of mastery, then they want learning of the second sort and not the first. This touches on the process whereby the learning takes place (see point (5) below).

(3) The proposition p can be true or false, or the belief that p may be a reasonable or unreasonable belief to acquire, or it may be a belief acquired on no good epistemic grounds whatever. In fact the belief learned could have either good epistemic credentials, or none at all, or quite bad ones.

The above establishes our thesis about the independence of learning from critical inquiry (where this centrally involves appeal to truth and rationality).[10] It can be expressed schematically as follows: For any proposition p about any subject matter, that person A learns that p does not imply any of: (a) p is true, or false; (b) it is rational, or irrational, to believe that p; (c) p has good or bad epistemic credentials; (d) person A uses the epistemic credentials of p as the means whereby A learns that p. Of course one can learn using methods of critical inquiry that provide the epistemic credentials of p. But this does not mean that critical inquiry is constitutive of learning; at best it is an extra "add-on", perhaps as an aim that any learning or education *ought* to have. Shortly we will look at a less minimal conception of learning that does have requirements of proper inquiry built into it.

(4) Nothing is said about how long one must retain the learned belief. It cannot be a fleeting belief, but must be something that we retain over a longish period. Since beliefs are dispositions, then these can last a while.

(5) The final point addresses the problem of what processes are involved in belief acquisition; these have been left wide open in the working definition. The processes of belief acquisition that comprise learning can range from behavioural conditioning and enculturation to the autonomous use of reasoning. What is to count as learning is a conceptual matter, and not an empirical matter to determine. Thus we should not rule out cases of rote learning *that*, as in the case of addition and multiplication tables, the vocabulary of a foreign language, a part in the script of a play, or even remembering of vast amounts of some sacred text of a religious society by constant repetition. But there can be cases of belief acquisition that do not count as learning. If a belief were put into A's head while in a hypnotic trance, or A got it through techniques of "subliminal advertising" (assuming these do work), or the belief got lodged in A's brain as result of trauma or drugs, then these would not count as learning processes, though they are processes of belief acquisition.

Commonly the learning process requires the intervention of others, such as teachers. They attempt to get pupils to not only learn *that* but also learn

how to, learn *to*, learn *how*, and learn *why*. Often all these kinds of learning accompany one another. Teachers can intervene in the learning process in a large number of ways. At the undesirable end of the scale would be learning by conditioning or by threats, or by rather blunt didactic methods, and so on. As Siegel puts the matter when he speaks of the inculcation of beliefs: 'We can inculcate beliefs in many different ways – e.g., by torture, brainwashing, lying, conditioning, propaganda, manipulation, indoctrination, peer-pressure etc.' (Siegel 1997, p. 47). In George Orwell's *Nineteen Eighty-Four* O'Brien, the torturing jailer of Winston Smith from the Ministry of Love, gets Smith to agree that the past could literally be altered, or that 2+2=5. And when he holds up the back of his hand with the thumb concealed he gets Smith to believe that he sees five fingers, and not four. O'Brien asks 'Which do you wish: to persuade me that you see five, or really to see them?' (Orwell 1987, p. 263). Smith eventually learns to see five fingers being presented, and so believes this. In this way he also *learns to* love Big Brother. Other kinds of learning processes can make the learner a more willing accomplice, but are nonetheless equally unsatisfactory modes of belief uptake. Thus one may learn from one's guru because one is a faithful follower of the guru's sayings. One can also learn from one's favourite pop-singer, or one's preacher, or one's favourite radio or TV commentator, and so on. All of these are further examples of a processes involved in minimal learning in which what is paramount is belief uptake by whatever process of inculcation.

At the more desirable end of the scale are teaching intervention processes that assist pupils over the difficult hurdles of learning they face, and do this in ways that excite pupil interest and enhance the belief and skill acquisition process. But there is nothing in such processes that guarantee that, in the learning *that,* or learning *why*, etc, what is learned is true, correct or rational and reasonable. People have, in the best of learning conditions, learned *that*, and learned *why*, where what is learned is now deemed to be false or unreasonable or ill-founded. All that is required for the minimal account of the learning process given above to be successful is that there be some process in which there is an uptake of beliefs and/or skills, however the process takes place (but with wayward casual processes being excluded).

In this book we wish to focus on learning processes that have at their centre what we will call "processes of critical inquiry". Critical inquiry, we claim, is a core aim of education, and thus of learning. But it is not merely an aim. It is also one of the processes whereby we can learn through acquiring new beliefs and discarding old beliefs. Further, and quite crucially, it is the application of critical inquiry that provides the grounds for the rational foundation for belief, yielding for us degrees of *rational* belief, rather than mere belief; ultimately it transmutes belief into knowledge. Spelling out the multifarious character of critical inquiry and its links to rational belief and

knowledge is the central topic of Parts I and II of this book. It is also the reason why, so far in this chapter, we focus only on the role of belief in learning (as spelled out in our minimal working definition). From this point on more will be said about the processes of critical inquiry in the formation of rational belief and knowledge.

Granted a notion of critical inquiry we can now propose a normatively laden conception of learning. In the working definition of minimal learning *that*, it was left wide open as to what the process of belief acquisition may be. The normative notion of learning restricts the processes of learning to those used in critical inquiry. Thus in so far as the various kinds of learning *how to*, learning *why*, etc, involve learning *that*, our normative notion of learning becomes learning carried out in the light of critical inquiry. As we have made amply clear, the norms of critical inquiry are not intrinsic to learning and have to be added on to obtain a normative conception of learning that takes us down the path to rational belief and knowledge. As we will make clear, no means of belief acquisition other than those of critical inquiry will take us down this particular path to rational belief or knowledge.

1.5 SOME CONNECTIONS BETWEEN LEARNING AND CRITICAL INQUIRY

In the previous section we set out a minimal definition of learning. In this section we explore just two of the many empirical theories of learning that have been proposed in psychology and education. Our task will be to discover whether or not a notion of critical inquiry is built into these theories. The two to be investigated are a version of Skinnerian behaviourism and Piaget's theory of learning. Out verdict will be that critical inquiry is not part of these theories. This supports our contention that the minimal definition of learning, as well as some theories of learning, entails nothing about critical inquiry. Of course it is open to educationalists to propose a theory of learning that has conceptually associated with it the notion of critical inquiry. Some advocates of strongly cognitive theories of learning might do this. But on the whole the norms of inquiry remain outside the scope of most empirical theories of learning. This is not on objection to these theories. But it does show that the scope of these theories is more narrowly confined than is evident.

1.5.1 Skinner's Behaviourism, Learning and Critical Inquiry

Though there is a vast literature on behaviourism and learning, we will look at only a minute portion of it in relation to learning and critical inquiry. Skinner is blunt about what is wrong with learning:

> Everyone has suffered, and unfortunately is continuing to suffer, from mentalistic notions of learning in education. It is a field in which the goal seems to be obviously

> a matter of changing minds attitudes, feelings, motives, and so on …. Yet the point of education can be stated in behavioural terms: a teacher arranges contingencies under which the student acquires behaviour that will be useful to him under other contingencies later on. The instructional contingencies must be contrived; there is no way out of this. (Skinner 1974 p. 184)

There is much to be said in favour of Skinner's positive appeal that teachers can make to the contingencies of reinforcement in the classroom – and much against it. Also the conception of learning is couched only in terms of behaviour. This is a limitation already criticized in a similar definition mentioned at the beginning of Section 1.4 which is also too narrowly focused and makes no mention of belief (a notion that a strict behaviourist might regard with suspicion). But these matters are not our concern here. Our concern is with what link there may be, if any, between the theory of behavioural learning briefly sketched above and central aspects of critical inquiry such as truth and rationality. Our view is that there is none. This is not an objection to Skinner's theory. But what it does show is that such behavioural learning cannot be part of any norm-laden theory of learning that is conducted according to the norms of inquiry.

Suppose in the quotation above we restrict the range of behaviours to those of verbal behaviour, such as uttering the English words 'the Earth does not move' (in given conditions of stimulation), that is standardly exhibited by those who possess the underlying disposition which is *having the belief that the Earth does not move*. The members of the Inquisition who interviewed Galileo had that disposition, while Galileo had the different disposition to utter the negation 'the Earth does move'. Now pupil learning will occur when, as is said above, the teacher arranges contingencies under which the pupil acquires the verbal behaviour to utter 'the Earth does not move'; and such learned behaviour will be useful to him or her under later contingencies in which uttering 'the Earth does not move' is required. It will be useful to pupils when they, say, talk to the Inquisition, or sit their examinations and try to pass (given that their teachers will mark the exam). But such behaviours will put them seriously at odds when it comes to talking with Galileo about the matter, or most subsequent scientists. In this case they need to have their behaviour modified under new conditions of teaching reinforcement, if they are to get on under these new conditions with their new regimes of reinforcement. And, of course, such has been the case when different verbal behaviours have been reinforced by teaching as science itself changes.

Now it is clear that, from such a behaviourist theory of learning, nothing follows about the verbal dispositions which are being reinforced having to reflect beliefs which are true rather than false, rational rather than irrational, or provide correct as opposed to incorrect explanations why, and so on. The Skinnerian account of learning supports our view that, on the whole, learning is independent of the notions we associate with critical inquiry, such as truth

and rationality. This does not mean that we endorse the Skinnerian theory; this would be an unsound inference. Rather it is a view of learning which is consonant with the independence claim. The requirements of critical inquiry will, for the Skinnerian behaviourist, have to be a contingent "add-on" to learning. Or else they are just more of the regime of reinforcement. But, of course, to claim this is to miss the normative force of the principles of critical inquiry when we apply them to our beliefs.

It might be objected that the strict behaviourist might not acknowledge such an "add-on" since they might not be able to give a behavioural account of it, or find a behavioural need for it. Even for Skinner all talk of "knowledge" is simply more in the way of verbal behaviour, though there is an appeal to correctness that does take us in the direction of one of the aspects of critical inquiry. Thus Skinner says of knowledge: 'The entity which is traditionally said to be maximized by education is called "knowledge" (Skinner 1953, p. 408). He goes on to say of some knower: 'He "knows the capital of Peru" in the sense that he will correctly answer when ask what the capital is or will make statements about the capital in discussing Peru, and so on' (*loc. cit*.). Once an account of knowledge has been set out (see chapters 2 and 3) it will be seen how lame Skinner's notion of knowledge is. For the sake of argument let us accept Skinner's view that evidence for knowledge claims is to be exhibited in verbal behaviour. But now note that Skinner has added the word 'correctly'. This must be added in as part of the evidence; without the requirement of truth, then there is no reason not to accept any old answer. What is needed for knowledge is at least the correct answer. So an appeal is made to correctness, truth and the like, all of which are aspects of critical inquiry. But this only arises if one wishes education to maximize knowledge (which in turn requires correctness), rather than just expand one's beliefs. This is clearly a contingent "add-on" to the behaviourist theory of learning and is not essential to it.

1.5.2 Piaget's Theory of Learning and Critical Inquiry

This sub-section attempts to answer the following questions. Is Piaget's theory of learning independent of critical inquiry? Or does it contain some special aspects on the basis of which it does entail something about critical inquiry? We discuss Piaget because of the significance he has for constructivism in science education, a doctrine aspects of which will be discussed elsewhere in this book. What follows is a brief outline of his theory which hopefully does justice to his complex position and which shows that an answer to either question is not straightforward and depends on how one understands his theory. Overall the first question gets a positive answer and the second a negative. And this accords with the view of learning developed in this chapter.

All people are born with certain innate dispositions to behave in certain ways such as to look, reach out, grasp, suck, etc. The ability to do this is due to certain cognitive structures in each of us. To use Piaget's technical term, each of these cognitive structures is a *schema*. In the plural we possess many such *schemata* at the beginning of our lives, and develop others through learning as we grow. Thus reaching out is not something which arises as behaviour due to a conditioned stimulus alone; rather we all possess from the beginning sufficient cognitive structure, the "reaching out schema", which makes reaching out possible for us. Each individual responds to their environment in accordance with one or other of these schemata; this is called the process of *assimilation*. Thus if the child has only the schemata of looking, reaching out, grasping and sucking, then the child will *assimilate* their environment in accordance with these schemata only, viz., their current cognitive structures. Moreover, they will have to deploy their schemata in a particular order; that is, they look before reaching out or performing grasping actions and, on the whole, not in the reverse order. Not to do so will lead to failure in assimilation. If they do not already have a schema which enables them to deploy their other schema in that order, then they need to acquire one.

Piaget uses the term *accommodation* for cases where a child (or more generally any organism) has to modify a schema in order for assimilation to take place, or develop a new schema (cognitive structure). Without an ability to accommodate, a child would simply continue to assimilate the world according to their schemata without changing in any way their mode of assimilation. Accommodation enables them to learn through the modification of already given cognitive structures, or the development of new ones (the brain presumably having the capacity for such development of schemata due to the child's encounter with the environment). Thus a child, when presented with a rattle will assimilate it to the general grasping schema. But if it has not grasped a rattle before, then it has to accommodate its cognitive structures to grasping it in a particular way, such as arranging fingers and thumb appropriately, grasping by the handle and not the spherical rattle-part, and so on.

Suppose the child finds that the rattle makes a noise when it is moved. The child then engages in new rattle-shaking activity, again through a process of assimilation to old schemata and accommodation to modified or new schemata. But note that the child has first to learn that it is *its* moving the rattle that makes the noise, and it will have to do this through other assimilations and accommodations. Though assimilation and accommodation are intimately related, it is the activity of accommodation in which cognitive structures, or schemata, are modified, or built anew, that underlies the process of learning. To illustrate again, a child given a spoon will assimilate it to the

grasping schema, but not the shaking and rattling schema of the rattle. But it does discover two new accommodations it can make to other schema, such as banging on the table (the rattle will do this too); but in contrast it can scoop things up, and enable self-feeding, neither of which the rattle will do.

What are important in these processes are those aspects of the environment that cannot be assimilated to prevailing schemata (cognitive structures). Thus the spoon does not rattle, or the rattle does not scoop things up. In this context there is a lack of harmony between schemata and the environment, or a lack of *equilibrium*, in that aspects of the environment cannot be assimilated. Here Piaget speaks of an innate drive of *equilibration* to remove disharmony and lack of equilibrium. (Sometimes he speaks of self-regulation.) The role of accommodation is to provide new cognitive structures that will restore harmony and equilibrium; they enable the individual to "cope" with their environment when prior to accommodation they could not. In effect the combination of accommodation along with the drive for equilibration simply stands for the adaptive function of newly built cognitive structures in restoring equilibration between cognitive structure and world. It is these notions that are directly employed in Piaget's theory of development in children, and in particular his theory of learning.

Given this brief sketch, what can we say about a possible connection between Piagetian learning and critical inquiry? The model above is quite general; it can apply to any biological organism learning to adapt to the world. But this need not, and often cannot, be assimilated to rational learning in the light of critical inquiry for such organisms. A purely causal story of constant feedback between disequilibrium, accommodation and restoration of a new equilibrium can do just as well. And it might also do just as well for infants who are not, as yet, users of critical inquiry. Looked at this way, there is no connection between Piagetian learning and critical inquiry; the two are independent in the sense that learning and restoration of well-adaptedness can take place in the absence of any use of critical inquiry.

However there is a sense in which Piaget's model of learning has features in common with a model of critical inquiry, that of trial and error. But again talk of trial and error need not be understood in terms of the application of critical inquiry to hypotheses; it may merely be complex causal feedback mechanisms at work finding ways of restoring well-adaptedness. Along the way to achieving this, the organism may try a number of different accommodations, some of which do not work – but eventually one does work. Here the processes of accommodation must be given time to sort through possible solutions in order to arrive at the right kind of cognitive structure to do the needed job of overcoming disequilibrium and allowing assimilation to reign once more. In the case of evolutionary processes there may not be time for such sorting to be done. Those organisms that

accommodate by hitting on the right solution in building the right cognitive structure first, or fairly quickly, are the one's to survive; other builders are too slow and die off in the process of sorting for the right cognitive structures – or they die off never finding them because there are no solutions for them.

Talk of a trial and error model might lead one to draw an analogy between this understanding of Piaget's learning model and the kind of simple model of scientific investigation often attributed to Popper (and others as well). This is the model of "conjectures and refutations" in which hypotheses are proposed, then are critically examined for their faults which do eventually appear, and finally they are refuted and replaced by new conjectures which are then, in turn, subjected to the process of critical examination …, and so the process goes on through a sequence of conjectured theories. Note that this is analogy only. We need to understand Piaget's more general talk of disharmony or lack of equilibration more specifically as the application of some theory in some context, and the discovery that it is inconsistent with a range of observations and/or experimental results. Also we have to see the inventing of a theory as, somehow, an accommodation in which new cognitive structures are built.

Crucially, what analogy is there between Piaget's model and the process of critical inquiry to which Popper says each conjecture must be subject? It is not clear, but perhaps one could appeal to all those assimilations which the Piagetian cognitive structures enable, and then note the failure to assimilate – and it is this that provide the drive for restoration of equilibrium. However this is analogy only and bears no strong resemblance to the methodological procedures advocated by Popper whereby critical evaluation of theories is to take place.

Popper himself does draw connections between a general activity of problem solving and his own theory of scientific method; but he also notes differences (Popper 1999, chapter 1). Thus he recognizes that the following three-stage model does have wide application in biological evolution:

(1) problem → (2) attempted solution → (3) elimination.

Item (1) is akin to the emergence of something that fails to be assimilated, or in evolution is an environmental change for which adaptation is needed for survival. Item (2) is the emergence of possible solutions to the problem. Item (3) is the detection and elimination of mistaken solutions, of attempted solutions which in fact fail to be satisfactory solutions. In contrast, successful solutions are learned and are part of the on-going flourishing of the problem-solver.

But for Popper there is an important difference between any organism to which the above three-stage model applies and the application by a scientist of a similar, though crucially different, model. On his view the progress of science as a four-stage "conjecture and refutation" model with the important

addition of the conscious application of a critical method for conducting inquiry (*ibid*., pp. 7-9 and p. 14):

> (1) an old problem → (2) formation of tentative theories → (3) attempts at elimination by the use of critical inquiry → (4) new problems that arise through critical inquiry.

Let us set aside an issue in the context of invention, *viz*., how we come up with tentative theories at stage (2) in the first place. The focus is on stage (3) and the context of justification. We are given some tentative theories and then, using methods of critical inquiry, show which is to be eliminated and which, if any, passes muster (for the time being). For Popper there are some similarities between the three-stage model applicable in biological evolution, and the four-stage model applicable to scientific change. But there are several differences as well, a crucial difference being the role played at stage (3) by the application of the canons of critical inquiry specific to science. In fact, since Popper thinks that "all life is problem-solving" the three-stage model will have a place in any account of the history of the evolution of species. But the four-stage model has a crucial role for problem solving for not just each individual life, but also our social lives as we attempt to find solutions to our individual and social problems.

How do these two models relate to Piaget's model of learning? At best only the three-stage model is close. But this needs no role for critical inquiry. Learning can take place using purely causal feedback models of selection for the successful solution to a problem. Piaget's model is quite distant from Popper's four-stage model in which the application of principles of critical inquiry plays a central role. Perhaps one might consider that humans, as they grow and develop, "internalise" something akin to the four-stage model. Then a case for claiming that Piagetian learning is like Popper's model of critical inquiry might be possible. But this is a long shot. There is no good reason to suppose that we are so rational as to have internalised such a model. Moreover, there is much evidence against it from the investigations in cognitive psychology as to how well we do in fact reason in our problem situations that involve making refutations. (See Manktelow and Over 1990, especially chapter 9 on how badly we tend to perform on the Wason's selection task; this crucially shows that we are not good at recognizing refutation conditions, See also Sternberg 1999, Chapter 12).

In fact there are grounds for claiming that a Piagetian model of learning need not take us to the true and the rational. It is quite possible that such an account of learning *how to* (say, use an invalid rule of inference), or *why* (say, the planets move as they do) or *that* (say, the Sun orbits the Earth), can be realized in the Piagetian model. Thus consider learning, say, the incorrect rule known as the Gambler's Fallacy. This might be learned by Piagetian means as an accommodation which restores equilibria if one applies the rule

after a streak of bad luck – and it turns out that on the next bet one has a big win. The rule is in fact incorrect; it is not a reliable guide to its conclusions. But a cognitive agent might mistakenly believe that it is correct given the lucky circumstance in which the cogniser's equilibration in life was restored. Nothing in the learning process requires the application of correct, as opposed to incorrect, modes of inference; nor does it require the learning of correct, as opposed to incorrect, rules. Thus the case for the view that Piagetian learning will take us, *ipso facto*, to the true and the rational, is not made.

This is not intended to be a criticism of Piaget's theory. Rather it serves to underline the point made in this chapter that in general (but not always) learning *that,* or *how* or *why,* is independent of critical inquiry; and Piaget's theory about learning is no exception. Only in the remote case in which a Piagetian learner had taken on board, and unconsciously uses, something like a Popperian theory of critical inquiry would this be so. But even Popper does not generally hold with this since he talks of our *conscious* (and not internalised or largely non-conscious) application of the canons of critical inquiry in scientific investigation. Of course, we all can consciously apply the canons of critical inquiry successfully in some circumstance; but this does not necessarily make us Piagetian learners.

1.6 EDUCATION AND ITS RELATION TO CRITICAL INQUIRY

In Section 1.3 we separated the different kinds of learning *that*, learning *wh*y, learning *how*, etc. As important as it is to recognize that these are conceptually distinct, in actual learning situations they often come together. Thus in learning *that* p (say, that 17 is a prime number), we may have to exercise what skill we have obtained in learning *how to* prove this*,* and apply our understanding obtained through learning *why* (by giving a proof that 17 is prime).

Though it is unclear exactly what education involves, learning must have something to do with it; for the idea that some education has taken place but no learning has occurred seems to be incoherent or contradictory. Bringing these points together yields our minimal working definition of education:

> Education is the coordinated process, typically but not always involving teachers, in which the goal is to bring about pupil learning (including learning *that*, learning *how to*, learning *why*, etc.) and the processes has a successful outcome.

Some comments need to be made on this non-normative conception of education before we see what needs to be added to produce a norm-laden conception of education.

First, teachers need not always be involved in education, as there are always autodidacts, or the self-educated, who conduct their own processes of

learning. However, we will normally assume that teachers are involved in the education process, and to some extent control, influence or shape it in accordance with whatever educational process they employ.

Second, there is no limit to the range of subject matters about which one may receive an education: algebra, etiquette, hang-gliding, safe-cracking, brain surgery, drumming, etc. In this book the object of education is the many facets of science; so some of the points made about this will not always carry over to other domains of education. Education in science will involve learning *how to*, say, perform mathematical calculations, perform various kinds of experiments, apply various theories, and so on. It will also involve learning *why*, say, bodies fall as they do, chemicals combine as they do, and so on. Also pupils learn *what* explanations science currently provides of various phenomena; and in so doing pupils increase their understanding within science. Finally pupils also learn *that*, for example, learn *that* the sequence 1/2, 1/4, 1/8 … $1/2^n$ converges on 1. But they may only learn *that* this is so by proving it, that is, by using the skills they have acquired through some specific mathematical learning *how to* (viz., learning *how to* construct a proof which shows that the sequence converges on 1).

Third, the minimal working definition of education leaves open the range of processes involved in learning, some of which have been mentioned in previous sections. The main process to be discussed in this book is the use of critical inquiry in education. But as we have argued, unless we build critical inquiry into the concept of learning by legislation, there will be no essential link between education and critical inquiry with its notions of truth and rationality. So the minimal working definition of education is not conceptually or logically linked to critical inquiry, and is without any normative force.

At the end of Section 1.4, a normative notion of learning was introduced in which, through the application of principles of critical inquiry, one advances from mere belief to rational belief or to knowledge. In the same way we claim that there is a norm-laden concept of education, as well as another that is descriptive only and is given in the working definition. There are not two words in English that mark the distinction; one word does double duty. Education is a broadly descriptive notion when it is linked to any processes whereby one learns. In contrast education becomes normative when all the processes of learning are conducted in accord with the principles of critical inquiry that lead to knowledge (as distinct from belief). Thus one would say that a person would not have received a minimally adequate education if they could not think for themselves and apply critical inquiry to a range of matters thereby having reasons for their beliefs. And the inadequacy is not just a contingent lack; the failure is such that it takes that person out of the realm of those who count as being educated, or at best on the fringe of the educated.

There is the minimal requirement on being educated that one has mastered the proper use of some aspect of critical inquiry (in some domain) that goes beyond our mere biological inheritance as reasoners. (Of course one can meet the requirements of being educated in varying degrees according to the extent that critical inquiry has been employed; but this is not the point being made.)

On this basis we claim that there is a notion of education that is norm-laden. Education should not give us merely beliefs. It should, as Anderson requires (see the *Introduction*, Section 2), give us the grounds for belief, and/or reasons as to why each belief *ought*, or *ought not*, to be held. That is, education gives us, as the products of critical inquiry, knowledge and understanding (and much else besides, as will be seen). Without the norms of critical inquiry, learning in education is not much better than the soaking up of a sponge in which we acquire information, take on beliefs or adopt opinions. But none of this is acquiring knowledge.

In order to be critical inquirers do we have to learn what the methods of critical inquiry are? If so, this would be a form of learning *that* such-and-such are the methods of inquiry, and learning *how to* apply them. As highly rational agents we might wish to learn *why* such-and-such methods are the principles of critical inquiry. Here there is a role for philosophy in setting out some of the principles of critical inquiry found in logic, epistemology, methodology and elsewhere, and then in providing a justification for them. However, as we will see in the Socratic model of education (Section 3.3) this might be too demanding. True, there is no barrier to our being educated in the methods of critical inquiry; nearly all of the Socratic dialogues touch on this issue at some point in the debate. But for Socrates we can all carry out our critical inquiries without engaging in such high-level learning in the first place. For Socrates we all have an innate ability to reason. With this we can agree (up to a point).[11] But what we need to do in order to know, by learning through applying Socrates' critical method, that, say, the Earth orbits the Sun, is to go through a process of inquiry that leads to this claim. Not to do so, but merely to pick this up as a piece of our culture's received wisdom is not sufficient for knowledge, as will be argued in subsequent chapters. But to do this it need not be necessary to launch into a full inquiry into the methods of critical inquiry themselves. But it would be a crucial for a complete education to have engaged in aspects of such an inquiry at some point.

NOTES

[1] Conceptual change has proved difficult to analyse, even though we do talk of it readily. One problem is that it is unclear when a concept remains the same while aspects of it change, or when we have different concepts arising from some change. This is the problem of the criterion of identity for same, and different concepts. Beliefs and belief change do not have quite this problem. Another issue is that according to some we have no good theory of concepts either in cognitive psychology or in philosophy; on this see Fodor 1998 and 2003.

[2] Evidence for this is cited in Gaulin and McBurney, 2001, chapter 7, and in Pinker (2003).

[3] Most evolutionary psychologists are aware of, and are critical of, simple ways in which their claims have been cast. Most textbooks, but alas not popular presentations, are aware of the problems here. See for example the useful text Gaulin And McBurney 2001, especially chapters 1 to 3. Their views on learning are in chapter 7.

[4] On the modularity view of the mind adopted by most evolutionary psychologists, see, for example, the recent collection of papers in Carruthers and Chamberlain (eds.), 2000. Just how far the mind can be "modularized" is a matter of current dispute.

[5] See Ryle 1963, pp. 125-6 and pp. 143-7 for his reminder of an old distinction between words which indicate activities (such as running, attempting to spell, etc.) and words that indicate success or achievement (winning, spelling, etc.) or failure (losing, mis-spelling, etc.).

[6] For an account of what is wrong with this inference, see Hacking 2001, chapter 3 entitled 'The Gambler's Fallacy'.

[7] See Scheffler 1965, p. 17. Chapter 1 of Scheffler's book also considers a range of different 'learning' locutions and their relation to belief, knowledge and teaching. Thus Scheffler correctly observes that the range of learning can outstrip contexts of knowing, as when one learns to be punctual, but it does not make sense to say that they know to be punctual (though to be punctual they may have to know *what* the time is).

[8] Mowrer and Klein 2001, pp. 1-2 review five similar broad definitions of learning and then propose one of their own saying 'these definitions share the common theme that learning is a relatively permanent change in the probability of exhibiting a certain behaviour resulting from some prior experience (successful or unsuccessful)' (*ibid*., p. 2). But they had also better say that the probability goes up rather than down. Also like other behaviourist definitions it is too narrow in that it concerns only behaviour resulting from experience and not broader processes of learning directed upon items other than behaviour.

[9] The distinction between *stipulative*, *descriptive* and *normative* definitions is commonplace in most philosophical accounts of what a definition is. It is also adopted by Scheffler (1960, chapter 1), except instead of *normative* he refers to *programmatic* definitions that lay down the way things *ought* to be.

[10] The independence thesis does not get much discussion in the literature on learning. Two philosophers, however do make this point; see Scheffler 1965, chapter 1, and Siegel 1998, p. 32-3 footnote 2.

[11] The extent to which humans are naturally good reasoners has been subject to empirical investigation in cognitive psychology. We turn out to be quite good at some kinds of inference and bad at others, especially probabilistic reasoning. There is a vast literature on this topic; we cite here only a survey books, Manktelow and Over 1990, and Sternberg 1999, Chapter 12, and the classic collection by Kahneman, Slovic and Tversky (eds.) 1982. The point being made here is that if we think of Plato's theory of recollection of what has been implanted in the soul (to be discussed in chapter 3) as a case of innate knowing, then we might not be very good as innate knowers. It seems as if our biological inheritance as reasoners has been a very patchy business, but just good enough to get us this far down the evolutionary track.

CHAPTER 2

KNOWLEDGE, EDUCATION AND CRITICAL INQUIRY

'The aim of education is to … put knowledge in the place of opinion' (Anderson 1980, p. 70) – and, we can add, in place of belief, or our own "constructs". In Chapter 1 we developed an account of learning that did not necessarily involve critical inquiry and which results in belief, or opinion. We will argue here that learning processes without critical inquiry cannot result in knowledge. Our point turns on the fact that the big difference between mere belief and knowledge is that knowledge only results from the application of critical inquiry to our beliefs (a claim that we will restrict in scope in important ways subsequently, especially in the case of direct perceptual knowledge). On the whole, it is only through learning processes that essentially involve elements of critical inquiry that we can get knowledge as the outcome of the learning, especially in the case of science. The difference that philosophers wish to draw between belief and knowledge is not something that is readily appreciated in science education circles, or in education generally. And it is something entirely muddled in postmodernist, constructivist and multicultural accounts of science education.

Knowledge is not the only outcome of critical inquiry. It can also yield reasonable or rational belief, or some reasonable degree of support for our scientific hypotheses that is not as full as the support in the case of knowledge. Not all science need reach the standards for knowledge of the kind we will describe here, though much does. To mark this difference some speak of *conjectural knowledge*, but the adjective 'conjectural' tends to undermine the following noun 'knowledge'. And again some wish to speak of probable knowledge; but talk of probability again undermines the status of the knowledge claim. Quite often science employs methods which tell us which is the best of a number of rival hypotheses, either on the basis of evidence or on non-evidential grounds, such as offering a better explanation or having wider scope, etc. We will explore issues concerning scientific method more fully in Part II. There we will consider modes of critical inquiry that do not necessarily give us knowledge in the sense defined here, but nonetheless give us good grounds for rational belief in science. Probabilistic reasoning will play a big role here, something largely ignored in most accounts of scientific method found of science education.[1] In this chapter we will consider only the nature of knowledge and the modes of critical inquiry that are needed to produce it in any process of education.

What is an education that produces knowledge rather than mere belief or opinion? One of the first philosophers to ask, and answer, these questions

was Plato in his dialogue *Meno*. His answer, roughly, is that we have knowledge if and only if we have a belief which is true and for which we have a justification for its truth. Since this theory is important in its own right, this chapter will be devoted to spelling it out, along with an account of what an education that produces knowledge is like. In the next chapter we will consider how this conception of knowledge arose in the Ancient Greek philosophy of Plato, and the role it plays in Socratic inquiry and education. Both chapters will place the notion of knowledge at the core of critical inquiry and consider its implications for theories of learning and education.

We focus on these ancient sources since they contain a richness of epistemological and educational theory that has not been surpassed, and the lessons of which, in our view, need to be revisited by every generation. Plato further qualified his account of knowledge in the *Meno* in later dialogues (such as the last third of the *Theaetetus*, not discussed here). In Chapter 4 we will consider some of the more recent reasons for adopting modified versions of the traditional account. Though the conception of knowledge is qualified in further analyses, the central notions of truth and evidence are definitely not abandoned; rather they are modified to meet problems that arise in developing a fully articulated theory of knowledge that meets some quite high philosophical demands (such as meeting the challenge of scepticism). And we will discuss in Chapter 4 whether there are foundations for knowledge, such as perceptual knowledge, that do not result from critical inquiry, or whether they too can be subject to critical inquiry.

Section 2.1 begins by clearing away potential confusions due to the wide use of the word 'knowledge'. It occurs in many contexts of use such as 'knows *how to*', 'knows *why*', 'knows *that*', and so on. Once these important distinctions are made the focus of the chapter is then on 'knows *that*' and how it might be defined. Section 2.2 briefly returns to the topic of belief again and investigates some of the different constructions in which the word 'belief' occurs, in order to focus on belief *that*. It also considers the idea of degrees of belief and the rational grounds for partial, if not full, belief.

Section 2.3 discusses briefly the notion of truth. Many in science education, and education generally, have caught a bad dose of truth phobia. Such has been the unfortunate influence of a range of views from scepticism to postmodernism where a very weak conception of truth is current. Here we will openly embrace a realist notion of truth, and leave until chapter 5 one of the main reasons that constructivists in science education have for rejecting it; in our view they are bad reasons, but nonetheless they have had a wide influence in philosophy and educational theory. Section 2.4 pulls the threads of the previous sections together and spells out reasons why knowledge *that* cannot be merely a matter of having a belief which is true, a point which is as old as Plato. The idea of knowledge as *justified* true belief is introduced and illustrated with examples that can serve as model in an educational context.

Section 2.5 investigates the notion of knowing *why*, that is, of explanation and understanding. At this point three important aims intrinsic to inquiry in science will have been introduced in the discussion of knowledge, viz., truth, knowledge and explanation. Section 2.6 considers knowledge as an aim of critical inquiry in relation to normative conceptions of learning and education. The final section considers some of the accounts of knowledge current in writings on science education and highlights some of their deficiencies in the light of theory set out in this chapter.

The role we assign to critical inquiry in obtaining knowledge, or rational belief, in the context of education and learning is not widespread in science education. But there are some who adopt conclusions close to ours, but perhaps not for the same reasons. Some authors do stress the need for 'inquiry-based instruction' and say: 'Students with scientific-thinking skills go beyond learning facts or doing hands-on activity and become able to use logic and reasoning skills to develop scientific knowledge and an understanding of scientific processes' (Bruning *et. al.* 2004, pp. 350-1). They then go on to do something important that is largely outside the scope of our book, namely provide empirical considerations about how best their model of inquiry-based learning can be used in the classroom. Where we agree is over the role of critical inquiry in instruction as the way students are to obtain knowledge, rather then mere belief. Where there is a difference of emphasis is that Bruning *et. al.* see their position as evolving out of constructivism. In contrast we see our version of inquiry-based learning and teaching evolving out of the kind of theory of knowledge we set out here, its application in examples of rationally based models of inquiry such as that of Socratic learning (see Chapter 3), and the use of the history of science as a vast repository of rational inquiry that can be adapted for use in the classroom.

2.1 THE VARIOUS CONTEXTS OF THE WORD 'KNOW'

The English word 'science' comes from a Latin verb *scire*, 'to know', from which the Latin noun *scientia* was formed. So, etymologically there is a link between science and knowledge. The English word 'knowledge' comes from the Old English *cunnan*, 'to know *how to'*, or 'to be able to', i.e., to have a skill or ability. It is related to German *kennen*, 'to know', and is not unconnected to our current word 'cunning', those in the "know" being said to be "cunning" unlike those who do not know.

Here we will be proposing a theory of the nature of knowledge. The topic *theory of knowledge* is also called *epistemology*. This last term come from the Ancient Greek *episteme*, often translated as 'knowledge'. It too has its origins in the sense of a skill or ability; but it can also be translated as 'understanding', or 'science'. The Ancient Greek Presocratic philosopher Xenophanes was perhaps the first person to draw a distinction between

episteme, knowledge, in contrast to *doxa*, belief or opinion. He was also the first sceptic in claiming that we can get little knowledge and must remain in the realm of belief and opinion. Plato (428-348 BC) was one of the first to develop a theory around the difference between *episteme* and *doxa* in the course of which he laid the foundation for a more optimistic non-sceptical account of epistemology. In Chapter 3 we will look at his reasons for drawing the distinction and exploring his theory of what knowledge means in contrast to belief, or opinion. In Chapter 4 we will look at a few subsequent developments in philosophy that start with Plato's basic themes.

There are also similar themes in Buddhist philosophy roughly contemporaneous with Ancient Greek philosophy. To make the topic manageable, we will restrict the scope of the discussion to just a few major thinkers in philosophy as it developed in the West. It is important to recognise that other philosophical cultures also explore the same themes as Plato, and either do, or do not, draw in their respective languages the distinction between knowledge and belief or opinion. We will consider only the distinctions drawn in English and their philosophical background; we leave it to others of different cultures with different languages to explore just how the various epistemic concepts introduced are expressed, and whether they are expressed in the same way. We will see that English is deficient in some respects in not having in ordinary language all the distinctions that philosophers want to draw. There is no reason to suppose that the ordinary language any of us speak should reflect all the philosophical concepts and distinctions in epistemology that need to be made. In fact this is part of the problem in coming to terms with theories about the nature of knowledge.

In developing a theory about knowledge it is *not* helpful to keep using the abstract term 'knowledge'. Here we will investigate a number of grammatical constructions of the form 'A knows …' where 'A' is the name of a person. We leave it open as to whether creatures that are not human beings can be knowers. We will also set aside other locutions that do not mention persons, such as Popper's idea that there is knowledge without knowing subjects; this is a matter that would take us too far a field. Even though we might use subjectless expressions such as 'it is known that …', we will assume that there is some person somewhere who knows, even though we do not know, and we do not even know who is the knower. Thus we can, for example, say 'it is known the chemical composition of aspirin is acytel-salicylic acid' without knowing which chemist knows this. Also we can allow that there is group knowledge as when 'A' stands for groups such as Australians, astronauts, architects, algebraists, and so on. Though group knowledge will not be discussed here, similar conditions applying to groups as a whole as apply to each individual. The main point is that the locution 'A knows …' emphasises the fact that there are humans who are knowers; items of knowledge do not float about in some disembodied fashion without being

lodged in the mind of individuals, or groups of people. Consider, now, some of the different constructions.

2.1.1 Know How To ….

The blank can be filled by phrases such as 'speak Chinese', 'play the piano', 'navigate a boat', 'use a computer', 'differentiate equations', 'crack a safe', and so on. The completing phrase commonly denotes a skill or ability. The ability is usually learned, but sometimes it is innately known, as in the case of neonates who know *how to* cry and suckle, or in the case of newly born whales that know *how to* swim. Sometimes the skill has become "second nature", as in know how to walk, know how to speak one's native language, how to drive a car, etc. Knowing *how to* parallels our earlier talk of learning *how to* in Section 1.3. In fact they are closely allied to one another; in the case of the successful outcome of A undergoing a process of learning *how to*, then A will come to know *how to*. Knowing *how to* is a central aspect of science education because of its links to learning *how to* and the acquisition of skills, or the development of abilities, in connection with science, such as conducting experiments, making calculations, gathering observational data, applying theories in some new context to solve problems, and the like. In fact the *know how* of problem solving is a central aspect of all science learning as is emphasised in the empirical research outlined in Bransford *et. al.* (1999, Chapter 2). Their concern is with pupils who have developed expertise, and how they acquire it. What is important for our purposes is their emphasis on the role of 'well-organised knowledge' (*op. cit.* p. 36-7), a matter we canvas here concerning the different kinds of knowledge, including *know why* discussed in Section 2.5.4. *Knowing how to* can go by different names, cognitive psychologists often referring to it as *procedural knowledge*.

2.1.2 Knowledge by Acquaintance

In the phrase '*A knows …'* the blank is filled by a name of an object or a description of some item, with which a person is acquainted through direct experience. Thus we have the locutions 'A knows the Pope', (i.e., A can pick him out in a line-up), 'A knows the Prime Minister' (is a friend), 'A knows the way home', 'A knows a fossil (as distinct from a rock)', etc. In all these cases either A is acquainted with the object; or A can recognise, identify, or classify some item correctly. This is commonly called *knowledge by acquaintance*. In French such constructions are indicated by *connâitre* and in German by *kennen*. However, in English knowledge by acquaintance is described using the same word 'know' that is used in a host of other contexts. Hence some consternation for English speakers learning French or German in mastering a distinction in these languages not marked by a special word in English.

Much of our observational knowledge is based in direct acquaintance through experience. Thus we are directly acquainted with the shapes, colours and textures which objects external to us possess; and we are directly acquainted with, and thus know, items internal to us such as pains and tickles. Being able to properly identify, or classify, such items may, however, take us beyond mere experiential acquaintance. Thus a person walking through a New Zealand forest will be directly acquainted with all the trees, through direct visual experience, or a person may hear all the sounds of an orchestra (assuming their visual or auditory systems are working well). But they may not know how to name, or classify what they see, or hear. On a bush walk a local may be able to say of some trees 'that is a Kauri', 'this is a Totara', and so on, but the foreigner will be unable to do this. And the conductor of an orchestra will be able to say 'that is a violin' but 'this is a viola', or 'that an oboe' but 'this a clarinet', and so on, while a person who cannot so classify correctly will fail to make the right classifications. To such kinds of classificatory knowledge many of us may remain strangers, but for everyday needs of classification we may get on quite well. The difference here is all-important. We can all have experiences of trees, or musical sounds; but it does not follow that we have correct beliefs about our experience, or alternatively can apply concepts correctly to what we experience. This difference is indicated in philosophy by talk of sensation *versus* perception. There is the "raw" input of sensation; but there is the application of concepts by us that leads to perception (a conceptual activity). The word 'experience' is often ambiguous between these. The next kind of knowledge *that*, as opposed to knowledge by acquaintance, essentially involves the application of concepts through our ability to express beliefs.

2.1.3 Knows That ...

In the above the blank is to be filled by a complete sentence. Thus: 'A knows *that* the Earth rotates on its axis', 'A knows *that* aspirin relieves headaches', 'A knows *that* absence makes the heart grow fonder', etc. This kind of knowledge claim is commonly called cognitive or propositional knowledge because it involves a complete thought, or belief with propositional content. Cognitive psychologists often call this *declarative knowledge*. A further more detailed investigation, and definition, of this central kind of knowledge is given in Section 2.4. (Note that in French this kind of knowledge is usually described as *savoir*, and in German as *wissen*. Here again English makes use of the omnibus word 'know'.)

Once various constructions for 'know' are recognised, a question arises as to what connection, if any, there may be between them. Thus, how much know *that* is involved in know *how to*? There is no single answer here. Clearly knowing *how to* ride a bicycle involves a minimum amount of know *that*, such as 'A knows that this is a pedal and must be pushed by the foot',

etc. But much of the skill in riding and remaining upright cannot easily be put into explicit know *that*. However other kinds of know *how to* do involve much know *that*. For example in knowing *how to* solve cubic equations or speak Turkish, much know *that* is required as background knowledge of mathematics, or knowledge of the meanings of words, and to what items they can be correctly applied.

2.1.4 Knows Why ...

Commonly such knowledge involves giving an explanation or providing understanding. Thus: 'A knows *why* the Earth rotates on its axis', 'A knows *why* aspirin relieves headaches', etc. Note that the examples given in (3) can hold while the corresponding examples in (4) need not. Most of us allege we know *that* aspirin relieves headaches, but most of us do not know *why* aspirin relieves headaches. Again some of us may know *how* to navigate a boat successfully, or know *how* to do long division; but we may not know *why* doing what we do (using certain navigation techniques, making certain arithmetical moves) leads to the successful outcome. In Section 1.3.3 we explored some of the links between knows *why* and learn *why*, that is, learning the currently accepted explanation. Learning *why* is to be allied to the best of the explanations that our methods of pure critical inquiry can produce. Further discussion of *knows why* is left to Section 2.5 where we investigate some of the characteristics of adequate explanation and understanding.

2.1.5 Knows How ...

Examples of filling in the blank are : 'A *knows how* aspirin relieves headaches, 'A *knows how* the heart pumps blood', 'A *knows how* electric meters work', etc. Such *know how* involves providing an explanation and/or displaying understanding; but this need not be the same kind of explanation as in A knows *why*. What is the difference between ‘A knows *why* the heart pumps blood’ and ‘A knows *how* the heart pumps blood’? The latter is answered by an appeal to the mechanisms in the heart, or surrounding body, which enable the heart to pump blood. The former asks for a different kind of explanation that has teleological overtones; an answer might appeal to the way in which the heart developed to do the things it does. Again we can ask ‘how does a thermometer work?’ citing the expansion and contraction of its contained mercury amongst other things. But an answer to the question ‘why does a thermometer work?’ can be answered by appealing to its design, or to the laws that govern differential thermal expansion and contraction. But in the case of aspirin relieving headaches, the explanations of *how* and *why* seem to be much the same.

2.1.6 Knows What ...

Here the blank can be filled by phrases such as 'a cube is', 'the Greek letter π stands for in mathematics', or 'is the chemical composition of aspirin'. The locution 'know *what*' in English is not always a separate kind of locution; it may simply be a way of conveying knowledge *that*. Thus the locution 'A knows *what* a triangle is' can be readily replaced by 'A knows *that* a triangle is a plane three-sided closed figure bounded by straight lines'. In the case of the letter 'π', its use is governed by a convention, or a stipulative definition, that can be expressed as knowledge *that*, viz., that this letter is to stand for the ratio of the diameter to the circumference of a circle. In other cases the blank can be filled in by something that spells out a nature or essence, as in the case of aspirin.

There are more locutions using 'know', such as know *which*, know *where*, know *who*, know *when*, and the like.[2] Without exploring these notions, the above shows the wide variety of "objects" of knowledge, and the hopelessness of any epistemological investigation that does not make clear what "object" of knowledge is being discussed. Bland talk of 'know' can be confusing and misleading. Talk of 'knowledge' in the abstract, as in much talk of 'indigenous knowledge', obscures the following distinct uses of 'know': A knows *how to* navigate a canoe between Pacific islands (a skill); A knows an area of the Pacific between some islands (i.e., knows by acquaintance and can identify it), A knows *that* doing-such-and-such enables one to navigate a canoe across the Pacific (cognitive knowledge); A knows *why* such-and-such navigation techniques get one across the Pacific (offer a correct explanation); A knows *what* navigation is (i.e., its nature); and so on. Talking of knowledge in the abstract obscures the ways in which we can know and the ways in which we do not know; as the above shows, one may know *how* and know *that* but not know *why* or *what*.

2.2 BELIEF AGAIN

2.2.1 Constructions Involving 'Belief'

This builds on the material in Section 1.2 on belief. We can now ask, in the light of the wide variety of "objects" that the words 'learn' and 'know' can take, whether 'believe' also admits of such a wide variety. Its use is not as wide, and only one will be the focus of attention here, namely, 'A believes *that* …' where the blank is filled by a sentence that expresses a complete propositional content. However, there is one special use of 'belief' as in 'A believes *in* …' where the blank can be filled by 'God', or 'what A's guru says' or 'humanity' or 'religion' or 'one's political party'. On closer examination most of these cases turn into a form of 'believe *that* …' as in 'A

believes *that* God exists', or 'A believes *that* religious claims express truths', and so on. But such replacements might not always capture what is intended by belief *in*. In the case of belief in God, or a political party, these do not merely reduce to belief *that*; what is missing is an extra element of commitment to conduct one's life according to God's commands, or to further the party's overall programme. So belief *in* can be a *sui generis* construction that incorporates belief *that*.

We can now pose a sharper question: what relationship is there between knowledge and belief? More specifically we want to know the following: what is the connection between 'A knows that p' and 'A believes that p' (where p can be any propositional content at all)? The connection is a one-way entailment: there is no knowing without believing. There are some cases where this might appear not to be so. A person might say that once they *merely believed*, say, that the Earth orbits the Sun annually; they got this by hearsay from others but never critically evaluated the claim for themselves. But now, having considered all the scientific evidence, they *know* that the Earth annually orbits the Sun. Here belief and knowledge appear to be exclusive. There is another way of explaining what has gone on here that preserves their compatibility. The person has studied all the relevant scientific evidence by consulting sources from Copernicus up to recent astronomy, observational and theoretical. Rather than stop believing, they have upgraded their cognitive attitude to one of knowing based on scientific inquiry. There is no exclusivity here but rather continuity of belief – except with the additional ground of reasoning based in science, this being sufficient to upgrade the belief to knowledge. As will be set out in Section 2.4, what needs to be added to mere belief *that p* to get knowledge *that p* are the requirements that p be true, and that the person possess sufficient evidence that p.

In what follows we will take believing and knowing to be states of mind in which, logically, knowing involves believing, but not conversely. There are many cases where we merely have a belief (as in the example above of the Earth's orbiting the Sun) but we do not know. Importantly, we will consider rational belief based on evidence as a step on the path towards knowledge. How this is so we will explain now, since both rational belief and knowledge are the products of critical inquiry.

2.2.2 The Idea of Degrees of Belief

A crucially important feature of belief explored here is that it comes in degrees. This is an important difference between belief and knowledge. Knowledge is an "all-or-nothing" affair. Either A knows, or A does not know; there is no halfway position. Any wavering is an indication that A does not know. Instead, A might have a rather high degree of belief, but not full belief, the wavering indicating a less than optimal degree of belief. Or it

might indicate that the evidence A has for some proposition p might not meet the standards for knowing that p (to be set out in 2.4); but it does meet the standards of a reasonably based belief. Since there can be quite different degrees of belief, philosophers often suggest the idea of a scale on which the degrees can be measured and compared. That is, each person's degree of belief can be quantified, and it is quite legitimate to talk literally of '*personal degrees* of belief' (for a person at some time, for belief in each proposition).

For any person A, it is possible in principle to set up a scale from 0 to 1 to quantify their degrees of belief. Since knowledge involves belief we could give this very special sub-class of beliefs the highest number 1 on the scale. All other beliefs would then be placed on the rest of the scale between at less than 1 down to 0. Some suggest that we ought to admit the notion of *full belief* that takes the value 1 as well.[3] If this is the case a further distinction needs to be made between *full belief* and *knowledge* even though both are accorded 1 at the top of the scale (what this distinction might be we leave until later). *Partial* belief is less than *full* belief. A person's partial degrees of belief could be quite high and range between just less than 1 down to, say, ¾. Those beliefs about which A is indifferent either way could be placed at about half; and yet other beliefs about which A is dubious might be less than half, with others placed at the bottom, 0, viz., those which A definitely rejects. This may seem to be an idealistic fiction, particularly when one might attempt to claim that A's degree of belief in some proposition is, say, 0.672. But for those who have investigated the idea of personal degrees of belief this is not all that far-fetched; they have developed a theory which all of those interested in science education should take an interest since it has become one of the leading contemporary accounts of scientific method.

As well as attempting to attach quantitative numbers in the range [0,1], they also attempt to compare degrees of belief. This can appear to be more promising. Thus given any two propositions p and q, person A might say that their personal degree of belief in p is greater than that in q, even though they cannot attach numbers to their degrees of belief. For another person B their comparative personal degrees of belief may be around the other way and they accord greater personal degree of belief to q than to p. This underlines the *personal* character of degrees of belief.

What is being measured when we talk of 'degrees of personal belief? The philosopher Frank Ramey, who first suggested that we take the idea of degrees of belief seriously, considered two ways of approaching this problem (Ramsey 1990, chapter 4 Section 3), though he often preferred to talk of *partial belief* and *full belief* (in the case of awarding 1 as the degree of belief). The first approach is one in which we take something which is perceptible to a person when they believe: 'beliefs differ in the intensity of a feeling by which they are accompanied, which might be called a belief-feeling or a feeling of conviction, and that by degree of belief we mean the

intensity of this feeling' (*ibid*., p. 65). So what we attempt to measure is the degree of the intensity of this feeling for a given belief in a person at a time. This importantly recognises that for the same person and the same belief the intensity might change over time, and that different beliefs can have quite different intensities accompanying them; also for any two people that believe the same proposition their intensities of belief can differ.

To some extent this does capture an important psychological aspect of belief. As all teachers can recognise in their pupils, they may have an absolute conviction, or be unshakeably certain, that some belief is correct, and resist all suggestions to the contrary. Alternatively, they may be only moderately committed to other beliefs such as, for example, whether or not Newton's laws of motion are correct; or they may, unfortunately, be quite indifferent. As important as the phenomenon of intensity of belief is, there are at least two problems if we use it as a way of measuring degree of belief. The first is that it is quite unclear how we might measure the intensity and assign numbers in order to set up a scale of degrees of belief. The second, as Ramsey points out, is that, from a phenomenological point of view, there are beliefs that have nothing in the way of a feeling intensity associated with them, yet these are beliefs we simply take for granted and would not abandon.

Rather than focus on an inner state such as intensity to get a measure of degrees of belief, Ramsey looked for something more external, objective and measurable. As we noted in Section 1.2, our actions are prompted by our beliefs (and also our desires which also have a belief content). It is this idea that Ramsey exploits. To illustrate, if one is standing at a bus stop, to what degree does one believe that a bus will arrive in the next five minutes? This need not be something upon which one need have an inner belief intensity. But it is still something that one steers ones life by, in Ramsey's phrase, since one still waits at the bus stop. If one really believed that a bus would not arrive, then, since one is, say, in a rush, one hails a taxi. In this case that a bus will come within five minutes is not something one believes, and so not something by which one steers one's life.

Ramsey takes the *actions* we perform on the basis of our beliefs as a way of measuring degrees of belief, and not the inner intensities that accompany them. But it is a special kind of action that he focuses on – our betting behaviour. He asks: what odds would we bet on a bus arriving in the next five minutes? We might give odds of 10 to 1, or 5 to 2, or 1 to 3, and so on; others might give different odds. In general we can say: where the odds we give for a bet on the outcome of a happening are m/n, then the degree of belief we ought to assign is m/(m+n). (Since there might be a range of odds one might be willing to bet, Ramsey took the lowest odds as the measure of degree of belief.) Thus if we wish to get at the degree of belief we have in the bus arriving in the next five minutes, and we are willing to bet on odds of

5 to 1, then the degree of belief is 5/6, which is rather high. But if we are only willing to give odds of 1 to 3, then the degree of belief is low at ¼ (so we then act differently and try to hail a taxi).

As can be seen, we still have a person-centred way of measuring degree of belief, but it is not based on a rather inaccessible inner feeling of intensity, but the more accessible overt actions we perform, such as our willing to bet on the correctness of a belief. Ramsey developed his approach in the 1920s; since then it has become one of the foundational aspects of theories of degrees of belief.[4] But much more than this was established by Ramsey that has become the cornerstone of all theories of *rational*, or *coherent*, degrees of belief. It is this that forms the foundations of a theory of rational belief (which is a step on the path to knowledge), a whole epistemology, and the basis of much contemporary theories of scientific method. It is this that we will explore more fully in Chapter 9, as part of our theory of probabilistic reasoning in science. The significance of this sub-section is to make a firm distinction between, on the one hand, a pupil's inner feeling of intensity that can accompany their beliefs, and on the other, something more objective that will give us a 'handle" on the idea of measurable degrees of belief, a prerequisite for any theory of rational degrees of belief. This latter idea is quite crucial for some accounts of probabilistic reasoning in which probabilities are just rational degrees of belief – as will be seen in Chapter 9.

2.2.3 Degrees of Support Evidence Gives to Hypotheses

In science we are often interested in the support some evidence E gives to some hypothesis H. But not only in science. Thus a doctor collects evidence E on the basis of observable symptoms and reports of medical tests; and then they consider the degree of support E gives some hypothesis H viz., that the patient has such-and-such a cancer. Or in a court of law evidence E presented and the jury has to decide which of two hypotheses is correct, the person charged is guilty (H), or the person charged is not guilty (not-H). In all these cases the evidence may not be fully conclusive. That is, the evidence E does not give us *knowledge that H* is so; nor might it give us *knowledge that* not-H is correct. We will give examples in Section 2.4.3 where evidence is sufficiently strong to give us *knowledge that*. Here we merely wish to signal that there are two important cases two consider. Evidence can be quite conclusive, and give us *knowledge that* in the sense to be defined shortly. But in other cases it might not. Evidence E might only give some degree of support for H but not fully conclusive support. Or in the case of two hypotheses it might not show that one is true and the other false; rather it supports one more than the other.

In assessing the degree of support E gives to some H, we would not want the matter to be decided merely by the inner feelings of intensity that a person has about their belief about the support E gives to H. This is

especially so in the case of a jury hearing evidence, or in science where alternative hypotheses are to be examined. We would want their belief at least to be a rationally based belief. Perhaps we would like more than this; we would like the degree of support E gives to H to be quite a quite objective matter. Are there such objective degrees of support that can be used in science, courts of law, and throughout life more generally, where evidence has to be weighed? This is a matter that we will leave until Part II, especially Chapter 9. Here we merely wish to signal the fact that belief in hypotheses in science can come in degrees, and that the degrees are not arbitrary and can be rationally based, and even objective. Importantly the degrees of support may fall short of giving us full belief that H, or even knowledge that H. In the remainder of this chapter we will focus on the case in which the support is the best possible, as this will play an important role in giving a definition of *knowledge that*.

2.3 TRUTH

This is one of the important aims of science to be discussed in Chapter 6. Our beliefs, as we argued in Section 1.2, can be either true, or false. If knowledge *that p* could also be either true or false, then there would not be much point in attempting to distinguish between *belief that* and *knowledge that*. Unlike *belief that*, truth is a necessary condition for *knowing that*: if a person A knows that p, then it follows logically that p is true. That is, we cannot know what is false; knowledge excludes error. The word 'know', as Ryle (1963, pp. 143-7) indicates, is a success or achievement or a "got it" word. It is not a word of process or of trying; nor is it a failure or "missed it" word. If p is false, then A cannot be said to know that p; at best A can only *claim* to know that p, or *think* or *suppose* they know when they do not. This is one central, but not the only, difference between *knowledge that* and *belief that* that is often over looked by many theorists' of education. Since truth is so essential, we can ask: What is truth?

> The classic answer is given by Aristotle in his Metaphysics (1011b, 25-8):
> *To say of what is that it is not, or what is not that it is, is false; while to say of what is that it is, and of what is not that it is not, is true.*

What Aristotle offers is a general schema, an instance of which is the following. Take a bit of what is, such as (the fact that) there is now ice at the South Pole; then to say *that there is now ice at the South Pole* is to say something true; but to say *that there is now no ice at the South Pole* is to say something false. For truth, *what we say* and *what is* must be in accord. Some such answer has been given by many philosophers after Aristotle right up to our own times. However many non-philosophers, especially those in theory of education, have not taken this message on board.

The correspondence theory of truth is of a piece with Aristotle's definition, though it develops it in a special way; truth is understood in a more particular way as a correspondence between, on the one hand, a proposition or what is said or a belief, and on the other hand, a fact or a state of affairs. The central task of this theory, adopted by Russell and the early Wittgenstein, is to spell out in what way the correspondence holds. However, not all theories of truth that endorse Aristotle's dictum need be correspondence theories of this sort. What such a theory primarily claims is that truth has to do with a thought-world relation. This idea is also central to the *truthmaker* theory of truth: 'The truthmaker is whatever it *is in the world* that makes a truth true' (Armstrong 1997, p. 13; see also Armstrong 2004)). The world comprises facts, states of affairs or *truthmakers*, and these make our utterances or propositions true, or false. Again, it is the task of truthmaker theory to say what it is in the world that is a truthmaker, and how our truths relate to it. Dummett[5] also expresses a version of the theory of truth we endorse, and can paraphrase as follows: "the belief/proposition/statement that p is true if and only if there is something in the world in virtue of which p is true". The 'in virtue of which' talk is deliberately neutral in that it leaves open whether it is a fact, or a state of affairs, or more generally a truthmaker, that makes that p true. It also leaves open matters concerning how the semantic relations between the bearers of truth the truthmakers is to be cashed out, correspondence being just one of these (also including the somewhat misleading construal of correspondence as copying or reflecting).

It is not clear that the above formulations provide a fully adequate definitions of the word 'true', a point famously made in the twentieth century by the logician Alfred Tarski (1956, chapter VIII) who adopted quite high standards as to what is to count as a definition. One of his central claims is that, when properly expressed, the above so-called "definitions" are more akin to schema involving the word 'true'. Such schemata, of which there are as many as there are assertoric sentences, are best understood, not as definitions, but as a necessary, or an adequacy, condition that any definition of 'truth' should meet. Thus Tarski gets us to consider a number (there are an infinity of them) of what are called 'T-schema' for any (assertoric) sentence in a language (either natural or formal) of which the following are just two examples:

(the sentence) 'snow is white' is true if and only if snow is white;

(the sentence) 'electrons are negatively charged' is true if and only if electrons are negatively charged.

[Alternatively we could say: (the proposition) that snow is white is true if and only if snow is white; and so on. Here there is a difference between saying that a sentence (token or type) is true, and that a proposition is true.

Details about what are the bearers of truth, such as sentences, propositions, etc, need not concern us here.]

For some, the (infinite) list of sentences of the T-schema is all there is required for a definition of truth. Such an infinite list is, of course, quite unlike an explicit definition of the word 'truth' which says something like the following: 'truth $=_{Defn}$ X, Y and Z' (e.g., 'vixen' = 'female fox'). Philosophers who say that all that is required is the infinite list are commonly called *minimalists* about truth, or they are said to be *deflationary* about truth. For others, and on some understandings Tarski is one of them, the T-schemata, while necessary for truth, are not sufficient. They want to add some extra correspondence, or coherentist, or pragmatist, condition. In this respect they are said to adopt an *inflationary* theory of truth. Tarski does point out, however, that the T-schemata are something that all theories of truth should capture; if any theory does not, then it cannot be an adequate theory of truth. And he also says that the T-schemata are neutral between rival metaphysical theories of truth such as the correspondence, coherence or pragmatist theories of truth. He also argues that his formulation in terms of T-schema is a more adequate, and a less formally problematic, account of truth in that it avoids semantic paradoxes about truth (a matter we need not pursue here). Tarski's approach represents a new way of dealing with problems that traditional accounts did not avoid, and has been the central pivot of contemporary debates about truth.

What should be noted is that none of the above formulations are epistemic in character; they are in fact thoroughly realist in character. They do not involve epistemic matters such as methods of test, or criteria for truth; these are quite separate matters from attempts to *define* 'true', or to say something *constitutive* about it. It is important to avoid what is known as the epistemological/ontological confusion. By distinguishing between (i) what may be true (or false), and (ii) *how*, or *whether*, or by *what test procedures*, we find out that a statement is true (or false), the confusion can be avoided. This is a distinction not always observed by those who work in the area of science education when they come to issues to do with truth. This is the crux of the difficulties with constructivist epistemology discussed in Chapter 5.

A final comment needs to be made about approximate truth, or the stronger notions of truthlikeness or verisimilitude. These notions are not epistemic; they do not reflect our uncertainty about what we believe. Rather, like truth, they are an objective relation between a proposition and the world. Truth and falsity are "all-or-nothing" notions; they either apply or do not. They do not come in degrees, unlike verisimilitude. The basic idea of verisimilitude is that a proposition can be false, yet it does relate to the world to some extent, and better than some other proposition. Thus suppose the following fact: the time is now 9 am. Now consider two claims made about the time both of which are false: 'it is now 8.55 am' and 'it is now midday'.

There is the strong intuition that even though both of these claims are false, the first claim is closer to the truth than the second claim. We say that the first claim has greater truthlikeness or verisimilitude than the second.

Can this intuition be extended to whole theories of which we say that one theory has greater truthlikeness than another theory? This Karl Popper attempted to do. However, his initial definition was beset with problems about how the comparison is to be carried out. Further research has overcome these difficulties, but is of a technical nature and will not be considered further here (for one treatment see Niiniluoto 1999, chapter 3.4 and 3.5). Thus we may say of two false theories that one has greater truthlikeness than the other. This comparison does not require us to have a full grasp of what are the truths of reality; rather it involves a comparative measure of greater truthlikeness of one theory with respect to another. Nor is it an epistemic notion that tells us something about our degree of certainty with respect to the theory. In fact we may be quite certain that a theory is false but recognise its greater truthlikeness with respect to other extant theories. Newton's theory had greater verisimilitude than those of his predecessors even though it is false. The same applies to Einstein's theory of relativity; it has greater verisimilitude than Newton's theory, but it may well still be false for all we know.

2.4 THE DEFINITION OF *KNOWS THAT*

2.4.1 Why it is Wrong that Knowledge is a Belief Which is True

As has been pointed out in Section 1.2, the claim that A believes that p can hold regardless of whether the propositional content, that p, is true or false. So why not define 'A knows that p' simply as: '(1) p is true, and (2) A believes that p'. It is important to see why knowledge is not just a matter of having a true belief. As we will argue, what makes the difference is not just truth, but that we get the true belief in ways that relate to the evidence for the truth, and that the evidence is reliable for the truth. Now it is obvious that our beliefs can be true, and we "hit" on the truth, but in ways that have nothing to do with any evidence for the truth, and so have nothing to do with knowledge. How is this so?

Imagine that hypnotists have the ability not only to put people into a trance but also to make suggestions to them that remain lodged in their memories long after they come out of the trance. Suppose that a hypnotist gives a person (who, let us suppose, knows no physics) a lot of false information while they are in a trance; but they do give one truth, say, that positrons have exactly the same mass as electrons but they have a positive charge. Suppose that the same person latter appears in a quiz show and they are asked to mention one way in which positrons and electrons are similar

and one way in which they are dissimilar – and they answer correctly! Here they have a belief that is true. But the belief is not one that is based on any kind of reliable evidence, either through reading authoritative physics books, or doing some science. The belief got lodged in their brains by means of a quite non-standard process, and one that is not even reliable for the truth (the hypnotist gave a lot of false information as well). So they do not know, or even have a reasonably based belief. Learning, alas, can also be a trance-like transmission of true beliefs!

Again one may guess a truth, and also believe it, but not know. Suppose a $10 note is placed under one of three inverted cups and a person is told that they can have the money if they guess under which cup it is placed. They consult their beliefs for the intensity of each belief feeling – the strongest telling them that it is under (say) the middle cup. And they are right. Here again there is true belief but without any evidence apart from the inner feeling which is hardly reliable for the truth, but which in this case just lucks out. Once more there is true belief but neither knowledge nor reasonable belief.

As another example, consider a very persuasive orator in a court of law who gives a compelling speech as to the innocence of a person on trial; and you, as a member of the jury, believe the orator. Plato uses a case like this in the *Theaetetus* (200d –201c) to show that speeches by lawyers in front of courts may persuade us as to the truth of some matter; and what they persuade us about is true. But do we thereby have knowledge? Part of the point he makes is that lawyers can be persuasive as to the truth, but it does not follow that the process of persuasion is always a reliable process for getting beliefs, particularly when no evidence is cited in the speech. Even though we may sometimes be led to the truth by persuasion, it is the lack of reliability of persuasion itself that undermines this is a way of getting knowledge.

In the same passages in the *Theaetetus*, Plato extends his point to teachers. They, too, may be very persuasive in their manner of informing pupils about something, say, Pythagoras' Theorem (which is true), and the pupils are so convinced by the teaching that they come to believe what has been taught. If the kind of persuasion is based on tricks, or entertainment and/or rhetoric, then this is not a process of belief formation that is highly reliable for the truth. But being an unreliable process for belief formation is not the only fault in the case of such teachers; tricks of persuasion and rhetoric are simply the wrong sort of processes for forming beliefs about geometry. The wrong causal paths of belief formation are followed, rather than the right ones based in reliable reasoning. What one needs to learn about geometry from one's teachers are the right kinds of belief forming processes, viz., reasoning processes that are guaranteed to take one to the correct conclusion.

What we will set out in the next section is one very important way in which we can reliably get true beliefs, and thus knowledge. This is by giving a proof, or providing conclusive evidence, as is often the case in science. Of course evidence may not always be conclusive and may only give us reasonable belief rather than knowledge. Since one of the central aims of critical inquiry is gathering truth and avoiding error (see Section 6.3.2), then what we want are processes of inquiry that reliably take us to this goal; gathering evidence and making correct inferences from it is one such reliable process.

Finally, it is important to note that there are a number of sources of belief that do not count as sources for knowledge. Classically, perception and reason are regarded as sources of knowledge, in part on account of their reliability (on the whole) in taking us to truths and avoiding error. But a number of other sources, in part because of their unreliability, are at best sources of belief only. Thus our traditions and our authorities in society can be a source of belief, and even on occasions they do have matters right; but they are not sources for knowledge because of their less than high reliability for the truth as processes of belief formation. Traditions and authorities can equally reliably transmit our beliefs regardless of their truth or falsity; what they cannot do is reliably assess those beliefs for truth or falsity.

Faith and conviction can be a source of belief, and they may even take us to the correct beliefs on occasions but, because of their unreliability and their chancy character, they cannot be a source of knowledge. And the same can be said of revelation, intuition or even supernatural experiences (such as clairvoyance or "second insight"). These can be a source of belief, and sometimes produce true belief; but their unreliability, their failure to do better than chance as much empirical investigation attests, counts against their being a source of knowledge. Knowledge is harder to get than these sources claim.[6]

2.4.2 The Classical Theory of Knowledge as Justified True Belief

The following is the starting point of nearly all discussions of the definition of *knowledge that ...* in epistemology; but it is by no means the final answer, as will be seen in Chapter 4. Suppose 'A' is any individual person and 'p' is any sentence expressing a proposition, say, that the Earth orbits the Sun. Pulling together the many threads of previous sections we have:

A knows that p = $_{\text{Defn.}}$

(i) [Truth Condition] p is a true;
(ii) [Belief Condition] A believes that p;
(iii) [Evidence Condition] A has evidence (justification, reason) that p is true.

The definition, and the Diagram 1 of this subsection, both need explaining. We all believe a range of things. In fact there appears to be a

human propensity to believe; thoroughgoing sceptics who not merely doubt most claims but suspend belief are quite rare. Our beliefs will vary over time; when we are young we have few beliefs, but acquire them quite quickly. When we come to investigate our beliefs we might refine or reject old ones, perhaps, but not always, replacing them with new beliefs; or we might just acquire quite new beliefs unrelated to the old ones we have. In Diagram 1 our set of beliefs at any given time is indicated by the large circle.

Some of our beliefs may be true, others false. There is a fundamental Law of Classical Logic that says that all beliefs (or all propositions) are either true or false. This law is represented by the large rectangular box of Diagram 1 that is divided in half into 'The True' and 'The False'. Given this, how do our beliefs relate to the true/false divide? It has a big circle depicting our set of beliefs. Where should it be placed within the rectangular box?

(i) Could the belief circle fall entirely within the False box? That would be tragic. But perhaps this is not possible. Consider the belief that most of us can entertain, viz., that I am a thinker who exists. Descartes argued on the basis of his 'Cogito, ergo Sum', 'I think, therefore I exist', that this is a belief that, as soon as we entertain it, it must be true. Other beliefs we hold are analytically true, that is, true in virtue of the meanings of the words they contain, example of which are simple arithmetical claims like '1 + 1 = 2', and so on. Thus it may not be possible for all our beliefs to be false and the circle to fall entirely on the right-hand side of Diagram 1.

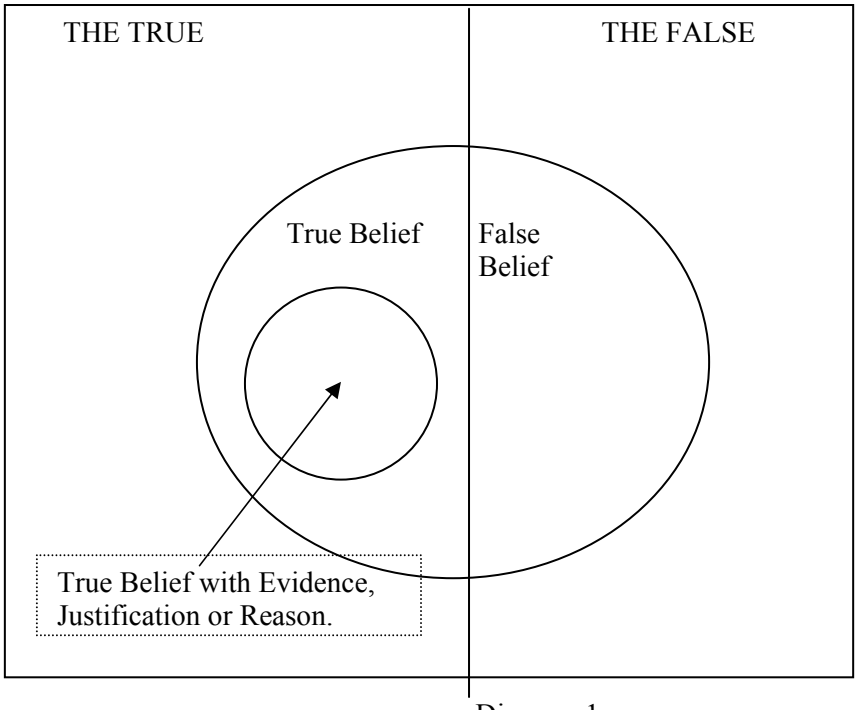


Diagram 1

(ii) Could the belief circle fall entirely within the True box? This is quite possible; there is no contradiction in supposing this. In fact it is the goal of many of us to have only true, and no, false beliefs. But perhaps the frailty of the human condition is such that in fact we always have some beliefs that are false.

(iii) The most realistic possibility, which reflects our human condition, is that depicted in the diagram in which our circle of beliefs overlaps the true/false divide. Note also that there may be propositions that we have entertained but about which we have suspended belief (this is the position of an agnostic concerning the statement 'God exists'); they lie within the rectangle but outside the belief circle. There are also many true propositions that we currently do not believe or have never entertained; these would fall on the left-hand side of the rectangle but outside the belief circle. Similarly, there are many false propositions that we currently do not believe or have never entertained; these would fall on the right-hand side of the rectangle but outside the belief circle. It is possibility (iii) that is represented.

In the light of the discussion in the previous sections, it is clear where knowledge is to be placed. The “knowledge circle” is properly contained *within* the “true belief circle”, since not all true beliefs count as having knowledge.

Some comments are in order about each of the three conditions. The first is the Truth Condition. In knowing that p, the statement p must be true; there can be no knowledge of what is false. Why? One reason is this. We already have a notion of belief that p in which, as we said, p can be true or p can be false. If knowledge that p could be either true or false, then knowledge would not differ from belief; there would be no distinction to draw between them and the two notions would collapse into one. Of course we can *claim to know* that p when p is false. But *claiming to know* is quite different from *knowing*; it is just another way of talking about belief. By not noting this difference people often claim that we can know what is false and say 'we once knew that the Earth was flat'. But the correct response is to say: 'well, people then *thought* they knew, or *claimed* to know, but they did not *really know* that the Earth is flat'. The word 'know', it is often said, is a success word. Just as 'winning' is a success word because it carries the implication that you came first (you cannot win and yet come second, or third), so 'knowing' is a success word in that it latches you onto the truth and not the false.

Some might argue that at best we can only ever claim to know; we do not ever know. This is tantamount to a global scepticism in which we never

know anything, though we can have beliefs of varying degrees of strength. Further comments on scepticism can be found in Section 4.2.

The second Belief Condition says that knowledge involves belief. But would it not be possible to claim that A knows that p yet A does not believe that p? This we have discussed in Section 2.2.1 where we drew a distinction between mere belief (got by hearsay) and belief based on evidence which counts as knowledge.

The third Evidence Condition is what turns a mere true belief into knowledge. What the definition importantly requires is that person A must possess the evidence, justification or reasons, for the truth of p; without it, as suggested in the previous sub-section, there is no knowledge. Note that it will not count as knowledge if someone other than A has the knowledge; A must possess it for himself or herself. Thus we all have the true belief that, say, the Earth rotates daily on its axis. But few actually possess the evidence for this; so strictly on the definition they do not know. Perhaps their evidence for this is the authority of scientists and what they say; or the claim is set out in some authoritative textbook. It can be argued that scientists do make reliable truth claims about their field. The institutional arrangements for science tend to guarantee this since the truth sayers remain in the scientific community while sayers of the false tend to get weeded out. But here the processes whereby beliefs are generated in A are different, and can have different degrees of reliability. In the one case it is knowledge by authority; in the other it is knowledge by evidence, justification and/or reason. Authority will do little for us when it comes to the initial challenging knowledge claims that Copernicus, Galileo and others made about the Earth's daily rotation. And these reasons can become an integral part of the processes whereby that belief is formed and maintained, thereby making it knowledge. The authoritative pronouncements of scientists or their textbooks will not establish knowledge in this way, though they might give us a reasonable belief. For most of us reasonable belief (based in authority) is all we have.

2.4.3 Illustrations of Knowledge in Science as an Educational Model

How do the above three conditions apply in the case of scientific knowledge? If we set aside the case of gaining scientific knowledge merely by observing, or the collection of data, either by human observation or by means of recording devices, then much of science turns on making inferences from what we can observe, or more generally from evidence. Scientists make inferences to generalisations (which may be universal in form, or statistical) or to hypotheses and/or laws (statistical or universal), or to theories. But as was indicated in Section 2.2.3, not all hypotheses need be of this form; they can be singular claims about a particular person, say, about their medical condition. Some illustrations will be given of knowledge as justified true

belief; these will also serve as models within science education for the acquisition of knowledge.

The examples have the following form. There is some proposition or hypothesis H that person A claims to know, for example a general hypothesis in science, or a particular claim such 'the Earth rotates daily about an axis'. And there is some evidence E that A has to have, and some pattern of inference that A makes from E to H. Both E and the pattern of inference are the grounds on which A is said to know that H. What this immediately reveals is that we need to know the evidence E, and know the pattern of inference and how to apply it in the case of E to reach H. And this suggests that there may well be a regress of knowledge claims in support of the original knowledge that H. The examples will reveal this regress, but it will not be discussed further until Chapter 4. Here we merely propose a definition of knowledge; in Chapter 4 we set out a fuller *theory* of knowledge in which the definition is encapsulated. The task here is to see how the definition relates to education.

The first illustration is this. How do we know that the Earth is curved and not flat? This was well known long before astronauts went out in space to look back on the Earth. Not unsurprisingly, this is often the only reason pupils give for our knowledge that the Earth is curved. And they are not wrong in this. But the astronaut's knowledge is obtained by direct perception. Pupils do not know it in this way; at best they have picked this up as a piece of true information (i.e., a true belief) that circulates in our culture. That the earth is curved was well known to the Ancient Greeks from about the 6th Century BCE (if not earlier), and so could not have been obtained by direct perception. Instead this knowledge was obtained by inference from observational evidence. What is the evidence?

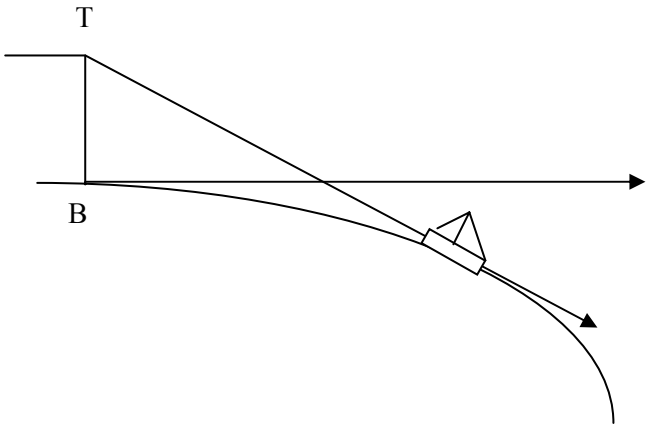


Diagram 2

It is well known by observation that if one sits at the top of a hill that slopes downwards quite strongly, then when one's companions walk up to meet you, they are not initially in full view; their head appears first, then their torso, then their legs and finally their feet. The full length of their body is obscured by the curvature of the hill. The Greeks noticed the same phenomena at sea.

Consider Diagram 2. From the top T of a cliff BT a ship is in the line of sight of an observer, while at the bottom B of the cliff a boat sailing towards the cliff is not observed. For those at the bottom, the ship slowly comes into view, mast first, then the deck and finally the full ship. The same phenomenon is also noted at sea; from the deck of a ship the crew cannot see either the distant land, or another ship, that can be seen from a lookout at the top of a mast. What explains this phenomenon? The problem cannot be with the sight of the observers, since they can see much greater distances, say to the Sun, Moon and the stars. Note also that it is assumed that light travels in straight lines, and does not curve around.

The best, and perhaps the only, explanation is that the Earth must be curved. Let H = the earth is curved. And let E be the above observed phenomenon, along with the assumption that light travels in straight lines. Now a mode of inference, called Inference to the Best Explanation (IBE), tells us that the best of a satisfactory set of explanations is true. It turns out that H is a very good explanation of what is observed, given the geometry of the situation and the assumption about light travelling in straight lines. In fact H is the only explanation that is available; in the absence of H no explanation is possible. Using this information as evidence and the pattern of inference IBE, then we can infer that H is true. Here we have an illustration that fits the definition of knowledge, in which appeal is made to evidence, and to an inference from the evidence.

Knowledge of the curvature of the Earth was commonplace amongst all sailors in the Ancient World, and those sailors who later ventured from Europe down the coast of Africa and out to the Canary Islands in the Atlantic. Importantly it was also known to Columbus. He never thought the Earth was flat. Where he differed from his contemporaries was over the size of the Earth. He thought it was a lot smaller than it was, and developed the idea, first commonplace with the Romans, that by sailing west one could reach India. The idea that people thought that the Earth was flat is a nineteenth century invention that does not fit, and in fact blatantly falsifies, what early sailors, or the educated, actually believed from Ancient times to Columbus.[7]

The second illustration of knowledge in the sciences follows on from the above. If the Earth is curved, what knowledge of its size can we determine? The Ancient Greek Eratosthanes (third century BCE) accepted the sphericity of the Earth and then provided one of our first estimates of the size of the

Earth that is roughly correct.[8] He suggested a geometrical model which, when given appropriate observational input as evidence E, would give us knowledge of hypothesis H, the size of the circumference of the Earth (with a degree of error equal to that of the observational input). We do not know his exact result because the surviving record contains observations that have been rounded off; and the linear measurements are in terms of stades. This is an ancient unit of length that is now not precisely known by us, but some indication can be given since it was used to give the length of a sporting stadium. Still, the rounded off data, the model, and the inferences that can be made, give us a good idea of the distance, which is correct within the order of magnitude.

Diagram 3 of this section is a model of a real site in Ancient Egypt. S is the position of the city of Syene (near current day Aswan), where Eratosthanes observed the Sun to be directly overhead at midday on the summer solstice; he noted that the Sun lit up the water at the bottom of a well. This suggested to him a way of measuring the circumference of the Earth using another point A, in the more northern city of Alexandria. There the midday Sun was not overhead; but it would be possible to measure the angle of the midday Sun on the same solstice day. To measure this he used the shadow cast by a vertical stick, called a *gnomon*, which is placed upright in the ground (this is indicated by AG). The shadow the gnomon casts at midday is on the ground and is represented by the segment AB. The short, dotted line is the tangent to the Earth's surface at A which intersects the line BG. Finally point A in Alexandria was at a distance said to be 5000 stades due north of point S in Syene.

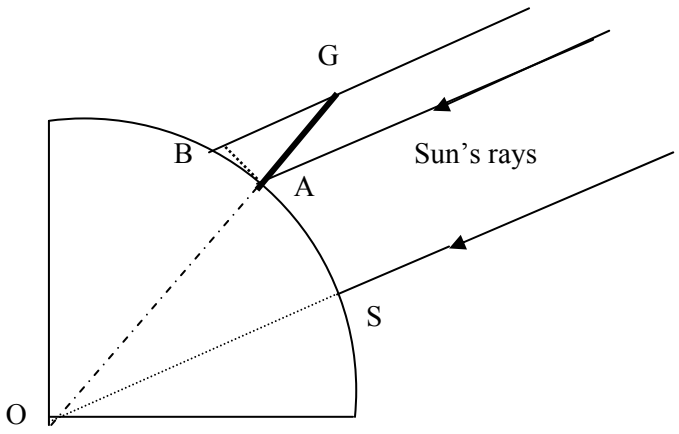


Diagram 3

Since the triangle BAG is right-angled (the gnomon at A is upright with respect to the earth's surface), it is possible to measure the angle AGB that subtends the shadow cast on the ground at Alexandria. This is done by measuring from the ground angle GBA, and then subtracting that from a right angle. It is reported that Eratosthanes found it to be 1/50 of a circle, that is, about 7.2 degrees. (It is known that the report of his measurement rounds off his original figure, as it also does the distance between the two cities.)

The geometrical aspects of the model are as follows. It is assumed that the Sun's rays are parallel, the ray passing through S being extendable to O, the centre of the Earth. The line of the gnomon, since the gnomon is upright, also passes through O. Now it is possible to know the size of the angle AOS, the angle subtended at the Earth's centre by the distance AS. A theorem of Euclidean geometry concerning parallel lines (the Sun's rays) intersected by a straight line AOG, tells us that angle AOS is the same as angle AGB.

It is now possible to deduce the diameter of the Earth. Since 5000 stades subtends 1/50 of a circle, then the full circle subtends 50x5000 = 250,000 stades. Though it is not possible to be sure, this figure is within 2% of the actual circumference. Setting aside the unsatisfactory records we have of Eratosthanes' data (which we know to have been more accurate), we still have a good idea of his method, and of the result he arrived at.

We can now say that Eratosthanes *knows that* the size of the Earth is 250,000 stades. Or if one still has scruples about the data, we can still make this a knowledge claim, but one that includes information about an error factor. And this is appropriate because in science we encounter quite often propositions that also include information about errors that we also know. Thus we do say that we now *know* that the circumference of the Earth at the equator is, say, 40,075.16 ± .02 kilometres, and this knowledge also contains information of an error factor. So we can equally as well say that Eratosthanes *knew* that the magnitude of the Earth's circumference was 250,000±500 stades (an error factor of 0.2%). This, in form anyway, is as good a knowledge claim as any found in science.

What is the evidence on which the knowledge is based? There are four crucial items. The first is (E_1), observational evidence obtained by measuring an angle using the naked eye and a measuring device. The second is (E_2), a long distance on the surface of the Earth. Third, (E_3) is a theorem of Euclidean geometry. Finally (E_4) is an idealising assumption about the Sun's rays being parallel. Putting these together, knowledge that H is arrived at through deductive inferences from E_1, E_2, E_3 and E_4. This provides a useful illustration of our definition of knowledge.

This raises a number of questions for discussion. One has to do with the invention of a method to measure the circumference. Possible, but wholly impractical, methods might be either to sail around the Earth attempting to make the measurement, or by boring a hole through the Earth. The ingenuity

of Eratosthanes' method becomes quite evident; there is no direct way to get the knowledge; it can only be obtained by inference from other information.

We can also ask questions about knowing *that*, *what* and *why* (see Section 2.1). With this example it becomes evident that knowing *that* the Earth has a size of about 40,000 km (= H) is linked to knowing *what* the size of the Earth is (viz., it is 40,000 km). But it is very clear that neither of these is the same as knowing *why* the earth has the size 40,000 km. The evidence for H does not provide an explanation of *why* H. In fact no such explanation seems to be forthcoming from anything like the evidence E. (A follow-up question would be: what would explain H?)

The next questions to be raised concerns the evidence for H. One could ask how accurate the observational evidence E_1 is, and whether it could be improved. One can also ask about how the distance between Syene and Alexandria was determined and whether it could be improved. (It remains a question for further historical investigation to determine how the ancients in fact measured the distance.) A further matter concerns the proof of the correctness of the geometrical theorem used as evidence E_3. This also hints at something about the difference in the character of the two kinds of evidence. Two items of evidence are based in observations made by measuring, and are thus empirical in character. The third is based on reasoning from axioms of geometry. But are the axioms and the theorems known on the basis of observation, and so are known empirically? Or are they known independently of observation, and are thus known *a priori*? Since the latter is so, we have an example in which the empirical and non-empirical, or *a priori*, come together to provide what is a piece of empirical knowledge. This is a very common state of affairs in science indeed; it shows that even though science is often said to be an empirical matter, it is not without its *a priori* aspects when it uses mathematics.

The final bit of evidence, if we can call it that, E_4, concerns the assumption that the Sun's rays are parallel when they are near the Earth. This, of course is an idealisation. We devote Chapter 10 to the topic of ideal models as an important part of scientific method. There are several idealisations in the above model, some of which Eratosthanes was aware, and others not. Thus, he assumed that the Earth was a perfect sphere and that Alexandria and Syene were on the same meridian line. But he consciously assumed (so it seems) that the Sun was so far away that its rays of light reaching us were parallel. This assumption is not unreasonable as there would be little noticeable difference between the actual rays of the Sun and those in the ideal model, as far as the degree of observational error with which he was working would have been concerned.

Students might have difficulty in making this assumption when they consider the real model of the Sun and the Earth, and the idealised model with its parallel rays. As Feigenberg, Lavrik and Shunyakov, (2002) argue,

not only do students have problems with envisaging the parallel rays, they also have problems fitting this into a model with the Earth's surface curved rather than flat. The authors base their study, in part, on student understanding of Eratosthanes' reasoning and his model, saying that the student's own mental model make it difficult for them to understand large distances and scaling. While this may well be part of the problem, another aspect, not envisaged by the authors, is that of building and constructing models of reality where these models tend to go against one's own experience of reality. This is a matter which Galileo, one of the first to employ idealised models to a large extent, was aware (as will be seen in Chapter 10). What is necessary in education would be to get students to see the value in idealisations which do not correspond to their "experience", and may even go against it – a difficulty that even Galileo's own contemporaries felt in studying his work. Here what Wolpert (1993) calls the 'unnatural nature' of science makes itself felt. Overcoming such difficulties in the education of students so that they grasp the use of idealised models, is an important piece of research to be advanced along the lines of Feigenberg *et. al*.

2.5 *KNOWING WHY*: EXPLANATION AND UNDERSTANDING

In Section 2.2 we have pointed out that *knowing why* is a different kind of knowing than *knowing that*, *knowing how,* and so on, and that one can know *that* p without being able to explain *why* p is the case. *Knowing why* involves providing an explanation for why things are the way they are. But what is explanation? This is an extremely general question. Since our topic is science education, we will confine ourselves to scientific explanations and ask: what is scientific explanation?

Minimally, a scientific explanation is an answer to a why-question that can take various forms: why did this happen? What caused it to happen, and the like. In general, why-questions can be expressed in the form 'why is it the case that p?', where p is a complete statement that describes a phenomenon (an event, a fact or a state of affairs). They are obviously expressions of human curiosity, and adequate answers to them are expected to satisfy it. A second general characteristic of scientific (indeed, all) explanations is that they consist of two parts: the *explanandum* and the *explanans*. The *explanandum* is a phenomenon to be explained. The *explanans* are further phenomena that do the job of explaining. A third general characteristic of scientific explanations is that the explanans appeal only to natural phenomena (which we can take to include an account of mental phenomena as well). Explanations in terms of supernatural items such as gods, spirits and so on are not admissible. Many explanations given by 'ethnosciences' of local, indigenous cultures are pseudo-scientific precisely because they lack

this characteristic. As we shall see in Chapter 13, this is a point often ignored by the champions of epistemic multiculturalism in science education.

In the voluminous philosophical literature on explanation, there is general agreement on these three characteristics of scientific explanation; but a wide variety of models are advocated. For our purposes these boil down to two basic approaches. One of them is known as *the covering law approach*, which takes explanation to be a matter of subsumption under laws and consequently a matter of unification. The other is called the *causal-mechanical approach* according to which to explain an event is to place it in a causal nexus and specify the mechanism that brings it about. The most important representatives of these approaches are Hempel (1965) and Salmon (1984) respectively.

2.5.1 The Covering Law Approach

According to this approach, an explanation is an argument, which shows that the explanandum was expected in virtue of the explanans. The explanans essentially contain statements of law that subsume or cover the event to be explained. Statements of laws may be exceptionless universal generalizations or they may be statistical. In the former case the explanation will take the form of a deductive argument and will be called *deductive-nomological* ('nomological' comes from the Greek root *nomos* which means law), and in the latter case it will take the form of an inductive argument and will be called *inductive-statistical*. Here we deal only with the former.[9]

As an example, let us ask why the length of the mercury column in a Torricelli barometer decreases with increasing altitude. This was first observed by Perier, Pascal's cousin, by climbing Puy-de Dome, a mountain that is about 4800 feet high. The explanation is roughly as follows:

> (a) At any location, the pressure that the mercury column in the closed branch of the Torricelli apparatus exerts upon the mercury below equals the pressure exerted on the surface of the mercury in the open vessel by the column of air above it.
>
> (b) The pressures exerted by the columns of mercury and of air are proportional to their weights; and the shorter the columns, the smaller their weights.
>
> (c) As Perier carried the apparatus to the top of the mountain, the column of air above the open vessel became steadily shorter.
>
> (d) (Therefore,) the mercury column in the closed vessel grew steadily shorter during the ascent. (Hempel 1966, p. 50)

Here, (a), (b) and (c) are *explanans*, and (d) is the *explanandum*, which follows deductively from the explanans. (a) and (b) are universal statements of laws which express regularities (uniformities) in nature, and (c) describes certain particular facts. In view of the explanans, the explanandum is

expected. To put it differently, (d) is deductively subsumed under (or covered by) the law statements in question.[10]

Such deductive-nomological (D-N) explanation is set out as Schema 1:

Schema 1

$$\begin{array}{c} L_1, L_2, \ldots. L_m \\ C_1, C_2, \ldots. C_k \\ \text{----------} \\ E \end{array}$$

where L's refer to universal laws and C's to particular facts, which collectively form the explanans, and E is the explanandum.

Hempel (1965, pp. 247-248) cites four conditions for the adequacy of D-N explanations (otherwise one has at best explanatory sketches only):

(1) The explanans and the explanandum must form a deductively valid argument. More specifically, the explanandum must be logically derivable from the explanans.

(2) The explanans must contain at least one general law essentially; that, the law must actually be necessary for the derivation.

(3) The explanans must be testable in principle by observations or experiments.

(4) The explanans must be true.

It is easy to see that the barometer example above satisfies these four conditions. Note also that in that example the explanandum was a particular event, namely the decreasing level of the mercury column at a particular space-time location.

The covering law approach applies equally well to uniformities or laws of nature as well as particular happenings. For example we can ask: why does the Galilean law of free fall, which we can express as $s=1/2gt^2$, hold? We can also explain laws like this by deriving them from more general laws of motion, in this case Newton's laws of motion. Let m be the mass the object subject to free fall. Also assume that it is dropped from the top a building of height s. Let t represent the time the object takes from the top of the building to hit the ground. g is the acceleration on Earth due to gravity.[11] Then the explanation roughly goes like this (we leave the details, such as computing the constants of integration, to the reader):

(1) A ball, initially at rest, is dropped from the top a building of height s.
(2) F=ma (Newton's second law)
(3) F=mg (the force exerted on m by gravity)
(4) ma=mg (from 1 and 2)
(5) a=g (from 3)
(6) $a=d^2s/dt^2$ (definition of acceleration)
(7) $d^2s/dt^2 =g$ (from 4 and 5)
(8) $ds/dt=gt$ (integrating 6 and using 1)
(9) Hence, $s=1/2g\ t^2$ (integrating 7 and using 1)

This is a deductively valid argument in the form of a mathematical derivation and fits the D-N scheme. (1) to (8) jointly comprise the explanans, and (9) is the explanandum. So, we see that general laws are explained in the same manner as particular facts: by subsuming them under (more) general laws. Since the more general the laws that figure in the explanans are, the more phenomena they can explain, D-N explanations have also the virtue of unifying diverse phenomena. Think of all the different kinds of phenomena that are explained by Newton's laws of motion: the law of pendulum, projectile motion, motion of tides, Kepler's laws, and so on. They can all be subsumed under Newton's laws and thus be unified.

The last point raises an important issue about the explanation of laws. Laws have been used to explain in the two examples given above. But the laws in turn are not explained by anything; they play the role of unexplained explainers. So what would explain laws? Other more general laws can be used to explain why a less general laws holds, as in the example of Newton's laws explaining more specific laws. But what explains other laws? Either they remain unexplained, or deeper, more general laws explain them. But ultimately all explanation will stop at fundamental laws which are not open to any further explanation with in science. This raises deep issues in the philosophy of science that do have further discussion in metaphysics.[12]

Despite the appeal of the covering law approach, it has some limitations. What these are need not concern us here. These are discussed in detail in Salmon (1984), who have developed an extremely sophisticated alternative account of scientific explanation. In what follows we will be content with the barest outline.

2.5.2 The Causal-Mechanical Approach

The starting point of the causal-mechanical approach is an ontological assumption about the structure of the world: it assumes that the world has a built-in causal structure. And to explain an event is to fit it into the causal structure of the world. More specifically, an event is explained by citing its antecedent causes, by specifying the causal mechanism that leads to it. Because of this, the causal-mechanical approach has an ontic conception of explanation. By contrast to the covering law approach, it does not see explanation as a matter of giving an argument where certain statements (premises) stand in a logical relation to other statements (conclusions). Thus, according to the causal-mechanical approach, for example, we explain the expansion of a metal bar by pointing out that it was heated and that heating metals cause them to expand. The further question of why metals expand when heated is explained by appealing to the atomic structure of metals in the same manner. We point out that all substances including metals are made

of little particles called atoms and that absorption of heat causes them to oscillate, which in turn result in the expansion of the metal. To explain an event, therefore, often involves capturing the underlying, often hidden mechanism behind it. Thus, to understand why blue-eyed parents have blue-eyed offspring, we appeal to genes; to explain throat infections we invoke viruses or bacteria, and so on. It is these unobservable entities and hidden mechanisms that provide explanation of many phenomena.

It may seem that the most important difference between the received view and the causal-mechanical view is that while the former appeals to laws for explanation, the latter does not. This is not true. For the causal structure of the world, according to the latter, may reveal itself either as exceptionless regularities or statistical uniformities. Therefore, to the extent to which these are manifestations of the underlying causal structure of the world, they will be taken as laws by the defenders of the causal-mechanical approach.

The two approaches differ from one another crucially with respect to their notion of causality and the role causal relations play in explanation. As we have seen, the essence of explanation according to the covering law approach is deductive or inductive subsumption under laws and the unification thus obtained. Causal relations play a secondary role here. This is because the covering law approach has the same account of laws and causes. Both are defined in terms of regularities. The covering law approach takes laws as describing regularities, and for its advocates casual relations are nothing but regularities. Thus, they believe that invoking laws is enough to capture causal relations. More explicitly, the received view operates with a Humean-regularity account of causation. This means that causal relations take place between discrete, localized events and that an event A is said to be a cause of another event B if and only if A occurs before B and whenever events of type A occur events of type B follow regularly.

For the proponents of the causal-mechanical approach causal relations are both primary and prior to deductive or inductive systematisation, so explanation depends on them. Moreover, they have a different account of causality. They characterize causal relations in terms of spatiotemporally continuous processes, which they take to be ontologically irreducible to events. Events are then defined as intersection of such processes. Thus, explanation is a matter of fitting an event understood in this way into a discernable causal pattern.

Despite such differences, however, the two approaches are not entirely unrelated to one another. Often, the discovery of underlying causal structure of many diverse phenomena (such as in terms of atoms, genes, etc. and their properties) enables us to unify them. Thus, unification is a respect in which the two approaches overlap considerably. Also, sometimes a causal explanation can be fitted into a covering law approach as well, as in the case of explaining the expansion of a specific metal bar when heated. In the

classroom science teachers need not worry about the subtle differences between the two approaches. They can appeal to either or both depending on the case at hand. The important point is that for an account to qualify as a scientific explanation, rather stringent conditions must be met. That science can often provide explanations that satisfy them is a remarkable achievement.

2.5.3 Pragmatics of Explanation

There is another aspect, called the pragmatic dimension, of explanation, which needs to be discussed, as it is directly relevant to education. Ordinarily, we think of an explanation always as an explanation of something to somebody by someone else in a certain context. Understood in this way explanation involves a pragmatic component. To explain what this means, we must appeal to a customary division made by philosophers and linguists. When they analyse language, they divide it into three parts: syntax, semantics, and pragmatics. Syntax deals only with the formal relationships among a given set of symbols called the vocabulary without reference to their meanings. It consists of rules of formation, which tell us which sequences of symbols of the vocabulary form well formed formulas (they might be called sentences), and also of rules of transformation, which enable us to infer one sentence from a set of others. Semantics is concerned with symbols (including sentences) and what they mean and refer to. Meaning and truth are among the most important semantical concepts. Finally, pragmatics deals with symbols, their meanings *and their use* by people in various contexts. It is in this sense that explanation involves pragmatics: there is a person who does the explaining to someone in a specific situation.

The pragmatic dimension makes explanation highly variant; what counts as an explanation will depend on the interest and the background knowledge of the person who requests it. Suppose someone asks why Brutus stabbed Caesar to death. She may be asking why *Brutus* (rather than someone else) stabbed him to death, or why he stabbed Cesar *to death* (as opposed to merely wounding him), or why he *stabbed* Caesar to death rather than knocked him out, strangled him or shot him, and so on. Each of these questions requires a different explanation, and each explanatory story will emphasize a different aspect of the historical phenomenon. The amount of explanatory information to be provided will depend not only on the interest of the questioner but also on how much she already knows.

The pragmatic aspect of explanation is obviously highly relevant to science education, especially in the classroom context. How much a child can learn and absorb naturally depends on her age, on her mental development and the like. School curricula on science naturally take such factors into account. Thus, for most young schoolchildren an explanation of why objects fall in terms of the gravitational pull of the Earth may suffice.

But older students may be exposed to more sophisticated explanations in terms of Newton's laws. The teacher should also pay careful attention to the student's why-questions in the classroom to understand exactly what it is she is asking. Is she, for instance, asking why metals expand upon *heating* rather than cooling? Is she asking why they *expand* rather than shrinking? Or perhaps why *metals* but not something else like wood?

It should be noted, however, that in scientific explanations the pragmatic dimension is an add-on to the relationship between the explanandum and the explanans, regardless of whether one adopts the received view or the causal-mechanical approach to explanation.[13] In other words, explanation cannot be reduced to mere interests, or more generally to mere pragmatics. Unless an appropriate relation holds between the explanandum and the explanans (subsumption according to the covering law approach and causal connectedness according to the causal-mechanical approach), no explanation can be satisfactory. In either case, that relationship is objective; it does not change once the pragmatic aspects are fixed. This is a point often missed by some of the constructivists in science education. Although explanations are human constructions in the sense that they are provided by human beings, their adequacy depends satisfying certain objective conditions Thus, it is of vital importance that students acquire genuine explanatory knowledge as part of their science education.

2.5.4 Explanation And Understanding

In ordinary language there is not much difference between the locutions 'A know *why*' and 'A understands *why*'. Here to explain is also to understand, and conversely. For students to increase their understanding of matters in science is just for them to make explanatory connections, and to see the connections for themselves. Of course, both differ from 'A knows *that*'. A person may *know that* a lot of things hold, but understand nothing in the sense that they make no links between the various things they know. Using an example above, they may know both Newton's and Galileo's laws of motion, but not see that one is explanatorily linked to the other. As such their understanding of the interconnectedness provided by explanatory links between higher and lower level laws is missing.

The word 'understanding' however gets a use in contexts where 'explanation' is not appropriate. Thus we are said to understand the meanings or words, or languages; and we understand signs and symbols. In these cases it is not explanation that is intended. Perhaps because of the connections with such kinds of meaning, we are also said to understand the meaning of human actions by, say, coming to know the "meaning" of a person's actions, in the sense of their reasons or motives (conscious or unconscious) for doing something. Orwecome to understand their actions through coming to know of their intentions or goals in so acting. Or we come to

understand the significance of a human institution or some ritual by citing their purpose or function. But it is not clear that the word 'explanation' is inappropriate in these latter contexts. Thus we can equally as well be said to explain human action in terms of reasons, motives, intentions and goals, or explain
human institutions by pointing to their role or function. In our view there is no significant difference between explanation and understanding even when applied to human action.

That there is a difference between explanation and understanding has been urged by those who see that in the physical and biological sciences law-covering and casual models of explanation must apply; but in the human, historical and social contexts such models are alleged not to apply, but others instead. To mark the difference, these other models are often said to be models of understanding and not explanation. This is not a matter we wish to enter into here, largely because the kinds of sciences we are considering in a book on science education are the natural and not the human sciences. There is much literature devoted to the topic, and we wish to merely signpost this fact.[14] Though we will not argue the case, in our view the differences are not as great as are sometimes alleged and the same kinds of models apply in both the human and the natural sciences.

Some science educationalists put emphasis on the idea of students having *well-organised* knowledge, this being an necessary condition for a pupil to acquire *know how* with respect to problem solving (see Bransford *et. al.* chapter 2, especially pp. 24-30). Though the requirement of having *organised knowledge* can be quite broad for pupils who are expert learners as opposed to novices, in large part it is a kind of *know how*, an ability to construct explanations through their understanding of the interconnections and relations that are possible in their science. An example of this is given above; it is the construction of the two deductive-nomological explanations given in Section 2.5.1 of phenomena in physics. Bransford *et. al*. (op. cit. p. 27) cite related examples from physics (in this case explanations of objects moving down inclined planes) to show the difference between novices and experts in solving problems. The success of the more expert pupils is due to their ability to construct explanations using principles and laws of physics. In contrast novices do not use the principles of physics as a way of organising their knowledge. Rather they rely more on superficial and often irrelevant observable similarities as the basis of their organisation of knowledge, that is, problem situations are treated the same because "look the same". But such surface similarities are no guide to whether or not there are deeper underlying similarities (or differences) based on the sameness (or difference) in the explanatory laws to be employed. Clearly *know why* in the sense of explanatory understanding is crucial in realising the requisite level of the organisation of the other items of knowledge possessed by a pupil, thereby enhancing their ability to solve problems.

2.6 LEARNING, TEACHING, KNOWLEDGE AND CRITICAL INQUIRY

In this section we pull together some of the various threads linking learning *that* and *why* with knowing *that* and *why*, and what connection they might have to critical inquiry. As we argued in Chapter 1, learning *that* or *why* can be successful in so far as it leads to the uptake of beliefs. But importantly this can take place in the absence of any critical inquiry. In contrast, in coming to *know that*, critical inquiry is essential in being part of the very process whereby knowledge is acquired. What is needed is learning through a process of critical inquiry which does at least two things: it takes us to the evidence for the truth of some proposition that p; and we think about this so that we see that it is genuinely evidence for the truth of p. Without this no knowledge that p is obtained; at best one gets just a belief or an opinion that p. The same applies for knowledge *why*. If we have knowledge *why*, then it must be the correct explanation that we have and not some generally accepted explanation that might not be correct. Moreover we need to see that it is the correct explanation, otherwise we do not have the knowledge ourselves. Without these two conditions, no learning which culminates in knowledge *why* can take place; at best we have only opinions or beliefs about the alleged explanation. The norm-laden notion of education we have distinguished thus involves learning *that* and *why* through the application, by the learner, of the principles of critical inquiry that establish knowledge *that* and *why*. Again, with out the application of critical inquiry by the learner himself or herself, no such education will have taken place but, at best, something much less.

Some of the points we make here have already been carefully drawn in Scheffler (1965, chapter 1). He argues on a minimal account of learning: person A learns that p entails that A believes that p. But from this it does not logically follow that A knows that p. Only when A's learning is in the light of critical inquiry can it be possible for A to know. But as Scheffler points out (*ibid*., p. 8) there is a "success" or "discovery" use of 'learn' where the entailment to knowledge does hold. For example reporters may claim, when they investigated, say, the revelations of the informant "Deep Throat" about President Nixon's cover-up in the Watergate scandal: the reporters learned, after careful investigation, that the revelations were correct. Here the sense of 'learn' does entail that what is learned is knowledge. What makes it knowledge is that one has come to learn something through investigation, or some related process of critical inquiry, and has thereby had success in making a discovery. One has not learned something in the sense of being schooled in the claim. So, even though there are some uses of the word 'learn' that do involve 'knowing', this does not hold for other uses,

especially talk of 'learning' *via* educational processes that do not involve critical inquiry.

What of teaching *that* p? 'Teaching' is a two-place relation involving teacher T and pupil A. So what we need to investigate is the teaching/knowledge link. Alas, there is not one. Consider 'T teaches A that p'. This does not even entail that A learns that p. Even in the best of teaching conditions, 'T teaches that p to A' can be unsuccessful; so the claim 'A learns that p' does not logically follow. However if the teaching is successful then it does follow that A has learned that p. But as can now be readily seen, even in the case of successful learning there can be no further logical link from teaching to knowledge; 'T teaches A that p' does not entail 'A knows that p'. Teaching can have learning and/or knowledge as a goal, but it does not logically guarantee either; it can proceed quite happily and successfully in the absence of the acquisition of knowledge in contrast to belief. Obviously the converse relations do not hold; one can learn, believe or even know that p without being taught by anyone.

The upshot of the above is that in the cases of teaching and learning that p there is no logical link to knowing that p. But note that the above applies only to cognitive 'knows *that*'; the case of explanation and understanding as in the phrase 'A knows *why*' needs a separate investigation. However much the same conclusions follow. Successful learning *why* need not culminate in knowing *why*. Collectively, these results support our claim of the logical independence of learning from knowledge. Learning can only involve knowing under proper conditions of critical inquiry. But this is something that can only be achieved in learning when critical inquiry is deliberately made an "add on" that informs and guides the process of learning.

2.7 MISCONCEPTIONS ABOUT KNOWLEDGE IN SCIENCE EDUCATION

We end this chapter on the nature of knowledge by contrasting our account of the various facets of knowledge with some of the unsatisfactory accounts of knowledge to be found in the science education literature.

Consider the conception of knowledge according to radical constructivism, which is the most popular theory of knowledge. Radical constructivism defines knowledge as 'conceptual structures that epistemic agents, given the range of present experience within their tradition of thought and language, consider *viable*.' (Von Glasersfeld 1989, p.124). Since we will discuss in detail the inadequacy of this conception of knowledge and how radical constructivists are erroneously driven to it in Chapter 5, here we simply note that truth as a necessary condition of knowledge is totally missing. This tendency unfortunately pervades most of the literature on science education influenced by radical constructivism. Thus, for example,

Bettencourt's, Driver's and Roth's definitions of knowledge repeat Glaserfeld's almost verbatim (see Bettencourt 1993, p. 43; Driver 1988, p.135 and Roth 1995, p. 13). Furthermore, radical constructivists talk about the 'construction of knowledge' without distinguishing between different senses of 'know', and when it is clear from the context that it is *know that* they are talking about, they tend to confuse construction of beliefs or representations with the construction of 'knowledge'. While it makes sense to talk about the construction of beliefs or representations, it is wrong to talk about the construction of knowledge unless both evidence and truth have an essential role in the constructing (see Chapters 3 and 5 for more details). The aforementioned confusion usually results in a subjectivist conception of knowledge, as in Von Glasersfeld, Carr *et. al*. (1994), and many others. Nor does it help to insist, following Vico, that all knowledge is *know how*. This reduction of all the various kinds of knowledge to just one kind is clearly wrong.

On the other hand, some moderate constructivist approaches to knowledge are not without their problems. Brown *et. al* (1988), for example, claim that 'knowing … is inextricable situated in the physical and social context of its acquisition and use' and that 'Knowledge is fundamentally a co-production of the mind and the world' (*loc. cit*. pp. 1-2). But the first remark does not tell us what knowledge is. Rather it tells us what few would deny, viz., that knowledge is "situated" in the use to which we put knowledge. As for its acquisition, if *knowing that* is intended, then nothing is said about the evidence condition, which, in another sense, situates our knowledge in the context of giving reasons. And it would be a long stretch of the imagination to interpret the second remark to be about the truth condition on *knowing that*. Bednar *et. al.* (1992) take a commendably objectivist stance. But, unfortunately, they regard the mind as a computer in which the symbols it manipulates get their meaning by being mapped onto the world. From this they conclude: 'Knowledge, therefore, is some entity existing independently of the mind which is transferred "inside the mind" (*loc. cit*., p. 20). If only our minds were that transparent! *Knowledge that* (assuming it is this kind of knowledge under consideration) would then get into our minds without satisfying the bothersome evidence condition!

As a final example, consider the following non-constructivist formulation by Brickhouse and her collaborators: 'One of the goals of science education is the acquisition of justified belief, or knowledge, about the natural world' (Brickhouse et. al. 2000, p. 19). The emphasis on justification as a condition of knowledge is certainly to be commended, and indeed the very title of their article draws attention to the importance of evidence and warrant for knowledge. But, unfortunately, because they use 'knowledge' and 'justified belief' interchangeably, their notion of knowledge is spoiled by the conspicuous absence of truth. We are struck by the large number of science

educators who seem to suffer from truth phobia! What is even more surprising is that there are some science educators who, though they have a realist and objectivist approach to knowledge, sometimes shy away from explicitly using the term 'truth'. For instance, Halloun writes:

> Knowledge consists of conceptual structures and processes that have been corroborated in specific respects. Corroboration consists of some sort of objective evidence, the most reliable of which being empirical or real world evidence that meets specific norms. (Halloun 2004, p. 6)

Since Halloun talks about corroboration and evidence, he must have in mind *know that*; after all, it is propositions or beliefs that are corroborated or not. But this is obscured by talk of 'conceptual structures and processes' as the bearers of knowledge. As we explained earlier in this chapter *know that* is propositional knowledge; but one is left wondering how a *process* is to be corroborated. At any rate, evidence is an essential part of *knowledge that* as Halloun insists. Nevertheless, if we take the phrase 'consists of' in the first sentence literally, then Halloun's characterization is inadequate because it ignores truth as a necessary condition of knowledge. Later on, Halloun introduces the notions of 'experiential knowledge' and 'scientific knowledge' (*ibid*., pp. 8-11); according to him, while the former *could* correspond to an objective reality on some occasions, the latter always corresponds. Now, although it is not at all clear why 'experiential knowledge' *qua* being knowledge does not correspond to reality, we are pleased to see that at least scientific knowledge does. Despite this, Halloun refrains from calling a belief or conception 'true' when it corresponds to reality! Indeed, when he talks about the relationship between a model and the world, he prefers to talk of 'model viability' rather than truth (*ibid.,* p. 68). Truth phobia seems well entrenched in science education circles.[15]

Much of the above does become understandable if one considers belief rather than knowledge. But the whole point of this chapter has been to emphasize the way in which belief and knowledge differ. Even though *knowing that* involves *believing that*, it must involve a lot more, as has been argued. One of the few papers on knowledge in science education to which we can give considerable endorsement is Smith and Siegel (2004). Like us they wish to distinguish the different kinds of know (as in Section 2.1) and they make much of knowledge and its links to understanding. Their primary concern however is how to deal with a pupil who says 'I understand the theory of evolution, but I don't believe it'. In a similar vein another pupil might say 'I understand what the Bible says about creation, but I don't believe it'. Understanding what is going on here does take a careful approach to the epistemological concepts involved, often obscured in the literature. Dealing with issues of what one ought to believe, or accept (or any of a range of cognitive attitudes here) takes us into the terrain of critical inquiry. This is a matter only the surface of which we scratch in Part II.

NOTES

[1] One exception is the excellent Gauch 2003 which links issues in science education to a well-presented account of scientific method and probabilistic reasoning in general. This is a book that all interested in theory in science education should consult for its account of scientific method.

[2] Cognitive psychologists are beginning to recognise these and other kinds of knowledge. See, for example, Sternberg 1999 chapters 7 and 8, and the index of Brunning *et. al*., 2004, under 'knowledge' and 'metacognition'.

[3] Some philosophers adopt the radical position that there is no such thing as knowledge at the top of the scale [0, 1]. What goes there is just full belief. See for example Jeffrey, 1992, especially chapter 3 in which the idea of probable knowledge is eschewed in favour of full belief. This rejection of the traditional idea of knowledge can give no comfort to postmodernists, constructivists and the like who also reject the idea of knowledge. Jeffery is a strong advocate of the notion of *rational* degrees of belief of the sort set out in the text to which they also give no credence.

[4] For some of this literature see Ramsey 1990, chapter 4, or Jeffrey 1992 and Jeffrey 2004, chapter 1.1.

[5] See Dummett (1978), chapter 1 'Truth', in which Dummett adopts the above formulation of the realism that lies in the background of all formulations of the correspondence theory of truth that do not beg questionable correspondence relations such as copying, reflecting, etc.

[6] For an evaluation of the claims on behalf of such dubious sources to provide not only belief but also knowledge, see Hospers 1970, chapter 2 Sections 7 and 8. See also Popper 1963 'On the Sources of Knowledge and Ignorance' for an account of the limitations on all sources that do not employ the canons of critical examination.

[7] See Russell 1991 for the persistent myth, invented in the nineteenth century, that sailors up to the time of Copernicus though the Earth was flat. They all knew it was spherical. What is of interest here is how this myth has come to pervade education. Where Columbus differed from most others was in the much smaller size of the Earth adopted by him. In this respect Columbus was wrong about how far China and India were from Europe, but lucky in that a large landmass lay between Europe and East Asia at roughly the right distance he believed (wrongly) to obtain between Europe and Asia.

[8] The details set out above concerning Eratosthanes' investigation can be found in Kuhn 1957, in the 'Technical Appendix Section 4, Ancient Measurements of the Universe'.

[9] Readers interested in inductive-statistical explanations can consult Hempel 1965, part IV.

[10] For purposes of brevity, we will use 'law' and 'law statement' (or the statement of law) interchangeably from here on since the context makes clear which one is meant. It should be noted, however, that strictly speaking they are different. A law statement is a linguistic expression of a law of nature; a law of nature is a certain kind of state of affairs in the world that typically manifests itself as a regularity, or uniformity. See also Section 6.5.

[11] For the purposes of simplicity, we assume that g is constant and that there is no air friction. These are idealizing assumptions that play an important role in science; see chapter 10.

[12] On the matter of the ultimate end of scientific explanation see Sober 1991, lecture 7.

[13] A notable exception to this is van Fraassen (1980) who takes pragmatics as essential to scientific explanation. For a critique of this view see Salmon 1984, especially chapter 4.

[14] For one account of alleged differences and similarities between models of explanation and understanding, and their application in difference sciences, see von Wright 1971, especially Chapter 1. For the many models of explanation that can be employed in the social and

historical sciences that relate to this issue, see the excellent collection by Martin and McIntyre (eds.) 1994.

[15] There are, of course, exceptions. There are several science educators who build both justification (evidence, reason) and truth into their conception of knowledge. See especially Siegel 1998 and also Matthews 1994.

CHAPTER 3

PLATO ON KNOWLEDGE, AND A SOCRATIC MODEL OF INQUIRY

In Chapter 2 the classical definition of knowledge was set out and some of its consequences were explored. We will defer examination of the theory behind the model until the next chapter. In this chapter we will consider a similar account of knowledge, first set out by Plato in his dialogue *Meno*, and the model of critical inquiry that he also develops.[1] Here our focus is entirely on the account of propositional knowledge *that* p found in the *Meno*.

In most of Plato's dialogues Socrates appears as the questioner of other people's beliefs about matters such as piety, courage, justice, virtue, rhetoric, knowledge, and so on. His procedure, outlined in the next section, provides a model of what is commonly known as the 'Socratic method'; it has a central place in any theory of critical inquiry. Socrates is the person who *par excellence* wishes to engage in critical inquiry as he tells us in the dialogue *Gorgias* (457e-458a):

> I'm worried about subjecting your views to a thorough examination, in case you assume that the target of my argumentativeness is you, when all I really want to do is clarify the facts of the matter. If you're the same kind of person as I am, I'd be glad to continue questioning you; otherwise, let's forget it. What kind of person am I? I'm happy to have a mistaken idea of mine proved wrong, and I'm happy to prove someone else's mistaken ideas wrong, I'm certainly not *less* happy if I'm proved wrong than if I've proved someone else wrong, because, as I see it, I've got the best of it: there's nothing worse than the state which I'd been saved from, so that's better for me than saving someone else. You see, there's nothing worse for a person, in my opinion, than holding mistaken views about the matters we're discussing at the moment. Anyway, if you tell me that you and I are alike in this respect, then let's carry on talking; but if you think we'd better forget it, then let's do so and call a halt to the discussion right now. (Plato 1994, pp. 21-2)

It is evident from this passage that, according to Socrates, the goal of critical inquiry is to discover the truth and eliminate error (cf. Section 6.3.2). If this goal is not shared, then, Socrates warns, critical inquiry should be abandoned. The context of Socrates' remark is important. In the *Gorgias* Socrates is combating the idea that rhetoric could ever reliably achieve this goal; since rhetoric can only rely on persuasion to achieve its goal, it has no place within critical inquiry. The passage above also draws attention to the important point that, in conducting critical inquiry, if he, Socrates, (or anyone else), shows that someone has a mistaken view, then this can either have a good or a bad effect. The good effect is that if someone accepts the criticism, then they can eliminate a false belief. And they can do this without

necessarily either discovering what correct belief they ought to hold, or filling the vacuum created by belief elimination with some other belief that has not yet been subject to critical evaluation. The bad effect is that the person who has had their belief criticised resents this and turns against their critic (as the Athenians later turned against Socrates – see *Apology* 23a-e). What is important about Socratic inquiry is that it can be carried out by anyone at anytime regardless of culture, social standing, system of belief, or whatever. It employs only the rationality we all possess.

In Section 3.1 we present Socrates' method as a model of critical inquiry. In Section 3.2 we briefly discuss Plato's conception of knowledge in order to show that the standard definition of knowledge as justified true belief goes back to the Antiquity. In the final section we relate the Socratic model of inquiry to learning. In the *Meno* there is a special episode with a Slave Boy in which Socrates gives a display of how the methods of critical inquiry can be adapted to the situation in which a person comes to learn, or discover, what is the correct answer to a problem, in this case a problem in geometry. Socrates plays a non-didactic role as a teacher, guiding the Boy at certain points towards an answer that he arrives at largely by himself. Attentive readers will not fail to notice that Socrates exemplifies what is best in the constructivist approach to learning and teaching while avoiding, at the same time, some of its less satisfactory aspects. This will become clear in Chapter 5 when we discuss the constructivist pedagogy in detail. What Socrates' episode with the Slave Boy shows is that the good aspects of a constructivist approach can be combined with a model of critical inquiry; there need not be an abandonment of critical inquiry in Socratic constructivism.

3.1 A MODEL OF SOCRATIC INQUIRY

Most of Plato's Socratic dialogues exhibit the method of Socratic critical inquiry, though with varying degrees of application. In the *Symposium* speeches are given about the nature of love with only one short episode of Socratic critical examination. In contrast, a dialogue like the *Meno* has Socrates relentlessly engaging in critical examination. In yet other dialogues (such as much of the *Republic*) there is a lot of almost sycophantic 'yes' and 'no' in reply to Socrates that is not always in response to critical examination. So one needs to be careful to ensure that critical examination is taking place rather than the mere appearance of it. What is Socrates' critical method? Just a few of its central features can be mentioned here.

An important contrast is drawn in the Meno between *eristic* and *dialectic* that needs to be examined as it is one of the central features of the Socratic method. In a more literal translation of a passage from the *Meno*, Gilbert Ryle uses these two technical terms, saying:

> If my questioner were a professor of the eristic and contentious sort I should say to him: I have made my statement; if it is wrong, it is your business to examine and refute it. But if, like you and me on this occasion, we were friends and chose to have a discussion together, I should have to reply in some milder tone more suited to dialectic. The more dialectical way, I suppose, is not merely to answer what is true, but also to make use of those points which the questioned person acknowledges that he knows. (*Meno* 75C-D, Ryle 1966, pp. 127-8).

The term 'eristic' derives from the Greek for a contest or battle, particularly the battle of intellects; in contrast the term 'dialectic' has its origins in the Greek, both for language or discussion, and for debate or skilled logical argument. Though they have features in common, they mark an important difference between two kinds of argumentation. Eristic battles were played out as part of a young man's education. As Ryle (1966, chapter 4, Section 2) describes them, eristic duels took place in the form of a question/answer contest concerning some thesis (such as, justice is in the interest of the powerful). One person, the answerer, could only answer 'yes' or 'no' to a question put by another, the questioner. The goal of the questioner was to drive the answerer into a contradiction on some thesis as the subject matter of the duel; and the questioner had to do this within a set time. In this contest there can then be a winner and a looser; either the answerer escapes being driven into a contradiction within the set time, or the questioner successfully drives the answerer into a contradiction. Roles can readily be swapped and the same duel played out on the same thesis by the same people; or the duel can take place over the negation of the thesis (i.e., justice is not in the interests of the powerful), or over some other thesis.

As useful as such intellectual duels may be (e.g., in moots or courts of law), they are hardly part of anyone's education these days. Ryle (*loc. cit*.) lists several reasons, cited by Aristotle, as to why the young should engage in such eristic tournaments as part of their education. First, it is an important gymnastic exercise in which pupils can train and sharpen their wits. Second, being defeated in such an eristic contest can puncture one's complacency and one might be more willing to look for better ways of defending one's views, or modifying or abandoning them. Third, it is an absorbing, exciting and competitive game to watch, or in which to engage. Fourth, there can be champions who win in public contests and to whom others might go to get the best training in eristic duelling. Fifth, given the nature of Athenian public political life, and the growth of a legal profession, all need to have some training in eristic in order to be participatory citizens, or at least not be duped by those more successful in public eristic.

Finally, people may engage in such an exercise for the interest in the conceptual issues being examined. Such an eristic procedure seems well suited for conceptual matters that cannot be settled by empirical methods of investigation. In fact it seems hard to see what method might be used for the

examination of matters that an appeal to experience cannot resolve. It is here that eristic becomes a tool of philosophical investigation into concepts or definitions for which many want an answer, such as 'What is the nature of justice?' (for example, is it what is in the interests of the powerful or not?). In fact eristic becomes a means in which any of Socrates' 'What is X?' questions can be answered, where 'X' can stand for matters such as piety, courage, love, justice, rhetoric, knowledge, and so on. It is here that the common Greek public practice of eristic merges with what Plato called 'dialectic'.

The word 'dialectic' has several uses in Plato depending on whether one considers his earlier as opposed to later dialogues. To add to confusion the term was adapted by German philosophers from Kant onwards to mean several quite different things. Here we will focus on the nature of dialectic in so far as it is exhibited in those dialogues of Plato, such as the *Meno*, in which the best aspects of eristic are incorporated into dialectic. Socratic dialectic uses the same question/answer technique of eristic but less rigidly. More significantly the goal of dialectic is to get at the truth rather than score victories over another. What eristic can do is provide a method for uncovering contradictions in one's overall views on some matter. Eristic ensures that one's overall views are consistent and contradiction-free. When a contradiction arises, as in the case of the Slave Boy's first two answers to the geometrical problem Socrates poses to him (Section 3.3 below) then there appears to be no way forward. This is an example of a Socratic *aporia*, which literally means 'no way out'. Here an intellectual impasse has been reached, and some way needs to be found to get around it. Most of us cannot remain in a state of *aporia* for long about our intellectual problems and seek a way out by examining other hypotheses, or by taking on other views that have so far escaped eristic contradiction. What eristic can do is at least to ensure that our beliefs, as far as can be determined, are consistent – no small matter when no other method of investigation seems available.

But consistency is not enough; what we wish to aim for is truth. So the goal of dialectic is to get at the truth. But it does so indirectly by a negative pathway. The Socratic path of dialectical examination is littered with beliefs that have been shown false. At each step along the dialectical path, an earlier refuted belief is, hopefully, replaced by a later better one that overcomes the problems that faced the earlier belief. But it, too, must be subject to eristic examination to ensure that it contains no error. If a further contradiction is revealed, then the second belief needs to be replaced. Perhaps at this point one is in a state of *aporia* and no new third proposed belief is available for discussion. Some Socratic dialogues simply end with everyone in a state of *aporia*, since no one is able to come up with a way of overcoming the difficulties exposed in their views. But if a further proposal becomes

available, then the procedure of Socratic dialectical examination can begin over again.

There are two conclusions that can be arrived at by a repeated application of eristic (driving to a contradiction). Either the inquiry ends with all the disputants in a state of *aporia* and there is no satisfactory answer to the original question; or a new proposal has been made and after critical examination no contradiction or other difficulty can be found concerning it. Does this second alternative mean that the new proposal is correct? No, it does not. Only after persistent attempts to critically evaluate it could one safely say that it is something to which we might give tentative acceptance. But it would not be correct to say that we have proved definitively that the proposal is correct. The dialectic method, based in eristic, cannot do that; it is not powerful enough to show truth, but it can expose error. So, can there be progress in the examination of our beliefs?

Yes, one can remain optimistic about progress in the Socratic examination of our beliefs. One kind of progress is made when a belief is refuted. One thing we know is that the belief is wrong. And that may well be a satisfactory thing to know. We are not to be dogmatically content with an unexamined belief for it may habour serious difficulties and contradictions. This is something that Socrates regarded as important. As he says in the remark from the *Gorgias* cited above, he is in a much better state when a belief of his has been refuted, and he knows this, than when he entertains beliefs that are faulty but their faults have not been bought to his attention. A second kind of progress is made when we have a belief that we have not refuted even though we have subject it to eristic examination. Here we have a belief which has, so far, passed our best methods of examination; so it might be worthy of our assent – at least for the time being until further examination shows it to be defective in some way. The conclusion to be drawn is that, either with or without refutation, critical examination using eristic methods (or any other method for examining beliefs, if there is a reliable one available) can yield progress. Inquiry makes progress when it exposes error, or when a belief survives – so far – attempts at eristic refutation. It is the added notion of the goal of truth, or at least that of error elimination, that turns unpromising eristic duelling for mere victory over an opponent into the important method of dialectic used to examine our beliefs in order to have a victory over our ignorance.

How does Socrates use eristic methods for dialectical ends? This is illustrated in diverse ways in many of the Platonic dialogues in which he is the central questioner. Here we can discuss only a small section of one dialogue, and briefly. The example comes from the investigation of the notion of courage in the dialogue *Laches* (which is so named after a prominent Athenian general). The first third of the dialogue is taken up with

a discussion of whether certain skills are worth teaching to young men. The skill in question is (friendly) fighting in armour. We often ask a related question: is it good for people to engage in sports or not? One common set of answers is that it is at least exercise, it builds the body and it develops skills; but importantly it builds character and enables one to deal with defeat or victory in a game. All of these contemporary answers can be found in the *Laches*. But others as well. Since the Athenian state was often at war, the young were to be trained early in some skills that could benefit them in the future as citizens who must defend their state. As some say, military training is also character building as well as training for specific military tasks. But what sort of training of the character would learning to fight in armour provide for the young Athenian? The answer is that it leads to a number of human excellences, and one in particular: courage. You may or may not agree with some of the points just made; and they are contested in the first third of the dialogue. We will focus on the remainder of the dialogue where an attempt is made to answer the question 'What is courage?' And we will consider just a few suggestions and their dialectical examination.

The general, Laches, begins with the definition: a man is courageous = the man does not run away but stands at his post and fights against the enemy (*Laches*, 190E). Socrates drives him into contradiction in the following way. He gets Laches to admit both: (i) many of the following actions are courageous, but (ii) they do not fit his definition. His definition is too narrow because it does not capture the following cases. There are Scythian horsemen who are admittedly courageous in battle but do not stay at the one place, but ride their horses back and forth in battle. This is a quite literal attack on Laches' definition which requires one to *stand* at one's post; other counterexamples are not so literally intended. Laches also admits that sailors can be courageous at sea in a storm, others are courageous in politics, or in surviving in adversity and poverty, and so on. None of these fall under his definition. Not being bereft of further ideas he proposes another that is meant to capture all the above examples ruled out by his first too narrow definition. His second proposal (*ibid*., 192 b-d) is: courage is an endurance of the soul (or a persistence in the face of a difficulty).

Socrates does not himself make the second proposal; someone else does. He can still claim that he is ignorant of the nature of courage; he does not *know what* courage is. But given that there is a new suggestion on the table, he subjects it to the one thing he does know, viz., he *knows how to* critically examine any proposed idea. His response is quick; he gets Laches to admit that his definition is to broad because it allows cases of foolish endurance. What would such a counterexample be? A modern example might be a person who puts up with the loud music from one's neighbour for hours on end without asking for it to be turned down. Endurance all right; but foolish

when something can be done about it (let us assume the neighbour is friendly). And if the person never makes the request one might conclude that they are rather weak or even cowardly. At this juncture Socrates does not proceed by counterexample. Rather he tries a different tack to drive Laches into contradiction. He gets him to admit that courage is a noble thing while foolish endurance is ignoble and even hurtful. The upshot is that Laches is forced to admit that some acts of endurance of the soul are really ignoble and hurtful. So his definition cannot be satisfactory.

On the basis of Socrates' point against him, Laches makes a third suggestion: courage is a *wise* endurance of the soul. Socrates now exercises his critical abilities on this definition showing that it is both too wide and too narrow. What would Laches say of the doctor who proposes a cure for an ailment that requires the patient to, say, avoid taking any drink. The doctor wisely endures the pleas of the patient for water. But is the doctor being courageous? This fits the definition but it is agreed by Laches that the doctor is not being courageous at all; at best he is simply doing his job. Socrates then gets Laches to consider the case of two men in battle; one undergoes training so that he develops battle skills, while the other has had no training but when confronted with a marauding enemy takes up arms and defends himself and his family and property (possibly to death). The question is: who is the more courageous? If one admits that the second person is more courageous, as Laches does, then a contradiction arises between the definition and features of the example. The second man has been unwise in not getting training and might even thereby be careless or reckless rather than courageous. In contrast, the first man has been wise in getting training so that battle is almost second nature to him, but he is said not to be as courageous as the second.

Another similar counterexample is that of an expert swimmer who dives into the water to save a drowning person in contrast to a person who is not very good at swimming but who also jumps into the water. Who has the greater courage? The first has wisdom in their endurance in the sense that they have a skill through training; but this lessens the grounds on which one might say that they are courageous because swimming is no problem for them. On the other hand the person who has little swimming skill, but who jumps in, might be said to be courageous; but their endurance is unwise. Perhaps they are foolish, heedless or even reckless. What one needs at this point is a conception of courage that distinguishes it from recklessness; but one is not available. At this point Laches recognises that his third definition is a failure. But he has run out of suggestions and is in a state of *aporia*.

The dialectical encounter with Laches has run its course and he drops out, with another general, Nicias, making further suggestions. But he, too, falls foul of Socratic questioning. (We leave the reader to follow up Socrates'

encounter with Nicias.) And there the dialogue ends. Have we learned nothing and the encounter with Socrates has been unproductive? Certainly there is mind-numbing *aporia* all round at the end. But there is a lot that has been learned. Many cases of courage (or the lack of it) have been canvassed and agreed upon, several important distinctions have been drawn, and even though the various definitions of courage have been refuted some progress has been made in clearing away wrong conceptions and in zeroing in on the conceptual area where further discussion ought to take place.

Plato does not feel that in the dialogue he has to answer all possible questions that might be raised; that is up to us using the methods he has suggested in the dialogue (and other methods as well). As outsiders we might feel that had we been present we might have done better. If so, then do better! All the issues raised in the dialogue are still under active consideration. Moreover they have to be engaged in by each new generation of young people in order that they become intellectually mature. Not only do we want a rich enough conception of, say, courage as part of our understanding. It is also part of the training through such dialectical examination of an idea that we develop the intellectual abilities necessary for a properly conducted investigation, and the attempt to make a positive contribution to any discussion. After all Socrates did think that we all had the ability to do this, and in the right circumstances can use the methods of critical inquiry for intellectual ends.

Though this is a book about education in science, the above comments on courage in the *Laches* are not entirely out of place. The dialogue can serve as model for discussion in a class about the nature of courage, something that all might wish to know about. But the dialogue also moves at another level, that of philosophical method. More reflective students can also think not just about the nature of courage itself, but also the methods whereby they are making their critical points – and this does genuinely take students into the domain of critical inquiry. The Socratic dialogue is often an excellent educational tool that can be used as a primer for the investigation not only of a substantive topic (e.g., courage), but also the methods of critical inquiry to be employed in any discussion.

3.2 KNOWLEDGE AS "TETHERED" TRUE BELIEF

Plato's Socrates begins his discussion of the difference between belief and knowledge in the *Meno* in the following way:

> Socrates: If someone knows the way to Larissa, or anywhere else you like, then when he goes there and takes others with him he will be a good and capable guide, you would agree?
> Meno: Of course.
> Socrates: But if a man judges correctly which is the road, though he has never been there and doesn't know it, will he not also guide others aright?

> Meno: Yes, he will.
> Socrates: And as long as he has a correct opinion on the points about which others have knowledge, he will be just as good a guide, believing the truth but not knowing it.
> Meno: Just as good.
> Socrates: Therefore true opinion is as good a guide as knowledge for the purpose of acting rightly. That is what we left out just now in our discussion of the nature of virtue, when we said that knowledge is the only guide to right action. There was also, it seems, true opinion.
> Meno: It seems so.
> Socrates: So right opinion is something no less useful than knowledge.
> Meno: Except that the man with knowledge will always be successful, and the man with right opinion only sometimes.
> Socrates: What? Will he not always be successful so long as he has the right opinion?
> Meno: That must be so, I suppose. (Plato, Meno 97A-98A; Plato (1956), p. 153)

To illustrate, suppose that you are standing at an intersection of five roads but there is no sign that indicates the road to the town of Larissa. Which road should one take? Decide by tossing a coin, or a die? See which way the clouds are moving? None of these are reliable for picking the correct road. But suppose you are standing there with another person, a fellow traveller, who has been at the intersection before, and who, after following, say, Road #3, got to Larissa, and who also remembers correctly their past success. That person would *know that it is Road #3* on the basis of experience. Such past experiences, and current memory of them, are usually highly reliable ways of forming true beliefs about which is the correct road. Since the *belief* is formed in a *reliable* way a person can be said to *know that* #3 is the correct road.

Socrates does not argue for exactly this point in the above passage. Though it is not made explicit, it would be agreed that a false belief about which road to take would not get one to Larissa. In contrast a true belief would. The point the above exchange makes is that following a *true belief* that it is Road #3 will achieve exactly the same success (viz., arriving in Larissa) as would *knowledge* that it is Road #3. So what important extra does *knowledge* that it is Road #3 have over a mere *true belief* that it is Road #3?

There is a hint that getting beliefs that are true is a rather chancy matter. The beliefs one acquires could well have been false. Compare this with the toss of a fair die and getting a six. There is one chance in six of getting the highest number. But sometimes one gets lucky and one does toss a six! Similarly, the processes that lead one to form a belief could be chancy. There may be no guarantee that the process is reliable for the truth. But one can still get a true belief – even though the chance that it does yield truths is low.

The situation with knowledge is different. Knowledge arises when the belief forming processes are highly reliable (100%, or near) for the truth. There may be belief forming processes that are much less reliable for the truth. Nonetheless, as unreliable as they are, they can, and do, produce true

beliefs on some occasions. However they will not produce knowledge because of their low reliability. Importantly this difference is not to be cashed out in terms of the success that follows from acting on a belief that is true; both a true belief that p and knowledge that p can be equally successful when acted upon. Rather the difference is to be cashed out in terms of how the belief arises in the first place. The requirement on knowledge is that belief arises from a process highly reliable for the truth, and not an unreliable process. Granted this, it will follow that both knowledge and true belief will yield a truth; and success in action in either case is guaranteed.

In the above we have talked of reliable and unreliable processes of belief formation as a way of distinguishing between a true belief and knowledge. In the *Meno* Plato does not spell out his position in this way. Rather he uses a metaphor of beliefs being akin to the life-like statues of the sculptor Daedalus that are liable to run away rather than stay stationary. To hold them down one must tether them. True beliefs will stay in the mind if they are tethered, i.e., got by a reliable belief-forming process. And this is knowledge. But if the true beliefs are not got by a reliable process then they are likely to run away since they are untethered by a reliable process. Continuing the above quotation Meno says:

> Meno: In that case, I wonder why knowledge should be so much more prized than right opinion, and indeed how there is any difference between them.
> Socrates: Shall I tell you the reason for your surprise, or do you know it?
> Meno: No, tell me.
> Socrates: It is because you have not observed the statues of Daedalus. Perhaps you do not have them in your country.
> Meno: What makes you say that?
> Socrates: They too, if no one ties them down, run away and escape. If tied, they stay where they are put.
> Meno: What of it?
> Socrates: If you have one of his works untethered, it is not worth much; it gives you the slip like a runaway slave. But a tethered specimen is very valuable, for they are magnificent creations. And that, I may say, has a bearing on the matter of true opinions. True opinions are a fine thing and they do all sorts of good so long as they stay in their place; but they will not stay long. They run away from a man's mind; so they are not worth much until you tether them by working out the reason [*aitias logismos*]. That process, my dear Meno, is recollection, as we agreed earlier. Once they are tied down, they become knowledge, and are stable. That is why knowledge is something more valuable than right opinion. What distinguishes one from the other is the tether. (Meno 97D-98A; Plato (1956), pp. 153-4)

For the time being set aside the last remarks about recollection (to which we will return). Plato bequeathed to epistemology the problem of the tether which, when properly understood, marks the difference between mere true belief and knowledge. Knowledge is *tethered* true belief; untethered true belief can slip away or run from our minds unlike knowledge that is tethered, stays in the mind or is stable and does not run or slip away. True beliefs can arise in the mind in ways that have nothing to do with what makes the belief

true (such as a fact or state of affairs), for example by hypnotism. As such they are chancy and formed in a quite unreliable way. We get to the truth by a lucky accident while not even knowing that we have got it accidentally, for example in the way as benign hypnotist decides to put only true beliefs into our minds when we are in a trance. But knowledge is different; we get it by processes that are not chancy but are reliable for the truth. Reliable belief forming processes are the tethers that produce stability in our beliefs akin to physically tied down statues.

With some retrospective hindsight we have used the idea of belief-forming processes that are reliable for the truth as an account of Plato's metaphor of "tethering". This may not initially appear to be an entirely correct way of understanding how Socrates draws the true belief/knowledge distinction.[2] But tethering is said to be 'working out the reasons' or *aitias logismos*, *aitia* being cause or more generally explanation or reasons, and *logismos* being calculation. The whole expression could also be rendered as 'reasoning out the explanation', or even 'giving a chain of proof'.[3] In the light of this, reasoning processes are paradigm cases for tethering our beliefs. Moreover when they are properly employed, reasoning processes are highly reliable for producing truths;[4] they do not yield true beliefs in chancy ways.

Examples of forming true beliefs in reliable ways, and thus of knowledge, have already been given. In Section 2.4.3 our knowledge of the curvature and the size of the Earth turned precisely on the possession of evidence, and of reasoning from the evidence to these pieces of knowledge. Reasons and reasoning are clearly one of the paradigm ways in which reliable processes lead to true beliefs, and thus knowledge. Reliability for the truth then becomes an important feature of the principles employed in scientific method and in critical inquiry. It would be an important part of any reflective theory of method and inquiry to *show* that the principles we use are reliable for the truth (a matter raised in Chapter 6).

The example of knowing the road to Larissa might appear to be of a different character in that it does not involve much in the way of working out reasons. But different processes are used which are also reliable. Here the reliability is based on two things: (i) previous success in using one of the roads to get to Larissa, and (ii) a memory of this. (i) is a brute fact which has been discovered by practical experience to be so by some traveller. (ii) is a current memory of this previously discovered brute fact. A well-working memory is generally a reliable process in that for earlier input of truths it gives, when we recall from memory, the same truths. (Because our memories generally work well its outputs will be true if its inputs are true.) Even in this case Plato's metaphor of "tethering" beliefs, understood as reliable processes for the formation of true beliefs, still holds.

3.3 LEARNING AND PLATO'S SOCRATIC MODEL OF CRITICAL INQUIRY

Down the ages the works of Plato have been an important source for educationalists. The *Meno* provides us not only with the first attempt at a definition of knowledge as opposed to belief but also, in Socrates' encounter with the Slave-Boy late in the dialogue, a model for pedagogy that is non-didactic and thus important for both constructivists and non-constructivists alike in education. The *Meno* begins with a question in ethics: 'What is the nature, or definition, of *virtue*?' A number of different considerations are canvassed. But quickly the dialogue shifts to a different level and asks methodological questions such as 'what counts as giving an adequate definition?' The *Meno* gives us the beginnings of a theory of what it is to understand what something is, in the sense of providing a real definition, as opposed to a stipulative definition. In the dialogue attempts are made at satisfactory real definitions of, for example, *virtue*, *colour*, *shape*. But this then raises a further question: 'How do we know, when we say, for example, that X (e.g., virtue) = A&B&C, that this is the correct answer?'

The participant in the dialogue, Meno, offers Socrates a tricky conundrum that he picked up from a sophist (see *Meno* 80D and 81E; Plato (1956)). It applies to all inquiry that attempts to answer any 'What is X?' question; it claims that inquiry is either superfluous or impossible. The argument can be summarised as follows:

(1) If person P knows what X is then P's inquiry into X is superfluous;
(2) If P does not know what X is, then it is impossible for P to inquire into X;
(3) Either P knows what X is or does not know;
(4) Therefore, inquiry into X is either superfluous or impossible.

The argument is valid: but is it sound? The crucial premise is (2). If correct it would be fatal to all inquiry in that it could never get off the ground. One consideration on behalf of (2) is that if we genuinely do not know what X is, then even if we hit on the right answer, say that X = A&B&C, we are not able to recognise that this is the correct answer. In the light of what we have already said, what we need is compelling evidence, or reason. And this has to be so compelling that it shows to us that it is the correct answer – something that might be hard to achieve.

Socrates does not challenge premise (2) by showing that it is based on faulty considerations. Rather, he provides a counterexample to it. He talks to a nearby Slave Boy and shows that it is possible for him to inquire into some matter and recognise that several answers are wrong and that a further answer is the right one; and the Boy can do this even when it is clear that before the inquiry started he did not know anything about the matters subsequently discussed. Thus it is possible to inquire into what one does not know. So the

above argument is refuted. The refutation involves further methodological points, this time about the nature of knowledge and of learning.

So let us pick up the dialogue at the point where Socrates is faced with Meno's conundrum, and refutes premise (2) with the counterexample of the Slave Boy who does come to know something through inquiry that he did not previously know. What we will consider is an overview of the *Meno* 82B to 86A and what it tells us about learning, education, knowledge and critical inquiry.

Socrates makes it clear at the beginning that the Boy does not know the answer to a 'what is X?' question. In this case it is a geometrical question: 'What is the length of the side of a square double the area of a given square the side of which is 2 metres long?'. After a process of critical examination, the Boy comes to know what is the correct answer (it is the diagonal of the given square). Like any good teacher, Socrates starts with what the Boy already believes, or thinks he knows. Here Socrates is in agreement, as any commonsense person would be, with constructivists about the starting point of his encounter with the Boy. However unlike nearly all constructivists the starting point is not with what the pupil *knows*. This is often a mischaracterization. On the whole the starting point is with what the pupil *believes*. And in this case of the Boy one of the beliefs is, unfortunately, false. Like constructivists Socrates is non-didactic and does not tell the Boy that he is wrong. But he does attempt to get him to see that he is wrong by other means.

Initially the Boy thinks that the answer is double the side of the given square, i.e., 4m. Using a question-answer method, Socrates gets the Boy to work out the areas of the two squares whose sides are 2m and 4m long; that is the Boy shows that they are, respectively, 4 square metres and 16 square meters. So 4m cannot be the right answer. Another answer seems plausible to the Boy, viz., that the length is 3m; but in the same way Socrates gets him to see that the answer is wrong. While doing this Socrates emphasises his non-didactic approach towards the Boy's thinking about the geometrical problem when he says to Meno: 'You see Meno that I am not teaching [telling] him anything, only asking' (84E). Socrates does not *tell* the Slave-Boy that his answer of 4m is wrong; rather through the question-answer method the Boy *comes to realise himself* that his answer is wrong. Socrates' approach to the Boy's learning that his answers are wrong is definitely non-didactic, even anti-didactic.

Often teachers strike such unfortunate starting points: 'college students often have developed beliefs about physical and biological phenomena that fit their experience but do not fit scientific accounts of these phenomena. These preconceptions must be addressed in order for them to change their beliefs' (Bransford *et. al*. 1999, pp. 10-11) But how addressed? This is a

crucial matter for all accounts of learning that aim at correctness, including constructivism – providing that it is not so radically understood that truth is no longer an aim of learning. Bransford *et. al.* continue saying:

> A common misconception regarding "constructivist" theories of knowing (that existing knowledge is used to build new knowledge) is that teachers should never tell students anything directly but, instead, should always allow them to construct knowledge for themselves. This perspective confuses a theory of pedagogy (teaching) with a theory of knowing. (*ibid*., p. 11)

We can agree with much of this – except in our view the term 'knowledge' is wrongly used in place of belief. If what the students construct is not in accordance with principles of inquiry then at best they construct beliefs. This gives point to the earlier remark of Bransford *et. al.* that what students often believe is not in accord with science, which is (on the whole) knowledge obtained by critical inquiry rather than student "constructions" of beliefs. Interestingly Bransford *et. al*. go on to add: 'there are times, usually after people have first grappled with issues on their own, that "teaching by telling" can work extremely well' (*loc. cit*.). Thus there are times when it is appropriate to be didactic; a teacher can tell students the right answer once they have made (unsuccessful) attempts to find the right answer. If correct, then this does count against one of the central tenets of most versions of constructivism. But again, as we will show, mere telling cannot provide knowledge. It merely gives true belief – a point already emphasised in Plato.

Returning to the dialogue, Socrates does not resort to "teaching by telling". He still has sufficient pedagogical techniques up his sleeve to get the Boy to see that he is wrong without telling him anything. Socrates still adheres to the tenets of constructivism, but entirely in a framework that employs the principles of critical inquiry. It now remains to see how the Boy finally gets to the correct answer, and to see that it is the correct answer. But even here Socrates would not say that the Boy has knowledge. A further step is to be made – from hitting upon a true belief to converting it to knowledge. Here Socrates distances himself from a misunderstanding common amongst many constructivists; they fail to observe the difference between true belief and knowledge.

After two wrong answers the Boy becomes perplexed; he does not know what other answer might be correct and, moreover, he knows that he does not know. He is in a state of *aporia*. Socrates turns to Meno and says:

> Now notice what, starting from the state of perplexity, he will discover by seeking the truth in company with me, though I simply ask him questions without teaching him. Be ready to catch me if I give him any instruction or explanation instead of simply interrogating him on his own opinions. (84 C-D: Plato (1956)).

Since the Boy has run out of suggestions Socrates gives him a hint about what geometrical move he should try next. Socrates does not tell the Boy the answer. Rather he helps the Boy by making a further third suggestion of his own. Once the Boy takes up this suggestion he is then invited by Socrates to

exercise his reasoning capacities and draw some consequences from it. In this way the Boy gets to the correct conclusion, viz., the diagonal of the given square of side 2m. Does Socrates ask 'leading questions'? This is not necessarily the case. What Socrates offers the Boy is a hint as to what further suggestion he might follow up. It is not necessary that Socrates knows that this is the right hint to try. But one can assume that in the dialogue it is intended that Socrates does know (as would teachers instructing pupils). But Socrates could equally as well not know and simply provide a suggestion to see whether it is correct or not. Socrates could be in exactly the same position as the Boy and not know. In this respect he could be in the same position as the first person who discovered the correct answer; they did not know that this was the right hint until they proved it to be so.

There are many matters that this episode raises. Consider first a pedagogical matter. Socrates asks Meno: 'Has he, the Slave Boy, answered with any opinions that were not his own'. The reply is 'No' (85B). At each stage of the questioning process the Slave-Boy has acquired, or, if you like the metaphor has 'constructed', for himself the reasons that show that his first two answers are wrong and that the third answer is correct. He has done this with minimal assistance from Socrates, and by largely drawing on his own abilities to reason (an ability which was not taught to him but, like the rest of us, he does possess). In being opposed to didactic teaching and learning of reasons that underpin knowledge, Socrates and Plato are the first constructivists in education. This is part of the quite plausible core of constructivism that few would deny. But, as will be argued, even though Socrates and Plato could admit a 'constructive' element in arriving at the reasons that eliminate false belief and turn true belief into knowledge, they do not endorse a constructivist account of the nature of knowledge itself.

The second point concerns knowledge directly. Socrates comments on the correct answer given by the Slave-Boy:

> At the present these opinions [that the answer is the diagonal] being newly aroused, have a dream like quality. But if the same questions are put to him on many occasions and in different ways, you can see that in the end he will have a knowledge on the subject as accurate as anybody's. … This knowledge will not come from teaching but from questioning. He will recover it for himself. (85C-D; Plato (1956)).

In Socrates' view, students do not acquire knowledge through picking up bits of (true) information didactically conveyed to them. Even being led through a question-answer session does not provide, by itself, knowledge; at best the process can only leads pupils to the correct belief. Only when they can go through the steps of reasoning by themselves and thereby make fully explicit to themselves the reasons for the correct answer will they have knowledge. Re-expressing this more metaphorically, only by 'constructing' for oneself the reasons for a true belief can one acquire knowledge. (Of course this does not show that no *belief* whatever can be acquired through telling someone

something or conveying information to them; rather, in this case and many others in science and mathematics, discovering the reasons for something is an important necessary condition for acquiring *knowledge* as distinct from belief.) Grasping this important point requires that the notions of belief and knowledge be understood to be quite distinct – something not commonly grasped in science education.

This leads to the third point. Socrates' answer to the initial conundrum posed by Meno is that we can recognise that we have knowledge when we have satisfactory reasons for the truth of what we believe. This is spelled out in the account of knowledge as the "tethering" of belief by working out the reasons:

> True opinions are a fine thing and do all sorts of good so long as they stay in their place; but they will not stay long. They run away from a man's mind, so they are not worth much until you tether them by working out the reason. … Once they are tied down, they become knowledge, and are stable. That is why knowledge is something more valuable than right opinion. What distinguishes one from the other is the tether. (97E-98A; Plato (1956))

Surprisingly, Plato's tether of reasons goes almost unmentioned in constructivist accounts of knowledge; merely constructing beliefs is often regarded as sufficient for knowledge. By omitting the third Evidence Condition for knowledge constructivists omit a crucial condition for knowledge, viz., that knowers themselves are the possessors the evidence or reasons.

The fourth and final point is that Socrates insists that one of the few things he knows is that knowledge and true opinion are different (98B). However the account of knowledge given in the *Meno* is also bound up with a theory of knowledge as the recollection of what has been implanted in our immortal souls. This is not something that we need endorse; nor does Socrates endorse it fully. Even though he expounds this doctrine Socrates says 'I shouldn't like to take my oath on the whole story' (86B) – and most commentators would argue that he is right to be cautious. However the recollection story can be reconstrued as a first primitive account of either innate belief or of *a priori* knowledge. (Here '*a priori*' has its traditional sense of 'known to be true independently of any appeal to experience'.) The Slave-Boy, like all pupils, must have at least some innate capacity to reason about geometrical problems since he has not been taught either how to reason or how to recognise Plato's tethering reasons as reasons for belief.

As a teacher, Socrates starts with the abilities and beliefs (not necessarily the knowledge) the pupil already possesses, something constructivists and non-constructivists alike can applaud. The ability to reason, and to recognise reasons for and against our beliefs, is not something that is always explicitly taught. It is taught in logic courses, but few school pupils actually have such lessons. More often than not they pick up ways of reasoning from their

friends, family or community; and they can be corrected by them when they reason in a faulty way. But all of us have the same evolutionary inheritance through which we have acquired the capacity to reason. The extent of this is a matter for evolutionary psychology to investigate, along with the important mater of just how well we have the innate ability to reason *correctly*. As some psychological studies of actual human reasoning show (see Manktelov and Over, 1990), we are often not good at reasoning, especially in the case of probabilistic reasoning. As Plato indicates in the dialogue, the Slave Boy has sufficient innate ability to employ his untutored capacity to reason. Socrates is able to exploit this ability to get the Boy to explore other unrelated issues, such as the answers to geometrical problems. In other dialogues Socrates questions his fellow citizens about a whole range of matters. In doing so he is also able to draw on their innate untutored reasoning abilities in order that there be a critical examination of further beliefs, such as those concerning courage, justice virtue, and even knowledge itself.

In an excellent paper 'Socratic Education" (in the fine collection in Rorty (ed.) 1998) Paul Woodruff considers whether a Socratic education is teacherless. He points out that Socrates thinks that each of us has sufficient personal resources to learn through the examination of our own beliefs, using various methods of (correct) critical inquiry. He also points out that Socrates often holds that he does not know enough to be a teacher, given his conception of what that ought to be like. Perhaps Socrates' position can, in part, be captured by the image employed in the *Theaetetus* (150A-151D; Plato (1973)) in which Socrates compares his role in education and inquiry more generally to that of a midwife attending a birth. It is not the midwife who is giving birth but someone else. The role of the midwife is to assist in the birth and then to tell whether the offspring is healthy or not. Now the first role of merely being an assistant to someone, such as the Slave Boy, who is giving birth to an idea does fit with the claim that Socrates himself knows very little. This is a claim made in the *Apology* (21A-E) where Socrates alleges that the only way in which he is superior in knowledge over his fellow citizens is that he knows that he does not know, whereas most others do not know this.

But what role does Socrates play that parallels the role of the midwife who examines the offspring? Here Socrates does have some knowledge in the form of a know *how to* critically examine an idea: 'The most important aspect of my skill is the ability to apply every conceivable test to see whether the young man's mental offspring is illusory and false or viable and true' (*Theaetetus* 150B-C). We have already seen in the *Laches* that it is the general Laches who comes up with a succession of proposals about the nature of courage, not Socrates who claims ignorance of what courage is. But what Socrates does know is *how to* critically evaluate Laches' claims. In general,

even though Socrates disclaims any special *knowledge that* … that would answer a 'What is X?' question, he does have a superb ability to *know how to* … when it comes to conducting critical inquiry. And since he has at least this *know how*, it follows that, if he has already engaged in some inquiry into what X is, he must have some *knowledge that*. This follows because he has already been through the critical examination of ideas about X and knows that some definitional claim, say that X = A&B&C, will not pass muster because it is illusory or false. Socrates might not know what X is, but he certainly can, and often does, know what X is not.

When it comes to education Socrates' disclaimer can have a positive role to play, at least in the sense that he is not didactically telling an inquirer what the answer is since he himself does not really know the answer. Nor does he present himself as someone who is "in the know", as many teachers might. Rather the disclaimer reinforces the point that all learning and education is to be conducted along the lines of the model of critical inquiry that he has developed. And this model downplays the role of teachers and plays up the role of pupils who can employ their own resources to arrive at an answer. Moreover the model is a collective one, and not necessarily individualistic. Though each person can use it by themselves, the model is best suited for two or more who collectively engage in critical inquiry through a mutual dialectic of question and answer.

The episode of Socrates and the Slave Boy give us one (but not the only) model of how learning can take place. And it leads to success, providing the conditions on *knowing that* are met by the end of the process. Why is it successful? It is successful because the Slave Boy uses principles of critical inquiry that take us to knowledge rather than mere true belief. In fact the principles themselves are reliable for the production of knowledge, rather than mere true belief. The process of learning itself might be bolstered by knowledge that educationalists have acquired from empirical investigations into learning. These may be strategies that tell us how to over come pupil's erroneous conceptions, strategies that alert pupils to erroneous reasoning, strategies that over come aporia or "blockages" in which pupils do not know where to go next, and the like. But at the core of it all lie principles of critical inquiry. If these are not employed then pupils can only obtain true beliefs. This is not knowledge as we have used the notion here. That it might be thought to be knowledge is due to much misunderstanding in science education, education generally, and constructivism particularly, of what counts as knowledge (rather then belief). What is missing is the crucial role played by the application of critical inquiry. On Plato's theory of knowledge, and many others, the only pathway to *knowledge that* is through the application, by the inquirers themselves, of the principles of critical inquiry.

No other pathway will take inquirers there; at best the alternative pathways only take them to belief, or opinion.

NOTES

[1] The *Meno* is Plato's first attempt at drawing the all-important distinction between knowledge (*episteme*) and belief (*doxa*). This is something that he discusses in later dialogues, such as the *Theaetetus*. He also develops, especially in the *Phaedo* and the *Republic*, a theory about our knowledge of the Forms, or Ideas. We will not discuss this as it involves knowledge by intellectual acquaintance with a quite different kind of "object". It is important to keep these matters quite distinct.

[2] Some see in the passages cited above from the *Meno* an embryonic account of a reliabilist theory of knowledge, e.g., Armstrong (1973), p. 159. A case can be made for theories of knowledge from Aristotle to Descartes being externalist and having elements of reliabilism about them. And even Descartes can be viewed as a reliabilist, albeit a supernatural reliabilist, since God is not a deceiver. In the history of epistemology only in the last few centuries have internalist accounts of knowledge been in the ascendancy.

[3] Standard commentaries on *Meno* 98A such as Bluck 1961, pp. 412-3, suggest a range of possible ways in which *aitias logismos* can be understood. See also Nehamas, 'Meno's Paradox and Socrates as Teacher' in Day (ed.) 1994, especially Part V, pp. 240-5.

[4] More strictly, reasoning processes are *conditionally reliable*, and not reliable *simpliciter*. This is best illustrated in the case of a valid deduction which is a belief forming process in that it takes us from one set of beliefs as premises to a further belief as a conclusion. Such deductions will, as a belief forming process, yield truths as conclusions, only on the condition that all the premises are true; if any premise is false, then valid deductive reasoning might yield truth or falsities as conclusions. However valid deductive reasoning will be 100% conditionally reliable.

CHAPTER 4

SOME PROBLEMS FOR THEORIES OF KNOWLEDGE AND THEIR RESOLUTION

In chapter 2 we set out the basic idea of *knowledge that p* as true belief that p, for which a person has reasons, evidence and/or justification for p. In chapter 3 we showed how this basic idea was the core of Plato's conception of *knowledge that p* in the *Meno,* and part of his important model of Socratic inquiry. However Plato, as is the case with most philosophers, found that while the definition headed in the right direction there was still much more to be said in order to tell a better and more comprehensive theoretical story. Such a story we will call a *theory* of knowledge to distinguish it from the more explicit and brief *definitions* already given. In a later dialogue, *Theaetetus*, we find Plato explaining more fully the theory behind his brief definition in the *Meno*. He shows why knowledge cannot be merely true belief, and argues, on a certain understanding of what 'logos' means as reason, account or explanation, that neither is knowledge merely true belief plus *logos*, as he earlier suggested.

We will not follow Plato's further account of knowledge. His initial definition was the generally accepted default position about knowledge adopted by most philosophers until the twentieth century when some generally agreed counterexamples to it were uncovered. Revisions to the definition were then proposed. In making the revisions the conditions of truth and evidence were not abandoned. Far from it. They were retained, but refined in order to provide a more careful account of knowledge not so susceptible to difficulties. We will not follow this story here that is told in many contemporary books on theories of knowledge.[1] Rather we will take a slightly different path through the complexities in order to set out some of the main theories of knowledge that impact on science education.

One of the more prominent theories of knowledge in science education is constructivism, or 'radical constructivism' as some prefer to call it. We have considered some aspects of constructivism already in relation to theory in pedagogy. But there is also an epistemological aspect to radical and moderate versions of constructivism. In this and the next Chapter 5 we focus only on what we will call *epistemological constructivism*. Our aim will be to provide a setting for epistemological constructivism within the context of other theories of knowledge more generally; this will enable a more adequate evaluation of it in comparison with other theories of knowledge. Epistemological constructivism is also a version of scepticism in that

downplays not just the role of truth in knowledge, but also of the idea that we can give an objective account of what the world is like, even in science. Our view will be that it is not a theory of science or of knowledge that should be endorsed and that it, along with relativism, has not been a healthy influence in science education.

We begin by stating one of the standard objections, based on the regress of reasons, to the definition of knowledge advanced in Chapters 2 and 3. The rest of this chapter investigates a number of responses to this problem within the theory of knowledge. The responses we investigate include dogmatism and scepticism (Section 4.2), relativism (Section 4.3) and foundations in certainty (Section 4.4), the coherence theory of knowledge (Section 4.5) and finally the theory that says that the best that can be done for knowledge is really a theory of probable belief (Section 4.6). This last section provides an important link between Section 2.2 and the probabilistic theory of scientific method discussed in Chapter 9, viz., Bayesian methodology. This is not a complete survey of responses to the regress problem in epistemology; we omit discussion of reliabilist theories of knowledge, contextualist theories of knowledge, naturalised epistemologies, many of the responses to scepticism, and the like. But the limited and incomplete aspects of the theories we do touch upon should some idea of how constructivism compares with its rivals as an image of knowledge and of science.

Given that a number of different theories of knowledge are explored in this chapter, an important point needs to be re-emphasised. Many of the rival theories do *not* jettison the idea that knowledge has something to do with truth, and something to do with reasons, evidence or justification. On the contrary they are important notions that are preserved. What the rivals do is give different accounts of how evidence or reasons enter into the definition of knowledge and how this is to be fleshed out in the form of a full theory. Very few theories abandon these central notions, relativism and dogmatism being two. In Chapter 5 we will look at how these matters impinge on the constructivist view of knowledge and science common in education.

4.1 THE PROBLEM OF THE REGRESS OF REASONS

We have already met the problem of the regress of reasons in Section 2.4.3. Consider the illustration of the definition of knowledge given, viz., Eratosthanes knows that H (= the earth is a curved sphere with circumference of approximately 40,000 kilometres). As indicated, knowledge for this is based on four pieces of evidence E_1, E_2, E_3 and E_4 and the inference from these to H. Now imagine a persistent pupil who asks, for every piece of evidence that is given, 'what is the evidence for that?' They begin by asking: 'How do we know evidence E_1, viz., that the angle is 7.2 degrees? Can we always trust our perceptual systems? Our eyes may not be working properly,

or we make a mistaken measurement and so on. Do we not need evidence for our observation?' Again, the questioning pupil asks: 'Do we really know E_2, viz., what is the distance between Alexander and Syene? Could we not have made a mistake in the measurements on the ground? What evidence do we have that these are right?' The pupil questions even further: 'How do you know that E_3, the geometrical theorem, is correct? We need to have a proof of the theorem from the axioms before we can accept it. But now tell me, how do we know the axioms are correct?' And finally they insist: 'How do we know that the simplifying assumption E_4, holds, viz., that as far as observations are concerned there is no difference between the actual Sun's rays and the assumption that they are parallel? We do not really know this unless we have evidence for it. So what is the evidence?'

Finally the pupil raises a different, but equally serious, objection: 'How do I know that the inference from E_1, E_2, E_3 and E_4 to H is correct? What is the rule of logic that backs it up? I need to know that this rule is correct before I can use it'. The pupil sums up their objection: You claim that we can have knowledge that H. But before we can get it we need knowledge of a lot of other things before we can know H! But this quest for further knowledge does not stop and seems to go on and on. There is a regress of getting knowledge for other knowledge, and yet for further knowledge, and so on indefinitely. There is a problem with your definition of knowledge here'.

The pupil is rehearsing an old objection to the definition of knowledge that goes back to the Ancient Hellenistic philosopher Sextus Empiricus. We need not follow the history of this objection. But we will follow some of the responses given to it after the objection is given a more careful formulation.

If people are not going to be dogmatists about their beliefs, such as H, then some evidential or supporting reasons have to be given for knowledge. So, setting this out in terms of the definition of knowledge developed so far we have the following:

Person A knows that H =

(1) H is true;

(2) A believes that H;

(3) A has good sufficient, evidence, reason and/or justification that H.

Clause (3) can be made more explicit as follows:

(3a) there is evidence E that gives strong support to H;

(3b) A has the evidence that E;

(3c) A reasons from E to H.

The conditions 3(a) to 3(c) are the more explicit account we will adopt of the justification condition in (3). Each of these needs a separate discussion.

Let us start with clause 3(b). What is it for A to *have* the evidence E? If this merely means that a *believes* evidence E, then A has an unsupported assumption that E; so A has not advanced beyond the position of the

dogmatist in retreating to claims about evidence E. What seems to be required is that A *knows* that E. So now we should change 3(b) to

3(b*): A knows that E (i.e., A knows the evidence).

But now the definition is circular. We want to define the phrase 'A knows that H'. But in the very definition we use the term 'knows' that we are trying to define.

How can we avoid circularity? One way is to reapply the definition of knowledge over again to clause 3(b*). Here we appeal to evidence F that is to serve as reason or justification for E, and E in turn is evidence for H. But now one can readily see that an infinite regress of reasons threatens. If A wishes not to be a dogmatist and give reasons for their belief then they cannot either rely on unsupported assumptions, or give a circular definition that presupposes the very thing to be defined, or end up in an infinite regress of reasons. It might seem difficult to avoid these objections; so the definition is exposed as deeply flawed. Epistemologists now find that their job of defeating dogmatism and putting proper beliefs on a solid foundation within a theory of knowledge is a much harder task than they had thought.

One immediate response would be to say that, in the case of observational evidence, the regress stops in observations that do not call for further evidence. What are these like? This is the classical, "foundations for knowledge" response that we will consider in Section 4.4.

We have not yet finished with the definition of knowledge; there are clauses 3(a) and 3(b) to consider. 3(a) simply says that evidence E gives strong support to H. But what do we mean by 'support'? This is intended to indicate a *logical* relation of support in the sense of: either (a) from E as premises we can logically deduce conclusion H; or (b) from E as premises we can find a strong inductive argument to H as conclusion (see Section 7.2 on these modes of inference). So, support is simply a logical relation of deduction or a strong inductive connection between evidence and what it is evidence for. So far this is relatively unproblematic. But it is not enough that E support H; A has to know that this is so, and reason accordingly.

Clause 3(c) adds the important requirement that person A actually makes the inference from E to H. It is not sufficient that E is simply strongly supporting evidence for H. A has to get this into A's mind and realise that E is evidence for H; that is, A has to do the hard work of inferring from E to H. Do we mean by this that A has to have some *know how* in getting, logically, from evidence E to H? Yes. Does this now lead to a circularity objection? What we are defining is 'knowing *that*', but we employ 'know *how*' in the definition. But more than this might be required: A has to at least *know that* the inference from E to H is a correct inference. This is captured in the suggested revision of (3c):

3(c*) A knows that E is a good reason for H, and knows that such-and-such a pattern of inference is a valid or correct pattern of inference to employ in getting from E to H.

It would seem that some such knowledge *that* is required about reasoning, viz., A knows *that the inference from E to H is correct*. We cannot just *accept* some pattern of inference, or more weakly merely *rely* on the correctness of some pattern of inference; this would be to adopt an unsupported assumption, or a belief. Our assumption about reasoning might be correct or incorrect; but even if correct, it remains merely a belief. If we are not to be dogmatists about reasoning, then we need to reapply the definition of knowledge over again to our patterns or rules of reasoning. But now a regress threatens about our knowledge of the very rules of reasoning themselves. Does this stop somewhere or does it go on indefinitely, or end up in a circle? Here we will not discuss the problem about how we justify the rules of reasoning employed in knowledge.[2] Here we will restrict our focus to the justification of our knowledge of observational evidence.

Given the problem of the regress of reasons, we can now compare rival theories of knowledge to see how they deal with the problem. The remainder of the chapter considers a few rival theories in this respect. Our target is constructivism as a theory of knowledge (and not as a theory of pedagogy). Even though it is not fully discussed until the next chapter we will see that constructivism, radical or not, helps itself to elements of scepticism (Section 4.2), aspects of relativism (Section 4.3), a problem the besets the classical foundations theory about the relation of experience to the world itself (Section 4.4) and aspects of conventionalism as a positive response (Section 4.5).

4.2 DOGMATISM AND SCEPTICISM

The dogmatic attitude is one of refusing to give reasons for one's beliefs. One just has one's beliefs – and that is that. The dogmatists' response to the regress is simply to stop at the point where it lands upon one of their beliefs. The business of giving reasons and evidence is effectively abandoned – and thereby any attempt at acquiring knowledge.

It is hard to see what role learning and education could have for dogmatism – unless it is simply getting others to be in accord with what the dogmatist believes. Why they should want others to believe what they believe remains unclear since they are not in the business of giving reasons for belief. Perhaps they are merely being prudent and find life easier with fellow dogmatists who believe what they do; in contrast life would be uncomfortable in a community of dogmatists who do not share beliefs. There is no reason to suppose that a community of dogmatists should all agree in their beliefs. But it remains hard to see how, if all choose to remain dogmatists, each might influence the other by reason or get the other to change their beliefs, unless

force is used or a form of mob psychology prevails. For a community of dogmatists, learning and education can only be that process whereby pupils are initiated into the belief system of other dogmatists. The processes of initiation cannot be anything like a model of critical inquiry. At best it can only employ methods of indoctrination, brainwashing, manipulation, conditioning, etc, which are contrary to the spirit of the educational model which has at its core methods of critical inquiry. The history of human kind is replete with methods of belief inculcation by dogmatists who attempt to get others simply to take on what they believe. Could one be a dogmatist but not try to inculcate others with one's beliefs and let a plurality of beliefs prevail? The liberal stance for a dogmatist seems to be rare, unless they are really closet relativists (see Section 4.3).

Sceptics are quite different from dogmatists. There is a healthy kind of scepticism that is always open to the questioning and critical evaluation of any belief. Science itself is often said to be a form of "organised scepticism" (Merton 1973, Chapter 14). There is no subject matter which is to be sealed of from scientific inquiry; all is open to investigation and nothing is accepted because it is sacred, or traditional, or even generally believed. Whereas dogmatists usually believe much, but offer no support for their belief, the sceptic often advocates suspension of belief until supporting evidence is found.

But there is a version of epistemological scepticism that is stronger than merely the willingness to subject any belief to examination. It maintains that there is often not sufficient evidence for a large number of different kinds of beliefs, either in support of them or against them. Thus consider sceptics about religious belief. They do not invite us to believe that p (for example, God exists) or believe that not-p (God does not exist); rather they invite us to suspend belief and believe neither p nor not-p. We become agnostic, this term coming from the Greek *a* for 'no' and *gnosis* for 'knowledge' (in the sense of recognising, but also particularly pertaining to, knowledge of esoteric matters). Again, there are local sceptics about other domains, such as our ability to ever have knowledge about other minds, or about the external world, and so on. Unlike local scepticism, global scepticism is not specific to any domain. It simply says that whatever the domain (religion, the external world, etc) we can have no knowledge and must suspend all belief.

Such epistemological scepticism is to be carefully distinguished from a common form of scepticism that arises from a relaxed and lazy attitude to beliefs. It advises us to be neither dogmatic adherents to some beliefs nor to be too active in searching for reasons for belief. Rather, just "lie back" and suspend belief. It is hard to see what role learning or education can have for such "laid-back" sceptics.

Epistemological scepticism is a quite different matter. The original meaning of the Greek word for scepticism was 'to look about'; but later it was used to mean 'considered examination' or 'inquiry'. When inquirers applied the appropriate standards for the successful conduct of critical inquiry, they found that few beliefs, if any, passed these standards. Hence the commonly accepted meaning of 'sceptic'. The principles of a pure critical inquiry should, however, not be set so high that mental hygiene is all too readily achieved by eliminating all beliefs as candidates for knowledge. They should enable us to filter out false from true beliefs, show where reason is absent or show that some beliefs are held for bad, reasons, or even held on irrational grounds. When standards are set too high then few beliefs might be warranted by the principles of critical inquiry. We will not enter into the many discussions there are in philosophy about the extent of, or even the viability of, scepticism as discussed by latter Hellenistic philosophy such as that of Sextus Empiricus, or the scepticism of Descartes, or contemporary versions of scepticism (see, for example Musgrave 1993, or Gascoigne 2002). But a version of scepticism about the external world will arise in Section 4.4 that is of importance to constructivism.

We can raise an objection to a version of global scepticism that says that we can have no knowledge at all. Suppose the sceptic uses the above regress argument to show that all attempts to obtain knowledge must fail because either unsupported beliefs will arise somewhere, or any definition will be circular, or any definition involves an infinite regress. So, there is no knowledge. But how does the sceptic know this? By giving the regress argument as evidence for it. So, the global sceptic knows at least one thing viz., that there is no knowledge. And such a sceptic knows this on the basis of good evidence (the regress argument itself). Thus global scepticism fails. This is a quick argument against the global sceptic that does have some force. But any determined, sophisticated sceptic can attempt to reformulate their position so that it bypasses this objection. But in so doing their position cannot be fully global.

Dogmatism and scepticism raise questions about the *ethics of belief*. We claim that if we freely perform an action (and do not perform it involuntarily, or under coercion) then we are responsible for it, and so we can be praised or blamed for it. Thus we freely talk, walk, think, eat food, see a film, drive our car too quickly, and the like; and we can be praised or blamed for these actions. Similarly when we believe something, an act of *believing that p* is performed (as distinct from the content, *that p*, that is believed – see Section 1.2). If people can perform acts of believing that are free, then they ought to also be responsible for what they believe. But if their acts of believing are not free, then they cannot be responsible for them. So are acts of believing free or unfree?

There is a large divide between philosophers on this issue. Hume thought that belief is largely involuntary; so we cannot be responsible for our beliefs. Though we think some acts of believing are involuntary (it is hard not to claim this about many beliefs we get from direct perception), on the whole we agree with the criticisms of Hume's position in Passmore (1980a, 'Hume and the Ethics of Belief '). We support the position of Descartes and Locke who argued that we are responsible for our acts of believing. Moreover we are responsible for ensuring a certain degree of mental hygiene in ensuring that what we believe is reasonably well supported by evidence. If it is not well-supported then we should not believe it.[3]

We can behave badly or irresponsibly as believers. We can be blamed for being dogmatic, or for being too easily persuaded by what others say, or not paying attention to available evidence, not even looking for relevant evidence, and so on. If this is so, then there are normative constraints on us as believers as well as constraints of logic and rationality. Not only can we be wrong about matters of logic and rationality; we can also be wrong in not being sufficiently attentive to the logical and rationality conditions that *ought* to inform our believing activities. An *ethics of belief* adds an additional constraint to those of logic and reason. Failure to obey these additional constraints is something like failing to do the moral thing. They are matters that a person who conducts any critical inquiry into any subject ought to pay heed. They ought to try to avoid error and try to get at the truth.

An ethics of belief is of extreme importance in education and learning. We ought to learn from the start the norms that govern both the process of carrying out a properly conducted critical inquiry, and the norms that concern the appropriate attitudes (of belief, disbelief, suspension of belief, etc) we should adopt towards the products of inquiry. Spelling out an ethics of belief is a large task that we cannot carry out here. But it is an important issue to consider for both dogmatists who are resistant to critical inquiry and for epistemological sceptics for whom over-strong constraints on inquiry can also lead them astray.[4]

4.3 RELATIVISM

Relativism is an old doctrine perhaps first advocated by the Presocratic sophist Protagoras and first roundly criticised by Plato in his dialogue *Theaetetus*. We will look at some of Plato's decisive criticisms shortly. Plato recognised that relativism can take many forms. He also recognised that since each new generation constantly re-discovers relativism for itself, then his arguments against it will have to be rehearsed for them, and so on down the ages. In our own time many postmodernists, (social) constructivists and sociologists of "knowledge" ally themselves with some version of relativism. So even now there is a need for the criticisms of Plato, and others who have

criticised the constantly changing forms that relativism can take. Very few philosophers have been advocates of relativism while many have criticised it. During the twentieth century, at the beginning writers as diverse as Lenin (1952, Chapters 1 and 2) and Husserl (1970, Chapter 7) were fervent critics of relativism; towards the end of the century a prominent critic is Putnam (1981, Chapters 5 and 7).

One impetus for relativism is scepticism borne out of despair of ever having knowledge, perhaps because of arguments like the regress of reasons. Another impetus is that relativists note that we do have beliefs, and that these beliefs, as a matter of sociological fact, vary widely from individual to individual, from culture to culture, from thought system to thought system, and so on. For example, given the same belief content p (such as *that the Sun orbits the Earth*) one person or cultural group might maintain 'p is true' while another person or group maintains 'p is not true' (= 'p is false'). Clearly disputes can then arise over which of the logically contradictory claims 'p is true' and 'p is false' is correct. Not both of these can be maintained; otherwise a classical law of logic, the Law of Non-Contradiction, is violated.

Relativists have a way of avoiding such clashes of opinion by adding relativisers such as 'true-for-me' and 'false-for-you'. That is, that the Sun orbits the Earth is true-relative-to-me (or-my-group), but that the Sun orbits the Earth is false-relative-to-you (or-your-group). If one relativises truth in this fashion, then there is no logical contradiction between 'p is true-for-me' and 'p is false-for-you'; or between 'p is true-for-my-culture' and 'p is false-for-your-culture'. Both pairs are quite consistent. So relativism is a way of reconciling what appears to be a clash of belief. From the relativist stance worries about knowledge and its objective truth condition fall away. Relativists can adopt a more relaxed position that does not insist that there is one definite truth to be found; rather there are a multitude of relativised truths to consider for each person, culture or thought system. Here scepticism about ever arriving at one objective truth in the pursuit of knowledge fuels relativism.

4.3.1 What is Relative to What in Relativism?

In the above relativism with respect to truth has been introduced. But there is another broad version of relativism to consider, the relativisation of justification to individuals, cultures, communities, thought systems, and the like. That is, the reasons I have for the belief that the Sun orbits the Earth are reasons-relative-to-me (or-my-culture); but they are not reasons-relative-to-you (or-your-culture). One immediate consequence of this is that there are no objective justifications to be found. Instead there is one kind of justification for the belief that p relative to some individual (or their culture, community or framework of reasoning), and another kind of justification for the opposite

belief, not-p, that is relative to different individuals (or their culture, community or framework of reasoning). From this stance, justifications and reasons, like truth, become relativised. Does this mean that "anything goes" as far as justification is concerned? This might be so for justifications relativised to each individual. Unless they happily coincide in what turns out to be the same justification-for-each, then there does not appear to be any ground on which individuals can come to agree about justifications, or be made to see that they *ought* to adopt one justification rather than another, or see that their justification-for-them is erroneous.

However community or culture relative justification does allow that a person can be corrected; this will occur when the person does not adopt the justification that is endorsed relative-to-their-community/culture. This is as far as correction may go since, on the relativist stance, what is ruled out are claims to the effect that a justification holds objectively and independently of any culture, community or whatever. For example, the rule of logic *Modus Ponens* (from premise A, and premise if A then B, validly infer conclusion B) is not objectively valid for the relativist. But if it is in the justificatory repertoire of a culture, then a member of that culture can use it. But if the member fails to use it when they should (by the lights of the community), or misuse it, then they stand to be corrected by the "higher" authority of the community. In contrast, if it is not in some community's repertoire then no member ought to use it; if they do use it community sanctions can be applied. The "ought" here is not the compulsion of objective rationality; rather it is the "ought" of what a community sanctions, or does not sanction.

The above indicates two ways in which the relativist can circumvent the regress of reasons. First, there is no objective truth to be found: there are as many truths as there are believers of the truth (or communities of believers). Second, there are no objective justifications to be found; there are as many modes of justification as there are believers about what constitutes a justification (or communities of believers). The regress is circumvented in a radical way by removing the objectivity of truth and reasons, and thus of knowledge.

Within relativism there are a plethora of relativisations of the form 'x is relative to y'. The x can range over, as indicated above, not only truths or justifications but also meanings, reference and reality. The y can range over, as indicated above, not only individual persons, cultures or communities but also social classes, epochs, languages, conceptual schemes, frameworks of thought, scientific paradigms, world views, and so on. Thus the meanings of terms and their reference are alleged to be relative to languages, to scientific paradigms or conceptual schemes or frameworks. Again what the world contains, or "reality" or "metaphysical commitments", are alleged to be relative to scientific paradigms, world-views, conceptual schemes or

frameworks of thought. In what follows we will focus only on truth claims of the form 'p is true-relative-to-x', where 'p' is some belief-content or proposition, and x is some individual person, a community or a system of thought or paradigm. However if the reader thinks of individual persons as the relativiser, then most of the considerations to be introduced can be quite easily carried over to other sorts of relativisers.

Most of the following criticisms can be found in one form or another in Plato's *Theaetetus*. His critique begins with the apparent citation of a hypothesis from a now lost book of Protagoras known as the *Measure Hypothesis*: 'Man is the measure of all things: of those which are, that they are; of those which are not, that they are not' (*Theaetetus* 152a; Plato (1973)). The dialogue immediately gives an example of the relativity of perceptual properties such as, in the case of a blowing wind, that the wind is hot relative to one person but cold relative to another. We will pass over the question of whether or not the doctrine of relativism applies to perceptual properties (not listed above as one of the kinds of thing that can be relativised). Even though the relativisation to individuals is most appropriate in the case of perceptual properties, this need not be the case for other kinds of items that can get relativised.

Once having teased out the relativism of the *Measure Hypothesis*, Socrates asks whether it entails that no person is wiser than another, and that no one can teach another anything, thereby turning the tables on Protagoras' claim that in advancing the doctrine of relativism he is thereby teaching somebody something.

> … as we've said several times, each person is himself the only one who can judge the things that he does judge, and they're all correct and true [to be more explicit Plato should add 'for that person']. But if all that's to be so, then how on earth can it be the case that Protagoras is wise, so that he can justly think himself fit to be a teacher of others at high fees, whereas we're more ignorant and have to go to his lessons, though each of us is himself the measure of his own wisdom? How can we avoid concluding that Protagoras is playing to the crowd when he says that? … and I suppose the same goes for the whole business of dialectic. It must be (mustn't it?) a long and protracted bit of foolery to set about inspecting and testing one another's appearings and judgements, if everyone's are correct; as they are if Protagoras' *Truth* is true …. (*Theaetetus,* 161d-e' Plato (1973))

The point is clear. The doctrine of relativism allows the following possibility: both p and its reasons are true-relative-to-x, and at the same time both are false-relative-to-y. It denies that there is any objective truth *that p*, or reasons for p. So on what grounds can one be said to be wiser than the other in believing or reasoning? There are none. Each has his or her truth, and reasons for its being their truth – relative-to-them; and that is the end of the matter.

4.3.2 Why Teach if Truth is Relative?

If no person can be wiser than another, then on what grounds do we engage in teaching? Can anyone learn anything from another? Given the relativism of truth and reasons, teaching and learning cannot show, say, that it is p, rather than not-p, that makes a greater contribution to knowledge and thus to the wisdom of the person who has it. The grounds of critical inquiry have been undercut; and so have the grounds for teaching and learning.

In subsequent passages, in order to give the absent Protagoras a fair hearing, Socrates turns to defending his doctrines. In reply to the above criticisms he conjectures that Protagoras might have said:

> As for wisdom or a wise man, I'm nowhere near saying there's no such thing; on the contrary, I do apply the word 'wise' to precisely this sort of person: anyone who can effect a change in one of us, to whom bad things appear and are, and make good things both appear and be for him. … In education, too, in the same way, a change must be affected from one of two conditions to the better one; but whereas a doctor makes the change with drugs, a sophist does it with the things he says. (*ibid*., 166d-e)

There is point to Socrates' attempt at a defence. If his reply is adequate, then he has shown how the core notions of teaching, learning and education, along with the notion of wisdom, can be maintained within relativism. So a non-relativist objectivist conception of knowledge appears not to be the only place where these notions can find a home. The stakes are high at this point because the very idea of objective human knowledge as the one and only home for such notions is being challenged. If the defence succeeds, relativists can do just as well, and perhaps even better, than objectivists!

To see where the defence may be weak consider the analogy with a doctor made in the dialogue (fuller details have been omitted but they follow from the above quotation). Let us suppose that a patient is in a condition that they find bad. They want a cure in the sense that they want to get to a condition that is good. When Socrates speaks of good, bad and better conditions does he mean that there are objective grounds for such claims that all can agree upon, the patient, the doctor and anyone else? This is one reading of the passage that commentators have pursed; and it is objectivist in tone. Or does he mean that the conditions are good bad or better only relative to the patient, and not the doctor or anyone else? To be consistent with relativism we will follow the second interpretation and adopt a relativised conception of what is good- (bad-, better-) -relative-to-the-patient (and not anyone else). As can be seen, the following considerations will apply to both.

Patients suffering from a bad, more strictly *bad-for-them*, condition do not know how to make the change in their condition for what is better (by their own lights); so they appeal to the doctor. The extra wisdom the doctor has over that of the patient is this: the doctor knows, but the patient does not know, how to bring about a change in the patient (by using drugs or whatever) from what is (objectively) bad, or the relativised bad-for-the-

patient, to what is (objectively) good, or (relatively) good-for-the-patient. So the doctor does know something, viz., *that always (or nearly always) doing A brings about a change in a patient from condition B (which is bad-for-the-patient) to condition G (which is good-for-the-patient)*.

Note that the italicised claim is not relativised; rather it is a scientific generalisation which is open to test by anyone. It is an objective truth, or falsity, within medical science about what changes can bring about, or remove, whatever conditions (judged good or bad relative to those who have them). If the italicised claim were a truth relative to one doctor but a falsity for a second doctor, then one would have to be careful in picking one's doctor if one wished to get out of a bad condition (bad-relative-to-oneself)! On the relativist understanding, one might be lucky with the first doctor and get the change one wants, but unlucky with the second doctor and not be administered to in a way which gets one to the desired condition. But the luck on the part of the first doctor could not count as a piece of medical knowledge on his part. This cannot be knowledge not only because of the relativisation of the claim, but also because of the lucky happenstance of getting the desired remedy. This brings us back to the account of knowledge developed by Plato in the *Meno* and discussed in Section 3.2 in which knowledge cannot be a true belief arrived at by a process whose outcome of truth is a matter of luck and where the process is unreliable for the truth. So Socrates' defence on the part of Protagoras relies on some non-relativised truths as part of the superior wisdom of the doctor due to his medical knowledge of what can bring about what desired change. It appears that the analogy with the doctor ministering to a patient does not avoid an appeal to non-relative truths.

Let us turn to the case of education. Successful teachers know something, namely, how to change a pupil from one state of belief to a different state of belief; unsuccessful teachers would not know even this. But what the successful know may be merely sufficient to fulfil the quite minimal conditions for learning set out in Section 1.3. Minimal learning was simply the introduction of a belief, or change in belief, by whatever means; and the extra knowledge the teacher has pertains to just these means. Also what this kind of successful teacher knows cannot be a relativised truth-for-the-teacher (this was noted in the case of the doctor). If it were, then once again any success in their teaching practices would be a matter of luck and not of knowledge. One may take it that much of learning theory comprises a body of tested and well established knowledge of just how one can successfully bring about the introduction of a belief, or belief change, in pupils.

If we pursue more fully the analogy with the doctor, then we would have to add that the pupil finds their current belief state undesirable-for-them and they wish to change to a belief state that is more desirable-for-them (even if

they do not know, at the time, what belief state might later prove to be more desirable-for-them). To keep the analogy, we have to allow that what counts as desirable and undesirable belief states is relative to the pupil. They may desire not to be ignorant, or desire not to be in a state of confusion or contradiction as is the Slave Boy as he wrestles with his geometrical problem. They may also desire to acquire knowledge they lack in some area for whatever purpose, such as self-improvement, or the need to get a better education to improve their job and pay prospects. This is a good starting point for the teacher that may not always be present in pupils, viz., that pupils actively desire to have a change in their belief state (for whatever reason). If it is not present then the pupil has either lapsed into dogmatism about belief, or lack of interest in it (the 'lazy laid-back' scepticism of 4.2). If either of these then relativism will not be and answer to their condition – unless it is another form of the lazy laid-back sceptic who simply rests with what-is-true-for-me, thereby resisting any attempt to renovate belief.

There is another way to put the analogy with the doctor, but in a social context. Rather than focus on what the individual pupil takes as a *bad-for-the-pupil* condition that the pupil wishes removed, one could consider the social case of the pupil having a *bad-for-the-pupil's-community* condition that the community wishes removed. The role of teachers is then to take pupils and render changes in pupils from what is *bad-for-the-pupil's-community* to what is *good-for-the-pupil's-community*. Teachers are then (more or less) successful agents for the community in this respect. Though the following comments are cast in terms of relativism with respect to the individual, they can also be taken to be a community relativism as well.

Matters cannot be left here in Socrates' defence of Protagoras' conception of teaching and wisdom, as is realised in the dialogue. What a teacher knows is *how*, and *what*, will bring about a change in pupil x from x's not believing that p to x's believing something p, or believing that q but desiring to replace it by something else because of its inadequacy (for whatever purpose). Let us grant that the teacher knows some non-relative truths about how to do this. What the pupil wants is to get rid of an undesirable-for-them state of affairs in which they are ignorant, or believe some false q, and get into a desirable-for-them state of affairs in which ignorance is relieved by acquiring the belief that p. So what is desirable about p and undesirable about q? It could be the truth of p that makes it desirable and the falsity of q that makes it undesirable. And if this is the case, Socrates' reply on behalf of Protagoras is no reply. What the pupil wants is not what someone takes to be true, or false, relative-to-them. They want the plain, unvarnished non-relative truths, or falsities. And these can only be obtained if one engages in a critical inquiry into what is true and false, something that has been ruled out by the doctrine of relativism.

What the above reveals is that successful teachers need to have some non-relativised beliefs about how to affect changes in the beliefs of their pupils. But matters should not stop there and the pupils be left content with beliefs whose truth and justification remains relativised. It seems quite arbitrary that there should be non-relativised truths only at the level of what teachers do while relativised truths remain at the level of what the pupils pick up as a result of teaching. Of course it is possible that some teaching involves bringing the pupils' beliefs into conformity with what the teachers, or other authorities, want the pupils to believe. That is, there are tried and tested techniques for getting a pupil from not believing that p to believing that p, where p is deemed true-relative-to-some-authority. But if critical inquiry is allowed to operate at the level of how to bring about such changes in belief, then it should be allowed to operate at the level of the beliefs that p themselves. Moreover the learning ought to take place in accordance with genuine principles of critical inquiry, and not merely in accordance with principles-which-are-correct-relative-to-x.

The above shows that if one is a genuine relativist then one should not profess to teach; but if one does, then one trades in some non-relativised beliefs about the efficacy of teaching methods.

4.3.3 Relativism as Self-Refuting

There are other lines of criticism of truth-relativism developed in the dialogue that will be mentioned only briefly. One of the main critical points it outlines is that the very statement of the relativist doctrine is self-defeating. One way of presenting this is to express the doctrine of relativism in the form: all truths are relative (call this 'R'). But is R itself a relative truth or a non-relative truth? If it is non-relative, then it cannot be generally true as there is at least one exception to it, viz., R itself. If it is a relative truth then sentence 'R' does not fully express this. What we should say is: R is true-relative-to-x. But in that case there may be some other person y, such as Socrates, of whom it can be said: R is false-for-Socrates. But as Socrates insists in the dialogue, this does not express his opposition adequately. It is not merely that 'R is false-for-Socrates', since this commits him to some aspect of the doctrine of relativism. Rather the proper expression of his position is this: R is false (*simpliciter* and with no relativisation).

The dialogue also touches on the following difficulty for relativism. Consider the claim

(1) p is true-relative-to-x.

On the face of it this is a relative truth. But it is also a non-relative truth in that it tells us what is x's truth for them; and this is a non-relative matter. So the whole claim of (1) is itself a non-relative truth. An example might help with this point. Let 'p' be the Roman Catholic (C) belief that, in the

celebration of the Eucharist, the wine and bread becomes the blood and body of Christ'. Then on the relativist doctrine we can express this as:

(1) (that wine and bread become the blood and body of Christ) is true-for-C.

Atheists (A) would hotly deny this and claim;

(2) (that wine and bread become the blood and body of Christ) is false-for-A.

And Buddhists (B) might be non-plussed by this and claim:

(3) (that wine and bread become the blood and body of Christ) is false-for-B.

Now each of these three communities, A, B and C, differ over what they take religious doctrine to be, viz., the contained proposition p = that wine and bread become the blood and body of Christ. But if they turn into anthropologists and investigate the religious doctrines of one another, is there no fact of the matter as to what they find as to what one another believe, or hold as their-truth-for-them? That is, do not A, B, and C, agree that each of (1), (2) and (3) are non-relative truths about what each religious group maintains? If they do then they are trading in non-relative truths. But if the doctrine of relativism is not to be arbitrarily about the beliefs to which it does or does not apply, then each of (1), (2) and (3) must be further relativised. Thus when the atheists investigate the Catholics what they might claim is:

(4) [(that wine and bread become the blood and body of Christ) is true-for-C] is false-for-A.

And it may transpire that when the Buddhists do their investigation they claim

(5) [(that wine and bread becomes the blood and body of Christ) is true-for-C] is true-for-B.

If one thinks that the Buddhists have it right and the Atheists have it wrong about what is the truth-for-Catholics, then one has abandoned relativism and settled for absolute truth at the level of one iteration of the truth relativiser. In a consistent application of relativism there is no agreement, no fact of the matter, between Atheists and Buddhists as to what the Catholics believe. It might happen that what each of the Buddhists and Atheists take as what is true-for-the-Catholics has the same content. But this is not a non-relative truth. To envisage this in (4) replace the second relativiser by 'true-for-A'. Then one still has what is true-for-A and what is true-for-B; and both of these are relative, and not absolute, truths. In this case agreement in the content is a matter of happenstance; it does not remove the relative character of each claim.

Relativism turns out to be a disaster for the possibility of an anthropological investigation into the religious, or any other, beliefs of one another. Rather what one obtains is a further truth-relative-to-the-investigator. Moreover, it is clear that the iteration can, and must, go on, as when the Catholics start to investigate what the Atheists and Buddhists uncover in (4) and (5). This is a genuinely self-defeating consequence of relativism. For

social investigators, such as anthropologists, who have relativist tendencies, the full ramification of their relativist doctrines undercuts the possibility of uncovering non-relative truths. At best all they can give are truths-relative-to-the-anthropologists! It is hard to tell what might be the canons of any critical inquiry they might carry out. And in any case it yields no knowledge. So relativism turns out not to be a viable response to the problems that face the theory of knowledge.[5]

4.4 CLASSICAL FOUNDATIONS FOR KNOWLEDGE

4.4.1 Some Preliminary Issues Concerning Foundationalism and Constructivism

The regress of reasons for knowledge of Section 4.1 shows: either (1) we simply stop at a belief which is unsupported by further reasons (so we are no better than dogmatists), or (2) circularity arises by using, in the definition, the very term 'know' that is to be defined; or (3) there is an infinite regress. The classical "foundations" theory of knowledge responds by saying: (a) we can reject (3) because there is a natural stopping point, and the regress does not go on indefinitely; and (b) we can reject (1) because we can stop at unsupported beliefs which are unproblematic without becoming dogmatists. These are unsupported because it becomes obvious that they simply need no further support; they are simply not open to any doubt. What are these stopping point, or foundational, beliefs? They are said to be beliefs about our individual experiences, or our sense impressions.

The "foundations" view uses an architectural metaphor. As in any building there are solid foundations upon which a superstructure is built. So in the case of knowledge the "foundations" will be the beliefs that constitute the natural stopping point. As will be shown on the "classical" foundations theory of Descartes, the natural stopping point is in beliefs that are certain and are not possibly open to doubt. The edifice constructed on the foundations will be all our commonsense knowledge of the world, and also the knowledge we have from science. Such knowledge is open to doubt; but when the connections are shown between this superstructure of knowledge to the foundations or knowledge then doubt is removed. It is important to note that even though Descartes advocated a method of doubt, and argued that many of our commonsense and scientific beliefs are open to doubt, he was not a sceptic either about our commonsense or scientific knowledge. He argued that such doubt can be removed by showing the complete edifice of knowledge that links foundations with superstructure. Many who accepted some (but not all) of Descartes' framework argued that he was not successful in removing doubt and that the appropriate position to adopt is scepticism.

As we showed in 4.1 the regress went back through two separate branches. One of these branches has to do with logic and reasoning. So the foundations response must urge that there is a natural stopping point of justifications for our beliefs in logic and reason; either these beliefs are self-evident, or they can be justified *a priori*, or they are true on the basis of meaning, or whatever. Whether there is a natural stopping point for justifications for reason and logic is an issue that we indicated in Section 4.1 that would not be pursued here. We wish to focus on the second branch. This has to do with whether there are some foundational beliefs about experience that are not to be further justified, but they provide the basis for the justification of all other non-foundational beliefs we have about the world, including the claims of science. If you like, what we are looking for are some 'unjustified justifiers' i.e., beliefs that stand in no need of any further justification (since they are in some sense self-justifying) and provide the basis of all other justification.

Aspects of what we will call here 'the classical foundations theory' can be found in later Ancient Greek philosophy, in the seventeenth and eighteenth century philosophy of Descartes, Berkeley and Hume (to name a few), in twentieth century phenomenalism and phenomenology, and in some varieties of positivism. Below we will follow, in outline only, what we can call *a Cartesian position* (but this will not be a scholarly presentation of the real Descartes); and we will mention other philosophers only in so far as they depart from some aspect of the outlined Cartesian position. The main aim is, however, to consider some quite general characteristics of "classical foundations" theories of knowledge. On all of these theories the regress stops in beliefs that are said to be *self-evident*, or *certain*, or do not need any justification, or are *indubitable* (not possibly open to doubt), or *incorrigible* (not possibly open to correction), or *infallible* (not liable to the possibility of error). In philosophy not all of these terms always mean the same thing; but for our purposes there is no need to explore their differences. Confining ourselves to just our experiences of the world, we need to ask: What are examples of such foundational beliefs? In what way are they foundational? If we accept them, then how can we construct the superstructure of non-foundational beliefs either of science, or any other body of belief, so that we can be said to have knowledge?

The last question becomes important because the foundations view is meant to provide an answer to the question 'what is knowledge?' by showing how we can get to knowledge of the world from foundational beliefs concerning our experience. Some might say (as we do) that it would be a disaster for this theory if we could not climb from the foundational beliefs into the superstructure of knowledge that is allegedly based on them and which is supposed to be about the external every-day or scientific world. Others might take a more positive view the failure and say that it provides a

successful argument for scepticism about most knowledge claims (for example, Hume). But both agree on the main point: on one understanding of what the foundational beliefs are, we cannot climb up from the foundations into the superstructural edifice of knowledge about the world. In order to get out this difficulty, if we try a different understanding of what foundational beliefs might be like, then they are not the foundations they are supposed to be; they, too, stand in need of justification. If both these claims are correct (as we will argue), then the classical foundations theory is a failure.

What is of interest for our project is how various theories of knowledge respond to the problems this classical theory faces. Of particular interest is the response of *epistemological constructivists*. On their view all "knowledge" is just that – a construction out of experience (made by individuals or groups). Moreover, can the edifice we construct out of our experience be said to be, in any sense, a representation of the world? Constructivists emphatically say 'no' to this. Yet other philosophers, such as Descartes who in many respects adopts a foundationalist stance towards knowledge, recognised this highly sceptical conclusion would follow if one were not careful, and that one would end up an idealist. Constructivists, like their empiricist predecessors such as Berkeley, embrace what can only be called the 'idealist' position. The considerations that lead constructivists to accept some aspects of the foundational picture and reject others will be set out in this section. In the next chapter we will show that epistemic constructivists in science education do adopt an idealist position about science and knowledge more generally. We will show how this strange position can be avoided.

4.4.2 Realism, Perceptual Realism and Their Rivals

Some technical philosophical terms, such as 'realism' and 'idealism', have been introduced that need careful defining since they will be of use in what follows. Realism about some domain of items claims that the items of that domain exist in ways independent of us humans. Thus realism with respect to the domain of the items in our common-sense external world maintains that there exist items like cats, rocks, water, etc, and they exist independently of human perception, thought, language or whatever; that is, they are said to be mind-independent in that if we humans were not to exist, then these items would still exist. However there are items, such as artefacts (tables, screwdrivers) or social items (such as money or marriage), that do depend for their existence on us humans and our beliefs about them. Though we will not argue the case here, a story can be told about these items that does fit within a realist framework, despite their mind-dependence.[6]

Realism about the items postulated in the domain of science maintains, in much the same way, that there exist items such as electrons, neutrons,

pulsars, tectonic plates, DNA molecules, etc, and they exist independently of us humans and our scientific theories and activities. In contrast idealism maintains that while the items in some specific domain do exist, they do not exist in ways that are *independent* of us. Rather they are dependent on our perception of them, our thought about them, our theories of them, and the like; without us and our mental activities there would be no such items.

There are two versions of idealism to be aware of. The first is *ontological* and says that no object (in the given domain of say, common sense or science) exists at all in the realist sense; if there are objects in these domains then they exist as something dependent on us. The second version of idealism is *epistemic* and adopts a sceptical attitude towards the independent existence of anything (in the given domain). If there are items in the domain that do exist independently of us, then we cannot know anything of them (beyond their bare existence, though even this can be a matter of scepticism). The only things we can know are those things that are, in some sense, dependent on us, or a "construct" of ours. How the items in the world are shown to be constructs is a matter that will be spelled later. But at least it is important to recognise the two kinds of idealism, ontological and sceptical, that stand in contrast with realism.

It is also important to recognise that there are realist and idealist stances in the theory of perception. Here we will spell this out briefly since it will become important in what follows. What do we see when we look about us? The commonsense reply is that we see objects, like tables, chairs, rocks, the Sun, water, and the like. This is the realist position on perception; the objects of perception are just ordinary objects we believe there to be in the world. We should also add that we can also see the properties of objects such as their shape and colour; and we can see their relational properties such as their position. We can also see non-substantial items such as shadows and rainbows; and we also see events such as the flight of a bird, processes such as the ebbing tide, and the like. Though these are all legitimate "objects" of perception in the broad philosophical sense, we need only discuss this doctrine taking the term 'object' with its common sense meaning (roughly as a continuing lump of stuff or matter). It also goes without saying that these objects of perception are, on the whole, to be understood realistically in the sense of the previous paragraph; they exist independently of human perceptual activities. Thus external objects such as rocks and cats exist independently of us. In this sense so do the items we make such as artefacts like tables, screwdrivers and coins (used in money transactions). Though these are artefacts which are human dependent in the sense that we make and use them for certain purposes, they are also realised in bits of matter and so have the status of objects which, from the point of view of perception, exist independently of our perceptions (but not other mental activities of ours).

This view is also sometimes called *direct perceptual realism*, because we do directly perceive the objects themselves (and not some intermediary).

In contrast the idealist stance towards perception says that ordinary objects such as tables, rocks, bits of water, etc, are definitely not the direct objects of perception. On this stance what we *directly* perceive are experiences, or sense impressions, or in more modern terms, sense-data. (In what follows we will use the terms 'experience' and 'sense impression' interchangeably.) Here a distinction between direct and indirect perception plays a big role. Commonsense objects are the *indirect* objects of our sense impressions; they are not *directly* perceived but are to be inferred from the complex of sense impressions we have. What we *directly* perceive are experiences or sense impressions. Alternatively the same contrast is drawn by talk of the *mediate* objects of perception, such as cats, rocks, etc, and something else that is said to be the *immediate* object of perception, such as sense impressions. It can be readily agreed that the direct, or immediate, objects of perception, viz., sense impressions, are mental. In contrast the indirect, or mediate, objects of perception are commonly said to be material in that they are the constituents of the material external world (or features of it such as shadows).

If what we directly perceive are sense impressions and not external objects, then such impressions must exist (but not mind independently). But what of the external objects themselves? Do they really exist? For philosophers such as Descartes and Locke, our sense impressions exist; and so do the objects which give rise to the sense impressions also exist. Thus there do exist, say, greenish-leaf sense impressions; but there also exist green leaves. In normal perception, and in our commonsense view of what goes on in perception, we do agree that our green-leaf-ish experiences are not illusory (though they may sometimes be); and there do exist objects, such as green leaves, which give rise to the experience. There must be some connection between these two items, so what is it? It is commonly said that such experiences do *represent* the independently existing objects. That is, there is something in the external world which is just the way our sense impressions say it is.

This gives rise to the doctrine of *representative realism* in which, while the direct object of perception is something experiential, the experience does correctly represent. This contrasts with direct realism which says that what we perceive are objects, and not something experiential such as sense impressions. But both representative realism and direct realism agree that there are external objects which we do perceive (in the one case directly, and in the other indirectly *via* a representative). Several questions arise immediately about representative realism, one of which is: how do we *know* that this representation actually and genuinely takes place most of the time? This is an important issue that we will discuss in the next chapter since

epistemological constructivists deny that we can ever know anything about the veracity of such representation. So constructivists are not representationalists as were Descartes and Locke in their different ways. And as will be seen they are not direct realists either.

Though the position of David Hume is complex, at one level he can be said to be a sceptic about the claims of representationalism. Thus for him the objects of perception are our mental impressions or our experiences; the 'objects" of perception are not the objects of common sense. Whether or not there really are objects out there in the world existing independently of our experiences is an uncertain matter about which scepticism might be the best policy. Thus on the issue of the realism of common sense objects, Hume can be said to be a sceptic. In contrast, consider Bishop Berkeley who also maintains that the direct objects of perception are sense impressions. It follows that for him the ordinary objects of common sense do not have an independent existence. Rather they are just a collection of experiences, or possibilities of experience. This makes them very mind dependent entities indeed. So his rejection of realism about ordinary objects is quite radical indeed; on his view there are no such things. Rather what exists is a mind dependent entity made up somehow of our experiences. Hence his strong ontological idealism about objects in which even representationalism is not possible, since there is nothing to be represented. This doctrine is sometimes called *phenomenalism*; but sometimes the more broad term 'idealism' is used, it being understood that it is an idealism with respect to the objects of perception.

The conflicting positions of perceptual realism, representative realism and phenomenalism (idealism) have been at the centre of attention of 20th century philosophy. The generally agreed verdict at the end of the century is that non-realist theories of perception are not viable. We cannot mention the vast literature on this topic. But one of the classical confrontations was that between A. J. Ayer (who was anti-realist) and J. L. Austin (who was realist). Readers should follow up this literature for arguments for and against these positions.[7]

The above briefly expressed definitions will give the reader some idea of the different stances that philosophers have taken concerning realism *versus* idealism (with respect to common sense objects). It also conveys some idea of realism about perception in contrast to different versions of non-realism from the mid-way position of representationalism, to the more extreme position of scepticism, and finally to the quite radical phenomenalism or idealism of Berkeley. What of the domain of the objects postulated in science? Ought we to be realist or non-realist about them? Clearly since most of the items postulated in science are not observable, they cannot be the objects of direct perception. But could we still be realist about them? Yes, say

those who are scientific realists (as we are). But there are rival positions of various shades of non-realism. One version of non-realism is scepticism about the existence of the non-observables of science, i.e., a suspension of belief either way. More strongly one could reject their existence entirely and become an idealist or a phenomenalist about scientific objects (instrumentalism being one kind of idealism).

Where does *epistemological constructivism* stand with respect to these positions? Its advocates are not realists about the unobservable objects postulated in science. So they are either sceptics (which allow that such items might exist but we can not know anything of them) or idealists (such items do not exist at all). On either position the items postulated in science can simply be 'constructs' we make out of experience. Note also that constructivists take a non-realist stance about the objects of perception. What we directly perceive are sense impressions. And what we know are simply constructs out of such sense impressions. This might be a surprise to many in science education who flirt with constructivism as a doctrine in epistemology. Such people might intuitively, like the rest of us, be realists about what we perceive; we do see tables, chairs and the like. But they have non-realist tendencies about the objects postulated in science. For them, the objects postulated in science are a kind of construct out of sense impressions (even though it is obscure how the constructs are made). To come out cleanly and say that they are constructs out of sense impressions has counterintuitive aspects. So they adopt an uneasy version of constructivism that does not pursue fully its own implications. But there are radical constructivists who openly do declare that not only are the items in science simply constructs out of experience, but commonsense objects as well. These are *radical* epistemological constructivists. It is this position that we will explore more fully in Chapter 5.

Given the above comments on realism, perceptual realism and their rivals, it can be seen that the issue with which the classical foundations theory of knowledge wrestles is of vital importance, viz., the connection between our mental sense impressions and the external objects of which they are said to be impressions. In the remainder of this section we will explore this matter further. It has an important bearing on constructivism. As will be seen constructivists accept some aspects of the dialectic to be presented but reject others in proposing their own theory of knowledge. Such a version of constructivism might have implications for pedagogy. However in this chapter we wish to keep separate the epistemological doctrine of constructivism, which applies to everyday knowledge and to knowledge in science, from constructivism as a theory of teaching. But it is clear that if one adopts as one's overall account of knowledge what we have dubbed epistemological constructivism, then one might also adopt consequences of it

as a stance towards teaching. Though this latter needs to be kept conceptually separate, we will look at constructivist pedagogy as a possible "flow-on" doctrine in the final section of Chapter 5.

4.4.3 What Are the "Classical" Foundations for Knowledge?

We already have an illustration of what the foundations might be in the case of our knowledge of the size of the Earth. Consider the observation made of the angle of the Sun in the example of Section 2.4.3 (this concerns evidence E_1). Let us suppose that the device used to measure angles has its pointer resting at '7.2' on its calibrated scale. Normally we would report what we observe by saying: 'the angle of the Sun is 7.2 degrees'. Now such a report is pretty close to what we experience, and is based on direct observation. But is it certain? Is it logically possible to subject it to doubt? Yes, we can easily imagine that we have not looked carefully enough, or that our eyes malfunctioned at a crucial moment, and so on. So this report is dubitable, or open to doubt. Or it is fallible in that we can envisage conditions under which we might discover that we have made a mistake and then revise it. Normally, for claims like the above, we do accept what we observe as being correct – but recognise that they are not indubitable and that they are open to an extreme doubt. However we might be lucky in that they are true; and it would then be no fault if we were to build some superstructure of *belief* on such truths. But would it then be *knowledge* that we have built? The foundations theory of knowledge says 'no'; it requires more than mere truths for our foundations. Nor would it be enough to build our edifice of knowledge on truths that we happen to believe. We also want to *know that they are true* in a way in which all doubt about their possibly being false has been removed.

One response to the regress problem is simply to say that we stop at observational reports like the one above, even though they are not absolutely certain. We found our edifice of knowledge not on absolute certainty in which all doubt is removed; rather we base our knowledge on fallible foundations. But then the fallibility get transmitted up the edifice to all the items of 'knowledge" in it. In the strict sense we have "fallible knowledge", which might not be knowledge at all. Karl Popper adopts a version of the regress argument and openly embraces a position of fallibilism, playing down any scepticism associated with it (Popper, 1959, Chapter V). His solution is to ride along with the infinite regress. But it is a regress that does have stopping points, not in classical foundations but fallible foundations. The stopping points are temporary only and they are open to revision latter, if need be. This is just one of many such responses to the regress problem in which the idea of foundations for knowledge does not involve certainty.

For epistemic constructivists such a solution is no real solution to the problems the regress raises. For Von Glasersfeld, radical constructivism

'starts from the assumption that knowledge, no matter how it is to be defined, is in the heads of persons, and that the thinking subject has no alternative but to construct what he or she knows on the basis of his or her experience' (Von Glasersfeld 1995, p. 1). One problematic notion here is that of *experience*. Using the above example, we commonly say that we know by experience that the angle of the Sun is 7.2 degrees; we just look. But often philosophers use the term in the special sense of meaning by our experiences our subjective sensory impressions. Which does Von Glasersfeld mean? It is quite clear in the context of his remark that he means the latter. And this is reinforced by remarks elsewhere when he says:

> All that we really know is that we have had, or are having, a perceptual, or as Piaget would say, a sensory-motor experience …. To conclude that, because we have a perceptual experience which we call "chair", there must be a chair in the "real" world is to commit the realist fallacy. We have no way of knowing what is or could be beyond our experiential interface. If we can reliably repeat the chair experience, we can only conclude that, under the particular circumstances, it is a viable construct. (Von Glasersfeld 1993, p. 26; shudder quotes in original).

The issue of how, within classical foundationalism, we get to the real chair in the world from our sensory impressions of a chair, rather than the "constructed chair", is discussed in the next Section 4.4.4, and in Chapter 5. The main point here is that Von Glasersfeld's radical constructivism accepts much of the epistemic doctrine of classical foundationalism with its radical division between sensory experience and objects in the independent external world. Perhaps he does not accept the aspect of classical foundationalism that requires that our sensory experiences, or reports of them, are certain and beyond doubt; it is not clear. In what follows we briefly examine this central aspect of classical foundationalism.

To illustrate the classical foundations theory, consider an example from Descartes:

> I see light, hear noise and feel heat. But it will be said that these appearance are false and that I am dreaming. Let it be so; so all the same, at least, it is very certain that it seems to me that I see light, hear a noise, and feel heat; and this is properly what in me is called perceiving … (*Second Meditation*, Descartes 1968 p. 107).

Let us expand on this, making a number of distinctions clear.

(1) There is a factual state of affairs in the world involving a light and its shining, viz., the light's being on.

(2) We have in our ordinary language a way of describing such states of affairs, and we can form beliefs about them. In this case it is the belief *that the light is on*. This is what we would normally call an *observation report* in science, based on our experience of the world.

In (2) we have a belief that the light is on. And if this belief is true, it is made true by a feature of the world, viz., that fact in (1) that the light is on, or the light's being on. According to one of the principles of belief we introduced in Section 1.2, we can have the belief that p yet p be false. That is, (2) can be

true while (1) is false. This is a logical possibility: a person does not have the requisite belief even in the presence of a light's being on and their visual system being in good working order and used appropriately. So (1) and (2) are claims that are logically independent of one another

On question we can now ask is: does there exist a special class of beliefs for which this principle does not hold? If 'yes', then for some range of beliefs, if A believes that p, then p is true. That is, there is a special class of beliefs such that as soon as any of us entertains the belief, then it must be true. This is one way some have defined what it means to have a belief that is beyond all doubt and is thus certain. We will use this in what follows as our definition of *certain* (i.e., indubitable – no logically possible grounds for doubt).

One class of such beliefs are first-person beliefs about our own mental contents, for example beliefs about our own inner experiences or sense impressions. That is, we have a special access to own mental contents that no one else can have, and moreover, we cannot be wrong about what is going on inside us when we form beliefs about our experiences.

Thus running parallel to the distinction between facts and beliefs about them, as in (1) and (2), there is another corresponding to experiences and beliefs about experiences.

(3) A perceiver has an experience of lights being on. The experience is something mental and in the perceiver. It is commonly had in the presence of lights that are on when one looks. But as will be seen the experience need not always be produced in this way.

Given these experiences, perceivers describe them using language, and form beliefs about their experiences. This leads to the fourth element, and turns on the large, and obvious, distinction between the experiences we can have and descriptions of, or reports about, or beliefs about, them.

(4) The perceiver has a belief that their experiences are of such-and-such a character. That is, they have a belief that is about their experience and it is expressible in at least the following ways: 'I am having lights-being-on experiences', or as Descartes puts it, 'it appears to me that the light are on'. (Such a report about our experience is not the same as the observational report of (2).)

The final point to note is Descartes claim of certainty.

(5) Such first person reports, or beliefs, about one's mental states are certain and not open to doubt.

We now have all the major elements of the classical foundations picture. The foundations are experiences, or more correctly, reports of these experiences. These foundational items are certain. This gives us the ground floor of knowledge in certainty upon which all other knowledge is to be

based. So it is important to examine whether there is such a foundation in the certainty of experience.

To begin, note that neither (3) nor (4) entail (1). A perceiver can have lights-being-on experiences, and beliefs about this, without (1) being true, viz., that there is a light which is on. Now (3) and (4) are, in normal conditions, caused by the factual state of affairs in (1). But conditions need not be normal. The perceiver might be hallucinating, or on drugs, or their brain might be malfunctioning and giving them such experiences. Or as Descartes famously imagined, an "evil genius", or (not far fetched these days) a neuroscientist, who might be so manipulating the cells of the brain of a person that they have lights-being-on experiences, but no factual state of affairs of a lights being on is the cause of this. Quite different casual processes, such as neurophysiological manipulation, can have the same experiential effect. So given or experiences or the beliefs we have about our experiences we cannot infer directly to the state of affairs causing it.

The fact that either (3) or (4) can hold without (1) holding is of utmost importance. It is the main consideration that generates scepticism about the external world. Experiences can be so-and-so, and our beliefs about them can also be so-and-so. But the world need not be the way we experience to be, or believe it to be; it can be quite other. This is an important feature of the radical constructivism of Von Glasersfeld in which given our experiences, or say a "chair", we cannot know that the world has a chair in it.

Now let us focus on the connection between (3), the having of experiences and (4), our belief about such experiences. Both of these are on the mental side of things. First, note that (3) can be said to be the *cause* of (4). But, second, can (3), an experience, *justify* (4), a belief **?** No. Only a belief can justify another belief, as is commonly said. Things that are not beliefs, including sensory experiences, cannot *justify* beliefs though they may *cause* them. So already there is a logical gap between experience and beliefs about the experiences. Moreover we can show that (4) can hold but not (3), that is, we can have beliefs about our sensory experience but the belief not be true of our actual sensory experience. We will build up to this important result.

Let us now focus on (4), a first-person belief about one's experience such as 'I am having lights-being-on experiences' We have noted that this can obtain independently of (1), the way the world is, viz., the light being on. What we need to consider is whether the belief in (4) can hold independently of (3), our having a "lights-being-on" experience. And we need to consider this is in relation to claims about certainty (5) is this: is this first-person belief certain, and so can serve as a foundation for other knowledge?

Recall what we said above that being certain might mean. It violates a principle of belief in that it claims that for a very special class of beliefs, those about our inner mental goings on such as sense impressions, we have:

"'I believe that p' entails 'p is true'". If this is to be doubted then we have to allow the following: it is true that A believes that they are having lights-being-on experiences; but yet they are not having an experience of light being on and their experience is other than this. Note what this is claiming. It is not that tomorrow I wonder whether the belief I had about my experiences yesterday (viz., I had lights-being-on experiences then) is certain. I can revise tomorrow this very belief about my experiences. Clearly I can be wrong tomorrow about what I experienced yesterday; so such beliefs are not certain. What we need to consider is my current experiences now and my belief, now, about what they are like.

To show this, it is possible to envisage an experiment that would show that my belief (now) of what I was experiences was not correct about my very experiences (now). The experiment to be described may not be technologically possible now, though perhaps actual science is not all that far away from realising it. But we are not concerned here with either technological possibility; rather it is logical possibility. Here is such an logically possible experiment. It would be logically possible for scientists to so wire up a person's brain that they detect not only what *beliefs* they have and their content (which is about their experiences at a time t), but also what *experiences* they are actually having at t. Suppose they investigate the part of their brain where their beliefs occur and they discover that, at time t, the person believes, say, that they are having lights-being-on experiences. Simultaneously they have detectors at work examining that part of their brain where they have experiences; and the detectors reveal that either they are having no experiences at all, or they are having experiences of some sort but they are not lights-being-on experiences. In this case we can have evidence that A has beliefs about their experiences but they are wrong about their experience.

This is very much a science fiction counterexample. But it is a logical possibility. And if it is a bare logical possibility, then that is all we need to undermine the claims of certainty, viz., that there is no possibility of doubt about our beliefs concerning our own experiences. There is much literature on counterexamples such as these that go into the details of the possibility and can be consulted. (See for example Armstrong 1968, chapter 6 section IX 'The Alleged Indubitibility of Consciousness'.)

We are less sure these days of claims about our knowing the contents of our own minds than were people in the past. We are all accustomed to cases in psychology where we mistake our own motive and desires; our real desires and motives might be hidden from us. It is not even necessary to go to Freudian psychology to discover this. The cases that Descartes envisages are very special ones in which what we are said to know, without doubt, are the very contents of our own sensory impressions. But contrary to Descartes, we

can be wrong about these, as when we misclassify pains as tickles, or when injured in a sports game we continue on misjudging the nature and or severity of the pain we have. And in the long run there are cases like the science-fiction example above; it undermines even claims about the logical impossibility of doubt. Thus even though their might be some beliefs that are immune from doubt (e.g., in mathematics or logic, etc), there are others that are not also immune. But alas, it is these other beliefs that Descartes needs to show that they are certain. But they are not. So there are no foundations for knowledge in beliefs that are certain. At best we always have dubitibility and fallibility.

Radical constructivists in epistemology may not take on board that aspect of the classical foundations picture that requires that our beliefs about our experiences must be certain. But, as we showed at the beginning of this section in the case of Von Glasersfeld, they accept the rest of the classical foundations picture with its radical division between our sensory experience or impressions and objects in the external world. An important consequence follows from this; because of this they abandon the view that we can have knowledge of the external world at all. This is a matter we will deal more fully in the next chapter. But it has an important counterpart to the classical foundations theory that we turn to next.

4.4.4 Can We Get From Foundations in Certainty to Knowledge of the World, and to Knowledge in Science?

Having set out the above, we can quickly show that we cannot get from the alleged certainty of classical foundational beliefs about experience to the edifice of knowledge about the world. This we have already shown: from claims like (4) as a premise we cannot infer claims like (1).

> I am having lights-being-on experiences (or it seems to me that a light is on);
> Therefore, a light is on.

The premises can be true, yet the conclusion can be false. And this is so regardless of how many extra reports of our experiences we use as premises. And it is so regardless of whether the first premise (in either version) is certain or is not certain. Its being certain contributes nothing to the validity of the argument. In the light of this point, radical constructivists must also accept the invalidity of this argument. And they do. But they make a virtue of vice by then embracing the view that we can have no knowledge of the external world based on our sensory experiences.

In fact Descartes did recognise that the above argument was invalid. And he tried to overcome it by, next, arguing for the existence of a God that was not a deceiver. In some such theological way Descartes tried to add extra

premises to get to the conclusion about how the external world is. But most commentators reckon that his ploy is not satisfactory.

Hume did not accept Descartes theological solution. Instead he explored the nature of the argument involved, recognising that it might be reconstrued as an inductive argument and not as a deductive argument. This might seem more promising; the conclusion does not logically follow, but has strong support. But this is not promising either for a number of reasons. One of these is that Hume also discussed the nature of inductive reasoning and showed that it cannot be rationally justified. So even if we view the inference as an inductive one it cannot be rationally compelling. So on some interpretations Hume adopted a scepticism about our ever knowing anything about the external world, or about the postulations of science. He recognised the peculiarity of this position when he also pointed out that in our philosophy we end up with conclusions that conflict with what we naturally believe, viz., that there is an external world and that there are the unobservables of science.

Bishop Berkeley was however more extreme. He accepted that the above argument was invalid. But he drew a different inference from this. This is that we are deluded in thinking that there is an external world at all. And if there is not, why are we trying to prove it existence? Simply accept the fact that that all there is to the world is strictly mental, either dependent on our minds or on the mind of God. If we drop Berkeley's theological stance then we do have a genuine ontological idealism in the entities of the world are not independent of us; they are all mind dependent. So all realisms are false.

The above is a very much a thumbnail sketch of some important issues in epistemology. But what they show is that the foundations approach to the regress problem for theories of knowledge is quite central in characterising a number of theories in epistemology. Of interest is epistemological constructivism. On some characterisations it accepts much of the dialectic of the above. It takes the idea that we are to start with reports of experience; but it sets aside matters concerning whether or not they are certain. It accepts that sense impressions are the direct or immediate objects of perception and not objects themselves (though there are some constructivists differ on this point). About how the external world is, either the common sense or scientific world, they adopt a sceptical position (akin to Hume) or an outright idealist position (such as that of Berkeley). So they are not realists in any sense. This clearly create tensions for them when it comes to consider the status of ordinary objects such as chairs, cats or water, and scientific entities such as electrons and DNA. They also reject the idea that any representation is possible, since either there is nothing to represent (the stance of strong ontological idealism), or if there is something out there at all we can know nothing of it (i.e., sceptical idealism). We will consider these issues in Chapter 5.

Since constructivists have no way of anchoring our experiences in reality, as the classical foundations theory aims to do (but fails to do so), then what support could beliefs get? The only remaining possibility is the way in which our beliefs about our experiences can hang together, thereby giving one another some support. This leads them to adopt a coherence theory of knowledge. We will consider it next as a theory of knowledge in its own right which tries to provide a response to the regress of reasons.

4.5 COHERENCE THEORIES OF KNOWLEDGE

The coherence theory of knowledge addresses problems raised by the regress of reasons by querying a number of its features; one of these is the "linear" character of the regress. The regress arises because we believe proposition H and look for evidence E that supports it; in turn this leads to a search for evidence F that supports E; and so on. It appears that there is a linear chain of relations of support to F, then to E, and then to H. But do the relations of support have to always be understood in this linear way? Can there not be a variety of different interconnections between H, E, F, etc, that are not linear but which are interlinked in a more "holistic" way? That is, we ought to look to a range of mutually supportive interrelationships between either all the beliefs in our system of beliefs, or a substantial subset of them. The mutual support provided by such systematic inter-relationships can be called the *coherence* of the set of beliefs. As such the coherence can come in differing degrees of mutual support.

Many theories of knowledge take on board some such notion of the degree of systematic inter-relationship of support amongst beliefs (as well as much else besides). The coherence theory makes such holistic interconnection a central virtue. In a nutshell the coherence theory of knowledge says: A knows that H = (1) H is true; (2) A believes that H; (3) H coheres with A's background set of belief set **B**; (4) the belief set **B** itself exhibits a sufficient degree of coherence (and this is not lowered by the introduction of H into **B**).

For any putative knower A, the coherence theory requires that they already have a sufficiently coherent set of beliefs **B** (where this comes from might be a problem for the coherence theory, but we can pass over this here). If B is not sufficiently coherent then that might undermine the claim that A has knowledge of the items in **B**. Suppose now a new proposition H comes along that is a candidate for knowledge. H is then added to the old set **B** to get the new set **B***. (Perhaps some other beliefs have to be deleted from **B** for H to fit in coherently to produce **B***.) Then for A to know H it is required that the new set **B*** with H in it has a greater degree of coherence, or minimally at least the same degree of coherence, as the old set **B** before H came along. If the degree of coherence of **B*** is lower than that of **B**, then H cannot be an item of knowledge.

Clearly such a definition depends very much on the, so far, undefined notion of coherence. As useful and important as this suggestion is, it remains to see whether talk of coherence is not just the name of a problem, viz., what the mutually supporting inter relationships amongst our beliefs actually are. Importantly the idea that coherence can come in different degrees needs to be spelled out more fully. It is recognised by both supporters and opponents of the coherence theory that mere logical consistency amongst beliefs is not enough. What needs to be recognised is that some beliefs can be highly integrated into the belief set **B**, while others may remain relatively isolated and out on the fringe, or periphery, of **B**. So an important question is just how supportively integrated the set **B** has to be before we can claim to have knowledge.

Often this feature is expressed metaphorically. The classical foundations theory of knowledge uses the metaphor of the "foundations" of a building that supports all the upper floors of a superstructure of belief turning the beliefs into knowledge. The coherence theory has its own metaphor vividly captured by Neurath's analogy with a boat made of planks; each plank gives mutual support to all the others as they collectively make up the structure of a seaworthy boat of knowledge. Our system of beliefs often stand in need of revision; so, according to the metaphor, repairs to the boat can be made only plank by plank while it floats on the sea and not demolished and rebuilt anew as some versions of the "foundations" view require.

Given this much about coherence, it can readily be seen that it provides a promising response to the regress problem. It does not retreat to dogmatism. And it exposes and challenges one of the key features on which the regress depends, viz., its linear character of justifications. Understood this way the coherence theory overcomes another aspect of the regress, viz., the idea that there are unsupported assumptions. On the coherence theory, each belief will receive some support from some other belief, and to some acceptable degree of support. And what of the possibility of infinite regress? It is not a problem, if it exists. Our belief set **B** will contain a potentially infinite number of beliefs. But the coherence theory can live quite easily with the fact that the belief set is infinite, or potentially infinite, without generating an infinite regress of justificatory reasons in a linear fashion. Thus the coherence theory meets some of the challenges to the classical theory of knowledge raised by the regress.

On the positive side the coherence theory does capture one aspect of our beliefs – they do tend to hang together to some extent in a coherent whole. (Note that this feature is not exclusive to the coherence theory and can be adopted by other theories of knowledge.) Thus whenever we find some evidence E for a proposition H, then one kind of coherence relation has been established between E and H. Again when a theory T is used to explain a

range of facts or observational phenomena, O_1, O_2, …, then a kind of coherence has been established between T and all those observational facts. This is an example of explanatory coherence already discussed in Section 2.5.4 where this is seen as a crucial aspect of understanding.

A further important feature of the theory is the following. It compares one belief with another belief, or set of beliefs. But note that the logical relation of support will hold between beliefs only. Such relations of support cannot hold between a belief and any non-belief-like item such as an experience (i.e., sensory impression) or an external world state of affairs, etc. But it can be asked how the coherence theory accounts for the fact that our beliefs must in some way relate to the world or our experience of the world. So far there is only the comparison of one belief with another and not belief with experience or the external world. This is an important feature of the coherence theory that some view as one of its virtues. But others (including ourselves) view it as one of its vices because it simply skirts the important issue that the foundations theory attempted to confront, namely how our beliefs are to be compared either with the world, or our experiences of the world. Such a comparison is crucial for providing a check on our beliefs about the world, and in the next chapter we will argue in detail against the common view that comparing our beliefs about the world with the world or experiences of the world is impossible.

On the foundational theory there is an attempt to make such a comparison. The regress of reasons stops at reports which are required to be indubitably true of the experience they purport to be about. There is a one-to-one confrontation between reports of experience and the experience itself. But for the coherence theory there is no such one-to-one confrontation between belief and experience. At best it is the whole system of beliefs that is to be compared with the totality of experience. As Quine puts it: ‘The totality of our so-called knowledge or beliefs … is a man-made fabric which impinges on experience only along the edges. … A conflict with experience at the periphery occasions readjustments in the interior [of the fabric]’ (Quine 1953, p. 42) Clearly this does not involve a one-to-one comparison of experience and belief but something akin to a holistic readjustment. But how is this adjustment to be made?

It is not within the scope of our project to investigate further the objections against, and defences of, the coherence theory; the literature already cited is replete with them.[8] But enough has been said to show how an epistemic constructivist, with their sceptical attitude to our direct knowledge of the external world and their rejection of the classical foundations view (though they accept much of its problematic), might gravitate to a coherence theory. Some versions of the coherence theory do attempt to give an account of how our set of beliefs makes contact with the world and what constraints

that might play on our beliefs. Yet other versions play down any such connection; it is these versions that are more congenial to constructivists.

However we will mention one difficulty for the coherence theory that can have a bearing on constructivism. This is the objection, raised by Russell and others, that there may be no unique set of coherent beliefs **B** into which H fits. There may be two or more rival (and so inconsistent) sets of coherent beliefs, **B** and **B***, into which H fits equally as well: 'there is no reason to suppose that only *one* coherent body of beliefs is possible. It may be that, with sufficient imagination, a novelist might invent a past for the world that would perfectly fit on to what we know, and yet be quite different from the real past' (Russell 1959, p. 71). Russell then goes on to describe such a situation. We commonly think that the world was created a long time ago, in fact millions of years ago. But Russell envisages that it might have been created five minutes ago by an all powerful creator who made it appear as if there was a long past, since he implants in us all the requisite memories, and makes the Earth with its geological layers so that it appears that it evolved, and so on. Thus there appear to be two rival bodies of belief that fit observations of the world equally well.[9]

Since epistemic constructivists adopt much of the coherence theory, then a limited version of Russell's problem can come to haunt constructivism, and particularly the pedagogy of constructivism. Their starting point is with a pupil's systems of belief. But as Russell suggests, this system of belief, while internally coherent, may be a weird alternative, like that of the all-powerful creator who made the world five minutes ago. The pupil has ignored the possibility of other alternatives that can have more plausibility and have been widely adopted in scientific systems of beliefs. So there is no reason to think that students' own beliefs about the world need have much correspondence with it. But then the idea of any connection between a system of beliefs and the world is something that is either downplayed or denied within some versions of the coherence theory; and it is often denied within radical epistemic constructivism as well (as will be seen). So a version of Russell's problem can arise when little attention is paid to the constraints provided by some correspondence with reality.

There is also the problem of what to do with experiences that do not fit with the pupil's initial system of beliefs. The pupil is then invited to adjust their system of beliefs so that some kind of internal coherence is restored. There are various ways, good or bad, to restore coherence. (Coherence for whom? The pupil?) But the restoration of internal coherence can often still be at the cost of lack of genuine connection with the world. These issues will be revisited in Chapter 5 where it will be seen that for radical epistemic constructivists connections with the world do not matter; they have simply abandoned the idea of comparing (systems of) belief with reality. The only

"reality check" is the one that arises from the alleged internal coherence of belief systems. The external world plays no role in such radical theories.

4.6 THEORIES OF RATIONAL DEGREES OF BELIEF RATHER THAN KNOWLEDGE

There are a wide variety of other theories of knowledge that provide a response to some of the philosophical difficulties that can be raised for knowledge, such as those of the regress problem. But not all of these theories are germane to our purposes here. (For a review of them see Williams 2001, Audi 2003, or Dancy and Sosa (eds.) 1992.) In this final section we will review just one further theory. This is the view that we should abandon the largely honorific notion of knowledge plagued with its problems of scepticism, lack of adequate definition, and so on. Instead we should replace it by the idea of a *high rational degree of belief*. This is a notion we have met before in the discussions of belief in Sections 1.2 and 2.2. The full significance of this position will not be revealed until Part II, especially Chapter 9, where we discuss probability and Bayesianism as a theory of scientific method. It provides one response to problems that arise in philosophical analyses of knowledge.

The probabilistic view is well expressed by Bertrand Russell who tells us that there are two important dicta concerning probability: 'The first of these dicta is Bishop Butler's maxim that "probability is the guide of life". The second is the maxim that all our knowledge is only probable …" (Russell 1948). What the first dictum emphasises is the very important role of probabilistic modes of thinking in the conduct of not just our sciences but also our everyday lives, even though we might not notice this fact. The second dictum is more radical; it claims that knowledge, especially conceptions of knowledge which cling to conceptions of certainty, are in fact nothing more or less than rational degrees of belief with a high degree of support

In a much earlier discussion in a chapter of a widely read 1912 book entitled 'Knowledge, Error and Probable Opinion' Russell finds a *via media* between erroneous belief and a strong conception of knowledge not unlike the classical theory which gives rise to the regress when he says:

> What we firmly believe, if it is neither knowledge nor error, and also what we believe hesitatingly because it is, or is derived from, something which has not the highest degree of self-evidence, may be called *probable opinion*. Thus the greater part of what would commonly pass as knowledge is more or less probable opinion' (Russell 1959, p. 81)

In this earlier position, Russell is willing to adopt a tripartite division of belief into the erroneous, probable, and then knowledge. In his later position, the last of these, knowledge, disappears into the second, that of probable belief of a high degree. An even more radical position would be to give up on the idea

of error and replace it by rational probable belief of a very low degree (even down to zero).

Since Russell wrote the above, the theory of rational degrees of belief, understood as a probability, has over the last 30 years or so, become a leading, if not the dominant, theory of "knowledge". It is now the dominant theory of method in science, our most self-conscious way of obtaining knowledge of the world. It also has a way of dealing with the regress of reasons problem. Are we to accord full belief, or at best only partial belief, to our reports of observational evidence E (such as that discussed in Section 4.4.3)? What the probabilistic theory allows us to do is to give up on the idea that our foundational beliefs have to be certain, i.e., we must always accord full belief in them so that probability (E) = 1. It is possible to accord them less than full belief, and thus a probability of less then 1 (though it may still be high). And when we do this, it still remains possible to assign a probability to the degree of support that E gives H even when E is less than certain. This would assist greatly in overcoming the problem of the regress while maintaining much of the 'foundational' view that seems correct. Some of these matters will get further discussion in Chapter 9 that is devoted to a discussion of probabilistic theories of belief as a theory of scientific method

This ends our brief survey of some of the theories of knowledge that have been developed in response to concerns about the problem of the regress. In the next chapter we will consider one theory that has become dominant in science education circles that draws on many of the issues that concern the classical foundations theory, the coherence theory and scepticism, viz., *epistemic constructivism*. We will approach this as a theory within epistemology that attempts to address some of the philosophical issues raised above, and attempts to give an overall account of the nature of knowledge, and in particular scientific knowledge. We also discuss some implications for constructivist pedagogy that flow from it.[10]

NOTES

[1] The counterexamples not considered here are generally known as the 'Gettier problems' for knowledge, originally due to Edmund Gettier but some of which are older and go back to Bertrand Russell. For some book length treatment of issues either passed over or mentioned all to briefly in the above see, for example Musgrave 1993, Williams 2001, Audi 2003 and Dancy and Sosa (eds.) 1992, and their bibliographical references to what is a very extensive literature.

[2] There is extensive discussion in the philosophy on the justification of both deductive and inductive inferences. We touch upon the justification of induction in chapter 7. For an introduction to the justification of deduction see Haack 1996, pp. 183-213.

[3] Descartes' position is set out in his *Meditations*, the Fourth Meditation. Locke' position can be found in Locke 1975, pp. 687-8.

[4] Those who discuss issues concerning the ethics of belief include Pojman (1995). A recent defence of the idea of an ethics of belief is Adler 2002.

[5] The line of criticism developed here is much more fully discussed in Burnyeat 1976, an excellent commentary that deals with some of the difficulties in Plato's own account in the *Theaetetus* of the self-refuting character of relativism. A different approach can be found in Putnam 1981. The above only focuses on relativism with respect to truth. Much of the voluminous literature in relativism, some cited above, discusses relativism with respect to items other than truth.

[6] We accept much of the account of artefacts and social entities and their role in a realist framework as set out in Searle 1995. He speaks of the "social construction" of these items. However this can give no comfort to constructivists since Searle carefully distinguishes his position from that of most constructivists one would find in science education.

[7] See Ayer 1940 and Austin 1962. For a more recent advocate of Austin's realist position by an erstwhile critic of various kinds of realism see Putnam 1994.

[8] See, for example Williams 2001, chapter 10, Dancy and Sosa (eds.) 1992 under 'coherentism', and the references found in both works.

[9] We have passed over in the text Quine's famous thesis of underdetermination, in which two or more theories are alleged to fit the observable facts equally well. On this see the first essay of Quine 1953, the second dogma. But many of Quine's other writings discuss this issue and a large critical literature has grown around it in the philosophy of science.

[10] There are many more theories of knowledge that could have been considered alongside those in the text but for reasons of space were not. One theory not considered is reliabilism; but it is employed in Sections 2.4.1 and 3.2 to illustrate certain features of the nature of knowledge. See the books referred to in footnote 1 for accounts of reliabilism.

CHAPTER 5

VARIETIES OF CONSTRUCTIVISM AND THE INACCESSIBILITY OF REALITY ARGUMENT

Constructivism is probably the most widely accepted theory among science educators. Indeed, it would not be an exaggeration to say that in the last two decades constructivism represents a paradigm change in science education. One indication is the enormous amount of literature devoted to it. According to one estimate, 2500 scholarly articles inspired by it appeared in science education journals by 1993 (Duit 1993), and no doubt this number has grown even bigger since then.

Constructivism is not a unitary theory, and it has proponents outside the community of science educators as well. It has many varieties, some being more plausible than others. First, there is constructivism in sociology of scientific knowledge. This is known as *social constructivism*, a view that says that even the very content of knowledge, including scientific knowledge, can be explained causally by social factors. Social constructivism owes much to sociologists of science David Bloor and Barry Barnes and will be taken up in chapter 11.

In the field of education the term "constructivism" refers to a theory of "knowing", teaching and learning. Although constructivism in science education is predominantly an epistemological and pedagogical theory, it also contains ontological, semantical, and cognitive aspects. Unfortunately, these distinctions are not always observed in the literature on science education, resulting in much confusion. Accordingly, in Section 5.1 we distinguish between four types of constructivism: cognitive, semantical, epistemological, and ontological. We argue that with the sole exception of cognitive constructivism, all other three versions are flawed in various ways.

The bulk of this chapter is devoted to a careful scrutiny of an argument, which we prefer to call *the Inaccessibility of Reality Argument* (IRA for short). This is the philosophical underpinning of what we distinguish as epistemic constructivism. IRA is an old and pervasive argument among not only constructivist science educators, but also philosophers with anti-realist inclinations. It attempts to undermine any realist account of truth and thus knowledge in science and elsewhere. This is the claim that we can never compare either our experiences of, or our beliefs about, reality with how reality is because, in order to check whether our experiences or beliefs correspond with

reality, further experiences or beliefs must always intervene. Thus we can never have direct knowledge, or more strongly any knowledge at all, of how reality is. Constructivists take this to heart and look for accounts of knowledge that are anti-realist and coherentist and/or constructivist in which the role of the external world in knowledge claims is thoroughly downplayed or non-existent. Since IRA is a central tenet of constructivism both with respect to scientific knowledge and learning, we have devoted several sections to it. Much of the groundwork for this has been laid in Chapter 4 in which various epistemological theories have been discussed. As has been pointed out, epistemological constructivism takes it cue by denying certain claims made in the classical foundations theory of knowledge and adopting aspects of the coherence theory of knowledge; but in effect it is an independent theory.

More specifically, in Section 5.2 we quote from constructivist literature in education, which shows how widely IRA is employed. In Sections 5.3 and 5.4 we focus on two of the most influential constructivist theorists in education, namely Piaget and Glasersfeld, who are advocates of IRA, and on some of the many philosophers who have endorsed IRA. Though widespread, IRA is not without serious difficulties, and an early protest came from the logical positivist Moritz Schlick in the 1930s. Schlick's is a refreshing common-sensical opposition that deserves attention. In Section 5.5 we distinguish two versions of IRA that argue for the inaccessibility of the external world of common sense. The first depends on the idea that there is a veil of perception that keeps the external world forever beyond our direct inspection. The other is that the very activity of applying concepts to reality somehow shuts off that reality from us. In Section 5.6 we focus on the second of these versions of IRA. We set out the impossibility argument and show how it can be disarmed. We do this in outline but indicate the philosophical literature where these arguments are much more fully developed.

After we dispel the spectre of IRA, we turn to a discussion of constructivism as a pedagogical theory in Section 5.7. It is important to distinguish two aspects of constructivism, epistemic and pedagogical. First there are the claims of constructivism to provide a theory of knowledge. But we have shown in the previous sections that it is really a warmed up version of scepticism or idealism, and that it is wedded to the quite counterintuitive claim of IRA that it is impossible to compare our beliefs with reality. Second, and quite independently, constructivism aims to provide a theory of teaching and learning. While there is much to commend in constructivism as pedagogy, it provides little novelty in this regard. Long ago it was anticipated by many, from those at the beginning of philosophy such as Socrates (see Section 3.3) up to the 20th century.[1]

5.1 FOUR VARIETIES OF CONSTRUCTIVISM

Any talk of construction elicits the following questions. Who does the constructing? What is constructed? Out of what and how? Getting clear on the answers to these questions is essential for understanding what constructivist theories say. The question "what is the material out of which constructions are carried out?" is typically answered as "experiences" by most constructivists, but we are never told how. As is well known, philosophers such as Bertrand Russell and Rudolf Carnap spilled considerable ink on how to construct meanings and "objects" out of sense data or cross-sections of experiences, but without much success, as they themselves have admitted (see Carnap 1967). Constructivists in science education seem not to be aware of the philosophical programme of what might be called 'logical constructivism' which attempts to construct all knowledge out of experience, using the tools of logic. Had they known, they might not have engaged in a programme doomed to failure from the start. As for the question of who does the constructing, they usually reply "individuals with cognitive abilities"; but groups are also considered in social constructivism. Finally, we can ask "what is it that is constructed?" In many ways this is the most crucial question and gets a variety of different answers. Depending on the answer, we get a plausible or a totally unacceptable version of constructivism. Below we focus on four responses. A fifth kind of constructivism, pedagogical, has been introduced in Section 3.3 and is further considered in Section 5.7.

5.1.1 Cognitive Constructivism

A modest proposal is that mental representations are constructed. We call this theory *cognitive constructivism*. It is the view that 'individual cognitive agents understand the world and make their way around in it by using mental representations that they have constructed' (Grandy 1998, p. 114). (Note that the question of who does the constructing is also answered: it is individual cognitive agents.) Cognitive constructivism is widely endorsed by constructivists (see, for example, Bickhard 1998, Grandy 1998, and Von Glasersfeld 1987 and 1989). Cognitive constructivism is such a minimalist position that it can be taken as the core of all constructivisms, a core which is compatible with realism as well. Indeed, we do not know of any realist contemporary philosopher who would deny that mental representations are constructed rather than passively received (but there are non-realists who are anti-representationalist). No philosopher today believes that objects simply imprint their properties on a passive mind which in turn merely reflects them as accurate representations, as mental copies. Even the arch-realist Karl Popper wrote that 'we are not passive receptors of

sense data, but their active digestors' (Popper 1963, p. 95). Therefore, we can leave this brand of constructivism aside as being uncontroversial.

5.1.2 Semantical Constructivism

By *semantical constructivism* we mean the view that the meanings of terms and statements are not readily found in nature or society, but are constructed out of experiences (i.e., sense impressions). Semantical constructivism is endorsed by many science educators (see, for example, Driver 1988, Fensham *et. al*. 1994, Gunstone 1988 and Von Glasersfeld 1995, chapter 7). Here is a typical formulation:

> Concepts are not inherent in things but have to be individually built up by reflective abstraction; and reflective abstraction is not a matter of looking closely but of operating mentally in a way that happens to be compatible with the perceptual material at hand. (Von Glasersfeld 1995, p. 184)

According to this view, the meaning of the term, say, 'apple', is abstracted (constructed by abstraction) from apple-experiences. But because, on their understanding, experience is taken to be totally private and subjective, each person has his or her own concept of *apple* even when tasting, smelling, touching and looking at the same apple. Therefore, no two people's concepts of apple are the same. This argument can be extended to all concepts. From this it follows that there is no sameness of meaning; and this creates an acute indeterminacy of linguistic communication. Indeed, Von Glasersfeld writes that 'language does not convey knowledge' but only 'constrains and orients the receiver's conceptual constructing' (*ibid*., p. 182). Since there is no sameness of meaning, one can have at best compatibility: 'your meaning and another's are at best compatible; in a given situation neither reacts in a way that the other could not expect' (Von Glasersfeld 1993, p. 32).

This theory of meaning is, however, seriously flawed. For one thing, incompatibility requires some sameness of meaning as when one asserts 'the litmus is red' while another asserts 'the litmus is not red'. Here words mean the same, but one denies what the other asserts. Disagreement only takes place against a background of shared meaning. If there is no shared meaning, then trivial compatibility arises; we discourse on different topics and mean quite different things, thereby constantly talking past one another.

Constructivists like Von Glasersfeld (at least in the above) do not recognize the public character of concepts and meanings of statements. They think that because these things are constructed out of mental representations, they can have no shared content. But they fail to see the distinction between mental *representations* and their *content*. While the former are private and subjective, the latter are public and intersubjective. I may *believe* that today is Sunday, and you

may *deny* it; somebody else may *hope* that it is Sunday. Each of us has different mental states (believing, denying and hoping), but they are all about the same content expressed by the statement *that today is Sunday*. No doubt, we may sometimes disagree about the content of a certain statement, but from this it does not follow that "my meaning" is different from "yours". All that it shows is that we have different intentional states toward it: I believe it, you deny it.

There are several theories of concept formation that do not rely on abstraction.[2] But semantic constructivism about concepts does turn on an account of abstraction. There are many objections to abstraction only one of which we will cite here. The incisive criticism by the logician Gottlob Frege is both instructive and amusing. The essence of abstraction is to neglect certain features of objects until only that which is common to all of them is left. Frege points out that this procedure has no way of distinguishing between a situation where one deliberately ignores the features and another where one simply forgets them. It is worth quoting him at length:

> Inattention is a most efficacious logical difficulty; presumably this accounts for the absentmindedness of professors. Suppose there are a black and a white cat sitting side by side before us. We stop attending to their color, and they become colorless, but they are still sitting side by side. We stop attending to their posture, and they are no longer sitting (though presumably they have not assumed another posture), but each one is still in its place. We stop attending to position; they cease to have place, but still remain different. In this way, perhaps, we obtain from each of them a general concept of Cat. By continued application of this procedure, we obtain from each object a more and more bloodless phantom. (Quoted in Coffa 1991, pp. 68-69)

We conclude therefore that there is nothing to be gained from constructivist theory of concept formation and learning.

5.1.3 Epistemic Constructivism

Epistemic constructivism is the most widely held version of constructivism. It is the view that all knowledge, including scientific knowledge, is constructed by cognizing agents (see, for example, Confrey 1990, Driver 1988, Gunstone 1988, Von Glasersfeld 1995, and Wheatley 1991). Epistemic constructivism is also claimed to be the most original contribution of constructivism to the nature of knowledge. Its greatest theoretician and defender is Ernst Von Glasersfeld. He calls his version of epistemic constructivism 'radical constructivism' and formulates it as follows:

> 1a. Knowledge is not passively received either through the senses or by way of communication.
> 1b. Knowledge is actively built up by the cognizing subject.
> 2a. The function of cognition is adaptive, in the biological sense of the term, tending towards fit or viability.

> 2b. Cognition serves the subject's organization of the experiential world, not the discovery of an objective ontological reality. (Von Glasersfeld 1995, p. 51; the numbering is ours.)

The extent to which knowledge involves representations or beliefs, 1a and 1b can be seen as an articulation of cognitive constructivism. This confirms our earlier judgement that the latter is the minimal core of all constructivisms, including epistemic constructivism. As we have pointed out, this minimal core as expressed by 1a and 1b is non-problematic and granted by non-constructivists as well. The crux of the matter lies with the definition of knowledge:

> For constructivists, therefore, the word *knowledge* refers to a commodity that is radically different from the objective representation of an observer-independent world which the mainstream of the Western philosophical tradition has been looking for. Instead, *knowledge* refers to conceptual structures that epistemic agents, given the range of present experience within their tradition of thought and language, consider *viable*.' (Von Glasersfeld 1989, p.124).

This definition of knowledge is markedly different from those of the theories of knowledge discussed at length in Chapters 2, 3 and 4. If we take Von Glasersfeld's term 'conceptual structures' to mean beliefs or, at least, to include beliefs, then it becomes clear that for him knowledge involves neither a third justification condition nor the first truth condition. Setting these lacunae aside, let us ask if his conception of "knowledge" in terms of viability is superior to traditional accounts of knowledge. To answer this question, we must first clarify what Von Glasersfeld means by a 'viable conceptual structure' or a 'viable conceptual construct'. Von Glasersfeld defines a viable conceptual construct as one that 'fits the purposive or descriptive contexts in which we use them' (Von Glasersfeld 1995, p. 14). But such a notion of viability is too weak to replace the conditions that are central for any kind of *knowledge that* viz., justification and truth, where truth is understood as at least a pairing of our beliefs with (bits of) reality. In other words, a construct that is viable, i.e., fits our purposes in the contexts in which we use it, can nevertheless be false because it may not describe things as they really are. However there is another way of understanding this that is left to later sections where it is discussed more fully.

Von Glasersfeld is explicit about his denial of truth as correspondence. He writes:

> Knowledge can never be considered true in the conventional sense (i.e., correspond to an observer-independent reality) because it is made by a knower who does not have access to such a reality (Von Glasersfeld 1995, p. 94).

> Constructivism does not deal with the traditional concept of 'truth' which would require that one knows, or at least believes, that an idea, a theory, or any conceptual construct is an accurate representation or duplication of something beyond the experiential field. (Von Glasersfeld 1993, p. 27)

The first quotation documents not only Von Glasersfeld's rejection of a minimal correspondence view of truth, but also his reliance on the Inaccessibility of Reality Argument (or IRA). We will discuss this argument in detail later in this chapter. For now we are content to point out that Von Glasersfeld mistakenly attributes to the 'traditional concept of truth' the requirement that if a belief is true, then it must also be known to be true. But the traditional account does not require this at all. What it requires is that if I know that p, then p must be true, but not that I must also know that p is true; otherwise, the definition would be circular. Von Glasersfeld confuses matters to do with the definition of truth with knowledge, or with belief about what is true. He also insists that correspondence must always be some kind of duplication. But no copying or duplicating is required in the bare notion of truth as a pairing of beliefs with what makes them true. In Section 2.3 we set out what this might mean in terms of Aristotle's definition of truth, or Tarski's account of what T-sentences are. No copying or duplicating is involved here. It is not necessary to foist these notions on the broader notion of correspondence.

We have seen that Von Glasersfeld wants to give a definition of knowledge that does not appeal to truth as any kind of correspondence, even correspondence as weak as the pairing involved in Aristotle's or Tarski's accounts of truth. He believes that his definition in terms of viability does the trick. But does it? That it does not is evident from the following passage:

> If a prediction turns out to be *right*, a constructivist can only say that the knowledge from which the prediction was derived proved viable under the circumstances of the case" (Von Glasersfeld 1993, p. 26; our emphasis).

But to say that a prediction turns out *right* is just another way of saying that an observational consequence of a theory came out to be *true*! Therefore, talk of the viability or unviability of conceptual constructs is merely a mask for talk of truth or falsity of predictions when matched against observation. Truth of the kind set out in Section 2.3 is not avoided; appeal is made to it!

Moreover, Von Glasersfeld is completely silent about what happens when a prediction goes wrong, and even if he did appeal to "non-viability" to make sense of it, one might wonder if this is another mask for falsification. This is not a trivial issue and is much discussed in the philosophical literature under the rubric of the Duhem-Quine problem (see Section 8.4.2). The notion of viability sheds no light on the solution of this thorny problem, and Von Glasersfeld has made no contribution in this regard.

Despite claims to the contrary, Von Glasersfeld's conception of knowledge in terms of viability is not original at all. It is a version of instrumentalism, a view much discussed in philosophy. That his conception is instrumentalist is evident

from claim 2b above. Cognition serves the function of bringing order to our experiences, but emphatically not 'the discovery of an objective ontological reality'. Von Glasersfeld openly admits that he is an instrumentalist (Von Glasersfeld 1998, p. 97; see also his 1995, p. 22). Accordingly, our 'conceptual constructs' (such as our theories) are not true or false, but just instruments or tools which organize our experiential world and enable us to cope with whatever problems we are confronted within it. Indeed, this is a world into which we are trapped, from which there is no escape: 'From the constructivist point of view, the subject cannot transcend the limits of individual experience" (Von Glasersfeld, 1995, p. 2). Thus, it follows that we can only know either our experiences or what we can construct on the basis of these experiences. We are back to the idea that reality is inaccessible. This brings us to the next version of constructivism.

5.1.4 Ontological Constructivism

Ontological constructivism is the most radical version of constructivism. It claims that not only our representations of the world are constructed, but also the very furniture of the world itself, presumably out of our (individual and private) experiences. Since ontological constructivism denies the existence of reality independent of our experiences or our minds, it is a form of idealism. As we pointed out in the previous chapter, the 18th century philosopher Bishop Berkeley held such a view. He thought that objects are just a bundle of sense-impressions. Are there any constructivists today that honestly hold this extreme view? Consider the following two quotations from Von Glasersfeld:

> The crucial difference between the realist and the constructivist, thus, is not that the one projects his cognitive structures beyond the experiential interface, while the other does not; the difference is that the realist believes his constructs to be a replica or reflection of independently existing structures, while the constructivist remains aware of the experiencer's role as originator of all structures, i.e., for the constructivist there are no structures other than those which the knower constitutes by his very own activity of coordination of experiential particles. (Von Glasersfeld 1987, p. 104)

> To conclude that, because we have a perceptual experience which we call a 'chair' there must be a chair in the 'real' world is to commit the realist fallacy... If we can reliably repeat the chair experience, we can only conclude that, under the circumstances, it is a viable construct. (Von Glasersfeld 1993, p. 26)

In these passages Von Glasersfeld seems to be endorsing the ontological constructivist (i.e., idealist) position. To believe that there is a real table out there in the world is to 'commit the realist fallacy'. If all structures are constituted by us out of 'experiential particles', then there are no objects that exist independently of us (our minds, experiences, etc.). This is simply idealism. If one

is to be consistent then even ourselves, which we take to be real items in the world, must be constructs out of experience. But whose experience? Our own, or someone else's? It would seem that ontological constructivism, extended as it should be to the existence of individual persons, is caught in the trap of solipsism.

Idealism is such a counter-intuitive position that is extremely difficult to subscribe to it consistently – at least in our times. Indeed, Von Glasersfeld vehemently denies that he is an idealist:

> For believers in representation, the radical change of the concept of knowledge and its relation to reality, is a tremendous shock. They immediately assume that giving up the representational view is tantamount to denying reality, which would indeed be a foolish thing to do. The world of our experience, after all, is hardly ever quite as we would like it to be. But this does not preclude that we ourselves have constructed our knowledge of it (Von Glasersfeld 1995, p. 14-5).

What should we make of this inconsistency? Why does Von Glasersfeld write as if he were endorsing idealism in one place and deny it elsewhere? A clue to the answer seems to lie in the sentence 'the realist believes his constructs to be a replica or reflection of independently existing structures'. Von Glasersfeld denies that our beliefs (his 'constructs') about the world can more or less accurately represent (bits of) reality. In other words, he is presupposing the inaccessibility of reality argument. In Section 5.3 we will look at some passages that indicate more clearly his endorsement of the IRA argument. For the present our concern is to understand Von Glasersfeld's inconsistency. Perhaps there is a slide between three positions that seem to be similar. One is that of deep scepticism of the sort advocated by some ancient philosophers in which there is an external world but we can know nothing of it. The second position is idealism in which there is no external world of which we can be sceptical as to its existence (see Sections 4.2 and 4.4.2). The third is a kind of Kantianism in which if there is a mind-independent reality inaccessible to us (the noumenal world), then it is quite reasonable to assume that the existence of reality somehow depends on us if we are to have knowledge of it (the phenomenal world). Von Glasersfeld seems to move swiftly between all three positions.

The view that reality is inaccessible to us human beings looms large in constructivist theory. It raises its ugly head every time constructivists talk about knowledge. As we saw, it has come up both in the epistemic and ontological versions of constructivism. It is time therefore that we turn to it.

5. 2 CONSTRUCTIVISTS IN SCIENCE EDUCATION SAY: 'WE NEVER SEE TABLES!' – 'DON'T WE EVER?'

You are sitting in front of a table. It has a computer, a lamp and some pens on it. Your eyes are wide open. What do you see? Clearly, you see the table, and the lamp, and some pens. What could be more obvious? Putting this more generally, we can say, we see ordinary objects.[3] This is the position of direct perceptual realism set out in Section 4.4.2. Strange as it may seem, many in science education want to deny that we see objects, including constructivists. In what follows we provide a small sample of quotations taken from the many claims of constructivists in science education. This will display the widespread denial of perceptual realism throughout the last twenty years of constructivism. We have seen this in the case of Von Glasersfeld in the previous section; but he is not a lone voice on this matter in science education which has a long involvement with its rejection.

Quotation 1. We do not see a table because it is hidden from us.

> Although we may assume the existence of an external world we do not have direct access to it; as public knowledge is not so much a discovery as a carefully checked construction. (Driver and Oldham 1986, p. 109).

This is a quite direct support for scepticism about our every seeing a thing like a table. We might assume tables exist but we have no direct access to them. (Is this so even when we sit on them?) This lack of access is so widespread for all of us that there can be no public knowledge of the very same table. At best there is a publicly available construct standing in for the table that we cannot access. What is the status of this second item? This is unclear. There now seem to be two tables, one to which we do not have access, and the other which we construct. (How we construct it remains unclear but it is not the way in which a carpenter first "constructed" the table.) One may be puzzled as to how we carefully check the constructed table. Since the real table is allegedly not accessible then whatever checking there may be of the constructed table it cannot be a check against the real table. Note finally that the author does not talk about the construction of the *concept table* (the construction of concepts by pupils is pertinent to science education); rather it is the construction of tables themselves that us up for consideration.

Quotation 2. If a table remain hidden from us, then we can make our own, and see it!

> Put into simple terms, constructivism can be described as essentially a theory about the limits of human knowledge, a belief that all knowledge is necessarily a product of our own cognitive acts. We can have no direct or unmediated knowledge of any objective reality. We construct our understanding through our experiences, and the character of our experience is influenced profoundly by our cognitive lens. (Confrey, 1990, p. 108)

Once again we can have no direct knowledge of any objective reality, which must include common sense objects such as tables, and the objects of science as well. And this supports the contention that knowledge is limited. But why? The last sentence is confusing with its talk of the construction of understanding rather than other items such as the objects that populate 'objective reality'.But it appears that 'our cognitive lens' plays a big role in both (a) making it impossible for us to have direct knowledge of reality, and (b)in constructing "understanding" through operating, in some manner, on our experiences, understood as sense impressions.We will discuss claim (a) more fully when we consider the Inaccessibility of Reality Argument (IRA) more fully in Section 5.6.

Quotation 3. Even in science we never see postulated objects such as electrons! So why claim we see tables?

> Scientific knowledge is invented in order to make sense of observations, that are themselves theory-laden. There is no great book of nature that can be consulted in order to check if the models or theories correspond to an ontological reality. (Desautels and Larochelle 1990, p. 236)

Setting aside the issue as to whether all observation is theory-laden, here we have IRA again, or the **"**no checking against reality**"** claim. But this time it is expanded to include the lack of any comparison between the unobservables postulated in our theories, or in our models, and an external world; for example, there is no comparison between our theories or models of an electron and real electrons. There might be some point to this claim because we cannot observe real electrons (supposing they exist) and so cannot directly compare what we have in our models or theories with what is in reality. So how do the scientists pull off that trick? Have we been kidded all along that there really are electrons, as our theories say?[4] Here the problem appears to be different from that of tables, since we think, or did so until the constructivists came along, that we could see tables; in contrast electrons are said to be unobservable so we cannot see them. But we can detect them, as in cloud chambers, and so on. Clearly a story needs to be told here about how scientists successfully detect the unobservable entities they postulate.

Quotation 4. We do not have any knowledge of tables. We merely have 'viable explanations' of our table-experiences.

> The Theory of constructivism rests on two main principles … Principle one states that knowledge is not passively received, but is actively built up by the cognising subject … principle two states that the function of cognition is adaptive and serves the organisation of the experiential world, not the discovery of ontological reality … Thus we do not find truth but construct viable explanations of our experiences. (Wheatley 1991, p. 10)

Principle one can be endorsed by even non-constructivists. We certainly are the makers of theories, and the makers of models of reality. And we also hunt up evidence, construct experimental apparatus, and make mathematical calculations.

We are very busy cognitively, but not so busy as to construct truth! Importantly, none of this precludes getting a check against nature. But this is just what the constructivists deny with their Principle two. We are super-active in that we organise, not the external world, but our experiential world. What our experiences are of, remains unclear. We just have them. They are the material out of which we do our constructing. What we construct is, if not a real table, at least a look-alike table. But we cannot really say that, as we cannot compare the look-alike table with the table of external reality, which in the above quotation seems to have gone AWOL. We just have the construct, and what it looks like goes by the board. Whatever the case, again we do not see tables.

Quotation 6: Finally John Staver expresses what he calls a 'root paradox' of knowledge using the common metaphor of a lens:

> … we humans, individually or collectively, possess but a single lens, that of experience, with which we can learn of the world about us. To independently check any knowledge claim we, individually or collectively, must check it through a lens independent of experience. To date, several thousand years after being set forth, no one has bought forth, tested and validated such a lens, and consequently, no one has resolved the root paradox' (Staver 1998, p. 505).

Here we have a version of IRA in its classic form; we cannot check any of our knowledge claims against reality. In order to do so we must have a grasp of the world independently of any intervening experience; but alas, we cannot have this. The reasons for this will be set out, and evaluated, in Sections 5.5 and 5.6.

Amongst the various claims that have been made in the above quotations, there is one central claim, viz., that for whatever reason we cannot check our experience against reality. This might happen because intermediaries (whatever they are) always get in the way between us and external world tables so that we cannot see tables directly. Or we are so cognitively active that we do not need real tables at all, and merely construct a "table" out of our experience – and then we see at least a "constructed table" if not the real table. These two different strands will become important later. But they both have the same upshot: we cannot make any direct comparison with an external object such as a table. Given the pervasiveness of this claim in the history of philosophy, and elsewhere, it is surprising that there is no agreed name for it. Two obvious names suggest themselves: the *No Comparison Argument*, or the *No Checking Argument*. We have adopted the name *The Inaccessibility of Reality Argument*, which is suggested by Philip Kitcher. Of this he says: **'**The IRA is a terrorist weapon which anti-realists employ with enormous confidence**'** (Kitcher 1994, p. 122; Kitcher 2001, p. 156),

The dialectic under investigation has a form commonly found in philosophy. We start off with a seemingly uncontroversial, even obvious claim.

One such claim is that we see tables; another such claim is that we can often compare our experiences, beliefs and/or judgements with the external world to see if they are true or not. Then an argument is produced to show that we do not see tables, or the comparison of our perceptual experiences or judgements with any external reality is always impossible. The upshot is that there is a conflict between our common belief, say that there are tables here about, and some philosophical argument which says that we can not ever know this because reality is inaccessible to us – or worse, that there are no tables present at all but just our constructs! Some find the argument so persuasive that they abandon their previous belief that such comparison is possible. Others, like us, find the argument deeply suspect; and then try to show what is wrong with it.

5.3 PIAGET AND VON GLASERSFELD AS ADVOCATES OF IRA

Piaget is one of the leading theoreticians to influence constructivists. We have briefly discussed Piaget's theory of learning in Section 1.5.2. There we contrasted it with Popper's account of the growth of science through problem solving and the application of principles of science method. The important point is that while Piaget's approach might superficially resemble a trial and error model of learning, his 3-part model lacks an essential ingredient, viz., the role of principles of critical inquiry. This is made clear in Popper's contrasting 4-part model with it specific role for rationally based critical assessment.

Although Piaget puts emphasis on scientific naturalism and places his theorising within this context, he nevertheless adopts a version of IRA, largely because of his doubts about the correspondence theory of truth. He tells us:

> While we may not know exactly what life is, we know still less about the meaning of cognitive "truth". There is a sort of general agreement that it is something more than a faithful copy of the world of reality for the very good reason that such a copy could not possibly be made, since only the copy could supply us with the knowledge of the model being copied, and moreover, such knowledge is necessary for the copy to be made. Attempts to make this copy theory acceptable have only resulted in a simple phenomenalism, in which the subjectivity of the ego is perpetually interfering with the perceptual datum – a theory which betrays the inextricable mixture of subject and object.

> If the true is not a copy, then it must be an organisation of the real world. … knowledge is *essentially construction*. (Piaget 1971, Section 23.1)

Piaget (or his translator) says that truth is something *'more* than a faithful copy'. But since the copy account is rejected, Piaget's positive account cannot include the copy view. In the light of this perhaps 'more' should be replaced by 'other'. IRA emerges through the claim that no *copy* (either faithful or, let us also suppose, less than fully faithful) can possibly be made. The impossibility does not arise because of matters to do with the degree of faithfulness of the copy. Rather it is alleged to arise for the following two reasons. The first is this. We need to be able to check the copy (call this 'C') against 'the world of reality' or as Piaget also puts it 'the model being copied' (call the bit of the world, or model, being copied 'M'). That is, we need to check C against M. But our only access to M is via C; there is no independent access to M. So the attempt to check collapses into a comparison of C with itself. Does this show the impossibility of a copy, as Piaget claims? No. It might be the case that we are lucky and C is in fact a faithful copy of M. What follows from this is not the impossibility of a copy being made but the impossibility of us ever *knowing* that the copy was a copy (of any degree of faithfulness). And this is as much as IRA requires. The second point concerns the remark 'moreover, such knowledge is necessary for the copy to be made'. The point here seems to be that to make a copy C of M we need knowledge of M, that is, knowledge *that* M is such-and so. But we can never get such knowledge *that* …, alleges Piaget. And if we cannot get knowledge of M, we can get no knowledge of the fact that C is a copy of M. Nevertheless, C might, by sheer luck, actually be a copy of M; but again this is something that we cannot know. What is dubious here is the claim that we can have no knowledge *that* … of M; but this is an issue we will have to leave until later sections.

Piaget does not use IRA to establish that there is no external reality, as would an idealist. He remains a scientific naturalist or realist. But in rejecting a copy view of truth for the "lack of comparison" reasons given above, Piaget clearly adopts a sceptical view of our every having any knowledge of M; all we can know is C. Piaget's constructivist view of truth and knowledge is not ontological but epistemic viz., a sceptical version of constructivism. This raises a problem for him when he goes on to say that 'truth … must be an organisation of the real world'. Three matters remain unexplained. First, Piaget helps himself to illegitimate talk of 'the real world' which truth is alleged to organise. Second, there is no account of what connection truth has to the real world. Third, truth does not leave the real world as it is, but is said to organise it in some way. Let us assume that the organisation is a conceptual organisation with truth, somehow, involving the application of concepts to reality. Whether this "organisation of the 'real world'" is a benign organisation that leaves the world

still accessible to us, is a matter to which we will return later. Putting these points together Piaget's version of IRA allegedly shows that no copy C is possible of any bit of the real world M. So all talk of M and the external world should be dropped; all we have are the copies like C with which we are to deal. But it is then illegitimate for Piaget to go on to talk of *the real world* at all. This is the thrust of the various version of IRA, including Piaget's, in which the real world is well lost. This position sits unhappily with Piaget's obvious commitment to a scientific naturalism; we have no access at all to the independent naturalistic world.

On a moderate interpretation, Piaget could be understood to be in the company of many modern-day correspondence theorists of truth who are realists (as is Piaget) but who do not hold to a crude copy, or "correspondence", view of the theory of truth. Instead they invoke other semantic relations between names and sentences and the world, none of which are like picturing or copying or modelling. Such theorists do have a view about the, largely semantic, connections that hold between beliefs or propositions and bits of the world (see Alston 1996, or Devitt 1997). In rejecting crude copying, Piaget owes us an account of how it is that the real world plays a role when it is the very thing that truth is said to organise. Moreover he needs to tell us how our experience, which is of the real world and a response to it, enters into our assessment of our knowledge claims.

A classic expression of IRA comes from Von Glasersfeld when he says approvingly of the Pre-Socratic philosophers that they

> … also noticed that there was a serious problem regarding the claim that knowledge could and should faithfully reflect a world held to be independent of the knower. They realised that there was no way of checking knowledge against what it was supposed to represent. One can compare knowledge only with other knowledge. The later skeptics have never tired of reiterating this irrefutable argument. (Von Glasersfeld 1993, p. 24).

Elsewhere he attributes this view to Xenophanes and says of it:

> To claim true knowledge of the world, you would have to be certain that the picture you compose on the basis of your perceptions and conceptions is in every respect a true representation of the world as it *really* is. But in order to be certain that it is a good match, you should be able to compare the representation to what it is supposed to represent. This, however you cannot do, because you cannot step out of your human ways of perceiving and conceiving. (Von Glasersfeld 1995, p. 26).

Von Glasersfeld says of this strongly IRA view of knowledge which is ineluctably bound up with our 'human ways', that 'All great philosophers of the western world have admitted the logical irrefutability of this argument'. (*ibid*., p. 27).

Elsewhere Von Glasersfeld endorses the constructivism he finds in Vico when he says: 'One of Vico's basic ideas was that epistemic agents can know nothing

but the cognitive structures they themselves have put together. …Consequently, God alone can know the real world, because he knows how and of what He has created it. In contrast, the human knower can only know what he has constructed' (Von Glasersfeld 1989, p. 123). Here it is not claimed that the real world does not exist. Rather it is inexplicably inaccessible to us humans (as has been amply indicated in Section 5.2). All we have is what we can "construct". This is a deep scepticism about our ever knowing what the external world is like rather than ontological idealism which says that there is no external (material) world.

We commonly claim, as all constructivists do, that our experiences do have some controlling effect on our beliefs. What that effect is will be cashed out, in the case of our common-sense belief, in terms of the reality that our experience is alleged to be about. In the case of knowledge more generally, and science with its appeal to methodology, again reality and the experience we have of it will have a controlling effect (it can confirm or refute our beliefs). But for constructivists none of these can, or should, have a controlling effect; rather they prefer to talk about how our beliefs are made *viable*. We have seen in Section 5.1.3 what is wrong with this talk of viability. What is important in this context is the character of the experience that is alleged to do some controlling. It cannot be raw, uncooked, unfiltered, unconceptualised experience; this is ruled out by IRA. So it must be cooked, filtered, conceptualised experience. But if so, then by IRA we have no reason to think that it is a copy, or a model, or even a reflection of any external reality. So all talk of any of our theorising being about the world must go by the board; it is simply construction all the way down, even to our very experience – of which we might like to go on to say that it is experience *of the world*, but cannot. It is just experience. If asked 'experience of what?' no answer is possible. Saying that it is *of the world* is ruled out by IRA. Constructivists who follow Von Glasersfeld cannot uncritically help themselves to the idea of the external world. The external world is well lost, hidden from view by all our constructions – and moreover not able to be compared to any of our constructions if IRA is to be taken in its full force.

5.4 PHILOSOPHERS ON IRA

The position under consideration gets expressed in many different ways, of which the following is a good example: '… we are bedded down in particular cultures and shaped by a particular language. We cannot, as it were, throw off the clothes of our language and stand naked before experience. For experience is always suffused and shaped by the presuppositions and values of a particular vocabulary.'[5] We cannot stand naked before experience, or is it reality? Here there is a deep ambiguity about what the word 'experience' that only contributes

to the confusing character of the remark. But this does express one of the characteristic themes of 20th century philosophy in which matters to do with our social context and linguistic inheritance have become prominent, especially in the way in which they are alleged to shape experience and even our thought (including our science). The shaping is allegedly so strong that it often leads to a deep form of relativism in epistemology (see Section 4.3). Or else the shaping somehow "blocks off" any direct access to how the world is. How this is so needs analysing since the consequence is so counter-intuitive to many.

IRA is not only a theme of contemporary philosophy; it is a pervasive feature of earlier philosophy as well. Philosophers, present and past alike, have argued that the human mind is such that we are bedded down in a particular mental or cognitive structure and that this also suffuses all we know, think and perceive. So, similarly, we can never stand naked before experience, or reality. Whenever we come to know or experience anything, we must of necessity provide for ourselves a garb, taken from the wardrobe of our cognitive make-up, with which we then confront the items of experience or reality. Continuing the metaphor, our garb is so extensive that it too clothes those very items of experience and reality. From this it is concluded that there can be no naked confrontation of ourselves with bare experience or reality. Or so the story goes, in some such metaphorical fashion. It is a story that, as Von Glasersfeld claims, had its origins in ancient philosophy. But it is a story that McDermid (1998) tells us[6] is to be found in Spinoza, Locke, Berkeley, Hume, Kant, Hegel, Schopenhauer, latter-day idealists such as Bradley, Royce, Bosanquet and Blanshard, pragmatists such as James and Dewey, logical positivists such as Neurath, Carnap and the early Hempel (but not Schlick with whom they disagreed), and recent USA philosophers such as Rescher and Quine (in some moods), Putnam, Goodman, Rorty and Davidson – to mention just a few who at some time have either conjured with, or advocated, some version of the above position. It would be a bad argument from authority to accept IRA because of the philosophers who have accepted it. Rather than discuss the role of IRA in philosophy we refer the reader to McDermid (1998).

IRA also appears to have been resurrected in recent cognitive psychology as Bickhard indicates: 'If I wish to check my representation that there is a desk in front of me, my only recourse would seem to be to again invoke my representation that there is a desk in front of me. Any such check seems to be unavoidably circular and, therefore, no check at all' (Bickhard 1998, p. 100). Here the talk is of representations, but the point is much the same, viz., no comparison is possible with our beliefs, even when it comes to beliefs about items in the external world like desks. Bickhard goes on to show that not only is IRA alleged to infect our theories of perception, but the same inaccessibility

emerges in recent theories of AI and cognitive psychology, a matter explored more fully in Slezak (see Slezak 2002).

Put this way, one might suspect that postmodernists would take to this argument like ducks to water. A linguistic version of IRA might well be the argument which gives life to Derrida's dictum that 'there is nothing outside of the text' (Derrida 1997, p. 158). All we have access to is some item A; and A is alleged to correspond in some way to a B. But no access to B is available (from which some infer illegitimately that B does not exist). In the case of texts, all we are left with is not any referent for the text but just the text itself. IRA also looms behind remarks of Lyotard who asks how we could ever "prove" that there is a correspondence between a sentence and reality. His cryptic rejection of the correspondence theory, using the medieval philosophical name of *adequation* for it, is: 'the rule of adequation becomes problematical. What I say is true because I prove that it is – but what proof is there that my proof is true?' And he goes on to say about how evidence might arise in such cases: 'Not: I can prove something because reality is the way I say it is. But: as long as I can produce a proof, it is permissible to think that reality is the way it is' (Lyotard 1984, p. 24). In Section 12.2 we comment further on Lyotard's use of a version of IRA.

Has any philosopher opposed IRA? Many. But one cheerful corrective is marvellously expressed by Moritz Schlick, the logical positivist, who responded to other positivist critics of his day, such as Hempel and Neurath who adopted some version of IRA.

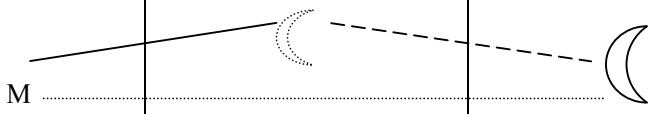
> I have been accused of maintaining that statements can be compared with facts. I plead guilty. I have maintained this. But I protest against my punishment: I refuse to sit in the seat of the metaphysicians. I have often compared propositions to facts; so I had no reason o suppose that it couldn't be done. I found, for instance, in my Baedeker the statement: "this cathedral has two spires". I was able to compare it with "reality" by looking at the cathedral, and this comparison convinced me that Baedeker's assertion was true. (Schlick 1935, p. 65-6)

Schlick's cathedral was the one in Cologne; and it still has two spires, as one can readily check. We can pass over his reply to his fellow positivists who wish to tar his position black by associating it with unsavoury metaphysics. What Schlick offers in response to IRA is the common person's protest that we do compare experience, statements, beliefs, or whatever, with bits of reality such as the Cologne Cathedral. For Schlick this is a brute fact not to be overturned by any fancy philosophical argument. But IRA purports to be such an argument. Does one accept IRA and spurn the common point of view? Or does one accept the common point of view and reject IRA by showing where it has gone wrong? Our view is that the latter approach is the correct one.

5.5 TWO VERSIONS OF IRA AND SOME CONSEQUENCES

IRA is advocated on two grounds. One has to do with the intervention of some kind of "go-between" which hovers between ourselves as perceivers and the worldly objects we perceive. Consider the diagram in this section.[7] Let us suppose that the box on the left stands for the mind M of some perceiver and cogniser (thinker). Suppose also that the box on the right stands for some object in the world (external to the mind in the left box), for example the Moon in its half phase. The box in the middle stands for some intermediary, or "go-between" for the object, the Moon, and the mind, M. Sometimes the "go-between" *represents,* or is a *reflection*, *model* or *copy* of the something represented, reflected, modelled or copied, viz., the Moon. In the history of philosophy the "go-between" has gone under various names; it could be an idea, or a sensory impression, or an experience, or a thought or belief. Here we will consider the first three of these (which we can assume to be the same). They give us the first version of IRA.

On the second version the "go-between" could be understood as the-Moon-as-it-appears-to-us-users-of-concepts; as such it is usually contrasted with the-Moon-as-it-is-in-itself. These two entities (if that is what they be) might be called (adapting J. L. Austin 1962, p. 70) "trouser entities" in that they always seem to accompany one another like the legs of trousers.

Individual Mind	'Go-Between' Moon idea/impression/sense datum/representation/ The-Moon-as-it-appears-to-us-concept-users	Item in External World Moon The-Moon-as-it-is-in-itself
		

Now consider some of the possible relations between these three items. What is denied in the view under consideration is that the long dotted horizontal line at the bottom linking the individual mind directly to the object, the Moon, stands for anything. That is, there is no connection of any sort that goes directly between a mind and an external object. What is denied here is the traditional theory of direct perception (see Section 4.4.2) which says that the items we perceive are generally objects, like the Moon; instead the item of "direct" perception is said to be a "go-between" such as an idea, impression or what have you. What connection there is between M and the Moon must go via the "go-between".

Now consider the right-hand side downward-sloping line from the "go-between" to the object, the Moon. This may be a representing, or semantic or causal relation which could go in different directions up or down the sloping line. Going "down", the line could be the relation of representing or of standing for, or of modelling, or of copying. Going "up" it might also stand for a causal relation, as when we say that the Moon is in some sense causally responsible for the Moon-"go-between" (of course in the presence of our mind). Note that not all philosophers want to say that a causal relation is possible here, e.g., Kant on some understandings of the diagram where the Moon is not an empirically given object but the Moon-in-itself.

Finally consider the upward-sloping line from the mind to the 'go-between'. There is presumably some relation that the contents of our experience or impressions bear to our mind, or that ideas have to the mind. The connection is quite intimate if they are thought of as states of the mind or mental qualities. If the connection were not intimate in some way, then one might have to postulate a further intermediary between the mind and the middle "go-betweens" by putting in a further column between the mind and the "go-betweens" thus postulating a realm of higher level "go-betweens" that somehow mediate between our minds and the lower-level "go-betweens". This raises the prospect of a damaging non-stopping series of levels of higher and higher "go-betweens". Such a hierarchy is not as fanciful as it seems because it does raise a question about our knowledge, not of external objects, but of the furniture with which this view populates our minds, for example our knowledge of our experience, or sense-data, or thoughts. Luckily this is a matter which can be set aside for our purposes.

Now we can see what the above-mentioned philosophers and science educationalists are getting at when they advocate IRA. There is a denial that the bottom line represents any viable relation between the mind and an external object; so strictly the bottom line is to be dropped out of the diagram all together. But the other sloping lines do have a role to play. But now an epistemological

question quickly emerges once one considers this picture. How do we *know* that "go-between" is anything like the object it purports to represent, copy, model or reflect? We do not, is the answer. We do not know that the "go-between" faithfully copies, reflects, represents or models its purported external object. Why?

Suppose we attempt to compare the "go-between" with its purported object. There are two cases to consider. First, we attempt to compare our moon impression or experience (say our experience of its curvature, or of its white-dark, mottled look), with the features of the Moon, or with the Moon itself. Then this presupposes that we have got the bottom line relationship back in place. We need to compare the middle item to which we have mental access with the right-hand-side item; but this comparison is not possible since there is nothing linking the mind to the Moon. All such relations must go *via* some "go-between". That is, we cannot compare, say, "go-between" #1 with any object. Suppose we wriggle a bit and try to get the object in our ken while still holding "go-between #1" in mind. However, given the very structure of things presupposed in the diagram, then another "go-between", "go-between #2", must pop up. But then we have our original problem all over again of comparing the newly popped-up "go-between #2" with the Moon. Second, suppose the "go-between" is a thought, viz., the thought *that it is the Moon*. Then we need to be able to compare this thought with a bit of reality, viz., the Moon or the existence of the Moon. But again there is no direct mind-object connection available for the comparison. And if we think such a comparison is at all possible then another thought "go-between" must pop up as an intermediary – and so we have our problem all over again. Conclusion: we can never compare our experience of thought with bare reality; at best we can only compare our experiences, or our thoughts, i.e., our "go-betweens", with one another.

Such is the position of one of the founding fathers of IRA, Berkeley; he denies that ideas 'are resemblances of any archetypes existing without the mind' and adds 'an idea can be like nothing but an idea' (*Principles of Human Knowledge*, Section 90). It is now easy to see that it is just a short step either to total sceptical idealism about the existence of the Moon, or to an ontological idealism which does away with any item in the right-hand-side box at all, viz., the Moon.This version of IRA is just like the veil of perception doctrine in which there is a heavy cloak over objects in the external world making the in accessible to us. But the veil of perception doctrine really leads us to the vale of tears, as much modern philosophy of perception shows, if it assumes that what we directly perceive are not objects but sense-data and the like. The arguments against this view are many and various and cannot be entered into here; but see, for example,

the classic Austin (1962), Dretske (1969) and Putnam (1994). But there is also a quite different strand of considerations leading to IRA. This is a Kantian way of looking at the distinction (whether it is Kant's is a matter to be left to scholars). Somehow, in the interaction between ourselves as active cognisers and our experience, any independent object, such as a table, either disappears or becomes inaccessible, or at least seems to go AWOL. Consider the-Moon-as-it-appears-to-us-concept-users and the-Moon-in-itself. Then there is an argument, using Leibniz' principle of the indiscernibility of identicals, which shows that they must be distinct items. The first item is perceivable by us but the second is not; according to Kant at least, the first item stands in causal relations while the second does not; and so on for other differences. Granted these differences, then they must be distinct items.[8]

However we can ask: do both of these Kantian items exist? Our view is that neither exists. They are the artefacts of a deeply misleading theory. Only the Moon exists. That this is so will emerge in the remainder of this chapter. In what follows we will mainly consider this "Kantian" strand of considerations on behalf of IRA.

A central aspect of epistemic constructivism is its antirealism as reflected in its adoption of IRA as one of its central tenets. As can readily been seen, IRA leads to a scepticism about the external world or to a denial of its existence. If the last ontological version is to strong for some, then the deep scepticism that IRA engenders renders any appeal to the external world quite unnecessary. The external world is a useless appendage doing nothing, except its postulation reduces disquiet about their being no world with which we are engaging – except of that engagement we can know nothing. Also it can be readily seen how epistemic constructivism takes a particular stance to the foundations theory of knowledge. It simply gives up on the problem of how experiences or sense impressions relate to the external world (it is either not there or we can have no access to it). At least the foundations theory makes a valiant attempt to deal with this problem, even though it is a failure.

Given that the external world plays a non-existent role in adjudicating our beliefs about it, epistemic constructivists are then free to adopt a versionof the coherence theory of knowledge. In particular they, perforce, take on board the view that we can only compare beliefs with one another and not the external world.It is not that the comparison with what we observe about the external world is hard to make; rather it is otiose and drops out. This position is supported by the rejection of the correspondence theory of truth. But we still have our beliefs to consider. Since there is no way to have an external check upon them, perhaps there can be an internal check, or they make a check on one another. Thus advocates of IRA often gravitate to some form of coherence theory

of knowledge since it is not possible to check of our beliefs against reality. All we can do is check our beliefs against one another, that is, make them internally cohere in some way but not externally fit within anything independent of our beliefs. Or as constructivists say, we make our beliefs "viable".

This is of a piece with the rejection of the correspondence theory of truth and often leads to a relativistic version of constructivism. Relativism (see Section 4.3) can sit quite happily with the alleged problems that IRA raises for the correspondence theory. Truth, and knowledge, cannot be anything 'absolute' but must be relative. Thus truth might be relativised to cognitive structures which, if they are common to all of mankind, do not raise a problem of conflict between various social groups, but would for ourselves in relation to non-human rational beings. Other relativisations do lead to genuine alternative systems of belief, as when the relativisation is made with respect to languages, cultures, social contexts, epochs, systems of frameworks of thought or paradigms, and so on. It should be no surprise that some epistemic constructivists do find relativism a congenial position given the problems that their commitment to IRA raises for them. This might also lead to a version for the coherence theory. But it is one in which it is difficult to say that our beliefs do not cohere because they are inconsistent. To define the very notion of inconsistency we need a notion of truth; but it is hard to see what this might be, and viability is too weak to do the job.

5.6 SETTING OUT THE IRA ARGUMENT AND ITS IMPOSSIBILITY CLAIM

So far we lack a fully expressed version of IRA. Here is one construction which captures most of its features. IRA makes a background assumption of some version of the correspondence theory of truth, and then attempts to show that it is an untenable assumption. To this end we can adopt the version of the theory of truth set out in Section 2.3:

> The belief/proposition/statement that p is true if and only if there is something in the world in virtue of which p is true.

The 'in virtue of which' talk is deliberately neutral in that it leaves open whether it is a fact, or a state of affairs, or more generally a *truthmaker*, that makes that p true; it also leaves open whether the truthmaker is part of the natural, biological or social world, or part of the inner mental world that each of us has.

Now we can set out IRA making clear its epistemic character:

(1) Epistemic requirement: A necessary condition for determining whether that p is true is that we need to ascertain what part of the world obtains in virtue of

which it is the case that p is true. That is, we need epistemic access to the worldly side of the truth relation, viz., some truthmaker, in order to tell whether it is the case that p is true.

(2) But (1) is impossible because the necessary condition cannot be satisfied.

(3) So, we can never determine that p is true.

Clearly the crucial premise is (2); and it is this that captures the point behind IRA. IRA turns on an impossibility claim of the form; it is impossible to have epistemic access to worldly truthmakers in virtue of which it is the case that p is true. Often this impossibility is expressed in metaphorical terms of our never being able to *nakedly*, or *skinlessly*, confront reality without some garb; or it is impossible to have *raw* experience but only experience *cooked* by conceptualisation; and so on.

Consider the two versions of IRA mentioned in the previous section. The first supported the impossibility claim by appealing to the inevitable interposition of a "go-between' such as an experience, or sense impression, or sense datum or whatever. It is now easy to reject this alleged impossibility, as has been indicated. It is a generally agreed verdict of current philosophy that there are no such "go-betweens" and their introduction into philosophy has been something of a disaster, one such disaster being this version of IRA. The second version is different. It appeals to the fact that the very act of conceptualisation itself makes the world inaccessible. We are always left, not with the Moon, but another entity, the-Moon-as-it-is-conceived-by-us. As to how the Moon is independently of us, that is a matter we cannot consider; all we ever have access to is the-Moon-as-it-is-conceived-by-us. As a result advocates of IRA speak of the Moon-as-it-is-in-itself. But critics of IRA should reject both of these hyphenated expressions as they only make sense in contrast with one another; give up one, then give up the other as well.

In discussing this version of the impossibility alleged in (2), Alston (1996, chapter 3, especially p. 90) draws an important and useful distinction between different impossibility claims contained within IRA. Here we will follow Alston in setting out the various kinds of impossibility he distinguishes as premises leading to the conclusion of an argument. If IRA can be established in the very central case of our experientially based beliefs then it is well on the way for making its case for all our beliefs.

(1) It is impossible for there to be perception which does not involve the active application of concepts. Or in other words, all perception necessarily contains a judgement, or a belief (i.e., there can be no experience without judgement or belief). That is, there is no pre- or sub- or primitive or primary experience that is independent of any judgement or belief.

From this it is concluded:

(2) So, it is not possible for us to be (directly) presented with external facts in the world (i.e., external to our thought) when we have perception; rather we are only directly presented with judgements or beliefs.
(3) So, it is impossible to get a bit of non-judgemental reality (fact, state of affairs) before our minds so that we can compare it with our judgement; all we get is another judgement.

Here there are three impossibility claims linked in an argument of which the conclusion expresses IRA. What we want to show is the negation of (3):

Not-(3): It is possible to get a bit of non-judgemental reality, e.g., facts, before our minds so that we can compare it with some judgement; we do not necessarily get another judgement.

This argument goes wrong right at the beginning with premise (1). If we reject (1), then (2) falls. We can, as is all too obvious (as Schlick pointed out), compare external reality (e.g., the Cologne Cathedral with its two spires) with a conceptually laden report of it, viz., the observational belief *that the Cologne Cathedral has two spires*). And if (2) falls, then so does (3). So what of (1)? It turns on the claim that we cannot have any primitive experience, that is, an experience in which no concepts are applied at all. But is this true?

Once we have arrived at this point it can be seen that IRA fails. We have nothing to add to the many considerations against IRA that have been introduced at this point. For example, we refer the reader to Alston (1996, chapter 3) who argues that we can have *visual presentations* that are primitive in the sense that no concepts are applied. The newly born have them all the time. And we have them too, even though we do apply concepts to them most of the time. Alston's crucial expression is 'visual presentation'. He uses this rather than use the philosophically ruined expressions 'sense data', or 'sense impression', or the highly ambiguous 'experience'. A visual presentation is what we have when our visual systems are in good working order as we have our eyes open, etc. They are to be contrasted with the kind of presentations we have when use our ears, or our hands (for feeling), or our inner mouths (for tasting), etc. It is such a visual, auditory, tactile, etc, presentations that we can have in the absence of any conceptualisation.

Dretske (1969, 1981 and 2000 Part II) is a sustained attack on the idea that there can be no primitive seeing. Rather he argues for the possibility of both of what he calls non-epistemic seeing, and epistemic seeing. Epistemic seeing is just seeing *that p* (note that the clause 'that p' is the object taken by the verb 'see'). This entails that if person A sees that p then it follows that A believes that p. It is the believing that p that threads concepts into the seeing. Thus, on a bush walk some of us may *see that it is a tree*, *see that is a kauri tree*, and the like. But what of the person that cannot make such identifications? Do they see nothing? Not so.

They are at least having what we called above a visual presentation. It is not as if a gap arises in their visual field because they cannot classify something they see.

The idea of non-epistemic seeing is a version of seeing which does not entail anything about believing. Thus the following necessary condition holds for non-epistemic seeing:

> If A sees (non-epistemically) an item X, then this does not entail that there is some belief that p, (either involving X or not) such that A believes that p.

That is, seeing can take place in the absence of believing (or applying concepts). Such ignorance does not impair vision; if it did our visual presentations might appear gappy. The difference, in part, lies in the important different in grammar: the world 'see' can takes a direct object or noun phrase ('the Kauri') and not a that-clause 'that it is a Kauri'). This idea of non-epistemic seeing is the same as primitive or primary seeing, and it takes place in the absence of the application of any concepts, or without and necessarily accompanying belief or judgement. Some examples will make the distinction clear.

There is a big difference not just in grammar between seeing X and seeing *that* it is an X. The former can take place with out the latter, but not the converse. Thus a person can see the child abduction, but not see *that* it is a child abduction; they can see a fossilized rock, but not see *that* it is a fossilized rock. This is also very clear in the case of hearing. One can hear the oboe (because one is near enough and one's ears are in good working order). But few of us would be able to claim that we hear *that* it is an oboe (as opposed to say, a clarinet). Yet skilled musicians and orchestra conductors do hear not only the difference but hear *that* the difference is such-and-such.

In all these cases A sees, or hears, X expresses the idea of primitive seeing or hearing in the absence of the application of concepts. It is only in the case of seeing *that*, or hearing *that*, etc, that concepts come into play. And so there is a notion of perception that is independent of concepts and/or beliefs in which the world is directly presented to us by our perceptual systems. And because of this we can get a bit of non-conceptualised reality in front of us in order to compare it with our beliefs about it. Putting it another way, in non-epistemic seeing, the visual presentation we have does reveal to us features of the very objects that we see. Supporters of IRA would have to deny this, viz., our visual presentations do not reveal any features of the objects of which they purport to be visual presentations. When put this way, it is hard to see that anyone would accept IRA.

Schlick is right: we can get the Cologne Cathedral with its two spires in front of us and check that our belief that the Cologne Cathedral has two spires is right (and our belief, if we had it, that the Cathedral has one spire is wrong). Much more needs to be said of this matter than can be presented here. The story we have told is endorsed not only in Alston (*ibid*.) and Dretske (*ibid*.) but also in

Crane (1992), Peacocke (2001) and several of the essays collected in Gunther (2003). These philosophers, along with many others, have done much to undo the bad influence of IRA in philosophy.

The above concerns constructivism as a doctrine in epistemology and its accompanying anti-realism that leads us to scepticism or to idealism. But none of these are satisfactory theories of either ordinary knowledge or scientific knowledge. What has all of this to do with constructivism is pedagogy? In our view constructivist pedagogy is independent of epistemological constructivism; as in the case of Socrates, versions of the first can be held with out the second. But often epistemological constructivism is wheel in to support pedagogical constructivism – to its detriment. It needs to be freed of it, as we will now argue.

5.7 CONSTRUCTIVIST PEDAGOGY

So far we have looked at varieties of constructivism, rejecting them all except one (namely, cognitive constructivism). Importantly we have argued against the IRA that is at the heart of epistemic constructivism. But constructivism is not just a theory of knowledge; it is also a theory of learning and teaching. Indeed, it is a pedagogical theory that has been considered to be highly original and enthusiastically embraced by a vast number of science educators. But is there anything valuable in constructivist theory of learning and teaching? How original is it? Is it inseparable from constructivist epistemology as its proponents claim? In this section we will try to answer these questions.

Constructivism in the field of education seems to be a reaction against what is known as "the transmission model of education" and the "realist" philosophy allegedly underlying it.[9] According to the transmission model, there is a fixed body of already existing knowledge that needs to be taught and learned. Teaching essentially consists of the transmission of this body from the teacher to the pupil. This model is criticized by the constructivists because it is didacticist and therefore teacher oriented, emphasizes content too much, encourages passive rote learning and memorization, and provides the student with no genuine understanding. Such pedagogy is blamed on "realism", by which it is meant an amalgamation of foundationalism, absolutistic objectivism, minimal and representational realisms. For this reason, "the transmission model" and the "realist" philosophy that supposedly nourishes it are replaced by a constructivist pedagogy and a constructivist philosophy underlying it.

The starting point of constructivist pedagogy, we are told, is the recognition that each student comes to class with his own understanding of concepts and "knowledge" (read: beliefs) constructed out of his experiences about how things work, and that any teaching strategy which does not take this into account is

bound to fail. This requires a will, an openness, on the part of the teacher to listen to students and find out about the differences in their learning needs. Thus, the teacher's first task is to help students articulate as clearly as possible their own ideas about a certain topic or problem, preferably in small groups, so that each becomes aware of what others think and "negotiates" his solution with others. In this way, the student becomes an active participant rather than a passive listener in the learning process, the result of which is genuine understanding and enlightenment rather than training. It is essential in this process that 'the teacher not tell students what concepts to construct or how to construct them', nor tell them that they are 'wrong', but simply prevent them from going in futile directions (Von Glasersfeld 1995, p.184). After all, there is no fixed body of truths to be transmitted, but only 'conceptual constructs' that are 'viable'. Indeed, the whole aim of science education, according to the constructivist picture, is not to help students learn or (re)construct the concepts and views that scientists have formulated and tested as a result of long and arduous cooperative work, but to 'enable [them] to function effectively in their world' (Gunstone 1988, p. 89). Accordingly, student assessment seems to consist of measuring the extent of active participation, conceptual novelty and creativity, concept mapping, and meaning compatibility between the teacher and the student on the one hand and among students themselves on the other, a task made especially difficult because of the indeterminacy of linguistic communication that results from a subjectivist theory of meaning (see the semantic version of constructivism in Section 5.1.)[10]

As Michael Matthews, one of the early critics of constructivism, pointed out, educational constructivism 'has done a service to science education by alerting teachers to the function of prior learning and extant concepts in the process of learning new material, by stressing the importance of understanding as a goal of science instruction, [and] by fostering pupil engagement in lessons' (Matthews (1998), p. 7). But these are pedagogical truisms, the recognition of which goes back to the critical inquiry developed by Socrates and Plato more than two millennia ago (see Section 3.3). It is also to be noted that the constructivist's linking of didacticism with realism is totally unfounded. The Socratic method of teaching and learning on the basis of the question-answer dialectic is an excellent non-didactic model for pedagogy for both constructivists and realists. The pedagogical truths stressed by constructivists are perfectly compatible with the direct realism that we have adopted above and can be decoupled from the constructivist theory of learning and teaching. They are not under the monopoly of constructivism.

We can agree with some of the constructivists' conclusions, but not their arguments. One area of agreement is that the transmission model is hopeless for acquiring knowledge, rather than belief (but see a caveat to this in Section 3.3

made by Bransford *et. al*.). As we emphasise in Section 2.3, this is a point well made by Socrates. If one learns *via* the processes of critical inquiry then one is on the one and only path to knowledge. But if one does not employ the processes of critical inquiry, then one can not end up with knowledge; at best what one learns is a (true) belief, or acquires and opinion, or picks up a bit of (true) information, or makes one's own "construction". This highlights an important difference between us and constructivists. The constructivists appeal at this point to the theory of knowledge embodied in epistemic constructivism to claim that the pupil has acquired knowledge. But alas their conception of knowledge is quite flawed. At best the pupils have a construct of theirs. But this construct has not been guaranteed to have been got by processes of critical inquiry. So the constructivists cannot claim that pupils have arrived at knowledge, as we and Plato and many others have defined it. Of course one can redefine the term 'knowledge' so that pupils do end up with a construct that is alleged to be "knowledge". But this is cheating by definition. It is also one reason why in Part I we have spent so much time on epistemological matters that are not well understood in science education, and have been muddied by constructivists.

Though some truths have been captured by constructivist pedagogy, constructivists have lost sight of other truths about teaching and learning and created unnecessary conceptual difficulties and confusions. To begin with, the conflation of "knowledge" with "belief" and the replacement of "truth" by "viability" resulted in the loss of the idea of a right and wrong answer in science and science education. Constructivist teachers are reluctant to tell their students that their representations may sometimes be misconceptions, and that their constructions can be misconstructions. Few constructivists confront head on cases where a student's understanding does not match the teacher's. What then? Do we explain to the student why she is wrong or not? As Grandy says, 'if we are unwilling to evaluate representations, unwilling to judge some representations and understandings as more accurate, more general, more consistent than others, then there is no need to teach science' (Grandy 1998, p. 116). It goes without saying that as teachers it is our responsibility to teach our students not what is false, but what is true and what the best existing theories say. We have shown in Section 3.3 how Socrates millennia ago showed us one way of overcoming these problems with his non-didactic approach to correcting pupils when they go wrong.

Many constructivist teachers naively expect their students to carry out "viable constructions", be they "constructions" of meanings of scientific terms or of explanatory scientific hypotheses concerning a given topic. But, scientific

concepts and truths do not come easily; they are the result of long and hard work. Moreover as we will show in Chapter 10, the whole idea of construction out of a pupil's experience is wrongheaded and will hardly help a pupil deal with matters such as model construction. That requires thought, and may go against experience, as Galileo argues. To some extent the non-natural character of many of the concepts and ideas in science needs to be recognised. As every teacher knows, even with much guidance some students will fail to discover them by themselves for various reasons. In such circumstances, teachers must guide their students toward them and make them see their mistakes if they go astray. Constructivist theory provides no well-defined mechanism by which students can be led to successful (re)construction of scientific concepts and hypotheses or their understanding (though there is some useful research in this direction). Not being able to guide the student toward what concepts or hypotheses to construct or how to construct them is not a virtue but a vice of the constructivist theory.

Finally, we would like to point out that constructivists often use a very misleading pedagogical discourse. We have seen an example of this in the talk of "viability". Another example is the talk of "negotiation". Constructivists are extremely fond of this piece of jargon, which they use to describe students' learning and discovery activity. Thus, students do not argue for their ideas nor do they try to convince others on the basis of available evidence, but "negotiate" them with their teachers and other students. But conclusions reached by "negotiation" need not be true. Moreover, such conclusions downplay or totally ignore the role of reality in providing an independent check against our beliefs – independent, that is, from the community of inquirers. It is here that constructivist epistemology does a disservice to constructivist pedagogy. As we argued in Section 5.1.3, evasive notions like viability or unviability cannot replace the time honoured talk of truth and falsity (or error). Constructivism thereby not only paints a distorting image of science, but also prioritises rhetoric and (uneven) power relations in scientific debates both in the classroom and outside. (The negotiating power of the teacher is certainly greater than that of the student.) Indeed, it would not be an exaggeration to say that describing rational discussion and argumentation as "negotiation" masks the essence of education, namely critical inquiry, as we have arguing through the course of this book. The main purpose of constructivist pedagogy seems to make students feel good rather than to turn them into critical inquirers.

In sum, our overall conclusion is that there are many pitfalls to constructivism in all its varieties. That is why in this book we have attempted to set out a conception of science and education that takes seriously both the realism of much epistemology and science, and its rationality.

NOTES

[1] Constructivists oppose didactic methods of teaching. But so do many non-constructivists. One does not have to be a constructivist to be anti-didactic. For example see the wise anti-didactic remarks made by Montaigne in 1850 and cited in Matthews 1994 p. 144. Only a small sample is cited: 'Most tutors never stop bawling into our ears, a though they were pouring water into a funnel; and our task is only to repeat what has been told to us. … *The authority of those who teach is often an obstacle to those who want to learn* [Cicero]'.

[2] See Fodor 1998 for a criticism of our current theories of concepts from the point of view of both philosophy and cognitive science. Fodor finds many of our current theories wanting; not only is abstraction criticised but also inferential role accounts, and accounts that they are definitions, stereotypes, prototypes, and the like. Even if one does not accept Fodor's positive views he does show that we do not really possess any problem-free theory of concepts. And along with this must also be included any unproblematic theory of concept change.

[3] Direct perceptual realism is the view that what we see are objects (and events of processes). It is introduced in the opening sub-sections of Section 4.4. It is the traditional theory of perception endorsed here; it has its defenders from Aristotle to the eighteenth century Thomas Reid, and to J. L. Austin 1962, and is now the most widely adopted view. Two of its rivals are representationalism and phenomenalism in which what we (directly) see is alleged not to be objects but sense-data, or impressions or experiential items, and the like. Philosophers who have occupied some of the various rival positions include Locke, Berkeley, Hume and Kant.

[4] We will not pursue here arguments for scientific realism in which we claim that unobservables such as electrons exist independently of us. For definitions and arguments see Devitt 1997, part I.

[5] This remark occurs in a review by Harries 2001, p. 25.

[6] For an even longer list of philosophers who have advocated the above view, and references to their work, see the paper by McDermid 1998, footnotes 4 to 35. McDermid, a supporter of IRA, provides an introduction to the history of the above view in philosophy over the past 200 years.

[7] Such a Diagram is suggested in Bergmann 1967, Part II Representationalism, Section Seven.

[8] I owe this argument to Alan Musgrave's unpublished paper 'Metaphysical Realism versus Word Magic'.

[9] While this linking of the "transmission model" with "realism" pervades much of the constructivist literature, its most explicit statement can be found in Pope and Gilbert(1983. See also Tobin *et. al*. 1990 and the literature cited therein.

[10] We have extracted this constructivist pedagogy from Driver 1988, Gunstone 1988, Tobin 1990, Tobin *et. al*. 1990 and Von Glasersfeld 1995, ch. 10.

CHAPTER 6

THE AIMS OF SCIENCE AND CRITICAL INQUIRY

Much has been made, so far, of the notion of critical inquiry and its links, or lack of them, to belief, learning and education. But what is critical inquiry? It is a many-splendoured thing. In Chapters 2 and 3 we considered the role of critical inquiry in relation to knowledge, and its connection to the Socratic model of inquiry. In Part II we will consider more specific forms of critical inquiry that give rise to the idea of scientific method and its norms. The very idea of scientific method is something that has come under heavy attack by postmodernists, sceptics and other anti-rationalists. And their views have come to infect much theory within science education, and in particular the idea of a "multicultural science" (see Part IV). We wish to challenge these prevailing views. We place considerable emphasis on the idea that even if there is no such thing as *the* scientific method, there are at least methods to be used in science in testing between rival hypotheses.[1] And we say much the same for the adjudication of rival hypotheses and theories in areas outside science (such as in law courts, everyday life, special commissions of inquiry, and the like). In our view there is a theory of rationality, and of critical inquiry, that should play a central role in any account of science and knowledge, and in any account of education, particularly science education.

As a preliminary to later chapters, in this chapter we will consider the connection between critical inquiry and the aims and methods of science. The first three sections deal with the distinction between extrinsic and intrinsic aims for science. Section 6.4 sets out our idea of what are the values, rules and methodological principles of science, the core of the idea of what constitutes critical inquiry. In Section 6.5 we say a little about what we mean by hypothesis, law, theory and model. In the final section we give our definition of what science is.

6.1 AIMS OF SCIENCE, OR SCIENTISTS?

The sub-heading is deliberately plural; that there is one unique aim for science is a strong claim in need of proof. But does it make sense to ask if science itself has aims? It might be more appropriate to ask what aim(s) (or goal(s)) particular scientists have rather than to ask whether an abstract thing such as science has an aim (or aims). A satisfactory survey of the aims of scientists has not, to our knowledge, been carried out. But most likely a wide range of

aims would be uncovered, including some that are not scientific aims, such as personal goals like getting a job with good pay, or having a secure position.

Here we need to distinguish between the aim a person can have in doing something, including some particular science, and the certain, or probable, consequences which follow from what they do even if they do not aim for this and it is an unintended consequence. This distinction is important when considering the activities of research scientists.

The example we will use as an illustration concerns those scientists involved in the invention, production and use of chlorinated fluorocarbons (CFCs) in refrigeration. These became widely used in all forms of refrigeration as the refrigerant, the cooling agent; they made possible the wide availability of refrigeration in the second half of the 20th century. However, a few scientists were concerned on theoretical grounds that a release of CFCs into the atmosphere might damage the ozone layer above the Earth. Such release would occur either accidentally or, more likely, when the refrigerator had passed its useful life, was dumped with the refrigerant still in it so that it slowly leaked into the environment. One of the first warnings came in a 1974 paper in *Nature* on the ability of CFCs to catalytically destroyed ozone. The authors, Rowland and Molina, received a Nobel Prize in 1995, the Nobel Committee citing them for saving the Earth from a potential disaster.

From the start, Molina and Rowland had taken seriously the socio-political implications of their work. Perhaps not unpredictably, companies such as DuPont which produced refrigerants and which would stand to lose from the banning of these products, mounted a campaign against attempts to limit or ban their production. Here we cannot go into the very interesting details of the reactions of vested interests at that time (for more details, see Christie 2001, pp. 35-6, and other literature on the politics of this episode cited there).

There was initially little direct evidence for the effects of CFCs, until ozone depletion was observed in the mid-1980s in Antarctica (an outline of the evidence is given in Section 7.6). Later it was definitely discovered that the depletion was due to the presence of CFCs that slowly rose from the Earth's surface into the upper atmosphere.[2] Scientists might have the aim of working for the production of CFCs in refrigeration; but none, unless they were excessively evil, would have as their aim the depletion of the ozone layer. However, they can come to realise that such depletion is an undesirable, and unintended consequence of their actions. What ought the scientists do once they realise the initially unintended consequences of their actions?

Each could give up their work on the refrigerant and investigate other alternative refrigerants that do not have this effect. Or they could take the strong view that no one ought to work on such refrigerants and advise all to abandon such research, or more extremely to leave their job altogether. But scientific activity rarely exists in isolation from the industries in which it is

produced and their commercial aims. Here the focus shifts from the aims of individual scientists to the aims of industries that use science. And the focus shifts from the aims of pure cognitive inquiry to a means-ends inquiry concerning the technological applications of science by industries and the risks involved, where their primary aim has little to do with science and much to do with running a successful economic enterprise. And this in turn leads to a focus on public interests and political and legislative interests in commercial enterprises using science. Given the problem that has emerged with the use of CFCs as a refrigerant, what are the alternatives that face the industries and their research scientists?

One response might be to require the collection of all used refrigerators and remove the refrigerant. This is a policy that could be backed by legislation (but sometimes resisted in part because of who might bear the burden of the cost of collection if not governments). The second would be to use even more science to find an alterative, less damaging refrigerant. To a large extent this has been successfully done. Once the companies that initially criticised the work of Molina and Rowland, and others, accepted the evidence of ozone depletion, then the search was on for less destructive refrigerants. But then there is the matter of getting other manufacturers to use new refrigerants, especially if they are more costly.

The third would be to get international agreements about the use of CFCs in refrigeration around the world. There are now the Montreal Protocols in force from 1987, with four amendments in the early 1990s to phase out even more quickly the production of CFCs. Despite these, international agreement has not been easy to achieve given the assistance many third-world countries feel they need to take the quickest route to industrialisation and economic growth. They tend to resist having restrictions placed on their growth through requirements such as the kind of refrigerant that is to be used. Once the problem became known, many western commercial enterprises were placed under legislative restrictions on the use of CFCs in their own countries. At one point this led to large stockpiles of CFC that could not be used or sold internally. But there were other countries (usually third-world where there were no such restrictions on use or sale) to whom they could, and did, sell their stockpile.

The rate at which CFCs eat up the ozone has not declined as fast as it might have. Some decline in CFCs has been noted in the Antarctic stratosphere accompanied by less severe ozone depletion; however in 2003 the ozone hole was larger than it has ever been. But now there are few ozone-depleting substances in use and it is hoped that by about 2050 the ozone "hole" phenomenon will have disappeared.

In cases such as these, the aims of scientists must also be considered in the social context in which their science is practiced. One needs to take into

consideration not just the aims of scientists, but also the aims of the technological use of science in industry by manufacturers, businessmen, shareholders, and other stakeholders. The unintended consequences of the actions of scientists might not be entirely due to their actions alone and may be in large part due to the commercial situation in which they also act. Whatever the case, these are all effects that pervade and surround science, whether aimed for or not. As can be seen from the case of ozone depletion, science can be a force that produces substances which can have lethal effects on our environment; but science can also be used to uncover such lethal effects and eliminate or replace these substances. But all of this takes place in a socio-political and commercial context which can promote or hinder the action of science and scientists.

6.2 EXTRINSIC AIMS OF SCIENCE

All of the above aims are *extrinsic* to science in the sense that science is not done for its own sake but as a means of realising some other aim. Extrinsic aims, which are often motivational, are of several sorts. On the part of non-scientists (and many scientists) their aims are to use science as a means to increased wealth. On the part of scientists, some aims are personal in that they use science as a means for, say, securing a well-paying job. Other aims might be professional, e.g., to form a successful research team, to edit the leading journal in the field, to obtain all the funding a research project requires, to be an influential adviser to the government on scientific and technological developments, to be the first to make an important research breakthrough, to patent technological applications of science, and so on. Scientists might also have specific political, religious, financial, humanitarian or social aims in doing their particular science that go beyond the personal and the professional. Thus there are strong moves by governments to develop the "knowledge economy" in which science would play a large role in enhancing economic performance and profit while offering at the same time the hope of a "trickle down" to greater job opportunities and employment in society as a whole. Yet other aims of scientists, and those who employ them, might be quite general, e.g., to enhance human power through prediction and control. All of these extrinsic aims are an important part of the social context of science.

What of the science that realises such extrinsic aims? Apart from a few lucky frauds, the science had better be good or satisfactory if the extrinsic aim is to be achieved. This suggests that, whatever extrinsic aims scientists might have, if they are to be realised the scientists also ought to have the main aim of doing *good* or *satisfactory* science. To see that this is so we need only compare the value of the following two relative probabilities:

(a) what is the probability that a person will realise their extrinsic aims given that they are employing good or satisfactory science?
(b) what is the probability that a person will realise their extrinsic aims given that they are employing bad or unsatisfactory science?

Most would agree that, the case of the lucky fraud aside, on the whole the probability of (a) is much greater than the probability of (b). The person who does not employ science that is warranted by the goals of pure inquiry is unlikely to realise his or her extrinsic ends, or the extrinsic ends that society might have in using science.

It remains to say what is *good* or *satisfactory* science. This brings us nearer to what can be called 'the aims of science'. We need to ask: are there aims that are not extrinsic to science but which are proper to, or *intrinsic* to, all science that any scientist ought to have regardless of their extrinsic aims? To produce good or satisfactory science is an answer, but hardly an informative one.

Should one include the following amongst the intrinsic, or the extrinsic, aims for doing science? For example: satisfaction of one's curiosity about something; the need to know why something happens in order to solve some problem; an interest in what will happen in the future (i.e., in developing a predictive science); the desire to understand how the world works. Scientists can often be caught up in enthralling intellectual passions of research and discovery in the attempt to realise aims such as these, thereby belying the common belief that scientists are cold and unemotional people. But are such aims directed upon the very content of science itself? Or do they rather concern the satisfaction of desires, wants, and curiosities of individual scientists? The latter seems to be the case and not the former. So while, say, shear curiosity might be a drive for many scientists, curiosity satisfaction is hardly a cognitive aim of science itself.

Consider, in contrast, how one's curiosity is to be satisfied. This is usually done by: getting to know the truth about what is going on; finding an adequate explanation; increasing one's understanding; being able to predict what will happen; finding hypotheses with strong evidential support; and so on. Fleshing out these answers by, for example, discovering an adequate explanation or developing a theory and testing it, takes us to the heart of scientific activity. One might then go on to ask even more general questions such as the following. How should one best go about achieving these answers? What are the proper methods of inquiry? What counts as an adequate explanation? What counts as increasing one's understanding? What is a theory? and so on. Answering these questions has become part of the province of philosophers; however scientists working either at the beginning of their science, or during a period of revolution in their science or digging down to the foundations of their science, are often moved by such questions

as well as questions specific to their particular science. Such has been the case with revolutionary scientists such as Galileo, Newton, Einstein and Bohr, but not most working scientists.

6.3 INTRINSIC AIMS OF SCIENCE

The extrinsic aims of science are to be contrasted with its *intrinsic* aims. These are aims which science realises for its own sake and not for some other end. A finer distinction can be drawn between intrinsic aims for science in this sense, and aims which are not only "for their own sake" but are constitutive of science or are essential to it, or which might define it. These will all be intrinsic aims; but not all intrinsic aims need be strongly constitutive (as some example will illustrate shortly). Sometimes intrinsic aims are also called 'cognitive aims', or 'epistemic aims' for science, or 'values' because they embody those features to which we may attach great value and wish our science to exemplify. Thus Thomas Kuhn[3] advocates the following values for our scientific theories: accuracy, consistency, simplicity, unity, breadth of scope (i.e., they explain a lot), and fruitfulness (they lead to new unknown results). Others suggest further aims such as: high evidential support, or higher evidential support than any other suggested rival; truth; overall coherence; elegance; absence of *ad hoc* claims; and so on. These are intrinsic aims applicable to most theories; but it is not obvious that they are all constitutive of science; we might still have science even though some of these aims are not realised (e.g., simplicity, or unity).

Other aims pertain to particular hypotheses, as when those who use mathematics in science might aim for hypotheses that are differentiable rather than those that are not, or aim for equations of a lower power in their variables as opposed to a higher power, or aim for those that are deterministic rather than stochastic, and so on. Such aims are hardly constitutive of science, but they might be aims that are pursued, not for their own sake as such, but because they realise some other desiderata that apply to science itself and not some extrinsic aim. Thus the aim of determinism, or of integratability of equations, might arise simply because of the deep problems that can arise in mathematics due its application to systems that are not deterministic, or the use of equations that are not easily integratable.

In what follows, we wish to highlight six intrinsic aims of science. The first of these, knowledge, was discussed more fully in Section 2.4; the second, explanation and understanding, in Section 2.5. He we will focus on testability, truth, prediction and fruitfulness. These are just some of the many general aims for science that most would not dispute, though they might give different accounts of them. Granted these aims for science, then we can say that the task of critical inquiry is to provide us, for the domain into which we are inquiring, theories and hypotheses which realise these aims, viz., they are

testable, are fruitful, yield predictions, provide explanation and understanding, are true (or approximately true) and advance our knowledge. All of these ends can be realized together; as will be seen there is nothing inconsistent about them and they do not pull in opposite directions. Nor are these the only values that critical inquiry involves; its methodological principles can also provide for simplicity, scope informativeness, accuracy, and so on.

6.3.1 Testability as an Intrinsic Aim

Testability is, for some, not just an intrinsic goal for science; it is a goal the realisation of which is constitutive or definitional of science. The young Popper noted that one of the characteristics of science was its ability to criticise and overthrow prevailing theories and replace them by new theories. Science is revolutionary and no respecter of prevailing systems of belief, no matter how well entrenched. In fact Popper was impressed by the way in which one of the best scientific theories that has ever been proposed, Newtonian mechanics, was overthrown by quantum theory and the theory of relativity. His view is that 'the distinguishing characteristic of empirical statements [is] in their susceptibility to revision – in the fact that they can be criticised, and superseded by better ones' (Popper 1959, p. 49). So, what analysis does Popper provide of the necessary conditions that must prevail for any criticism of our theories to take place, that is, to be open to at least provisional acceptance, or to revision or abandonment? This would appear to be an urgent issue for any account of critical inquiry.

According to Popper, for a theory to be testable it should be possible to bring it into relation with possible experiences we can have of the world; that is, it must be open to test by comparing it with either observations or experiments. If a theory is not capable of being compared with what we experience through observation or experiment, then it is not possible for us to critically evaluate it. This is not true of, say, mathematics; we do not (a few exceptions aside) test mathematical claims against experience. The role of critical inquiry in mathematics is, in large part, either to find a proof of mathematical claims, or show that they are inconsistent with some other mathematical claims that have been proved. Nor is this true of definitions; we do not evaluate them against experience. Nor is this a requirement of metaphysics, says Popper. But Popper does turn his requirement into a criterion for any empirical scientific theory worthy of the name that would distinguish it from mathematics, definitions, logical tautologies and metaphysics. Importantly it also provides one way of distinguishing science from theories that claimed to be scientific but were not, viz., the pseudo-scientific; that is, it demarcates good from bad science.

Putting the criterion a little more explicitly, for critical inquiry into any science to be possible Popper requires the following: for any hypothesis or

theory (under some conditions of application), it must divide all *possible* reports of experience into those with which it is consistent (some of which it would entail as predictions), and those with which it is inconsistent. If it does not do this, then it is not possible to test it by experience; and as a result, whatever else it might be, it cannot count as a piece of science. We discuss this idea more fully in Chapter 8 on the hypothetico-deductive method (see especially Section 8.6) since it draws on essential features of that method.

Popper talks of falsifiability, and Quine who also adopts a similar value, talks of refutability: 'some imaginable event, recognisable if it occurs, must suffice to refute the hypothesis. Otherwise the hypothesis predicts nothing, is confirmed by nothing, and confers upon us no earthly good beyond perhaps a mistaken peace of mind' (Quine and Ullian 1978, p. 79). Even if a hypothesis predicts, say, the observably testable proposition that p, there is another proposition, viz., not-p, that would count against the hypothesis; so the hypothesis gets into the arena of the scientific in virtue of its observable consequence, that p. What is important is that it yields at least one observational test consequence – the more test consequences, the more testable it is. In this case not-p is a potential falsifier for the theory. What is important is not actual refutation (when not-p is shown to be the case), but merely the bare possibility of refutation.

Note that a single, or a few, actual falsifiers are not sufficient to refute a theory; the conditions for refutation involve more than this (see Popper 1959, Part IV, especially Section 22). Both Popper and Quine recognise that it is also possible to evade any possibility of refutation by making adjustments to other hypotheses. So both propose methodological rules about which adjustments are admissible and which are not. This is a further important feature of the methods of critical inquiry in science that rule out *ad hoc* stratagems; we say more on this subsequently

6.3.2 Acquiring Truth and Avoiding Error as an Intrinsic Aim

Some definitions of truth were introduced in Section 2.3. Here we discuss the aim of critical inquiry which is said to be to uncover truths. Unfortunately this aim can all too easily be realised; simply believe everything that comes along and you will capture all the truths – but lots of falsity too. The aim of avoiding error is equally unhelpful; by not believing anything one avoids falsity – but truths are thereby missed. Instead the aim ought to be: maximise the acquisition of truth while minimising falsity. In accepting any theory one may have to take on board some truth along with some falsity. What we need to do is to select those theories that maximise available truth while minimizing falsity. This is the idea behind the notion of aiming for theories with a high degree of verisimilitude, or truthlikeness as is sometimes said.

Here we need to consider kinds of truth. Realists and non-realists about scientific theories are divided over the issue of truth as an aim for science. We all want our theories to fit all the observable facts and experimental laws we can gather. If they fit, we can accept them; if they do not fit and conflict with some observable fact or experimental law, we are inclined to reject them. People who are called 'realists' by philosophers of science, not only aim for (a) truths about what we can observe; they also aim for (b) truths about unobservables postulated in our theories. (Realists might settle for less in requiring only that our theories have some degree of verisimilitude rather than full truth.) A problem now arises for realists: granted that theories satisfy (a), how can they be sure that they satisfy (b)? That is, how can they tell that the unobservables they postulate, e.g., sub-atomic particles, genes, quasars, etc. really do exist and that the proposed laws about them are true. They have no independent access to such objects and to such laws other than through what can be observed as in (a). (Some replies to this concerning methods of test are addressed in the next three chapters.) Non-realists such as van Fraassen (1980, Chapter 2) who advocate a rival to realism called 'constructive empiricism', reject aim (b) of realists and settle for only the lesser aim (a). In their view truth is a legitimate aim for our theories *only* when we require our theories to fit with what we can possibly observe, i.e., our theories ought only be *empirically adequate,* as they say. Any more than this is scientific hubris on the part of the realist. Thus one of the aims of science, viz., discovering the hidden nature of unobservable reality, is highly contested. (We will not discuss this further here; but for one account and defence of realism and the value of theoretical truth it aims for, see Niiniluoto 1999).

6.3.3 Prediction as an Intrinsic Aim

Prediction is another intrinsic aim of science, indeed, one of the oldest. Ancient astronomers, for instance, were interested in predicting the positions of planets, the eclipse of the sun and the moon, and so on. In science quantitative predictions are preferred over qualitative ones, but this may be a tall order in some disciplines. For instance, while physics after Newton is extremely successful in producing very precise, quantitative predictions, disciplines such as meteorology, geology and earthquake science may be content with only qualitative ones in most cases, and Darwinian evolutionary biology may fall short of providing any.

Novel predictions are another category of predictions that are valued highly. A prediction by a theory T is novel if it has not been made by any theory before T. A prediction can turn out to be either true or false. While false predictions can be novel in the sense defined, they are not an indication of success. Rather what we want are novel predictions that are also true, that is, theory T anticipates a *novel fact*, unknown before the introduction of

theory T. There is also a slightly weaker sense of novelty in that a fact may be known but it has not been successfully incorporated into any theory before T and T is the first to predict it.

To give some historical examples, the phenomenon called *stellar parallax* was a novel prediction of the Copernican theory. The theory predicted that since the Earth orbits the Sun, its position at different times of the year should make a positive, changing angle with a fixed star. That is, the star appears to shift is position in the sky when it is the observer on Earth that is moving; in the same way distant objects change their relative position due to a person's motion on a merry-go-round. Though this was a novel prediction made by the Copernican theory but not its Ptolemaic rival, it is not observable with the naked eye. This was something first noted by Aristotle who entertained the idea that the Earth might orbit the Sun but rejected it on the grounds that we ought to observe parallax, but none can be observed (using the naked eye). Such parallax was first detected using telescopes by Bessel in 1838 thereby turning a novel prediction into a truth, viz., a novel fact.

Other novel predictions include: the existence of an eighth planet, namely Neptune, predicted on the basis of Newtonian mechanics in the 18th century; Hertz's prediction of radio waves on the basis of Maxwell's electromagnetic theory in the last quarter of the nineteenth century; Einstein's prediction of the bending of light when passing nearby a massive star in 1915.[4] Einstein's prediction of the precession of the perihelion of Mercury is a case of the weaker sense of a novel prediction. It had been known since the mid-nineteenth century that there was a missing gap of 43 arc-seconds per century that Newtonian theory could not explain. Einstein's general theory of relativity was the first to explain this missing amount in an entirely non-*ad hoc* way. This episode still counts as a novel true prediction since, even though the facts were known, they get their first adequate prediction (and so explanation).[5] Like quantitative predictions, novel predictions are not easy to come by, and that is one reason why they are so much valued (see more on novel prediction in Section 8.5).

There are some important relationships among prediction, testability and scientific method. If, for example, it is possible to deduce predictions from a theory, then that theory is testable in the sense explained above. The converse is also true; that is, if a theory is testable, then it is possible to draw a prediction from it. This shows that we can think of predictions as test implications of theories, and this is the core idea behind the hypothetico-deductive method that we discuss in Chapter 8.

6.3.4 Fruitfulness as an Intrinsic Aim

A theory is fruitful in two senses: first, in the sense of opening up new problems and new areas of research, and, second, in the sense of providing

unification. When Copernican theory predicted stellar parallax in the 16th century, it did not enjoy confirmation immediately. Many took this to be a refutation of the Copernican theory, but from the viewpoint of Copernicans this was a new problem that needed to be solved. Indeed, it turned out that the reason why stellar parallax was not observed in between 16th and early 19th centuries was that the existing estimate of the distances between the Earth and fixed stars was much smaller than the correct values. So, the observation of stellar parallax waited for new estimates, more powerful telescopes, and so on. In short, the Copernican theory was fruitful for raising a new research problem. Similarly, the prediction of radio waves by Hertz also opened up a whole new area of research in physics, which led to many practical applications later.

The second sense of fruitfulness is unification. A theory is fruitful if it unifies seemingly unconnected, diverse phenomena. Newtonian science provides an excellent example. Galileo had discovered the law of free fall and Kepler had discovered his three laws for planetary motion by the early 17th century, but before Newton's theory they remained as separate, unrelated phenomena. Newton was able to derive both Galileo's law of free fall and Kepler's laws of planetary motion from his theory, thus achieving a remarkable unification among diverse phenomena.[6]

Unification is an idea closely linked to explanation. Typically, when a theory has great explanatory power, it achieves a certain degree of unification among diverse phenomena, and thereby understanding (see Section 2.5). Thus, we see that these two intrinsic aims of science are also interrelated.

6.4 VALUES, RULES AND METHODOLOGICAL PRINCIPLES

The previous section discusses some of the values that our theories ought to exemplify (note the normative force of the 'ought'). But the values can also be expressed as rules (which also have normative force) as when we say: one *ought* to seek simplicity, or one *ought* to adopt fruitful theories, and the like. These rules express much the same as values that we adopt, providing the rules are not understood as always universally applicable, but are defeasible (that is, like most rules under appropriate conditions – whatever these be – they may be suspended). Though the difference in expression is slight, the idea of rules in scientific methodology does become important when we consider their wide use in critical inquiry.

The first important rules are those of deductive logic. Thus the rule *Modus Ponens* is a rule of permission that says: from the two premises A, and if A then B, infer B. There are also rules that do not permit certain inferences since they are invalid. Thus the fallacy of Affirming the Consequent is an inference which we are not permitted to make: from the two premises if A then B, and B, do not infer A. Inductive rules of inference can also be included here but

their discussion is left to the next chapter. Both kinds of logical rule are an important part of critical inquiry.

Following rules is sometimes advisable, but often obligatory, in critical inquiry because of the end, goal or value they realise. What is the goal in the case of deductive rules? The goal is that of truth, or more strictly preserving truth. Thus if the premises of an argument are true, and the rule is a valid rule, then there is a 100% guarantee that the conclusion is also true. Often the goal associated with a rule is not explicitly stated. But in a course on logic it is important to show that the goal is one of preserving truth, that is, of transmitting truth from the premises to the conclusion; and moreover that this is absolutely guaranteed. This is something that the truth-tables in logic readily show. As such the rules of deductive logic are important principles of critical inquiry that preserve truth (assuming the premises are true).

As well as deductive (and inductive) rules, the empirical sciences also have a special set of methodological rules that are an important part of critical inquiry as applied in the sciences. Isaac Newton proposes some important rules in his *Principia* Book III. These are his *Rules of Reasoning in Philosophy*. (Note that in Newton's time the word 'science' was not commonly used and that he often referred to his work in science as *Philosophy*, *Experimental Philosophy* or *Natural Philosophy*.) In the different editions of the *Principia* the rules differ. But commonly they are the following four Rules.

Rule 1: We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.

Rule 2: Therefore to the same natural effects we must, as far as possible, assign the same causes.

The first two are rules of parsimony for making causal claims. When it comes to postulating causes of what we observe, we are not to postulate too many, and when we do postulate some we should always assign like causes to like effects. These rules are not universally strict but are defeasible; as Newton says they should be followed 'as far as is possible'.

The next two are expressed more briefly than in Newton's text, but his overall intent remains clear.

Rule 3: The qualities in bodies … which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever.

Rule 4: In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, not withstanding any contrary hypotheses that may be imagined.

These two rules concern the use of inductive inference in science. Newton's worry underlying Rule 3 is: how do we know that the properties that we assign to bodies in our experiments done here on Earth also apply to bodies

elsewhere on Earth, elsewhere in the solar system, or elsewhere in the entire universe, past present and future? If there is no guarantee that what we postulate for objects here and now cannot apply at other time and places for similar objects throughout the entire universe, then science would be impossible. This was of acute concern to Newton since he wished to apply laws he had discovered here on Earth to all bodies in the Universe. And the only way to do this is to suppose some principle of induction (discussed further in the next chapter). The fourth rule concerns making inductions from experiments we perform to general laws. In fact Newton makes it clear that this is part of his conception of science since he makes the comment after he sets out the rule: 'This rule we must follow, that the argument of induction may not be evaded by hypotheses'. Only in the later editions of his *Principia* did Newton start to use the term 'hypothesis' in a pejorative sense to stand for speculation not grounded in induction from what we already know. Given this we can view Rule 4 as Newton's attempt to demarcate science from other matters such as hypothesising, speculating, and the adoption of any other systems of belief not founded in induction.

Part of Newton's genius is the ability to penetrate the common assumptions we make in conducting any critical inquiry in science, to state them explicitly and then to tell us throughout Book III of the *Principia* when he uses each of these rules in his reasoning. Usually scientists are not this explicit. For Newton these rules are at the core of his scientific enterprise and as such have had a central place in any account of what science must be. Something like these rules has become part of the lore of science. Of course the rules can be refined. In most books on either the establishing of causes, or the making of scientific inferences, something like Newton's rules are still present, though a lot more is now known about causal and inductive methods. Newton does not state explicitly what goal the rules are to realise, but the first gives us a clue that truth is one central aim: we want to know what are the true causes. And the last two presuppose that our aim is to know what are the laws that truly apply not only here on Earth in our experiments but also elsewhere in the universe. Thus once again truth seems to be the principal aim.

Science is replete with rules of method of all kinds. Some are controversial but others are generally accepted. Here, without entering into controversies about them, are some examples of rules of method:

construct hypotheses that are highly testable;
avoid making *ad-hoc* revisions to theories;
other things being equal, choose the theory that is more explanatory;
choose the theory that makes novel true predictions over the theory that merely predicts what is already known;
reject inconsistent theories;
accept simple theories and reject more complex theories;

in revising a refuted theory, choose the revised version that makes a minimum of changes in the refuted theory;
accept a theory only if it can explain all the successes of its predecessors
only use controlled experiments in testing casual hypotheses;
in conducting experiments on human subjects always use blinded procedures;
only admit testable theories as part of science.

These are just some of the many methodological rules to be found in science, or 'M-rules' as we will call them (these would also include Newton's rules). Here is not the place to justify these rules; that is a matter for meta-methodological investigation into the methods of science (see Nola and Sankey 2000). Our purpose is merely to illustrate the kinds of methodological rule that are part of scientific method and so part of critical inquiry that involves science.

The above M-rules are rules for revising or evaluating or choosing hypotheses and theories. But there are also M-rules for constructing or discovering laws, hypotheses, theories and the building of models (Chapter 10 contains a more extended discussion of the methodology of model building in science). The above rules have been stated without mentioning what is to be achieved by following them; that is, no aim, goal or value has been associated with them. In general rules may be of two forms. They may be categorical as when one says, without mentioning any goal at all: follow rule R. Here the goal is either implicit; or the rule is of a special sort that requires no goal because of its categorical nature (though in the long run this is often the goal of truth). But sometimes a value V is explicitly spelled out and the rule is a hypothetical of the following form: if you wish to achieve V then do R. Thus if you wish to obtain theories that are fruitful (V) then you ought not to accept any *ad hoc* theory (R); the goal of getting a theory that has lots of new consequences is hardly to be achieved by following the rule which says 'accept *ad hoc* theories'. Finally a rule might be comparative as when it says: you ought to follow R rather than R* if you wish to achieve V; that is, following R is more likely to get to you to V than following R*. Or even more precisely a rule might say: following R realises V m% of the time while following R* realises V only n% of the time, where m is (much) greater than n.

Understood as such most M-rules are instrumentalities. They are means-ends rules laying down a procedure to be followed for achieving given epistemic ends. The exception would be categorical rules that mention no value; they are not means to some end. Rather we are to follow them come what may. Perhaps the rules of logic are categorical in this sense.

We can now say what we mean by *principles of scientific method*. These are the norms that govern science in so far as it is a cognitive enterprise. Though they are not obviously expressed as principles, values do have

normative force as those features we wish our theories to exemplify; as such values can fall under the umbrella term of principles of method. And obviously we can include all the principles of reasoning of both deductive and inductive logic as they also have normative force in science. Finally we can include all the hypothetical and categorical M-rules mentioned above; they too have normative force in telling us what we ought to do in science. This collection of values and rules comprise the norms, or the principles, of scientific method; and these principles constitute the core of critical inquiry, especially as that inquiry is directed upon matters to do with the sciences (but they apply elsewhere as well).

6.5 HYPOTHESIS, LAW, THEORY AND MODEL

Science textbooks abound with terms such as 'hypothesis', 'law', 'theory' and 'model', but they rarely define or explain what they mean. As a result, there is much confusion about the meaning of these terms in the science classroom. 'Hypothesis', 'law', 'theory' and 'model' are not only used in different ways by different people, their meanings have also changed over time. Such factors resulted in a multiplicity in meaning and use, something students of science must be made aware of. Although it may be impossible to achieve absolute clarity and unanimity with respect to these terms, we may make some progress in that direction by making some distinctions. We will consider only current uses or meanings, bypassing what scientists such as Galileo or Newton meant by the terms in question (see, though, the previous section for Newton's use of the term 'hypothesis'). Let us then begin with 'hypothesis'.

The term 'hypothesis' has at least three meanings. First, a hypothesis is often said to be a *guess* or *conjecture* that enjoys no evidential support whatsoever. This is typically what is meant by the term when it is used by the legendary man in the street. As an example, somebody might say of the claim that there is life in other galaxies, 'Oh, that is just an hypothesis'. Sometimes, 'theory' is also used in this way. When, for example, creationists want to downplay the Darwinian evolution, they say 'it is just a theory', meaning that it is just a speculative guess or conjecture without any support. A second meaning of 'hypothesis' is: a contingent proposition that can be confirmed or disconfirmed (or refuted) and that is therefore revisable in the light of (empirical) evidence. In this sense, all scientific claims (including laws of science) are hypotheses. A third meaning is: a hypothesis is any contingent, revisable proposition that purports to solve a problem or explain a given phenomenon. This is how the term 'hypothesis' is typically used in science, and in this book we adopt this usage from now on. Notice that when a contingent proposition expresses a law of science or a causal relationship in the world it will have some explanatory power; so the second and third meanings become the same.[7]

The term 'law', on the other hand, is ambiguous between a state of affairs in the world manifesting itself as a regularity (uniformity) and a generalization as its linguistic expression. To avoid confusion, philosophers call the former 'law' and the latter 'law statement'. A law statement, therefore, is a statement that expresses a law (of nature). There is consensus among philosophers that, *minimally*, a law statement is a generalization, a universally quantified statement, which expresses a regularity in nature, in virtue of which it has explanatory power.[8] The more general a law statement is, the more explanatory it is. Philosophers also distinguish between fundamental and derived law statements. As an example, while Newton's second law of motion is a fundamental law statement, Galileo's law of free fall is a derived one (see Section 2.5). Clearly, fundamental laws have more explanatory power than derived ones. Law statements can also be used to make predictions. Explanation and prediction are arguably the two most useful functions of law statements.[9]

The term 'theory' has also several senses. We talk of Newton's theory, Darwinian theory, plate tectonic theory, even theory of knowledge and the like. Minimally, 'theory' simply means any (usually a small number of) interconnected set of principles (statements) about a certain domain. In science, the term 'theory' means more than this. What else is needed for a theory? At this point it may be helpful to distinguish between two senses: a wide and a narrow (technical) sense. In the wide sense, 'theory' in science means a set of interconnected fundamental law statements. Thus, when we speak of Newton's theory, we mean Newton's three laws of motion and the inverse square law, i.e., the law of universal gravitational attraction.[10] These laws are interrelated in several ways. For instance, they all involve force and mass; the second law (**F** = m**a**) specifies how a body accelerates when a force acts on it, and the inverse square law provides a specific, but very important force function which can be plugged into it, and so on.[11]

We define 'theory' in the narrow, technical sense in terms of the notion of model. A (theoretical) model is a system of idealized objects, with idealized properties and obeying idealized laws. Accordingly, a theory consists of theoretical statements and a family of models specified by them. Obviously, there is much that needs to be explained in this definition, but a full account must await Chapter 10, where we discuss the notion of a model in detail.

This ends our brief clarification of 'hypothesis', 'law', 'model' and theory'. We are well aware that much more needs to be said about these notions, but we believe that our treatment, though brief, is sufficient for most purposes in the science classroom. In a useful article McComas (2000) points out that there is a widespread myth in science textbooks and in science classes that hypotheses, theories and laws form an ascending hierarchy with increasing evidence: a theory is more secure (i.e., more supported) than a

hypothesis, and a law is, in turn, more secure than a theory. McComas criticizes this picture by saying that theories do not become laws with the accumulation of more and more evidence, and we agree. There are several reasons for rejecting this picture. First, as we just explained, in one sense of the term a theory is just a set of law statements; so no hierarchy of the kind envisaged as above exits. Furthermore, in our view there is nothing in the concept of a law statement requiring confirmation; in other words, a statement need not be confirmed in order to count as a law statement. Failure to recognize this would be to confuse matters of ontology with those of epistemology. All that is required for a statement to be a law statement is that it expresses a lawful regularity. Of course, to *accept* that statement as expressing a law of nature, we must have evidence that it does describe a lawful regularity; but this is an epistemic matter concerning acceptance. In short, being a law statement and accepting a statement as expressing a law of nature are different things.

It turns out, however, that McComas' reasons for denying the hierarchical view of laws and theories are entirely different from ours. He writes: 'Of course there is a relationship between laws and theories, but it is not the case that one simply becomes the other – no matter how much empirical evidence amassed. Laws are generalizations, principles or patterns in nature and theories are the explanations of those generalizations' (McComas 2000, p. 54). So, his reason is that whereas laws are generalizations, theories are the explanations of those generalizations. But this is a false contrast. Although we agree that laws (more correctly, law statements) are generalizations (and that is because they describe regularities), we also contend that they have explanatory power (as we saw in Section 2.5) and that theories have explanatory power in virtue of the laws they contain. Less general law statements are explained by subsuming them under more general ones, a point emphasized again in Section 2.5 on explanation (know *why*).

6.6 A WORKING DEFINITION OF SCIENCE

Scientific thought and activity is extremely rich and complex. Even when we confine ourselves to physical and biological disciplines as we do in this book, it is virtually impossible to give a simple and rigorous definition of science, a definition that would capture its richness and complexity and at the same time distinguish it from other human thought and activity. Nevertheless, it is important to have at least a *working definition* at hand, which could serve as a useful springboard in classroom discussions for more sophisticated definitions.

The literature on definition of science in science education is voluminous. We cannot do justice to this literature. But we think it is fair to say that most definitions in the literature are either too narrow or too wide. Instead of trying

to substantiate our impression, we will simply provide our own definition and then let each science educator to compare it with his or her favourite characterization.

Before we proceed, it will be useful to specify what we expect from a working definition. A working definition of science should be broad enough to capture the most important (if you like, paradigmatic) characteristics of physical and biological disciplines, but at the same time narrow enough to exclude non-scientific endeavours such as art, literature, religion, and pseudo-sciences as well. Clearly, there may be borderline cases, but they should be dealt with individually, as they arise.

We should also distinguish between *science as a thought and activity* and *science as an institution*. Here we are concerned with the former. The latter includes universities and research centres, scientific associations and academies, and so on. Science as an institution is basically a sociologically entity and can therefore be the object of social study naturally. By contrast, science as an activity and thought is essentially a cognitive entity and therefore must be treated as such. A failure to distinguish between the two gives rise to much confusion (some of which we discuss in Part III).

The following is our definition for science as an activity and thought.

> *We define science in terms of a manifold with six aspects: S = <Activity, Aim, Product, Method, M-Rule, Attitude>.*

First, science is, in part, a human activity. Typically, it includes such activities as observing, collecting and classifying data, setting up and carrying out experiments, calibrating scientific instruments, constructing hypotheses, theories and models, finding evidence, reading a scientific paper at a conference and so on. Second, scientific activity has a set of aims intrinsic to it (see Section 6.3). Knowledge, testability, prediction, explanation are among the most important. Third, by science we also mean the end products of this activity: reports of observations and experiments, laws (in the sense of law statements), theories, models, and, ultimately, knowledge. Thus, knowledge is both an aim of science and its product; in contrast, other products, such as hypotheses and theories may have other, different qualities (such as being justified by appropriate evidence, or being *ad hoc*, and the like). Fourth, science produces these results by using various methods. Some of these methods may be the material practises that students must master if they are to be successful experimentalists. A much more general conception of method includes the making of scientific inferences and testing, as in the use of induction, the hypothetic-deductive method, Bayesianism, and the like (each of these has separate discussion in the following three chapters). Fifth, we separate out those aspects of scientific activity guided by methodological rules, which we have called M-rules in the previous section. The final item in our list is a certain kind of attitude scientists are expected to conform to in

carrying out their scientific activities. These include: being open to free and critical discussion, a willingness to change one's opinion when presented with good reasons, being intellectually honest, using properly scientific methods to obtain results, being free in theory choice from religious or political bias, and the like. These are part of the norms that make up the ethos of science importantly described by Merton (1973, Chapter 13).

This definition, we believe, is both sufficiently sophisticated and simple enough to be effectively useful for the purposes of science education, that is, for giving students a good idea about the nature of science. It is also philosophically neutral as far as it goes, in the sense that it is free of certain philosophical commitments such as realism, idealism, relativism, universalism, and the like. One can adopt any one of these, depending on how one wants to fill in the notions of knowledge, truth, method and so on. Thus, science teachers can avoid discussing these difficult philosophical issues in the classroom if they wish to. Nevertheless, we hope that the fact that science is essentially a special form of critical inquiry comes out clearly from the working definition. This can be seen by noting the last three components: methods, M-rules, and the scientific attitude. All of these indicate the strongly critical essence of science by emphasizing that only those beliefs that are held with a certain attitude and that pass muster scientific method and rules are worthy of acceptance.

Another feature of our working definition is that it enables the teacher to focus on any of the six different aspects of science. Thus, for example, those instructors who would like to emphasize the social dimension of science can explore the first and last components. They can raise a number of interesting questions. Do scientists today typically work in teams or individually? Are there important differences among different disciplines in this respect or not? In what ways do they rely on each other's works? Is there only collaboration or also competition among scientists? If so, why and how? What sorts of reasons can lead scientists to deviate from the norms of openness and honesty? Are there tensions between the satisfaction of intrinsic and extrinsic aims of science? If so, how can one go about resolving them? And so on. Other teachers may wish to emphasize the nature and plurality of methods in science. Yet others may want to explore in what ways the various components of science are interrelated, thus drawing attention to the unity of the manifold. One can, for instance, point out the connection between avoiding *ad-hoc* revisions (which is what is demanded by an M-rule) and achieving the aim of testability. It goes without saying that our working definition is not complete for the simple reason that many concepts and ideas in it are left unexplained.

The remainder of Part II discusses the methods that produce scientific knowledge. The reason for this special attention is that while knowledge is the *ultimate* intrinsic product of science, the production of scientific knowledge is

impossible without scientific methods. But this is not the only reason. Unfortunately, most characterizations of knowledge and scientific knowledge in science education are either philosophically naïve or misleading or simply wrong. Most constructivist, postmodernist and epistemic multiculturalist accounts share these defects. Since scientific knowledge is a subset of knowledge, a poor conception of knowledge infects one's conception of scientific knowledge as well. That is why epistemology in general, not just philosophy of science, is so crucial for science education.

Earlier we pointed out that our working definition of science is philosophically neutral. But this does not mean that philosophical positions do not matter in science education. On the contrary, they are important, and it is our firm belief that a realist, rationalist, and universalist perspective gives a deeper and more grounded understanding of science. There are two levels at work here. The working definition is intended primarily for science students. This is the first (if you like, object) level. Our philosophical perspective about it constitutes the second, meta-level, which is intended for science educators. The meta-level affects how one goes about articulating and elaborating the working definition.

The notion of method is central to critical inquiry in science. Following this more general, introductory section on aims and methods, the next three chapters set out some of the central accounts of method that are at the core of critical inquiry in science. The next chapter considers the naïve inductive method; this is followed by a chapter on the hypothetico-deductive method, and following this a chapter which is intended as a primer on probabilistic reasoning and Bayesianism as an account of scientific method.

NOTES

[1] For an account of different theories of scientific method see Nola and Sankey 'A Selective Survey of Theories of Scientific Method' in Nola and Sankey (eds.) 2000. See also the whole of Nola 2003 which is a defence of theories of rationality of science against its detractors.

[2] An excellent account of this episode in the recent history of science, technology and business is given in Christie 2001. She discusses the extent to which scientists were or were not aware of the consequences of ozone depletion. This was recognised as a theoretical possibility by some; but it was not generally recognised that CFC molecules had the ability to rise from the Earth's surface to its upper atmosphere to the extent they actually did.

[3] For Kuhn's list of values see his 'Objectivity, Value Judgement and Theory Choice' in Kuhn 1977, pp. 321-2. Kuhn is much more explicit about the values he adopts than in the earlier Kuhn 1970, where it might appear that he does not advocate any "paradigm-transcendent" values at all. This impression is corrected in the 'Postscript" pp.184-6 to Kuhn 1970, first published in 1962.

[4] For a very readable discussion of these examples, see Chalmers 1999, pp. 138-41.

[5] For a much fuller account of this episode and other issues surrounding it, see Will 1995, Chapter 5.

[6] For the derivation of the law of free fall from Newtonian laws of motion, see Chapter 2, Section 2.5.

[7] For the explanatory role of law and causal statements, see Section 2.5.

[8] Philosophers also agree that this characterization is not sufficient to define a law statement because it is too broad; it lets in what are called accidental generalizations. To distinguish between the two, philosophers have devised various tools, which need not concern us here (see, for instance, Hempel 1965, Chapter 5.3). For the purposes of school science, the minimal characterization should suffice. There is also the further question of whether laws are *constituted by* regularities. This is a thorny issue in the metaphysics of law, which we need not get into (see, for instance, Armstrong 1983). We will simply adopt the more neutral position that laws often manifest themselves as regularities, without committing ourselves to any specific position concerning the question of constitution.

[9] As we indicated in Section 2.5, we will use 'law' and 'law statement' interchangeably for purposes of convenience. The context will make clear which one is meant.

[10] Strictly speaking, Newton's first law is a derived law; it can be derived from the second law. We keep it for historical and pedagogical reasons as most science textbooks do.

[11] In the fifties and sixties more was demanded from the notion of interconnectedness. Thus, some philosophers argued that a theory should be axiomatizable in first order logic. This idea was short lived; now most philosophers think of theories in terms of models. This brings us to the narrow, technical sense of the term 'theory', which we introduce below and discuss more fully in Chapter 10. For a very informative semi-historical account of different conceptions of scientific theories, see Suppe 1974, especially the long introduction by Suppe, pp. 3-232.

CHAPTER 7

NAÏVE INDUCTIVISM AS A METHODOLOGY IN SCIENCE

In science, philosophy of science and this book there is much talk of critical inquiry. This we take to include *the method(s) of science*. But it is unclear what these are. The word 'the' indicates it is supposed to be something unique. We will argue that there are different methods, depending on purpose, but all of these methods are part of a universalistic conception of scientific rationality. This does not preclude the obvious fact that scientific methods have evolved over time (methods in statistics being a prime example); we should allow that we have a growing bulk of methodological principles. Our own conception of science is decidedly realist and rationalist. In the following three chapters we will present several different accounts of scientific method. These accounts are not always rivals to one another. They have common elements, as will be seen; however they apply best in different domains. In this chapter we will discuss what we call *naïve inductivism* (NI) as a fairly common methodological approach in science. In the next chapter we will consider the hypothetico-deductive method (H-D). Even though these are different methodological approaches they should not be thought of as rivals; we will show that they in fact complement one another.

7.1 METHODS FOR WHAT?

Talk of scientific method invites the question: 'methods for doing what'? Are there methods for testing hypotheses? Clearly there are many different methods according to the kind of hypotheses under test. Are there methods for discovering hypotheses? Some have claimed that there are no such methods while others have claimed that not only are there such methods but that there are readily available computer programmes which, when the data used by, say, Kepler about planetary positions are fed into them, then they will yield as output Kepler's Laws. In statistics there are methods for finding the hypotheses which fit best a given set of data (such as methods of regression and the like). Are there methods for conducting experiments? Clearly in each science there are distinctive methods that students have to master for carrying out experimental investigations, such as the investigation of stress in model buildings under simulated earthquake conditions in engineering, the preparation of slides for viewing cells under microscope in biology, and so on. Are there methods for calculating? There are different methods of applying different kinds of mathematics in the various sciences.

Are there methods for observing? Clearly how astronomers observe the stars differs from how ethologists observe different kinds of bird-song, or psychiatrists observe odd behaviour in humans, and so on. Finally, there are methods that attempt to characterize, in some more general sense, the structure or nature of some aspect of science. In this chapter we will explore the character of naïve inductivism (NI) and the extent to which it occurs in science. The above illustrate ways in which talk of the methods of science is not empty or meaningless. Contrary to reports about the demise of scientific method by constructivists, postmodernists and their ilk, it is alive and well, and in flourishingly good health.

The image of science presented by NI is the following. Science is a 'bottom - up' affair. It starts with observational facts. In a strong version of NI, facts are prior to theory; facts are to be gathered independently of any theoretical considerations. But once gathered they can be used to infer hypotheses, laws or theories. The reporting of these facts can be made more precise with the development of observational techniques and instruments; and the stock of observational facts will increase over time. It is upon this allegedly firm foundation that the edifice of scientific knowledge is commonly said to be built, mainly using inductive inference to get from observational facts to generalizations or theory. There is something to this story, as will be pointed out in the following sections; but it is not the only approach.

In contrast to NI, the hypothetico-deductive method, H-D, treats science is a 'top - down' affair. According to H-D methodology, scientists always start with some hypothesis (usually proposed as a solution to some problem). It is then applied in a variety of particular circumstances, leading to test implications (sometimes predictions) that can be tested against what we can observe or discover by experiment. Here the hypotheses are the "drivers", not observations as in the case of NI. The application of the hypotheses determines what kinds of observational input are needed for their application, and what kinds of observation are required for their test. We wish to claim that neither method of science is exclusive of the other and that both capture important aspects of what we can more generally call "scientific method".

Often NI is presented as *the* one and only method of science. It is certainly the one that is most commonly presented as "the method of science". But it can only be part of the story, albeit a not unimportant part. What it captures are two important ideas. The first is that *sometimes* science begins with, and is built upon, the collection of facts or data, usually, but not always, produced by direct observation. (In this respect NI adopts certain features of the foundationalist model of knowledge discussed in Section 4.4.) As well as direct observation by humans, there is also the collection of data by measuring or detection instruments such as instruments for recording earthquake activity, remote sensing satellites, or the vast amount of astronomical data collected by radio-telescopes. Second, there are ways of

inferring from such data to hypotheses in science. Within science education students need to know about data collection, and know about how to make inferences from such data.

The following examples can be used to illustrate NI. Astronomers from ancient times onwards have gathered a vast catalogue of planetary and star positions, using the naked eye or more precise measurements with telescopes, most of which have a permanent place in the catalogue of astronomical facts. In contrast, it is our theories of planetary and star motion that have undergone change, not this body of fact.

Robert Hooke (1635-1703) was one of the first to publish an account of what he saw under the microscope. In his *Micrographia* of 1665 he described, and drew remarkable pictures of, what he could observe with the newly invented microscope. This included the cellular structure of the eyes of flies, their hairy legs, the point of a pin, the cellular structure of cork, the small organisms in water, and so on. All of this was an important record of what had never been seen before by anyone. He may have had some reason, now unknown, for selecting one sort of item, rather than another, to view under his microscope. Perhaps he was guided by the mere curiosity about regions of the micro-world previously inaccessible to us and about which there was, at that time, much interest and speculation. Hooke did not collect observational facts in the 'light of some theory", as some claim we should only do. Since Hooke's interest in collecting observational facts was pretheoretical, or carried out in the absence of any theory, we can say that his approach to science was *naïve* (in a non-pejorative sense). Hooke, we may say, was a *naïve* collector of all the facts he observed with his microscope. Often students can be excited by Hooke's approach when they, too, use microscopes for the first time.

Again Hooke was one of the first to suggest that one should collect weather information. We are now familiar with records of daily rainfall and daily maxima and minima of temperatures. Such fact collecting may not have had any immediate purpose apart from what might be gleaned from any seasonal trends. Now there is much interest in long historical sequences of such data because of the proposed hypothesis of global warming, an hypothesis that Hooke could not have had in mind as a reason for initially recording weather data. Without such naïve fact collecting of weather data, we would be bereft of important information about climate change.

Field workers in ethology often do not know what they are to learn from the behaviour of the animals they set out to observe. Thus Jane Goodall started, in Gombe Park, Tanzania, as a naïve observer of chimpanzees without prior knowledge of their behaviour and armed only with the desire to uncover information about their life histories. Since the early 1960s she has provided us with a vast catalogue of surprising chimpanzee behaviour. Students too can be acute observers of animal behaviour.

The ringing of birds' legs, or the tagging of fish, is done to discover something of their migratory habits. Often all that is learned about the birds or the fish is that they have been ringed or tagged in one place and then sighted or recovered in another place at a later time, the recoveries being a somewhat hit and miss affair. But with sufficient re-sightings the amassed data might yield important information that tells us something about migratory habits of birds or fish over time. Such facts have become crucially necessary for their proper conservation. It is hard to see how our knowledge of the migratory habits of species might begin to emerge without something close to the naïve fact collecting involved in ringing or tagging, and then recovering.

Discovering facts is one thing, describing them is another. Often the view that all observation is theory laden is taken to undermine naïve fact collecting. But there is no reason why our description of the facts collected need always be in some alleged theory-neutral observational language, if there be such. It would be an error for Jane Goodall to adopt a strictly behaviourist approach to the chimpanzees she observed and describe their behaviour in a language devoid of all purpose and intentionality. Clearly the chimpanzees choose whom they wish to groom, play with or have sex, and the descriptions of their activities (rather then mere behaviour) should reflect this. An opposite error would be to over-anthropomorphise such activities.

Importantly as we will see, the facts collected may be unobservable by us, or theoretical. As any realist would admit (but non-realists might have difficulty with this), there are both observable and unobservable facts for scientists to collect and from which to make inferences. Such is the case in the detection of the ozone hole by naïve fact collecting to be described in Section 7.6. Ozone is not observable by us humans, and is not observable by us in the upper atmosphere; we need detection instruments to do our work.

Some philosophers of science have been over-zealous in playing down the fact-collecting aspect of science. Thus Karl Popper is famous for his rejection of naïve inductivism and his espousal of the H-D method. This is clear in section V of Popper's paper 'Science: Conjectures and Refutations' (Popper 1963) when he reports that he often walked into a class of students studying philosophy of science and issued the command "Observe!". The students were correctly perplexed and did not know what to observe. From this Popper concluded that we always need a point of view or a problem, or even a theory to direct us in our observations. But there is a subtle difficulty here that needs to be avoided. Popper's basic point is correct: the command "Observe!" is directionless. Giving a direction can be done in many different ways. Arousal of curiosity may be sufficient to get one to observe carefully. Popper's error is to turn conditions that are sufficient to motivate observation, such as a theoretical interest, into a necessary condition such as the requirement that all observation must be in the light of theory. This is clearly erroneous, as the work of scientists from Hooke to Goodall shows.

Others influenced by Popper also reject induction in favour of non-inductive methodologies. Thus Lakatos characterizes strict, or radical, inductivist methodology as follows: '… only those propositions can be accepted into the body of science which either describe hard facts or are infallible inductive generalizations from them' (Lakatos 1978, pp. 103-5 and p. 170.) Lakatos argues that this is a seriously incomplete view of science, one of its problems being that it cannot explain why, in the absence of any theory, certain hard observational facts are chosen for attention in science rather than others. When it comes to the choice of what facts to start with, or with what problems to address in science, Lakatos says that NI 'allows only a [random] selection by the empty mind'. This is an exaggeration; a curious mind, which leads to fact collecting, is not an empty mind.

In contrast, Kuhn gives fact-collecting an important place in what he calls 'normal science', or within a particular paradigm. (Kuhn 1970, pp. 25-7). In this early book Kuhn is unclear as to whether the store of gathering facts is retained from one paradigm to another; but in his later writings he makes clear that this is the case. Fact gathering is simply part of normal science and takes place as a matter of course. There is a growing body of fact, which accumulates even across paradigm changes and with the differing descriptions of these facts that might arise in different paradigms. But, of course, in Kuhn's model of science as a sequence of paradigm changes, fact gathering cannot be the main, or the only, kind of scientific activity to consider. But it is not omitted or disparaged.

In sum, the methodology of NI, as characterised here, employs a host of methods for observing using human observational powers, or for collecting data using instruments or technology (such as remote sensing satellites), and then for using mathematical techniques on the data so that hypotheses can be drawn or established.

7.2 WHAT IS INDUCTIVISM?

7.2.1 Two Kinds of Argument: Deduction and Induction

We have explained why the NI model is naïve. But why is it inductivist? Here we need to consider the two broad, excusive and exhaustive logical categories into which all arguments fall. When we propose arguments we may intend them to be either *deductive*, or *non-deductive*, or as is commonly said, *inductive*. In *valid* deductive reasoning the premises entail the conclusion; or the conclusion logically follows from the premises; or the premises cannot be true and the conclusion false on pain of self-contradiction; or, if the premises are true, then the argument form gives the best guarantee possible that the conclusion is also true. (All of these come to the same thing.) If the argument is *invalid*, then none of the above holds. As is also said, deductive arguments are *non-ampliative*. The conclusion does

not *amplify*, or go beyond, what is contained in the premises; it simply draws out what is implicitly contained in the premises and makes it explicit. This is the kind of reasoning found in mathematics, and is captured in theories of formal logic by rules of deductive reasoning.

In contrast, in inductive reasoning the premises do not entail the conclusion; or the conclusion does not follow from the premises; or the premises can be true and the conclusion false; or we lack the best guarantee possible that the conclusion will be true if the premises are true. So all inductive arguments are invalid. But this does not mean that they are all bad; they may be *weak* or *strong*. For a *strong* inductive inference, we can say that the premises give *a high degree of support to*, though not a 100% guarantee for, the truth of conclusion. For *weak* inductive inference the conclusion gets little support from the premises. Importantly, we can say: in a strong inductive argument, the truth of the premises makes it *highly probable* (but does not give a 100% guarantee) that the conclusion is true. Here we invoke the notion of probability (to be explored later in Chapter 9) to define what we mean by a strong inductive argument.

Unlike deductive arguments, inductive arguments are *ampliative*; the conclusion *amplifies* the premises in going beyond what they contain. Given this characterisation, it can be seen that all inductive arguments, including the strong ones, are risky in a way that valid deductive arguments are not. Good inductive arguments should get us from true premises to a true conclusion; but since the content of the conclusion goes beyond the content of the (true) premises then there is a risk that the conclusion is false.

We can now see why inductive arguments loom so large in science. Given the span of our lives, and the life-spans of others, there is only a finite amount of observable fact that all of us can collect as evidence. But in science we wish to go beyond what evidence provides us. Inductive arguments enable us to leave the small island of facts we can observe or detect and to venture into the broad ocean of what has happened in the past when we were not present, what is happening elsewhere where we are not positioned (such as other places in the cosmos), and what will happen in the future where, perforce, we cannot be. In addition it is hoped that inductive inference from evidence will tell us of the basic laws of nature whereby the universe works. These important features of inductive inference are clearly recognised in Newton's Third and Fourth *Rules of Reasoning in Philosophy* (see Section 6.4) as basic to the scientific enterprise. If we do not have such modes of inference then we remain in the realm of naïve fact collectors with no knowledge of the past, the future or elsewhere, or of any of the laws of nature (assuming there are some). This important epistemological position is one worth emphasising at some point in a student's education in science; but it is all too often neglected. Coming to grips with this deep point removes some of the mystique that surrounds science and places it in a much more promising and exciting epistemological framework in which a student can get

an appreciation of the way in which inductive principles like Newton's are at work in all of science. They can also learn to shed the misleading idea that science deals only in certainties, a conception of science deliberately omitted in Chapter 6.

7.2.2 Examples of Deductive and Inductive Arguments.

These points can be best illustrated by looking at the following examples of arguments.

Argument 1	*Argument 2*
All humans are mortal;	All mountains are volcanoes;
Socrates is a human;	Vesuvius is a volcano;
So, Socrates is mortal.	So, Vesuvius is a mountain.

Argument I is *valid*; and it is also *sound*, since its premises are also true. But Argument 2 is invalid. Why invalid? Here we could rely on our reasoning intuitions to get a "feel" for its invalidity. (The reader is referred to any logic textbook to see why Argument 2 is invalid (i.e., the conclusion does not logically follow from the premises). It is also unsound because its first premise is clearly false (since not all mountains are volcanoes). Importantly it is both invalid and unsound despite the fact that the conclusion is true!

In contrast, consider two pairs of *inductive* arguments. The first pair illustrates statistical syllogism and the second pair illustrates enumerative inductive inference. Consider first a pair of statistical syllogisms. They differ from the deductive Arguments 1 and 2 in that the universal 'all' is replaced by the statistical qualification, in this case '90%'.

Argument 3	*Argument 4*
90% of humans are right-handed;	90% of humans are right-handed;
Fred is a human;	Leonardo da Vinci is a human;
So, Fred is right-handed.	So, Leonardo is right-handed.

For the purposes of illustration, suppose that the premises are correct. Note that both arguments are invalid in that the conclusions do not follow logically from the premises. But in both cases they give strong support to the conclusion drawn. This is even so in the case of Argument 4 in which the conclusion is false; we know that Leonardo was left-handed. So, in both inductive (i.e., non-deductive) arguments, even though the premises can give strong support to the conclusion, in the first case we get a true conclusion from true premises, but in the second case we get a false conclusion from true premises.

Consider now a pair of *enumerative inductive* arguments. They are so called because the premises enumerate a number of observed cases, and then draw a conclusion, in one case a generalisation, in the other a prediction

about an unobserved case. What reason do we have for the premises of Arguments 5 and 6? This is simply the observation that each member of humanity has made about other humans; they have been observed to die at some time. Here we enumerate persons a, b, c, etc, and observe of them that they have died. In Argument 5 we draw a general conclusion about all of humanity; in Argument 6 we make a prediction about some person n (now alive or yet to be born).

Argument 5	*Argument 6*
Inductive Generalisation	*Inductive Prediction*
a has died at some time;	a has died at some time;
b has died at some time;	b has died at some time;
c has died at some time;	c has died at some time;
. . . . .	. . . . .
So, everyone dies at some time.	So, n will die at some time.

Neither conclusion follows logically from the premises; so the arguments are invalid. But the premises give us strong grounds, and at one time the only grounds, for belief in our mortality. It should also be evident that Argument 6 gives stronger grounds for its conclusion than Argument 5. Argument 5 amplifies its conclusion into an exceptionless generalisation about all future as yet ubobserved cases, whereas Argument 6 only makes a prediction about one case.

We can now state a general *Rule of Enumerative Induction*:
When a finite number of items a, b, c, . . ., n, which have property A, have been observed in a wide variety of different circumstances and conditions, and each has been observed to also have property B, and none have been observed to lack property B, that is, it has been observed that n As are also Bs; then infer (1) the generalisation that all A are B, or infer (2) the prediction that the next unobserved item x which is an A is also a B.
The Rule requires us not to have come across counter-instances. And it requires us to take a wide and representative sample (where possible) of things that have property A. This is to avoid the problem of unrepresentative samples.

There is also a similar rule for statistical cases. Suppose a large sample of women selected randomly from a wide variety of circumstances (say 1000), are asked to apply a certain cosmetic C, and suppose that 850 of the women do not get a skin rash while 150 women do. Then we can say that of the total women observed, 85% who take the cosmetic do get a rash. What can we infer for the population of women as a whole, as distinct from the observed test group? Since the sample is wide and representative (there are accounts in statistical sampling of what this means), then we can infer, using the rule, to the population as a whole and claim: 85% of all women will not get a rash. Thus a rule of *Statistical Enumerative Induction* says:

When a finite number of items n have been randomly selected from a total population [of, say, women] with property A [woman applies given cosmetic to face], and m have been observed to also have property B [gets rash] then infer (where r = n / m): (1) r% of all A are B, or infer (2) the prediction that the probability that the next unobserved item which is an A is also a B is r%.
The Rule of Statistical Enumerative Induction is a more general case of the more simple Rule of Enumerative Induction which arises when n = m (and so r = 100%). In what follows we will consider only the simpler Rule; but the same considerations equally apply to the more general Rule.

The Rule of Enumerative Induction underpins the inferences we make from experimental results to general laws. An example of this arises in Section 7.4. Boyle made a large number of observations concerning the pressure P and volume V of a gas confined in a tube. Only eight pairs of data are given in the table; and they serve as the evidence E. From this evidence we can infer to the law, PV = constant, as the conclusion of the inductive inference. (Substitute each of the pairs of data into the law to see that they fit (within a small margin of error.)) What this shows is that induction has a wide application, particularly to how experimental evidence inductively justifies our belief in generalisations and laws of nature.

Let us now return to NI and the role inductive inference plays in it. In NI, science is said to start from observations about particular matters of fact. In this respect NI is like the examples of enumerative induction given in Arguments 5 and 6, and unlike the inductive Arguments 3 and 4. Given that we have a collection of particular observed facts, little of interest follows deductively. But if we allow inductive inference, then we can achieve several things. We can make inductively based *predictions* about the future or *retrodictions* (as they are called) about the past. And we can infer about elsewhere in the universe as Newton requires. We also hope to establish some generalisations, or laws, about what is going on. Finally we hope to establish theories (which usually involve at least two or more laws).

There are two final, important points to be made about induction. As Bishop Butler said, probability is a guide to life. Why? Consider the following. (1) In the past it has been observed that drinking water quenches thirst; so the next time we are thirsty we make the inductive prediction that drinking water will quench our thirst – and so we get a drink. Our life is guided by induction at this point. (2) Again, in the past it has been observed that the ceilings of rooms in which we have been have remained up for the time we were there; so the next time we are in a room we make the inductive prediction that the ceiling will stay up – and so we remain in the room. (3) Again, in the past we have noted that collapsing ceilings injure or kill people; so the next time a ceiling collapses on a person we expect injury – and so we avoid falling objects like ceilings. All of the above turns on accepting the conclusions of inductive inferences from past observations.

But what if we did not? Try the only alternative strategy of counter-induction. That is, accept the negation of each conclusion. So in the case of (1), despite all the previous success in quenching thirst by drinking, the next time we are thirsty we do not seek water to quench our thirst. In the case of (2) we do not remain in any building with a ceiling. And in the case of (3) we do not expect injury, and so do not avoid falling objects such as ceilings. Counter-induction is not a successful strategy, or guide, for life. Counter-inductivists have an unfortunate tendency to die out very quickly. While this is not a justification for induction, it at least offers advice as to which mode of inference is the prudent one. After all, we have observed in the past that books do not explode (even randomly); so you, the reader, can infer that this book will not explode. And so you continue to read it in safety. Counter-inductivists should have, already, abandoned this book (if they remain true to their policy of counter-inductively inferring).

This second point concerns the *justification* of induction. The examples and rules of inductive inference illustrate the risky character of inductive reasoning. What we want from induction is a guarantee that if the premises are true then the conclusion will be true (as in the case of Argument 3 but not Argument 4). That is, we want to know when we are *rationally justified* in claiming that the conclusion is true. Though we will not discuss this issue here, it is the famous problem of the justification of induction first raised in a clear way by the eighteenth century Scottish philosopher, David Hume (1711-1776). Hume's sceptical conclusion is that, even though we do, as a matter of habit or custom, make inductive inferences, there is no rational justification for the correctness of such inferences or our acceptance of their conclusions. This is a very important matter for us to recognise. We have put great store on the idea of critical inquiry and its underlying rationality. But now we point out that induction is central to science – and then add that Hume claims that all inductive inferences are without justification! We will not discuss this matter here; there are many evaluations elsewhere of Hume's sceptical conclusion in many books on philosophy and philosophy of science.[1] However this is a crucial problem for any theory of scientific rationality and the modes of inference that it relies upon.

7.3 NAÏVE INDUCTIVISM AS A METHODOLOGY

NI is not a straw-man view of science, as Hempel shows by citing someone who advocates a version of it:

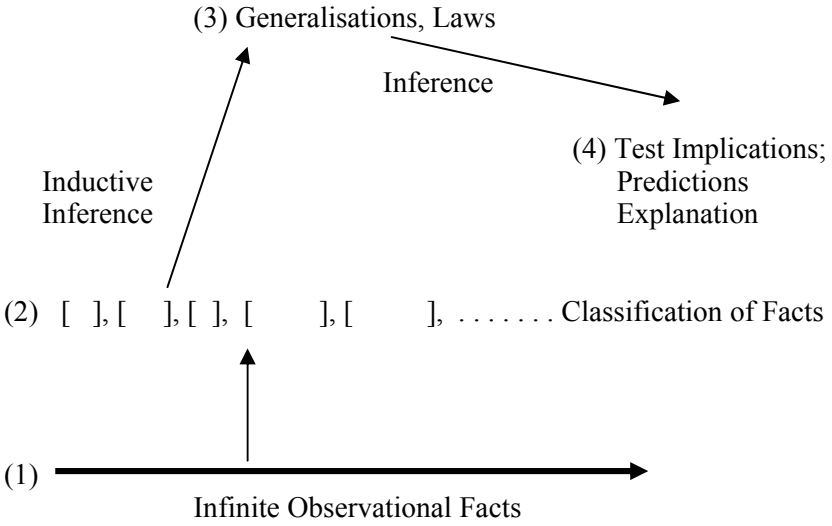
> If we try to imagine how a mind of superhuman power and reach, but normal so far as the logical processes of its thought are concerned, … would use the scientific method, the process would be as follows: First, all facts would be observed and recorded, *without selection* or *a priori* guess as to their relative importance. Secondly, the observed and recorded facts would be analysed, compared, and classified *without hypothesis* or *postulates* other than those necessarily involved in the logic of thought. Third, from this analysis of the facts generalizations would be

> inductively drawn as to the relations, classificatory or causal, between them. Fourth, further research would be deductive as well as inductive, employing inferences from previously established generalizations. (This quotation from A. B. Wolfe's 'Functional Economics' is cited in Hempel 1966, p. 11.)

There are four features to this account of NI:

(1) the collection of all observational facts independently of any selection process;
(2) the analysis and classification of observational facts independently of any theory;
(3) inductive inference from the classified facts to generalizations, laws or theories;
(4) further testing by deductions from the generalizations.

These features may be summed up in the diagram in this section.



Though there are some faults with this extreme view of NI, (3) and (4) do capture some aspects of some procedures in science that other theories of method have either downplayed or ignored. But the first two features of NI are misleading in a number of respects.

Contrary to (1), we clearly do not have to wait for *all* the facts to be collected before any science can begin, especially if we mean by 'all' either all the facts from the beginning of the universe to its end, or all the facts up until now, or all the facts 'relevant' (however that may be specified) to the science under consideration. The naïve fact collector submerged in an ocean of infinite facts is hardly a scientist. All that may be needed is an appropriate sampling of facts. (1) also assumes that in all cases observational facts are independent of any means of selection, in particular the use of theories to guide one towards relevant facts. In one sense this is quite correct. Suppose

the following fact obtains, say, that a dinosaur once stood on the ground currently under your feet. Dinosaurs are usually big; so there is no reason not to call this an observational fact even though no one actually observed that this is so, or ever could. It also remains a concrete fact even if no one ever selects it for any purpose. What is at stake is whether the mere collection of facts is a part of science even though no selection process filters the infinite number of possible facts. Curiosity, or interests, or problems or theories do direct our attention towards one kind of (observational) fact rather than another, and so do filter the infinitude of facts, thereby undermining an extreme reading of (1).

Once we have gathered facts, (2) tells us that we must analyse, compare and classify them without recourse to any scientific theory; only the 'logic of thought' is to be involved at this point. It is hard to know what this means since any two facts can be compared, analysed or classified in many different ways. Consider the following two facts for classification: (a) a tiger stalking in the jungle (now) and (b) a bee hovering above a flower (yesterday). There are many ways in which these can be placed in either the same or different classes. Thus they fall in the same classes when these are fixed by properties such as: being coloured, being yellow striped, being animals, being near vegetation, etc. But they fall into different classes when these are fixed by properties such as: being cat-like, having wings, being in motion, occurring today, etc. Given the indefinitely large number of same or different classes into which these two facts can fall, one cannot tell which are the significant classes for inquiry without invoking the non-logical hypotheses, or the interests, or a criterion of relevance, that have been ruled out in the very formulation of (2).

Again, most people, including early geologists before Steno (1638-86), recognised a number of rocks that they called *glossopetrae* – tongue-stones – found in great numbers in various regions around Europe, especially Malta. They were so called because of their smallish size and their tongue-like shape; they were sometimes used as charms and were believed to have curative powers. It was Steno who recognised that many of these, but not all, were fossilised shark's teeth. The re-classification of many tongue-stones was made largely on the basis of their causal origin as shark's teeth and their subsequent fossilisation. The importance of the re-classification was the recognition that the remains of quite old creatures were fossilised in rock formations that once had been under the sea but were now on dry land and even up in mountains. This led to a long-lasting controversy about the age of the Earth between the religious, who thought that God had created it within the last 5000 years, and those who thought that it might be millions of years old. The re-classification of many tongue-stones in terms of their causal origin requires some knowledge of how the world works that goes beyond knowledge of mere logic. The point of this example is that the classificatory description as fossils would be ruled out on the basis of (2) above. Even the

word ‘fossil’ as used at that time underwent a change in meaning. The word ‘fossil’ comes from a Latin word for ‘dug up’. With change of meaning in terms of causal origin, the word ‘fossil’ refers to a narrower classificatory class than merely those things which are dug up (see Rudwick, 1985). Are these examples of the theory-ladenness of description? Perhaps; but that phrase is itself quite unclear in its import in these cases and is better set aside. The upshot of these examples is that (2) is to be rejected.

In contrast, the other two features of NI do capture some important aspects of science. Feature (3) says that once we have “classified” the (observational) facts, we may then be in a position to draw inductive inferences about their relations. There are two ways in which we can understand this. The first is that the process of classification under (2) leads to *natural kinds* of things. Such has been the case in chemistry with the classification of kinds of substance into elements and molecules. Once we hit on natural kinds, then we can assume that each member of the kind has the same intrinsic properties as any other member of the kind. And so we can readily make inductive inferences from samples of the kind to any other sample. Thus if water is a natural kind, and we note that samples of water quench human thirst, or dissolve table salt, then we can infer the generalisation that *all* water quenches thirst, or *all* water dissolves table salt, and so on. It is part of our very idea of a natural kind that all samples of the kind share many features in common. So here is one way of overcoming the problem of induction. But it is overcome only on the assumption that the world comes divided into natural kinds, and this is something for which we need evidence. Induction would not work if the world is made up of things that are so disparate from one another that they do not form natural kinds and there is continuous variation amongst things.

A second way in which we could understand (3) is in terms of the collection of data and the derivation of a function which fits the data, within some degree of error. This is known as the *problem of curve fitting* in statistics.[2] Initially we must be able to sort out the observational data into that represented by an independent variable x and that represented by the dependent variable y (assuming there are only two variables to consider). Once this has been done then, by the procedures of curve fitting, we can discover a ‘best fit’ hypothesis linking them in the form of a function $y = f(x)$. This is one of the central aspects of scientific method that can be incorporated into NI. It is often overlooked by its critics who place too strong an emphasis on, for example, the hypothetico-deductive method and ignore the inductivist account of how one can arrive at generalizations by curve fitting. Clearly this is an important aspect of scientific method in which students of science should take in interest.

Though feature (4) in the Figure above is also not that specific, it does make a suggestion about what to do with the hypotheses, laws or theories arrived at using step (3). We can explain observational facts by making

deductive or inductive inferences to facts already known (see Section 2.5); or they can be used to make predictions about matters currently not known to be so.

Feature (3) is the most important phase of NI. To explore this a little further three case studies are given below. The first is the discovery of Boyle's Law. In science textbooks this is often presented as a case of NI in which data about the volume and pressure of a gas is collected. But such data collection was a theory-driven matter that does not support extreme versions of NI. The second case study concerns the attempt by researchers to find some factor relevant to the cause of 'cot death' in infants (SIDS, or sudden infant death syndrome). The third case study is one in which instruments, and not human observers, are used to detect some features, such as the level of concentration of ozone in the atmosphere above Antarctica. It was this that led to the discovery of the ozone "hole".

7.4 BOYLE'S LAW AND NAÏVE INDUCTION

Robert Boyle (1627-91) was the first to investigate the relationship between the pressure P of an enclosed amount of air and its volume V; he provides a classic example of the way in which the collection of data (viz., volumes of a gas at various pressures) can lead to a functional law (viz., Boyle's law PV = constant). However Boyle's procedures were not entirely those of a naïve inductivist. He did not suggest one day out of the blue to his co-worker Robert Hooke 'Let's see how pressure varies with volume!' and then collected data and inferred the law. Rather his experimental investigations were prompted by debates in the theory of pneumatics.

These debates were at the centre of physics in the seventeenth century and are an important episode in the study of the interaction between rival theories of the Earth's atmosphere and the collection of relevant evidence. It is singled out by James Conant (1957) as one of the crucial episodes in the history of science that should be studied by all science students. Here we make a few steps in that direction recommending Conant's fuller treatment as the basis of the construction of a classroom instruction module. The real debates are exciting, and the experimental evidence in Boyle's law fits into a much larger context not often recognised.[3]

Boyle had already argued in his *New Experiments, Physico-Mechanical, Touching the Spring of the Air and its Effects* (1660) that the air had both weight and 'spring' and knew that the spring of the air increased the more confined the volume of air. In the 1662 publication of this work Boyle added *A Defence of the Doctrine Touching the Spring and Weight of Air Against the Objections of Franciscus Linus wherewith the Objector's Funicular Hypothesis is also Examined*. His near contemporary, Franciscus Linus, agreed with Boyle that the air had spring but he did not always use its spring to explain some of its important features, particularly the vacuum created in

the Torricelli experiment. Since Linus was opposed to the very notion of a vacuum, he looked for a different explanation, not based on the spring of the air, for what can be observed in the Torricelli experiment. It is against this background that Boyle conducted his experiments by showing that the air retained its spring at pressures well above and below atmospheric pressure, a fact not well appreciated at the time.

In the experiment named after him, Evangelista Torricelli (1608-47) had filled a glass tube, somewhat over 75 cm long, with mercury. With fingers plugging each end of the full tube he inverted the column over a dish of mercury. Upon releasing his finger from the immersed end, the level of the mercury in the column fell to about 75 cm. He also reported that he felt a force sucking his finger into the space that appeared at the top of the tube when the mercury fell. What is our explanation of this phenomenon? We now claim, following Boyle, that the pressure of 75 cms column of mercury equals the pressure exerted by the atmosphere on the mercury in the open dish. The gap between our finger and the top of the mercury in the column is a vacuum; the force we feel is due to the absence of normal atmospheric pressure on our finger.

Linus had a different explanation. He postulated the existence of a *Funiculus* (from the Latin for 'rope' or 'cable'). This is a very rarefied substance allegedly pervading space; when extended it endeavours to contract itself. In Linus' view there is no vacuum at the top of the Torricelli apparatus. Rather there always remains the Funiculus; it acts to hold up the 75 cm column of mercury, not the downwards "push" of the outside atmosphere. Linus argued that the force we feel on our finger is due to the tension in the Finiculus which extends from our finger through the space at the top of the tube to the surface of the mercury. The force on our finger is due to the column of mercury which the Finiculus holds up! Boyle did many experiments on the Torricellian vacuum, including the effect of the absence of air resistance on a pendulum swinging in a Torricellian vacuum. Even though Boyle could not detect much difference, he notes wryly that the swinging of the pendulum, in whatever direction, was not impeded by Linus' cable-like Finiculus!

Boyle's measurements of the spring of the air (i.e., the pressure exerted) and its volume take place against his dispute with Linus about the existence of the Funiculus. He begins the presentation of his data by saying that Linus' hypothesis is needless since the spring of the air can, without the help of the Funiculus hypothesis, explain not only the phenomena of the Torricelli experiment, but also the ability of the air to hold back, albeit in a decreased volume, a wide range of pressures exerted by lengthy columns of mercury. By sucking air out of the longer arm of the J-shaped tube Boyle was also able to investigate the spring of the air under pressures less than that of one atmosphere. Boyle set out to demonstrate that air, as it occupies greater or

smaller volumes, displays less or greater spring, something which Linus' Funiculus is either not needed to explain, or cannot explain.

A selection of Boyle's data is given in the table in this section. Using a J-shaped tube closed at the shorter end and open at the longer end, Boyle was able to measure the volume of the air trapped in the shorter enclosed end by markings along the tube (assuming the tube was of constant diameter). When mercury was poured into the longer end the decreased volume of the trapped air could be measured. The mercury exerted pressure on the trapped air additional to that exerted by the atmosphere.

A	B	C	D	E	F
48	00	29 2/16	29 2/16	29 2/16	1398
40	06 3/16	"	35 5/16	35	1413
32	15 1/16	"	44 3/16	43 11/16	1414
24	29 11/16	"	58 13/16	58 2/8	1412
20	41 9/16	"	70 11/16	70	1414
16	58 2/16	"	87 14/16	87 3/8	1406
14	71 5/16	"	100 7/16	99 6/7	1406
12	88 7/16	"	117 9/16	116 4/8	1411

Boyle presents his data in several columns. In column A we find 'the number of equal spaces in the shorter leg, that contain the same parcel of air diversely extended'. In column B we find 'the height of the mercurial cylinder in the longer leg that compressed the air into these dimensions'. In column C there is the common factor of the pressure due to the atmosphere. In column D we find 'the aggregate of the last two columns B and C, exhibiting the pressure sustained by the included air'. In column E we find 'what the pressure should be according to the hypothesis that supposes the pressures and expansions (volumes) to be in reciprocal proportion'. An additional column F has been added; this is the product of the pressure given in D by the volume (in units of length of the glass tube) given in A. The data below is a selection of eight of the twenty-five sets of measurements and calculations Boyle reported. (See Boyle 1965, p. 160. Also see also Conant 1957, Chapter 1, for a discussion of, and extracts from, Boyle's pneumatic theories and experiments.)

Boyle did not, of course, use modern statistical techniques to arrive at the functional relation PV = constant. The obvious experimental outcome of increasing pressure and thereby decreasing the volume, suggests that there is an inverse relationship between pressure and volume; and this is born out by the data Boyle collected. In this respect Boyle's work does conform to feature (3) of NI. Most science textbook discussions of Boyle's work on pressure and volume commonly ignore the context in which its discovery took place, thereby suggesting that he is a naïve inductivist who 'hit' upon a

functional relation between pressure and volume. No mention is made of the rival pneumatic theory which Boyle set out to refute by investigating the spring of the air under various conditions. Boyle's very success has totally eclipsed the theory that gave impetus to his experimentation. But it is this false theory that played a role in guiding Boyle towards the collection and classification of particular observational facts.

7.5 THE STUDY OF COT DEATH AND NAÏVE INDUCTIVISM

Sudden Infant Death Syndrome, SIDS, or 'cot death', provides a case study which fits NI more closely than does Boyle's Law. SIDS is the sudden death of otherwise seemingly healthy young babies while asleep in their cots. It was noticed in the mid-1980s that in New Zealand there was a much higher incidence of SIDS than in other comparable countries; at its peak there were between 6 and 7 deaths per 1000 infants compared with much less than half that rate for most comparable countries. This was something that emerged from the fact collecting activities of epidemiologists in several countries. They collected data on overall infant deaths, and the known causes of such deaths. Statistical analysis of this data revealed a significant, but small, number of unexplained deaths that varied across countries that were otherwise similar in relevant respects. Several teams of investigators set about to discover why New Zealand was anomalous. Here it is data collection, along with statistical inferences from it that go beyond (but are of a piece with) the naïve induction so far discussed here, that is the core of the story. No theory is involved at all.

One issue that immediately presents itself is how SIDS was recognised in the first place. The recording of infant deaths from unknown causes is as old as that given in *Kings* in the *Bible*. What is more significant is the classification of infant deaths from seemingly unknown causes where there is a vast range of causes of deaths in children already known to medical science, such as congenital anomalies, infections, problems in the perinatal period, and so on. Hence the definition of SIDS along the following lines: the death of an infant between the ages of 28 days and one year which is unexpected by history and in which a thorough post-mortem examination fails to demonstrate an adequate cause of death. So defined, SIDS is a statistically identifiable phenomenon across many countries, races and social classes. So, why should it differ in similar countries?

What is of interest for NI is the manner in which factors were chosen which might be correlated with SIDS. Given that SIDS arises from unknown causes, there is a very wide range of possible factors to investigate in order to find out which are, and which are not, correlated with SIDS. This makes the hunt for causes a quite painstaking manner. Investigators drew up lists of possible factors that might be correlated and included them in questionnaires

for parents who had had a child that had died from SIDS. Some of these are included in the five groupings listed below.

(a) Physical factors could be considered such as: time of day (it was discovered that SIDS is more common at night than at day); the temperature of the day; the season; whether there were lights on or not; latitude and longitude. Concerning this last factor it was discovered that there was a significant correlation with this factor; SIDS increases from North to South in New Zealand with twice the rate in the South; in contrast in the USA there is an increase in risk from East to West but not North to South.

(b) Possible factors relating to the infant include: the clothing worn by the child and its tightness or otherwise; whether the child was on its back, side or stomach in the cot; whether the child slept with the parent(s); weight of the infant from birth until time of death; the food of the infant and its eating habits; age of the infant at death; sex of the infant; whether the infant was breast fed or not; the medical history of the infant from birth; whether starvation occurred while the foetus was in the womb; and so on.

(c) Factors concerning the mother might also be important such as: whether the mother (or both parents) smoked; whether the mother was married or not, or was a single parent; the age of the mother; the school-leaving age of the mother; whether addicted or alcoholic; the number of pregnancies of the mother; and so on. (It has been discovered that SIDS is more common where the mother starts having children before 20 and subsequently has a number of babies.)

(d) Factors relating to the birth might also be relevant such as: whether there were difficulties in the birth and how long the birth took; whether the child was premature and by how much; weight at birth; and so on.

(e) Socio-economic factors might also play a role such as: the marital status of the parent(s); their ethnicity; the quality of the housing including its building materials, etc.

Given the indefinite number of possible correlatable factors to investigate, it does appear that the SIDS researcher is initially in a position that is largely captured by NI. Feature (3) of NI clearly plays a role; statistical inferences need to be made from the masses of data collected from interviews of parents and doctors for each SIDS death for significant correlations to be discovered. What is of interest is whether Feature (1) is involved, viz., the collection of factors that might be correlated with SIDS in the absence of any selection process. Initially interviewers constructed lengthy questionnaires concerning any factor, plausible or not so plausible (such as latitude) which might possibly be involved in SIDS. After all it was quite possible that there was some factor linked to, say, the paint on the ceiling of the rooms of the infants that could be correlated with their death! Other factors were included in the list based on plausibility considerations; thus it had been suspected for some time that intra-uterine growth retardation is involved in SIDS. But other factors that were not known to be plausible had to be included in the list of

possible factors to investigate. It is this that makes the initial research into SIDS akin to Feature (1) of NI.

The first surprising discovery of the New Zealand research team was that infants sleeping prone (i.e., face down) were statistically significantly correlated with SIDS. It was common advice from infant specialists to recommend that children be put to sleep in the prone position. In fact the team discovered on the basis of their research that if the infant slept in a prone position, the mother smoked (in more than specified amounts) and did not breast feed then these three factors could account for about 79% of deaths from SIDS. Subsequently it has been discovered in several countries that not putting the infant in a prone position, along with the mother breast-feeding but not smoking, has considerably reduced the SIDS rate. These are not the only factors involved in SIDS. By investigation with microscopes it has been discovered that in the kidneys of 24 cot death babies there is a reduced number of nephrons, which are responsible for forming urine. This supports the view that there is some ante-natal factor involved in SIDS that concerns the development of the foetus in the womb. But again, like the factors already implicated in SIDS, it is not known what the causal links are.

What guides SIDS researchers in their choice of potential factors? The initial research into correlatable factors is not governed by any obvious prior sorting of observational facts; both plausible factors and those that are not initially plausible need to be considered in a thorough investigation into factors that might be correlated with SIDS. It is this that suggests that in some sciences, as in the case of SIDS, the initial investigation into potentially relevant correlatable factors proceeds with very little prior filtering of the factors that might be considered.

Such an episode in science could serve as a basis for instruction in the class. First the students could be asked about why the causes of SIDS are so important. Then they could be asked to consider relevant factors making up their own list of factors, and to make initial plausibility judgements as to whether they might be causally implicated. It would be advisable to initially keep the discovered solution from the class so that they realise what it would be like to look for real casual connections without knowing what they might be. Finally it would be useful to ask how the students would test for the causal factors. Such a case would be useful in getting students to realise the power of statistical methods in sorting out causally relevant factors from irrelevant factors.

7.6 THE DISCOVERY OF THE OZONE HOLE

NI can also apply to the collection of data using not just human observation but also data collected by instruments, such as remote sensing satellites, radio telescopes, various seismological detectors, and the like. Such sensing and data-collecting instruments are at the core of much modern science and fit

well with a suitably broadly understood NI. The example that we will briefly mention here concerns the use of instruments to detect levels of ozone in the upper stratosphere which ultimately led to the detection of the ozone "hole" above Antarctica. (Talk of a "hole" is simply a way of saying that the amount of ozone has been vastly reduced over Antarctica and in some regions there may be no ozone present at all.) In this section we draw on the excellent discussion in Christie (2001, especially Chapter 6) to which the reader is referred for more detail.

From the 1950s there has been a global interest in monitoring ozone levels around the world, not least of which had to do with the release of chemicals into the atmosphere from various sources from industry to high flying planes. At lower atmospheric levels ozone is a colourless but poisonous gas. At higher levels of the stratosphere (from 15km up to about 50km in altitude) it is an important shield against harmful ultra-violet radiation from the Sun. Here we will focus on the recording of ozone levels at the British Antarctic station at Halley Bay in Antarctica. A complete set of records of the daily stratospheric column of ozone above the bay from 1956 had been compiled.[4]

Ozone detection is commonly done through a machine known as the Dobson spectrophotometer, named after its inventor, G. M. B. Dobson. Also named after Dobson is the unit which measures the amount of ozone in a column in the stratosphere; these are called Dobson Units (DU). The normal amount is about 300 DU. If all the ozone in a normal column through the 35km of the stratosphere were to be compacted at 1 atmosphere and 0º C it would only come to about 3mm (or about 300DU). A small amount indeed, but an amount that has a vital function. It was recognised very early in the century that ozone filtered out much of the harmful ultra-violet rays reaching the Earth's surface from the Sun. This became another reason for monitoring the levels of ozone in the stratosphere; any drop in the level would be highly problematic for all animal and plant life on Earth.

Alas, this was what was detected at Halley Bay. A letter was published in *Nature* in 1985 reporting a significant drop in ozone levels (Farman *et. al*. 1985). Already in 1981 a drop had been noticed and the trend continued. However there was a backlog of unanalysed data from 1974 to 1980 which, when analysed in 1983, revealed a continuing drop in the amount of ozone from the mid-1970s. From the beginning of the observations in 1956 to 1974 there had been a similar pattern of ozone level for each year. Thus for the month of October over those years the amount of ozone averaged about 300 DU. But from about 1975 there was a noticeable variation in each October. This meant that at the beginning of each southern spring there were decreasing amounts of ozone present. The average amount of ozone for each October decreased from about 300 DU in October 1975 to about 200 DU in October 1985 – a decrease of a third (see Farman *op. cit.*, and Christie 2001

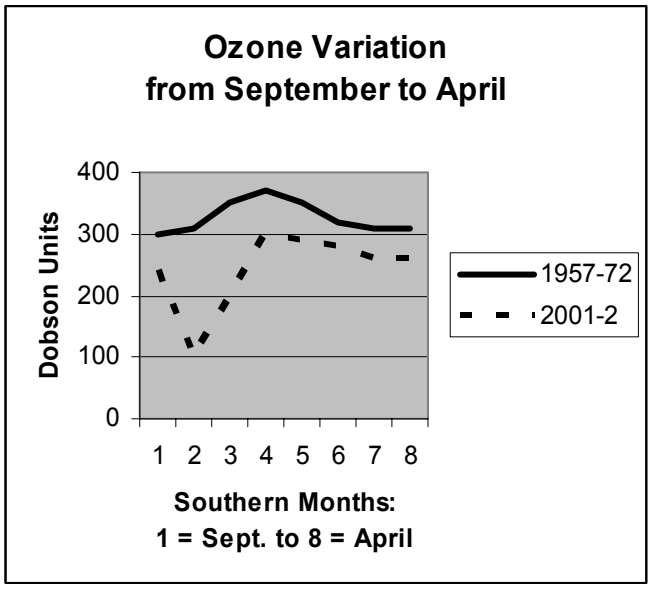
p. 48 for a useful graph). So by 1985 Farman and his team had the necessary data on the decrease ready for publication.

NASA had also been collecting data on ozone levels from weather satellites. Surprisingly they failed to detect the ozone loss. Christie (*loc. cit*.) has an interesting account as to why this was so. For our purposes it is necessary only to note that eventually the necessary data was collected from this source and it also showed ozone loss. The loss has continued up until now, sometimes 90% or more of the ozone being depleted over all of Antarctica and surrounding regions, including up to South America. We discussed in Section 6.1 some of the political ramifications of this loss. Here we only discuss the collection of the data using the methods of NI that lead to the discovery of the loss and its subsequent monitoring.

The diagram in this section sets out, in sketchy outline only, the data curve that can be plotted for each of the years 1957-72; this is represented by the top smooth line. The annual variation was much the same over the course of each of those years. From 1974 there is decreasing deviation with a dip beginning to occur in October. The dip becomes much more marked in subsequent Octobers. In the diagram only the later data from September 2001 to April 2002 is indicated (see bottom dotted line). The vertical axis represents the average daily column of ozone (in Dobson Units). The horizontal axis represents the southern spring to autumn months, September 2001 to April 2002 (each month is represented by a number). The drop in ozone over the spring, September to December, is dramatically represented with the big dip in October.

For ease of presentation the data for 2001-2 has been represented as a dotted curve. But this is not a true reflection of the actual data which is much more scattered about the dotted line. For a more correct account of the scatter the reader is referred to other sources.[5] This account is necessary for any classroom discussion to give a more complete over-view of the complexities involved in data collection, its scatter when presented graphically, and the attempt to construct an approximate curve.

There are several other ways of presenting the data collected by instruments. Importantly the data about the amounts of ozone has to be extracted from the overall data provided by the instrument by further analysis. Once extracted, the data illustrates one further way in which the methodology of NI is an integral part of science.



As we have seen in this chapter, the collection of data can concern a whole range of quite different phenomena, such as Boyle's pressure/volume variation, the incidence of infant cot death (SIDS) and ozone depletion. Such data collection illustrates an important aspect of science often neglected, or down-played in more theory-laden accounts of science. Once the data has been collected, a further task might be the discovery, using statistical techniques, of a functional relation between the variables in the form of an equation (to some degree of approximation of fit). This goes under a number of names, one of the common being *curve-fitting*.[6] The next task is to provide an explanation of the data, or the mathematical function. In the case of Boyle's data and the mathematical function that captures it, viz., Boyle's Law PV = constant, perhaps an underlying explanation is to be found in the kinetic theory of gases from which Boyle's Law can be deduced. In the case of SIDS the underlying explanation is to be found in the patient investigation of causal factors using further statistical techniques.

In the case of ozone depletion a number of possible explanations became readily available. In Christie (2001, Chapter 7) a number of possible explanatory hypotheses are canvassed. The first main hypothesis is that of Farman *et. al*.; the effect is due to increasing amounts of CFC's rising into

the stratosphere, an hypothesis that dates back to the 1974 paper of Molina and Rowland. The second hypothesis concerns possible patterns of air circulation in the Antarctica that create large regions with reduced ozone. The third hypothesis concerns the creation of various kinds of nitrous oxides high up in the stratosphere which are caused by particles emitted from the Sun in solar storms; these descend and interact with the ozone destroying it. Perhaps all of these describe some affect, but the first has been found to be the most directly responsible.

Here we leave NI to describe two other methodologies in the next two chapters. Often they have been contrasted with NI as if they were rivals; but this is wrong. There are hypotheses not obtained directly from data, but which are tested against data obtained by experiment, or evidential data collected according to NI. The first of these is called the hypothetico-deductive method and is the topic of the following Chapter 8. Chapter 9 looks at an even more general method of test within the context of probabilistic reasoning, in particular Bayesianism. Both methods rely on the collection of evidence, but they require more than just that.

NOTES

[1] Chapter 9 on probabilistic reasoning is, in part, a response to Hume' sceptical challenge, though we do not address this matter directly. Salmon (1967) is an excellent survey of responses to Hume's problem of induction and also spells out the nature of a probabilistic theory of inference that does provide some answer to Hume's sceptical problem. Gauch 2003, Chapter 7 also discusses related issues.

[2] For a further account of issues to do with curve-fitting, not discussed here, see Gauch 2003, Chapter 8, especially pp. 281-4. Most books on statistics carry an account of the method of least squares, and other methods of regression and curve-fitting, for finding hypotheses to fit a given body of data (and for varying degrees of accuracy of the data). Such methods are important in science for establishing hypotheses.

[3] This historical episode is also discussed in Matthews 1994, Chapter 4 pp. 60-70, and suggestions are made as to how this might be integrated into teaching in the classroom.

[4] A most striking set of graphs for most days in the years from 1957 to 2004 can be found on the web at http://chemistry.beloit.edu/Ozone/pdf/daily.pdf. What they show is the mean daily column of ozone (measured in Dobson Units) above the bay for all months of the year. They dramatically show the difference between what was initially observed each year from 1957 to 1972, in which the curves based on the data are very much the same over those years, and then the obvious collapse in the column mainly in the months from September to December from the 1980s onwards, with varying degrees of recovery, or lack of it, in other months of the year. Other diagrams of similar information appear in Christie 2001, Chapter 6.

[5] For sources see the website http://chemistry.beloit.edu/Ozone/pdf/daily.pdf, and many other related websites. See also Christie 2001, Chapter 6.

[6] The problem of curve-fitting is no simple matter and is an important part of statistical analysis of data. We do not discuss it here but refer to a useful introductory account in Gauch 2003, Chapter 8, especially pp. 281-4.

CHAPTER 8

HYPOTHETICO-DEDUCTIVISM AS A METHODOLOGY IN SCIENCE

The idea of hypothetico-deductivism (H-D) in science is an old one, its origins lying in Plato's dialogues where he talks of 'the method of hypotheses'. The role of H-D method became the centrepiece of a controversy in the nineteenth century about the nature of science between William Whewell who supported it and J. S. Mill who supported inductivism. It has been a pervasive model of many sciences. In contrast to naïve inductivism, which starts from observations and works its way towards laws or mathematical equations, the H-D method begins, as its name suggests, with hypotheses, or alternatively theories, laws, conjectures, hunches, speculative ideas, and the like. As the 'deductive' part of the name suggests, the hypotheses are tested by drawing consequences from them. This is necessary because in most cases the hypotheses are not open to direct test; they can only be tested indirectly by examining their consequences. Without something like the H-D method science would be poverty stricken if it could not get beyond what can be directly tested. The consequences are then compared with what evidence there is, obtained either from observation, experimentation or some naïve inductivist (NI) investigation. In the 'bottom-up' approach of NI, the search for observational fact is the driver. In the 'top-down' approach of the H-D method, the conjectured hypothesis is the driver. It leads one to look for specific kinds of observational fact, which might occur as part of the application conditions of the hypothesis, but more importantly will occur as evidence which tests the consequences drawn from the applied hypothesis. Unlike NI, the H-D method also makes it possible to test claims about unobservable entities; how this is so will become evident in the course of the chapter.

Once the H-D method has been set out in the first three sections some issues are raised about how confirmation of hypotheses are theories is to be achieved. This is not a straightforward matter, but it is touched upon in Sections 8.4 and 8.5. The H-D method is central to some conceptions of science. In Section 8.6 we discuss its importance for Popper's conception of science (though it is also important for many others such as Lakatos and Quine not discussed here). The material in this chapter is directed not so much towards teaching in the classroom but towards science educators and teachers; however it can illuminate what is done in the classroom. A grip on how theories can be tested is an important matter that needs to be introduced

gently to pupils. But it can be only done with teachers who are informed about what goes on in H-D testing and how it relates to the science they are teaching. In the final section we do give an example which can be useful in the classroom.

8.1 HUYGENS ON THE HYPOTHETICO-DEDUCTIVE METHOD

The Dutch scientist Christiaan Huygens (1629-95) in the 'Preface' to his *Treatise on Light* (1690) sets out one version of the H-D method as a preliminary to his use of it in his theory of light. Huygens was one of the first to adopt the wave theory of light. His basic theoretical hypothesis is that light is a multitude of very small wave-fronts advancing through space in the direction of the propagation of light. Being unobservable we cannot test directly for these wave-fronts and their properties, or use NI as a test procedure. Huygens' task is to show how his theory leads to a number of observable phenomena about light well known at his time; and it is these that can be tested. It is in the test consequences drawn from the theory that the H-D method can also meet the data provided by the NI method. Huygens wished to show that his theory could explain the well-known observable phenomena of reflection and refraction and the phenomenon of double refraction noted in the crystal commonly called Icelandic Spar. We will not go into the details of his theory. Rather we will look at his account of the H-D method in terms of which his theory and its results were presented. As he says in his 'Preface':

> There will be seen in it [i.e., the *Treatise*] demonstrations of those kinds which do not produce as great a certitude as those of Geometry, and which even differ much therefrom, since whereas the Geometers prove their Propositions by fixed and incontestable Principles, here the Principles are *verified by the conclusions to be drawn from them*; the nature of these things not allowing of this being done otherwise. It is always possible to attain thereby to a degree of probability which very often is scarcely less than complete proof. To wit, *when things which have been demonstrated by the Principles that have been assumed correspond perfectly to the phenomena which experiment has brought under observation; especially when there are a great number of them,* and further, principally, when one can imagine and foresee new phenomena which ought to follow from the hypotheses which one employs, and when one finds that therein the fact corresponds to our prevision. But if all these proofs of probability are met with in that which I propose to discuss, as it seems to me they are, this ought to be a very strong confirmation of the success of my inquiry; and it must be ill if the facts are not pretty much as I represent them (Huygens 1962, pp. vi-vii).

There are a number of important features of Huygens' advocacy of the H-D method. Both Euclidean Geometry and his theory of light proceed by deductions of theorems from axioms, or basic principles. However in the case of the geometrical axioms there is something evident, certain or incontestable about them; and this certainty is passed on to the theorems deduced from them. In contrast, there is nothing evident or certain about the

principles of his own wave theory of light; they are highly contestable and are yet to be proven. So how are the basic principles of his wave theory of light to be established?

For Huygens the only remaining possibility is by examining the deductive conclusions drawn from his wave theory. Huygens goes on to speak of *verifying* his theoretical principles. But this is too strong a claim. If we mean by 'verify' 'show to be true', then if the principles are perfectly universal in that they apply to all light everywhere at any time, they cannot be shown to be true at all. Being perfectly universal, there are an infinite number of deductive consequences of the theory of light to check out. No human, not even if all of us were to be collectively engaged, can inspect an infinite number of things; at best we can only inspect a finite number of things. No matter how many instances are in their favour, then, even if no counter-instance has been observed, we cannot show a perfectly universal generalisation to be true. This also follows from the nature of inductive inference. From a finite body of evidence we cannot prove deductively, and thus show true or verify, a conclusion which is quite general; at best we can only give *strong support* to the universal conclusion.

And this is what Huygens goes on to say. He drops talk of the verification of the theoretical principles of his theory of light; instead he talks of their *degree of probability*. Thus Huygens does not attempt to show that his principles are true, or verify them. Rather he wishes to show that they have some *degree of probability* on the basis of the evidence that he can collect on their behalf. We can also speak of his principles about the nature of light being *confirmed* (or *disconfirmed* as the case may be) by evidence. We will discuss the Bayesian method of science, which concerns probabilistic confirmation theory, in the next chapter. In fact some understand what Huygens says here as an instance of the Bayesian approach. Huygens, after all, wrote one of the first treatises on probability, something which he also applied to his theory of the nature of science as illustrated in his *Treatise on Light*. However what he says fits, to a large extent, the H-D method; and this is how his remarks will be viewed in this chapter.

8.2 A SIMPLE HYPOTHETICO-DEDUCTIVE SCHEMA

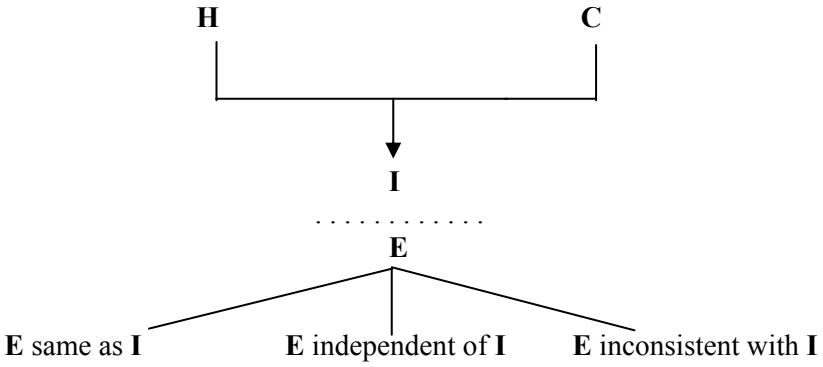
In this section we will consider, for the purposes of illustration, a simple account of the H-D method; in the next section a more sophisticated account will be presented.

In what follows let '**H**' stand for any hypothesis that is to be tested. The hypothesis can be a particular statement. For example, in medicine a hypothesis can be formed such as: **H** = patient A has lung cancer. If this is examined on the basis of the consequences it leads to, then it fits the H-D method. Or in a criminal investigation detectives may form the hypothesis; **H**

= suspect B knowingly acted as an agent to sell stolen property. It too can be H-D tested. More standardly hypothesis **H** is a generalisation, a law of nature, a set of laws, or an entire theory. These hypotheses will conform to the H-D method when they are tested through an examination of their consequences.

Let '**E**' stand for any piece of evidence with which **H** is to be compared. **E** can be a finite conjunction of observation statements, the minimum case being a single report of observation. Evidence **E** can arise in other ways. It can be generated by some other theory not under test, e.g., hypotheses in archaeology rely on evidence based on the theory of carbon dating, a theory which itself relies on yet other evidence in physics and the past history of the world's climate and vegetable and animal life, etc, for its testing. Here these other theories generate evidence for the testing of archaeological hypotheses. Importantly laws or mathematical equations obtained by NI can serve as evidence. We will give an example of this shortly. Thus what counts as evidence and what counts as hypothesis can be taken quite generally; any pair of statements can be regarded as evidence and hypothesis.

The diagram in this section sets out a general schema for a simple form of H-D testing. The top part of the diagram concerns the deductive part of the H-D method. **H** can be applied under special or initial conditions **C**; The conjunction **H&C** together deductively lead to the *test implication* **I**. Commonly **H** cannot be directly tested; rather it is **I** that is directly compared with evidence **E** (if **H** could be directly compared with **E**, the H-D method would be unnecessary).



In the bottom half of the diagram, we consider the possible relationships between evidence **E** and **I**. The H-D method tells us what specific evidence **E** can count for or against **I**; we look for **E** "in the light of" test implication **I**. There are three possible outcomes. **E** may be simply independent of **I**, so no test can be performed. But a test will arise when either **E** is equivalent to, or

entails, **I**; or **E** and **I** are inconsistent. That is, **I** either *passes* or *fails* in its test against **E**. We postpone until later what we can infer about the status of **H**, as this is not a straightforward matter.

To put some flesh on the bones of this schema a few illustrations will be useful.

8.2.1 Illustration 1: Newton's Laws and Galileo's Free Fall Law

In the first illustration we will use an example already set out in Section 2.5.1 of how Newton's laws can be used to explain a law obtained by NI. This is Galileo's empirically based discovery that the distance, s, a body travels in free fall is proportional to the square of the time, t, during which it falls, viz., $s \propto t^2$. Where does this proportionality come from?

It is now generally agreed that Galileo did experimentally determine that the distance a body travels in free fall is directly proportional to the square of the time. He obtained this law by using the NI method. Galileo announces this proportionality in his 1638 *Dialogues Concerning Two New Sciences*[1] and gives an account of the experiments in which he determined the proportionality.[2] One way to proceed was to drop a body on several different occasions from different heights and note the time of fall. While this direct method enables one to determine s accurately, in the absence of a good stop-watch it is difficult to determine the time t. All that Galileo possessed were water clocks in which one measures the amount of water that flows out in a given time. Also Galileo realised that a body which fell straight downwards in a laboratory would have a very short time of fall. So to make the time of fall longer, and so more accurately measurable, he rolled bronze balls down inclined smooth planes, sometimes even lined with parchment. He noted the different heights of the plane (inclined at different angles) from which the ball commenced to roll, and the time it took to descend to the bottom. In this way Galileo tells us that he was able to experimentally determine the proportionality $s \propto t^2$ within a satisfactory degree of error. It is in this obviously direct form of NI that Galileo obtained his free fall law.

Consider now the deduction set out in Section 2.5.1. It begins with Newton's inertial law, F = ma; this serves as the hypothesis **H** under test. It is employed in (2) of the H-D argument set out below. But it is tested under specific initial conditions, **C**, that of free fall close to the surface of the Earth (not the Moon or elsewhere). So a special case of the Law of Universal gravitation is invoked in (3) in which m is the mass of the ball and g is the acceleration due to gravity on the Earth's surface; this gives a specific value for the force acting on the ball. The rest is mere deduction from these two premises (hypothesis **H** applied under initial conditions **C**). And it leads to the test implication **I** indicated at step (9).

(1) A ball, initially at rest, is dropped from the top a building of height s.
(2) F=ma (Newton's second law – this is the hypothesis **H** under test)
(3) F=mg (the force exerted on m by gravity – an initial condition **C**)
(4) ma=mg (from (1) and (2))
(5) a=g (from (3))
(6) $a=d^2s/dt^2$ (definition of acceleration)
(7) $d^2s/dt^2 =g$ (from (4) and (5))
(8) ds/dt=gt (integrating (7) and using (1))
(9) Hence, $s=\frac{1}{2}g\ t^2$ (integrating (7) and using (1) – test implication **I**)

Now is the test implication the same as Galileo's proportionality obtained experimentally by NI? Yes, just drop the constant ½g and one arrives at Galileo's law.

This example illustrates a common procedure in science. First a uniformity, or a mathematical equation, is obtained experimentally by collecting data and then finding a generalisation, or a mathematical function, that best fits the data (using statistical methods). This is evidence **E** obtained by the method of NI. Then a range of hypotheses might be suggested as an explanation of the data; but sometimes there is just one such hypothesis available for test. By applying the hypothesis **H** in some special conditions it might be seen to fit **E**, or not fit, or fit **E** only to some degree. In this way **H** is subject to H-D test by **E**.

This story can be modified in several ways. First, it maybe the case that no evidence **E** is known prior to the investigation of **H**, and that **H** is simply applied in some circumstance leading to test implication **I**. This then suggests the kind of evidence that one ought to investigate to see if **I** is correct or not. Second, some assumptions may often be made along the way. Two assumptions are evident in the above. The first is that the free fall has to be close to the surface of the Earth; no variation is allowed in the force indicated in (3) due to the distance of separation of the ball and the (centre of gravity of) the Earth. The second is that there is no effect on the ball due to the drag of the air through which the ball falls. In Chapter 10 we discuss the issues raised here; they concern the use of models of varying degrees of idealisation in science. Also in the more sophisticated account of H-D method set out in the next section, allowance is made for suppositions about the model to which **H** might be applied.

Finally it should be noted that Newton's laws have a high degree of what we might call 'H-D power'. That is, there are many empirically determined functions that can serve as H-D test implications of Newton's laws. Thus Newton's laws pass H-D tests with respect to a number of free-fall laws where the body starts with an initial velocity; it passes tests with respect to Kepler's three laws of planetary motion; it passes tests with respect to the

motion of a pendulum; and so on for many such tests. And the same can be shown for hypotheses in electrodynamics, thermodynamics, light, and so on in many areas of the sciences other than physics such as chemistry, biology and economics. The H-D method is quite general in science.

8.2.2 Illustration 2: Qualitative Examples of H-D.

The above illustrates the way in which the methods of NI and H-D come together. But the H-D method can be used more widely than this. Commonly it is used to draw out a particular, or singular, test implication **I** rather than some empirically determined mathematical function. There are some classic examples of this that involve the H-D method. Here we will illustrate using only qualitative examples; quantitative examples often involve considerable mathematics and so are not used here.

In Section 2.5.1 we cite as an example of the covering law model of explanation, the decrease in height of a mercury barometer as one ascends a mountain. If we turn to the original historical context of this example it is really an illustration of the H-D method for testing a hypothesis, but in a qualitative rather than quantitative way. Originally Torricelli proposed the following hypothesis. Suppose that a long glass tube of about 80cms or more is closed at one end and filled with Mercury; then it is inverted over a dish of mercury. It is noted that the mercury in the tube falls to a height off about 75 cms. (This has already been described in Section 7.4). Torricelli's hypothesis to explain this is:

> **H** = the vertical column of air, from any point on the surface of the Earth up to the altitude at which the atmosphere ceases, exerts a pressure on mercury in the dish equal to the pressure exerted by the 75 cms of mercury.

How might one test **H**? It is impossible to do it directly. But it is possible to make an H-D test by applying it in some circumstance. And this is what Pascal proposed. Suppose one applies a test in different conditions, say, in conditions in which there is less air pressure, say at the top a mountain:

> **C** = if the Torricelli apparatus is transported up the slope of a mountain then the air pressure in the mercury in the dish is decreased.

From **H**&**C** follows a test implication:

> **I** = the level of mercury in the Torricelli apparatus falls.

What is evidence **E** that Pascal reported when the apparatus was taken up the mountain? The mercury level fell. **I** and **E** are thus equivalent.

So **H**, in conditions of application **C,** passes the test.

This result is not quantitative as it does not say what the amount of fall is. That would require a mathematical equation linking two variables. But it does illustrate the H-D confirmation of **H** in the absence of any direct test of it.

Science is replete with quantitative methods of H-D test. It would take us too long to go into the deductive details that many of them involve. For example, the deductive test of Einstein's General Theory of Relativity (**H**) which predicted that light would bend by a given amount as it passed the Sun, or any other large body (this is test implication **I** under conditions **C** of light passing the Sun).[3] Here it will be useful to review the example used in Section 8.1 that links to the long quotation from Huygens about the H-D method. Huygens can be understood to have used the H-D method to establish his wave theory of light. Huygens proposes a theory of light based on an analogy with the concentric ripples or waves which travel from the point at which a stone, when thrown, enters the water of a still pond. The transmission of light from a point source is just like the ripples travelling across the top of the water.

Huygens proposed a model of how each concentric wave is made up of a series of very small wavelets following one another in series. How might such a theory about the nature of light and its transmission be tested? There is no direct way. The only way is to examine its consequences. By applying his theory of advancing wave-fronts, and using some mathematical theory from geometry, Huygens was able to deduce the following: that refraction occurs (along with a quantitative account of the degree to which it occurs, when light travels from one medium to another, e.g., air to water); that reflection occurs and why the angles of incidence and refraction are the same: that the phenomenon of double refraction occurs, as observed in the case of Icelandic Spar; and so on. In this case his hypothesis **H** (or theory or model as we might also say) is one of a ray of light as a series of advancing little wavelets, supplemented with much theory from geometry. His evidence **E** is well known phenomena of reflection and refraction that any hypothesis ought to capture and thereby provide some supporting evidence. Huygens' wave theory is not the wave theory that we currently adopt, but it is an important precursor and provides a number of features still retained in some current wave theories.

8.3 A MORE SOPHISTICATED HYPOTHETICO-DEDUCTIVE SCHEMA

There is a more complex account of the H-D method that has the same logical structure as the simpler account; but it adds the following seven extra features that make it applicable to richer, and more real, cases in science.

(1) *The hypothesis, H, under test*. Sophisticated H-D allows for one or more laws, or a complex theory T comprising several laws, to be the test "hypothesis" **H**.

(2) *The model M to which the laws apply*. Often theories are applied to a model M that corresponds to a real system only to some extent. Thus

Newton's dynamical theory comprises his three laws of motion and the Law of Universal Gravitation, which we can list as the conjunction of the four laws: L_1, L_2, L_3, L_4. These are the hypotheses **H**. These laws can be applied to a variety of systems or models M, such as the solar system, a swinging pendulum, a harmonic oscillator, a gyrating top, the flight of a missile, and so on. All such models are, to some extent, *ideal* theoretical models that fit reality only to some degree of similarity. Thus a model of the solar system may only pertain to a few aspects of a bit of reality, such as, say, its dynamical aspects and not its colour aspects. And it may provide only a few of the real system's physical aspects (i.e., its dynamical but not its electromagnetic characteristics). The model can be simple as when the planets and Sun are treated as point-like bodies; or it can be made more concrete when the bodies are considered as massive, rotate on their axes and attract one another. All of these considerations are discussed further in Chapter 10.

(3*) The 'Sufficiently Isolated System', SIS, Condition*. Sometimes real systems do not exist in sufficient isolation from effects from outside the system and this is not reflected in the model. For example, even though objects in the solar system are under a dominant gravitational attractive force, there are other forces acting due to a number of extraneous factors such as stray objects pass through the solar system affecting it, explosions within the system, and so on. These will not be part of the model M of the solar system. Consequently one reason a test implication might not match the evidence is not only because the model does not fit adequately the real system but also because of the complex, sometimes random, interacting extraneous forces. It is hard to take into account all the stray effects that may arise in a real system into a model M, or a theory of the model. So the 'SIS' condition is a catch-all condition that requires that the system be sufficiently isolated from outside effects that the theory and the model make no attempt to capture; but nevertheless these are aspects of any real system that may well not exist in isolation from extraneous effects.

(4) *The initial conditions C*. Particular information needs to be added about the state of the system at a given time in order to deduce the state of the system at another time. Sometimes this is called a 'state description' or a set of 'initial conditions'. We will denote, as before, this additional information by 'C', the initial condition of model M.

(5) *Additional Assumptions of Theory B*. The H-D model needs to take into account any *additional theories* B that are assumed and must to be added to the premises, such as **H** and **C**, in order to arrive at the test implication **I**. There is no reason why additional theories cannot be assumed while hypothesis **H** is under test by evidence **E**. The test of **H** by **E** will then be relative to the acceptance of such additional theories **B**.

As an example consider the following. We wish to test some hypothesis concerning the drag caused by the atmosphere on, say, a swinging pendulum.

As additional theory **B** we will have to use Newton's theory of motion concerning the swing of the pendulum in the Earth's gravitational field. Though Newton's theory is required in order to deduce some testable consequences, it is not Newton's theory that is under test; it is the proposed hypothesis about the effects of atmospheric drag that is under test.

(6) *Any Theory D Employed in the Gathering of Evidence E.* When we gather evidence **E** we may do so, either with or without the help of some further theory. If evidence **E** can be gathered merely by observation then no theory will be involved. However theory is often involved when experimental apparatus is used to gather data. Call this *background experimental theory D*. As an illustration consider the following three cases.

(a) Thermometers are commonly used in the course of experiments to gather evidence. But the thermometer needs to be reliable, i.e., we wish to use only correctly calibrated thermometers. But such correct calibration in turn relies on a theory about the expansion of mercury in a confined space. Thus the theory of mercury expansion forms a background observational theory D for thermometers. In general, the use of any instrument to gather observational evidence will presuppose some background theory D.

(b) Another instance of the above is the use of telescopes in astronomy which rely on some background theory of the telescope, in particular some theory of optics. Not all such theory need impinge on the collection of observational data, e.g., data concerning the time and place at which a heavenly body is observed. For radio telescopes the matter is quite different; to get data about the time and place of a radio source on either a chart or a computer print-out, much theory concerning the workings of the radio telescope will have to be directly employed. Such theory may be relatively unproblematic; but nonetheless without it no test evidence **E** may be forthcoming.

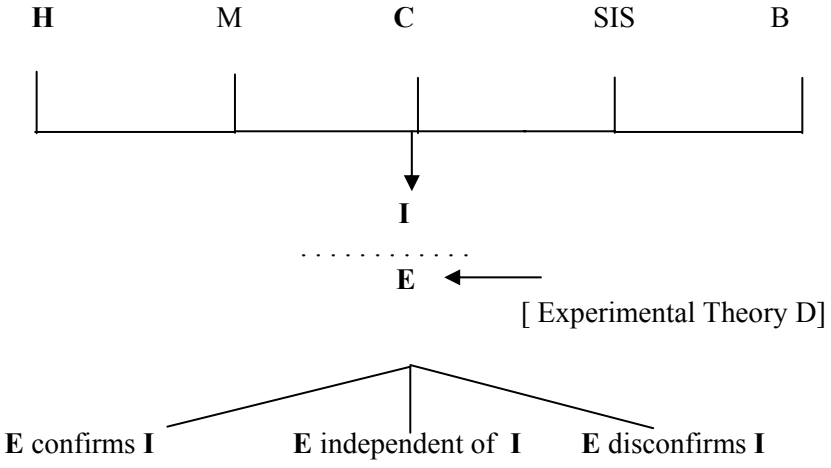
(c) In some cases the experimental background theory D used to generate observational evidence **E** may itself be problematic. Thus in archaeology we may want to test rival theories about whether some excavated site was occupied at the same or a different time as some other nearby site. The hypothesis under test may be of the kind: **H** = Site 1 was occupied before Site 2. One way of testing **H** would be to subject samples of organic material from the two sites to radio-carbon dating. In this case the theory of radio-carbon dating becomes a background theory D assumed true for the purposes of generating test evidence **E**. Archaeologists also make assumptions about the rate at which radio-carbon has been formed in the atmosphere and the rate at which it is absorbed. Any unusual production of radio-carbon in the Earth's atmosphere by, say, strong Sun flare activity during the year 3,500 BC, would increase the amount of radio-carbon at that time thus upsetting dating techniques based on the belief that there is a constant production of radio-active carbon. (For more on this see, for example, Renfrew 1976,

Chapters 2 to 4.) Thus the generation of test evidence **E** is itself dependent on the vagaries of theory and historical information.

Note that background theory D does not enter directly into the H-D test procedure; it is not involved in the deduction of the test implication **I**. But since it is involved in the generation of evidence **E** then, in the case of any conflict between test implication **I** and **E**, D may come under scrutiny as a possible cause of the conflict.

Finally we need to add the following logical precaution, viz., that **I** does not follow from M, C or B alone. That is, in the H-D method we must always include the hypothesis **H** under test in the premises from which **I** follows. In the H-D method, **H** must do some work and not be idle in the deduction of **I**; without **H**, **I** should not follow at all.

All of the above can be set out in the schema of this section. The main difference between the sophisticated and the simple H-D account is the addition of the extra factors from which the deduction of the test implication is made, and the addition of a theory from which experimental and, even observational, evidence may arise.



8.4 THE HYPOTHETICO-DEDUCTIVE METHOD AND TESTING

In the two previous sections we have set out a simple and a more sophisticated account of H-D testing. But how does the testing of Hypothesis H take place? So far we have spoken only of the relationship between the test implications **I** and the evidence **E**. But how does this relate to **H**, the very thing that we are trying to test? This is not a straightforward matter.

In what follows let **H**, as above, be the target hypothesis that we are trying to test. Let us call everything else, M, C, SIS and B the *auxiliary hypotheses*

A. They are auxiliary in that from **H** alone the test implication **I** cannot be deduced; but when some or all of the necessary auxiliaries are added and are conjoined as (**H&A**), then **I** can be deduced (symbolised as (**H&A**) →**I**)).

In some respects the H-D method is rather lame. It does not have a theory of confirmation or disconfirmation of **H** built into it. One has to be added. But at least we can say the following. If **I** and **E** are the same, or **E** entails **I**, then **H** has passed this test; but if **I** is inconsistent with **E** then **H** has failed a test. At best, the H-D method can only tell us if the hypotheses **H** under test has passed or failed the examination of a number of its test consequences $\mathbf{I_1}$, $\mathbf{I_2}$, $\mathbf{I_3}$, …, $\mathbf{I_n}$. In fact it is not always a simple matter to add some principles of confirmation to the H-D method. The following are some initial suggestions.

8.4.1 Passing a Test and Positive Confirmation.

If the test implication **I** is just the evidence **E**, and so **I** is a true consequence, then what we should strictly say is that the conjunction (**H&A**) has passed at least one test, and not **H** separately. If there are further test implications $\mathbf{I_1}$, $\mathbf{I_2}$, $\mathbf{I_3}$, …, $\mathbf{I_n}$ that fit the evidence, then (**H&A**) has passed more tests. We cannot say that **H** alone passes the tests; it is the conjunction (**H&A**) that passes. However if we have independent grounds for thinking that all of the **A** is true, as we often do, then it may be admissible to separate **H** and **A** and conclude that **H** by itself passes the tests.

Here there are two important principles of confirmation of which one ought to be aware, but which are not often stated in conjunction with the H-D method. Whether they are entirely correct is another matter (and for this reason some settle for the Bayesian methods to be discussed in the next chapter rather than adopt the following principles in an unsystematic way).

Confirmation Principle 1: if a hypothesis **H**, in conjunction with auxiliary assumptions such as **A**, has at least one true consequence **I** then **H** is confirmed (to some unspecified degree) by **I**, relative to **A**.

Confirmation Principle 2: if a conjunction such as (**H&A**) is confirmed, then each conjunct, **H**, and **A**, is separately confirmed.

(Some might have immediate qualms about the second of these. How can all the claims that make up **A** pick up confirmation so easily?)

Note that in the above we do not have any way of saying to what degree a hypothesis is confirmed. Nor can we make comparisons and say that it is better confirmed by evidence **E** than some other hypothesis. All we can make is the bare qualitative claim that the hypothesis is confirmed. And all this can mean is that it has passed at least one of its tests against evidence. This much should make clear that the H-D method needs to be further supplemented by yet other principles of qualitative confirmation. They do not normally come as an explicit part of the H-D method, but must be present. The same can be said of the associated principles of disconfirmation, which will be stated

next. (These are further reasons why one might adopt the Bayesianism of the next chapter instead.)

8.4.2 Failing Tests and Disconfirmation

Suppose the evidenced **E** counts against **I**. Then even if (**H&A**) have already passed several tests, the failure to past this test must count against (**H&A**) as whole. In fact (**H&A**) might pass several tests yet fail a number of others. It is reasonable to suppose that failing tests counts more heavily against (**H&A**) than passing tests counts in favour. In this case there is something wrong about (**H&A**) that needs to be investigated. Again there are some principles of disconfirmation to set out.

> *Disconfirmation Principle 1*: if a hypothesis **H**, in conjunction with auxiliaries **A**, has at least one false consequence **I** then (**H&A**) is disconfirmed (and not **H** or **A** separately).

Note that we do not disconfirm **H** directly, as the flow diagram shows. If we follow the diagram up from the false **I**, we reach a dividing point and do not know whether to go to the left toward **H**, or to the right, or go both ways. The problem could be with **A** and not **H**; and if the problem is with **A**, it could be that any one, two or all of M, C, SIS, B are wrong. But if we have independent grounds for being assured that all the **A** are unproblematic, then the disconfirmation can be directed at **H** alone. The problem is to find such independent grounds. Assuming that we do have them, we can endorse another principle:

> *Disconfirmation Principle 2*: if a conjunction such as (**H&A**) is disconfirmed, but we have good reason to accept **A** as true, then **H** has to bare the brunt of the disconfirmation.

This follows quite logically. However there is a difficulty that emerges much more starkly when we look at the more sophisticated account of the H-D method. It concerns what it known as the *Quine-Duhem thesis* which says: hypothesis **H** can never be sufficiently isolated from additional hypotheses such as **A** and so be exposed by itself to disconfirmation, or falsification. In other words, where there is a test failure all we can validly infer is that the conjunction (**H&A**) is false; but we cannot tell whether it is **H** or **A** that is responsible for the test failure.[4]

In fact the Quine-Duhem problem is wider than this. Suppose **H** is a complicated theory with many laws such as Newton's theory of motion and the Law of Universal Gravitation. The four laws can be represented as: L_1, L_2, L_3, L_4. Now the conjunction becomes not just (**H&A**) but the more complicated **({L_1&L_2&L_3&L_4}&A)**. Not only could the various components of **A** be at fault (or two or more of them), it might be that one or more of the component laws, L_1, L_2, L_3, L_4, are at fault. The problem is that we cannot

falsify one of these laws in the absence of the context of the other laws that might also be used.

There are two further important logical matters to note. The first is: when can we claim that **H** has been falsified, even if **H** can be sufficiently isolated? Is a single piece of counter-evidence **E** sufficient for the falsification of **H**? No. As Popper (1959, Chapter 3) argues, **E** could be stray observational claim that counts against some test implication **I**. We need to rule out alleged evidential claims that are stray happenings arising in unwanted ways, such as an artefact of observation, an instrumental malfunction, observational bias, random outside intervention in the experimental apparatus, faults in the theory of the experimental apparatus D, etc. That is, we need to ensure that **E** is a reliable observational outcome. How can this be assured? One way would be to ensure that **E** can be reliably repeated, that is the same instance of **E** can be replicated observationally or experimentally. Or that **E** is part of a sequence of observational claims that obey some regularity in the way they arise. And so on. So a necessary condition for the falsification of a theory is not a single piece of counter-evidence **E** but a reliable production of a kind of counter-evidence of which is **E** is but one sample.

The second logical point to note is that talk of passing tests and of confirmation is an appropriate way of circumventing The Fallacy of Affirming the Consequent. For, from a true test implication **I** (the consequent) we cannot validly infer that the premises from which it was derived are true. This involves the following fallacious pattern of inference well known in logic: From p infer q; q is true; so p is true. From the fact that a conclusion of an argument is true, it does not follow that the premises from which it came are also true; they could be false. As a simple illustration consider the following argument which has the same form as the valid Arguments 1 and 2 of Section 6.2:

All planets orbit the Earth;
The Moon is a planet;
So, the Moon orbits the Earth.

One can keep track of what has gone wrong here only if one observes the following distinctions: (1) whether the argument form is valid, or invalid; (2) whether the component statements are true, or false. Now the argument is guaranteed, by formal logic, to be valid. What this means is that if the premises are true then the conclusion must be true. It does not say the converse, viz., that if the conclusion is true then the premises must be true. In fact it leaves open the possibility that the conclusion is true while the premises are false. This is exactly the case in the example above. The conclusion is true, but the two premises are quite false – yet the argument is valid. This in fact reveals an important lesson in the philosophy of science: if

a theory has a true prediction we cannot infer that the theory from which it came is also true. This is to commit the Fallacy of Affirming the Consequent.

But doesn't a true conclusion (such as a true prediction) have some bearing on the hypothesis from which it came? Yes, but we cannot say that it shows the hypothesis to be true. What we might be able to say is that the hypothesis has passed one test, and this is a point in its favour. In fact we cannot show the hypothesis to be true because there are an infinite number of cases to investigate; at best we can only investigate a finite number of cases. This reiterates the point about verification in Section 8.1. We cannot verify general hypotheses if their domain is infinite; we can only do so if it is finite and humanly surveyable.

8.5 KINDS OF HYPOTHETICO-DEDUCTIVE TEST

Given that a theory yields testable consequences, do all tests count equally? The answer is 'No' and it is important to see why. In this section we will say a little about the way in which the H-D method has to be supplemented by three different kinds of confirmation that vary in their strength; this is something upon which the H-D method by itself is silent. This is a matter hinted at in the remark from Huygens given in Section 8.1 where he speaks of:

> things which have been demonstrated by the Principles that have been assumed correspond perfectly to the phenomena which experiment has brought under observation; especially when there are a great number of them, and further, principally, when one can imagine and foresee new phenomena which ought to follow from the hypotheses which one employs, and when one finds that therein the fact corresponds to our prevision.

As we will see, the kinds of distinction that Huygens draws between kinds of evidence have also been advocated by other methodologists of science.

Designer Evidence. When scientists propose a hypothesis **H** they might have in mind a range of observational facts or experimental laws $\mathbf{E_D}$ for which **H** was *specifically designed* to explain. We will subscript **E** by the letter 'D' to indicate that **E** is *designer evidence*, evidence on the basis of which **H** is specifically constructed or designed to explain. If **H** was specifically designed to explain $\mathbf{E_D}$ could $\mathbf{E_D}$ then count as evidence in favour of **H**? Many people would say 'No'. If evidential information is used to construct a theory then it cannot in turn be used again to test the theory; this is too cheap a way to generate support for a theory. Others might say that at best $\mathbf{E_D}$ could only weakly support **H**; after all **H** does accommodate some facts rather than none since it was designed to fit a range of them in the first place. Let us agree that at best the support that $\mathbf{E_D}$ gives to **H** is not very striking and is quite low. As an illustration consider the fact that Newton used Kepler's Area Law and his Harmonic Law to arrive at the Law of Universal Gravitation in Book III of the *Principia*. Once Kepler's two laws

had been so used, they could not be used again either to test the Law or as strong evidence for the Law.

Non-Designer but Known Evidence. Suppose that the scientists also know some other evidence that was not used in the design of **H**. After they have proposed **H** to account for $\mathbf{E_D}$, they discover later that when they apply **H** in a new situation it can account for this evidence. Call this evidence **'$\mathbf{E_K}$'**, the subscript 'K' indicating that it is evidence that was *known* by the scientists working on **H** but they did not use it in the design or construction of **H**. Would $\mathbf{E_K}$ count in favour of **H**? Many would say 'Yes', as does Huygens (Section 8.1). Evidence $\mathbf{E_K}$ was known but not used in the construction of **H**. It is a nice by-product of the newly invented hypothesis **H** that it can also account for independent evidence $\mathbf{E_K}$. As an example consider again Newton's Law of Universal Gravitation; it can be used to show that Galileo's law of free fall (distance fallen by a body subject to free fall is proportional to the square of the time fallen) holds true close to the Earth. This can be used as evidence in support of the Law of Universal Gravitation; the free fall law was known by Newton but not used in the derivation of the Law of Universal Gravitation.

Novel Evidence. Finally, suppose that much later the scientists think about a particular application of **H** in a quite new, or novel, situation. They note that **H** yields some test implication **I** that is not known to them. This is also envisaged by Huygens when he talks of 'prevision'. Call this a novel test implication (or prediction) $\mathbf{I_N}$, the subscript 'N' indicating that the prediction is *novel* or *'not-known'*. The scientists then hunt through the scientific literature and find that no one else has ever predicted $\mathbf{I_N}$. They then set up an experiment to see whether $\mathbf{I_N}$ is the case or not.

There are two possible outcomes for the novel evidence E_N that they uncover. They might be disappointed and discover that their theory has led them down the garden path to a novel prediction $\mathbf{I_N}$ that is false. $\mathbf{I_N}$ is contradicted by the novel evidence $\mathbf{E_N}$. This would be unfortunate for the theory. However they may be lucky and discover that the novel prediction $\mathbf{I_N}$ is true! $\mathbf{I_N}$ is just $\mathbf{E_N}$ (or entailed by $\mathbf{E_N}$). Evidence $\mathbf{E_N}$ is novel in the sense that it was not known before the theory suggested the novel prediction $\mathbf{I_N}$ and the subsequent experiment showed it to be correct. Does the novel piece of evidence $\mathbf{E_N}$ give support to hypothesis **H**? Most would say, as does Huygens, that it gives quite a confirmatory boost to **H**. As an example, once again consider the Law of Universal Gravitation; Newton used this to predict a novel fact, viz., that the Earth is an oblate sphere. It was shown to be about 28 kilometres further from its centre at the equator than at the poles (see *Principia* Book III, Propositions XVIII - XX). So the oblateness of the Earth is a novel prediction of Newton's theory that is correct and gives it a strong confirmatory boost (but we cannot say to what degree).

The above suggests that evidence can be of different kinds and have different weightings in support of **H**. These considerations must be added to the H-D method and are not something that arise from it. The different sorts of evidence and weightings with respect to a theory is set out in the table in this section.

All Evidence for Hypothesis **H**		
Evidence for **H** known at given time t		Evidence for **H** not known at time t
Designer evidence $\mathbf{E_D}$ (used at or before t)	Non-designer evidence ($\mathbf{E_K}$ known at t but not used at t)	Novel evidence $\mathbf{E_N}$ (known only after t)

The philosopher of science William Whewell (1794-1866) spoke of the different kinds of support that evidence can give a theory using the term 'consilience of inductions'. The word 'consilience' literally means 'a jumping together'. For Whewell different kinds of evidence might 'jump together' to give support to a theory when:

(1) a theory is supported by two or more different classes of phenomena (e.g., free-fall, the swinging of a pendulum, etc);
(2) a theory can be used to successfully predict cases of a *kind different* from those which were contemplated in the formation of the hypothesis' (this is $\mathbf{E_K}$ evidence);
(3) a theory successfully predicts or explains phenomena which, on the basis of our previous background knowledge, we would not have expected to occur (this is $\mathbf{E_N}$ evidence);
(4) a theory encompasses two or more different kinds of fact that were previously thought to be of a quite different kind.

In all these cases the evidence can give support to a hypothesis in varying degrees. But such considerations must be added to the H-D method and do not arise naturally from it. In this respect the Bayesianism of the next chapter is superior.

8.6 THE HYPOTHETICO-DEDUCTIVE METHOD AND POPPER'S PHILOSOPHY OF SCIENCE

Popper's philosophy of science proceeds on the basis of the H-D method and some quite simple but central points that flow from it. One of these points we have already addressed in the above, viz., his notion of falsification (see Section 8.4.2). We will consider only two further points. The first is his notion of the corroboration of a theory by evidence. The second is his idea of the demarcation of science from non-science in terms of the notion of falsifiability.

8.6.1 Corroboration

So far we have seen that theories are confirmed by evidence that follows deductively from them (with the help of auxiliaries). But the above shows that not all evidence will count equally. Evidence which is known but not used in the design of the theory will give good support to a theory; however, novel evidence will give striking support to a theory. Popper combines both of these notions in a measure of the degree to which a theory is, as he says, *corroborated* by the evidence. Popper deliberately used the term 'corroboration' since he wished to avoid the term 'confirmation'. This, the anti-inductivist Popper thought, was too closely allied to the notion of inductive or probabilistic support that a theory can receive from evidence. We need not go into Popper's anti-inductivism here. There are several ways of introducing Popper's notion of corroboration; the one given here is the simplest, introduced by means of the notion of a severe test.

Severity of Test. What does it mean to test a theory severely? Popper puts great emphasis on the idea of a severe test as opposed to those tests that involve evidence similar in kind to evidence already gathered for a theory. Consider the introduction of a new hypothesis **H** that is to be tested. Its introduction takes place against the background of older theories that **H** could displace if it passes the test. Let us denote some older theory, or background theory, with respect to which **H** is a new rival, by '**B**' and let **E** be some test evidence. (Note the use of '**B**' here is different from that in Section 8.3 where under (5) we spoke of additional background theories B used in an H-D test. These are not to be confused.)

Let us suppose that from the point of view of the new hypothesis **H**, **P** is an exciting novel prediction. (**P** is one of the test implications of **H** but is singled out because it is a prediction.) From the point of view of the older theory **B**, **P** is not even envisaged at all, or **P** is highly unlikely, or it is denied from the point of view of **B** that **P** could ever arise. We can say that with respect to the old theory **B**, **H** 'sticks its neck out' over **P**. **H** could be quite wrong about **P**; but it could be quite right. An example that might illustrate this is Einstein's prediction from his General Theory of Relativity (GTR – this is our hypothesis **H**). It makes prediction **P**: light which passes through a strong gravitational field, such as the Sun, would be bent. Given the prevailing wave theory of light at the time,[5] and the theory of optics, prediction **P** was not even envisaged, and might even have been thought to have been highly improbable given the view that light always travels in straight lines. So if it were to turn out that **P** were true, then GTR would be greatly boosted, given that it went against the prevailing wisdom of the background theories of the time. And this is what happened. The surprising prediction of GTR, surprising against the background of theories in the

absence of GTR, turned out to be correct. GTR got a confirmatory boost from its first radical prediction.

We can set this out a little more formally as follows. Let the expression 'prob(**E**, **H**)' stand for 'the probability of the evidence **E** given theory **H**'. This is sometimes understood as the *expectedness*, or the *likelihood*, of evidence **E** given **H**. It is quite distinct from prob**(H, E**) which is the probability of hypothesis **H** given evidence **E**.

Popper defines the *severity* of a test by comparing the likelihoods of the evidence **E** given both the new and the older background theories, viz., **H** and **B**:

> **E** is a *severe test* of **H** with respect to background theory B**,** or S(**E**, **H**, **B**), $=_{Defn}$ prob(**E**, **H**) is much greater than prob(**E**, **B**).

Consider the above illustration. Suppose that, as in the case of GTR, the theory actually entails the prediction **E** that light bends (to a given amount) when passing through the Sun's gravitational field. Given GTR logically entails **E**, then prob(**E**, GTR) = 1. In contrast given the background theories of the time, **B**, the prediction of **E** is highly unlikely. So prob(**E**, **B**) is low or close to zero, or actually zero. In the definition above, and if GTR is the theory whose severity of test is to be determined, we have the following values: prob(**E**, GTR) = 1, while prob(**E**, **B**) ≈ 0. Since 1 minus something close to 0 is still close to 1, then this is a very severe test.

If the notion of corroboration involves not just being subject to a severe test but also passing it, then GTR is corroborated by **E** (with respect to earlier background theory **B** before GTR was introduced). It would appear that Popper's notion of corroboration is not too distant from the notion of novel testing mentioned in the previous section. One difference is that Popper does not want to conclude from a theory's being corroborated that it is inductively confirmed in any way. Popper denies that science uses any kind of induction, so he denies that hypotheses can enjoy any *positive* support. According to Popper, corroboration of a hypothesis tells us only that we have failed to refute it; it does not mean that the hypothesis is confirmed or made more probable. This anti-inductivist position of Popper drew much criticism, but this is a separate matter which we are not going to discuss in this book.[6]

In the example above, GTR passes a severe test; in so doing it would have quite a large boost in evidential support. But if the prediction is false then the test is both severe and fatal for GTR. Sticking one's neck out against the prevailing wisdom of the time, the background theory **B**, and being wrong is no way to progress. Popper incorporates all of this into his idea of corroboration in which a theory passes a severe test. Such notions of severity of test and corroboration are part of the important ingredients that he adds to the idea of the H-D method in constructing his philosophy of science.

8.6.2 The Hypothetico-Deductive Method and Popper's Falsifiability Demarcation Criterion for Science

This section builds on the material in Section 6.3.1 on testability. Popper's notion of the demarcation of science flows quite naturally from the idea of the H-D method. Note that there is a big difference between the *falsification* of a theory, and the notion of the *falsifiability* of a theory to be discussed here. Falsifiability concerns the bare *possibility* of being falsified, not *actual* falsification. (This simple but important point is sometimes missed by postmodernist accounts of science. See, for instance, Chapter 12 on Lyotard.) Popper links the idea of bare falsifiability to his idea of demarcation. Instead we will employ the closely related notion of H-D testability.

What impressed Popper about the nature of science was its susceptibility to revision. This is something he observed in the sciences of his youth (the 1920s), especially when the best confirmed scientific theory of all time, Newtonian mechanics, was replaced by Einstein's theory of relativity and by Quantum mechanics. According to Popper, what made our theories susceptible to revision was the fact that they were open to test against observation and experiment; and it is this that makes it possible for them to either pass or fail their tests. Testability, we claimed in Section 6.3.1, is one of the intrinsic aims of science advocated by many philosophers, including Popper and Quine. Broadly, testability is the requirement that we need to bring any theory about the world into relation to what we can observe of the world directly, or by using instruments or by experiment. This is a necessary condition for our being able to test any theory against observations for their truth or falsity. If we cannot do this, then our theory would be untestable; it would remain in the realm of the speculative, or be quite hypothetical. And this would be so no matter whether the theory is independently true or false.

So how is this openness to test to be characterised? At this point enters Popper's idea of falsifiability which can be explained as follows. As we have noted, most conjectured hypotheses or theories cannot be tested unless they are applied in some situation by adding auxiliary hypotheses **A** (such as information about a model M or initial conditions C) in order to yield test implications. Often our conjectures remain in a limbo of untestability simply because there is no known means to supplement them, thereby deriving test implications from them.

Such was the case of the hypothesis, first proposed by the Ancient Greeks, that all matter was made of unobservable, indivisible particles called *atoms*. How is this claim to be tested, since atoms are not observable entities? The Greeks proposed this as a speculative hypothesis in answer to some purely philosophical problems. They had no way of showing this observationally or experimentally by means of the H-D method, or any other method. It was only in the late nineteenth and early twentieth century that the

hypothesis was applied to a specific theory of the nature of the atoms and some consequences were derived, which could then be tested against the evidence. For Popper the Ancient Greek hypothesis was untestable because there was no way in which it could be bought into relation with observations based in experience. It could be bought into logical relation to other philosophical ideas, and so evaluated with respect to them. But it could not be related to observational experience. This, in Popper's view, did not make it meaningless, as many positivists would say. Rather it fell outside the realm of science while still being a piece of meaningful speculative philosophy.

So what counts as falling within or without the realm of science? How might we set out the requirement of testability for theories in the light of the H-D method? What we require is that for any conjectured hypothesis **H** that there be at least some model M to which **H** can be applied, and that there be additional information C available about M, and some additional theories if needed, so that at least one test implication **I** can be deduced, which can be tested against observable evidence (the more test implications the better for the possibilities of testing). Clearly this requirement exploits the H-D schema. It does not matter whether **I** fits what we observe or conflicts with it. What is important is that there be some test implication **I** which can be bought into relation with a possible observational report that then either supports or undermines the test implication **I**.

This in a nutshell is the plausible core of Popper's position. We can spell it out as follows:

> Hypothesis **H** is *testable* $=_{\text{Defn}}$ there exists some possible application of **H** (usually involving a model M, initial conditions C, etc,) that entails that at least one test implication **I** follows from **H** so applied, and that **I** can be compared with some observational report O, whether actual or possible.

It follows that if there is no such test implication, **I**, that can be logically compared with a possible or actual observational report O, then **H** is untestable (but not necessarily meaningless or without an influence on science).

This is not quite the definition of 'testable' (or of 'empirical' or 'falsifiable' or 'scientific' – Popper often uses these terms interchangeably) that Popper gives in the *Logic of Scientific Discovery* §21, but it is very close to it. What Popper requires is that our theories divide all our reports about what we can observe into (i) those that **H** (when applied) permits, and (ii) those that **H** rules out. What is crucial for Popper is that there be *at least one* possible (not necessarily actual) observational report which **H** rules out, i.e., that there be at least one possible report that **H** does not permit. This is a weak requirement that the above definition does capture since it mentions a test implication **I** which has to come into logical relationship with a possible observational report.

Popper's demarcation criterion is often stated in terms of falsifiability, that is, in terms of the bare logical possibility of being falsified. Popper's reason for this is that in his view, since there are usually an infinity of test implications $\mathbf{I}_1$, $\mathbf{I}_2$, $\mathbf{I}_3$, …, then we cannot check all of these to show that the hypothesis **H** (or theory) under test is true. But it is possible for us to show that **H** is false; what is required here is not merely that there be a test implication that is inconsistent with an observational report but that there be a host of such reliably reproduced test implications. But this takes us into the realm of falsification. What we are considering here is a necessary condition for falsification; and this is that for some **I** there be a possible observational report with which it is inconsistent. The report does not have to be actual. In Popper's view all our theories are false (though some are less false than others). So there will be some test implication that does conflict with an actual report of observation, but we may not have found it, and may have some difficulty in finding it. In contrast if our hypothesis is true then there will be no actual observational report which conflicts with any test implication. But this does not mean that there are *no possible* reports of observation that conflict with **H**. If **H**, when applied in some situation, has test implication **I**, and **I** is the same as (or entails) observational report O (for example, 'the water in this beaker is at 12ºC') then there is a possible observational report that conflicts with **I** (for example, any contrary of O such as 'the water is at 87ºC'). And this is all that is needed for the definition above to work.[7]

The above definition shows that testability comes in degrees; some hypotheses or theories are more testable than others. The above definition requires that a hypothesis have at least one test implication. If it has just one its degree of testability is just above zero. An untestable hypothesis with no test implication has zero degree of testability. However all hypotheses and theories can, within the H-D method, be applied under a wide range of initial conditions C for the same model M yielding a wide range of test implications **I**. Further the hypothesis or theory can be applied to different models each with their own different ranges of initial conditions. And so the degree of testability goes up according to the range of models and initial conditions that can apply. Perhaps the test implications of a theory under the H-D method are infinite; in which case different methods need to be used to measure the degree of testability (a matter we need not enter into here).

So far we have only considered a version of Popper's definition for demarcating science from non-science. But Popper also proposes some rules of method that are to accompany his proposed demarcation criterion without which the definition would be rendered useless. Suppose a hypothesis **H** has a test implication which conflicts with what is observed. Then it is always open to a supporter of **H** to either add auxiliary statements to protect **H**

against possible falsification, or to alter the meanings of words in **H** to the same end. While Popper does not deny that these strategies should never be adopted, he imposes restrictions on their use. He proposes an anti-*ad hoc* rule which says: if some **H** has a false test implication, then adding auxiliary statement **A** is permissible only if the degree of testability of the new altered **H** and **A** taken together is increased and does not remain the same or is decreased. And a similar rule is proposed for semantic changes. Adopting such rules against *ad hoc* modifications has some plausibility in that we do want such changes to be independently testable. After all we do condemn some theories when we say that they are *ad hoc*, or are made to fit what we observe in an *ad hoc* way.

This, in outline, is Popper's proposals for demarcating science from non-science or pseudo-science. There is the core idea of a theory being bought into relation to possible observational reports about the world of which we can have experience, i.e., of being testable (which for Popper is understood to be falsifiable). And there are methodological prescriptions about how we are to behave towards our theories. They are to be maximally exposed to test and are not to be protected in illegitimate ways that minimize possibilities of test. These two core ideas of demarcation and methodology are at the heart of Popper's philosophy of science. Just how viable they are has been controversial, not least for a reason that Popper acknowledges, viz., that there can be rival theories of method, such as inductivism, conventionalism, and others, against which Popper has to defend his own theory. But we will not enter into these and other Popperian controversies here from Feyerabend, who is a critic of Popper's methodological rules (see Feyerabend 1975), to Popper's anti-inductivist critics (a matter not discussed here). What we have tried to present is the plausible core of Popper's ideas without necessarily adopting all of his philosophy of science.

8.7 APPLYING HYPOTHETICO-DEDUCTIVE METHOD IN THE CLASSROOM

So far we have discussed the H-D method, and related it to Popper's philosophy of science. In this section we present another example that will illustrate how the H-D method works. Although we have introduced many applications of the H-D method in this chapter, this example is based on such a simple experiment that it can be carried out in class (or in the laboratory) without difficulty. This example has several further advantages. First, it will enable us to demonstrate more concretely the connections between the key ideas of Popper and the H-D method. Second, since the experiment played an important historical role in the transition from the phlogiston theory to the oxygen based theory of combustion, it shows the relevance of the history of science to science teaching. Finally, it enables us to show, again more

concretely, not only how the H-D method embodies critical inquiry, but also the weaknesses of the constructivist pedagogy discussed in Section 5.7 *vis-à-vis* our approach that sees critical inquiry as the core aim of education.

Let us now describe the experiment, which is intended for middle school students. Children without exception are fascinated by the phenomenon of burning. They have observed it from early ages on and, indeed, it is something they have themselves probably done more than once. This experiment, which we have adopted from Boujaoude (1995), concerns the burning of a piece of steel wool, a kind of metal. The materials needed are a balance scale, a piece of steel wool, and a Bunsen burner. (Needless to say, certain safety measures, such as wearing goggles, clearing the demonstration area of all flammable materials and keeping a fire extinguisher nearby, should be taken before the experiment is done.)

Now, place pieces of steel wool on each side of the balance scale until they balance. Then ask students to guess what will happen when one piece of the steal wool is burned with the Bunsen burner. Most students will say that the balance will tilt toward the unheated side. Ask why they think so. They will probably say, on the basis of their previous experiences, that burnt substances (such as a piece of paper or wood) weigh less. Now, ask them to give an explanation for this. The typical explanation one gets is that stuff is driven off the burned substance. (Of course, this is vague because it does not indicate what "stuff" escapes upon burning, but that is all right for our purposes. The identification of that "stuff" is a too difficult question that we can expect middle school students to answer.) Let us call this explanatory hypothesis **H**. If true, **H** explains why substances such as wood and paper weigh less upon burning.

Then direct the flame of the Bunsen burner to the steel wool on one side only until the balance tilts in that direction. Students will be surprised because their expectation is violated. There is a problem that needs to be solved. This is a place to point out that, as Popper emphasized, violation of our expectations constitutes an important source of scientific problems. Ask students what the result of this experiment implies for their hypothesis **H**. Clearly, it conflicts with it. This can be expressed using the simple H-D method of Section 8.2, where

Hypothesis **H**: Stuff is driven off substances when they are burned;

Initial conditions **C**: Steel wool was placed on both sides of the balance scale until it was balanced; Steel wool on one side was burnt;

Test implication **I**: The balance will tilt in the other direction, indicating that the burnt piece of steel wool weighs less.

Evidence **E**: The balance tilted in the direction of the heated steel wool, indicating that it weighs more.

We can express all this schematically as follows, where the arrow indicates deductive entailment:

(H&C) → I

I is disconfirmed by **E**.

Therefore, (**H&C**) is also disconfirmed by **E** (**via** the application of the logical rule *Modus Tollens*). This means that either **H** is disconfirmed or **C** is disconfirmed.

But since **C** has been confirmed by the way the experiment was done, **H** is disconfirmed.

Given that H is disconfirmed and thus cannot explain why the burnt piece of steel wool weighs more, we need to develop a different explanatory hypothesis. At this stage, we can ask students what to do. Some might stick to their original idea, but revise it so that it escapes the disconfirmation:

H*: Stuff is driven off all substances except metals when burnt.

If this is what students come up with, we can point out, following Popper, that the empirical content of **H*** is reduced considerably and, therefore, that it is now less falsifiable. Furthermore, since the only motivation behind **H*** is to escape disconfirmation, the move from **H** to **H*** is *ad-hoc*. It is also worth bringing to the attention of students that **H*** still does not explain the evidence **E.**

Some students might drop **H** altogether and put forward an entirely new one. Clever ones may come up with either of the following:

$\mathbf{H_1}$: Stuff is absorbed by metals when burnt;

$\mathbf{H_2}$: Air is absorbed by metals when burnt;

$\mathbf{H_3}$: Air is absorbed by all substances when burnt.

The idea behind these new hypotheses is suggested by the evidence **E** gathered from the experiment above. Since the burnt steel wool is a kind of metal *and* has gained weight, metals (and, indeed, perhaps all substances) must absorb something from the environment. What could it be? Here background knowledge plays an important role. Students already know that we are surrounded by a sea of air, and that seems to be a good candidate, given the circumstances of the experiment (i.e., there seems to be nothing else around to absorb). So, if students come up with $\mathbf{H_1}$ only, they can easily be guided to $\mathbf{H_2}$ or $\mathbf{H_3}$ by appropriate questioning. That leaves students with $\mathbf{H_2}$ and $\mathbf{H_3}$.

At this stage, students may be asked the nature of evidence **E** with respect to $\mathbf{H_2}$ or $\mathbf{H_3}$. They are expected to note that **E** is designer evidence because $\mathbf{H_2}$ and $\mathbf{H_3}$ were designed (or constructed) to explain **E**. Students may also be asked to compare the two hypotheses and come up with a new test. Obviously, $\mathbf{H_3}$ has more empirical content and, thus is more falsifiable, than $\mathbf{H_2}$, so again following Popper's advice of "bold conjecturing", they may be led to entertain $\mathbf{H_3}$ rather than $\mathbf{H_2}$. Designing a new test for $\mathbf{H_3}$, however, may be more difficult (although testing the simpler hypothesis that air is required for burning is much easier; cover a burning candle with a glass

container and watch the flame go out after a while). There is the further question of why substances like wood and paper lose weight upon burning and whether this well known phenomenon is compatible with $\mathbf{H_3}$ or not, and if so, how? These are further food for thought for students.

The experiment discussed above provides teachers with a good opportunity to illustrate both the simple version of the H-D method and some of the important ideas of Popperian science: solving problems, bold conjectures, empirical content, falsifiability, and *ad-hoc* moves. It can also be used to show the relevance of the history of science for science education, kindle students' interest in it and give a sense of the difficulties involved in the process of discovery. As we know, the idea that "stuff " is driven out of the burning object constituted the core of phlogiston theory in the eighteenth century. Scientists named that "stuff" phlogiston and held that combustible substances were rich in it. Thus, they believed that heating drives off phlogiston into the air and cooling makes it less volatile. This commonsense idea explained many observed phenomena, including the fact that a burning candle enclosed by a container soon goes out. The explanation they gave was that 'the enclosed air gets saturated with phlogiston so that the phlogiston remaining in the wax has nowhere to go' (Giere 1991, p. 70).

In the 1770's Lavoisier, whom we consider today as the discoverer of the oxygen theory of combustion, carried out an experiment with mercury, similar to our experiment above, based on the investigations of Joseph Priestly, another famous scientist who was, however, an advocate of the phlogiston theory. Despite the fact that the result of the experiment with mercury conflicted with the phlogiston theory, the development of an alternative theory was not easy at all. There are many reasons for this, and we cannot do justice to this intricate story (see Kuhn 1970, Chapter VI). Combustion is a very complex phenomenon, during which many gases are released. Until the 1770's the composition of air was not known; the theory of elements and how they combined in various chemical reactions were not available until the next century. Indeed, it took Lavoisier several years to reach the conclusion that the gas released by heating the red oxide of mercury was a distinct species, i.e., oxygen, one of the major constituents of air. Priestly never accepted this conclusion and called it "dephlogisticated air", thus still adhering to the phlogiston theory.

The experiment with steel wool can also be used to display the weaknesses of constructivist pedagogy discussed in Section 5.7 *vis-à-vis* critical inquiry as the core aim of education that we defend in this book. Constructivist pedagogy correctly reminds us that each student comes to class with his own background set of beliefs, which should be taken into account by the teacher. Indeed, these beliefs do come up, as we saw, when students are asked to guess which side the balance will tilt. So, we agree with constructivist pedagogues that it is a good idea to ask them questions

that will bring out their preconceptions. Failure to do so will also mean to miss the opportunity to make a point about one important source of scientific problems.

Our agreement with constructivist teachers ends here, however. For they lose sight of the difference between right and wrong answers, naïvely expect their students to carry out "viable constructions" (in this case, concerning combustion) out of their experiences and "negotiate" them with others. The experiment above is an excellent example of a lesson we have learned from Galileo: scientific experiments and demonstrations often go against the common sense ideas we acquire through our daily experiences. Common sense tells us that many substances lose weight upon burning, so this leads to the expectation that the same will happen with steel wool. But it does not; the experimental result suggests just the opposite. The first thing students therefore must be encouraged to learn if we want them to become scientifically minded is to be cautious about relying too much on common sense. To be critical of such beliefs based on ordinary experience is the first step toward becoming a critical inquirer.

The second point is this. The H-D method is an effective way of finding out what is wrong with our initial beliefs. It enables us to subject them to critical scrutiny, and only those pass the severest scientific tests are worthy of acceptance. Thus, coming up with good tests of our pet beliefs or hypotheses is an important part of scientific inquiry, which is critical inquiry par excellence, as the H-D method makes abundantly clear.

Finally, without proper guidance through questioning and prompting, it is not easy to come up with such tests, nor is it any easier to hit upon explanatory hypotheses to be so tested to begin with. Is it not naïve to expect students to come up with experiments like the one above on the basis of their own experiences ("viable constructions?"), as constructivist teachers do? Furthermore, the scientific discussion between Priestly and Lavoisier concerning the interpretation of the experiment with mercury cannot be understood in terms of the constructivist notion of "negotiation". Oxygen theory and phlogiston theory are rival accounts of combustion, and one or the other must go. The talk of "negotiation" simply masks these facts. Informing students about some basic history of science enables them to appreciate the complexity of scientific research, to which one cannot do justice in terms of simplistic notions like "viable constructions" and "negotiations".

As we have suggested in the above, The H-D method highlights certain aspects of what goes on in science; but it has its limitations as well. We turn in the next chapter to a different theory of method that incorporates all the good aspects of the H-D method while avoiding some of its limitations. This is a probabilistic theory of method, or Bayesianism as it is sometimes called.

NOTES

[1] See Galileo 1954, Third Day, Theorem II, Proposition II, p. 174 (in pagination of National Edition, [209]). The entire section is entitled 'Naturally Accelerated Motion'.

[2] See Galileo, *op. cit.,* pp. 178-9 (in pagination of National Edition, [212-4])

[3] For fuller details see Will 1995, Chapter 4. There is also a long discussion in Will of another H-D case from Einstein, that of the deduction of the missing 43 arc/seconds per century in the perihelion of Mercury from Einsteinian relativity.

[4] Though we mention the Quine-Duhem thesis it does not mean that we endorse it. It has wide acceptance but is not without some critics who claim that it is not generally correct; see, for example, Grunbaum 1971. Also many Bayesians argue that there is a way around the problem the Quine-Duhem thesis raises. This is a matter we cannot discuss here.

[5] It is important to note that particle theorists of light could, and did, make this prediction over 100 years before Einstein made it. Will 1995, pp. 66-7 mentions three followers of Newton's corpuscular view of light who envisaged this possibility: Mitchell, Laplace and Soldner. For wave theorists the prediction of bending would have been a novelty.

[6] This is discussed in Stove (1982) and also Salmon 1967, p. 27 and p. 115.

[7] Here we pass over the objection, made by those who support the Quine-Duhem thesis, that it is not always clear that a theory does have sharp, or even any, falsifiability conditions. This is not an issue that we will enter into here. But if one does not put great store by the thesis (and we have mentioned some in Section 8.4.2 and in footnote 4 above), then this is not as serious an objection as it seems.

CHAPTER 9

BAYESIAN METHODOLOGY IN SCIENCE

The previous chapter noted shortcomings of the H-D method. No natural theory of confirmation flows easily from it and one has to be grafted on to it. It can only deal with one hypothesis at a time for testing. It is not able to readily compare a number of hypotheses to see how well they are doing with respect to evidence.[1] It has no way of taking into account the initial plausibility of the hypothesis under evaluation. It cannot easily accommodate statistical hypotheses for testing; though inferences can be made from them, it is not clear how evidence is to bear on them. Moreover, there is a large theory of statistical testing that seems to be by-passed by the H-D method. It offers no solution to the Quine-Duhem problem due to an incomplete account of confirmation. These, and other problems, have led methodologists to consider other methodologies.[2]

Is there a theory of method that overcomes these problems? There is! And it is one in which the good features of the H-D method come out as a special case. Moreover, it contains as a natural feature many of the plausible confirmation and other methodological principles that we added to the H-D method. This is the theory of Bayesian confirmation (named after one of the eighteenth century founders of probability, Thomas Bayes). This theory has had much discussion in the philosophy of science over the last several decades. Nevertheless, it is one that has made little impact on discussions within science education which has remained in the backwaters of the 1970s in the debates between Popper, Kuhn, Lakatos and Feyerabend. We will see that Bayesianism incorporates in a natural way many of their more plausible ideas, and much else as well. This chapter is intended as a brief primer to probabilistic thinking and Bayesianism within scientific method in order to lift science education out of its methodological "time-warp". It also suggests further research in this area not undertaken here.

In earlier Sections 1.2 and 2.2, we considered the ideas of belief, and degrees of belief. Building on this basis we introduce one of the main constraints that turns mere degree of belief into *rational* degree of belief. Degrees of belief are said to be *coherent* or *rational* if and only if the degrees of belief are distributed in accordance with the probability calculus, thereby enabling us to treat degrees of belief as probabilities. This is a central result that we will not prove, but it can be found in most books on Bayesianism (such as Earman (1992), Howson and Urbach (1993)). Here we wish to eschew the technical side of Bayesianism which can be formidably off-putting for those trying to approach the subject for the first time. We will

pass over technicalities in order to get at the core of this theory of method. Unfortunately not all technicalities can be by-passed; but they will be kept to a minimum.[3]

Section 9.1 sets out some of the basic principles of probability, as applied to belief, that leads to Bayes' Theorem. In Section 9.2 Bayes' Theorem is introduced in some of its various forms. Section 9.3 considers one of the characteristic rules of Bayesianism, the rule of conditionalization. This is in effect a rule about updating the probabilities of our hypotheses with the growth of evidence. It is this principle, along with others, that goes a long way towards realising a theory of rational probable belief that rivals theories of knowledge of the sort envisaged by Russell and discussed in Section 4.6. In Section 9.4 Bayesianism is applied to some of the principles of method mentioned in previous chapter.

Bayesianism captures, in a systematic way, some of the principles of method advocated by other methodologists. This is a theme further developed in Section 9.4 that shows how many of Kuhn's own methodological claims (made after he wrote *The Structure of Scientific Revolutions*) can fit into a Bayesian framework. Section 9.5 mentions some of the problems that confront Bayesianism and how they might be solved. It also introduces, but does not discuss in detail, vexing matters that arise concerning subjectivist, or extreme personalist Bayesianism, less subjective or tempered personalism, and objective Bayesianism.

In some cases it is useful to show that some of the claims made on behalf of Bayesianism can actually be proved. We will not mention any proofs here; the reader will be referred to some of the standard accounts of Bayesianism. We should also make it clear that we do not think that all is rosy in the Bayesian garden; it is not without its problems. (In fact neither author of this book is a committed Bayesian.) But these are part of the frontier of research into much current thinking about methodology which cannot be avoided and which needs to be extended to science education.

Section 9.6 draws out some important implications of Bayesianism for science education. So far as we know, this is a new area hitherto uninvestigated by science educators, and we realize that we have barely scratched the surface. We hope, therefore, others find our efforts to incorporate the Bayesian approach into science education worthwhile and give it the attention it deserves, an attention which we believe to be well overdue.

Two final points. Bayesianism provides a natural way of understanding one aspect of constructivist pedagogy. As will be seen, Bayesians, like constructivists, start with the beliefs of learners. But they also consider the degree of belief that the learners have for each particular belief they hold. Importantly there is a rationality requirement for learning on the basis of acquiring new information that leads them to revise their degrees of belief appropriately. This is not something emphasised in constructivism, but it is

something we have emphasised under the rubric of critical inquiry in learning. Though what Bayesians propose is something of an ideal model for learning, it is suggestive as to what goes on. The import of this will become clear in what follows. The second point is that there are several "interpretations" of probability discussed in most texts. These include the classical theory of probability, the frequency theory, theories of probability as a logic of partial entailment (largely due to Keynes and Carnap), theories of objective chance or propensity, and so on. The theory of rational subjective belief is also a further interpretation of the axioms of probability. Anyone interested in probability should also consider these theories. Since they are not germane to our task here, they will be omitted. We will focus exclusively on the interpretation of degrees of belief as probability.

9.1 THE PROBABILITY CALCULUS

9.1.1 Degrees of Belief and Probability

Here we will build on what has been established in Sections 1.2 and 2.2 about belief. In Section 2.2.2 we introduced the idea of beliefs that people can hold with varying degrees of strength. It is important to distinguish between (a) a psychological sense in which a person might have, at a given time, a degree of belief in some proposition p akin to a strength of commitment (high or low), from (b) a *rational* degree of belief that a person might have in p (which can also vary over time). Here our focus is entirely on (b) and not (a) at all.

In Section 2.2.2 we introduced the idea of an individual person's degree of belief in some proposition p. And we considered Ramsey's way of measuring that degree of belief in terms of their betting behaviour. We now develop an important aspect of Ramsey's theory that is the cornerstone of a theory of *rational* degrees of belief. Consider an individual person A (for Alice or Arthur) and some proposition p that A believes. (The reference to person A underlines that it is *personal degrees of belief* that we are considering.) Suppose A is waiting at the bus stop. Also suppose that p = the bus will come in five minutes and that A believes that p. Suppose we also examine the lowest odds A is willing to bet on this and it yields A's degree of belief of, say, ⅜. Now we examine the odds on which A is willing to bet on not-p = the bus will *not* come in five minutes. Suppose A's degree of belief turns out to be ⅞. Now most would find this peculiar. And so it is! Surely, you would say, that if A's degree of belief that the bus will come is ⅜, then A's degree of belief that it will not come *ought* to be 1 - ⅜ = ⅝. It cannot be ⅞! Or, if A's degree of belief that no bus will come is ⅞ then A's degree of belief that a bus will come *ought* to be ⅛ – it cannot be ⅜. Here the 'ought' is important. It suggests in this case a normative constraint, viz., that a person's degree of belief in p, and in not-p, ought always add up to 1, whatever the

value assigned to the person's degree of belief that p (or the value assigned to their degree of belief that not-p).

As a matter of fact people can and do bet on their beliefs in the peculiar way indicated, and so have degrees of belief that do not add to 1. But *ought* they? What is shown in the literature (but will not be argued here) is that if people do not bet in *coherent* or *rational* ways, then in bets about the behaviour of buses they will lose, or never win. What is meant by *coherent* or *rational* in this context? It is simply that *assignments of values to degrees of belief must be in accordance with the probability calculus*. This is an important normative constraint that has to be added to our degrees of belief to transform ordinary degrees of belief into *rational*, or *coherent*, degrees of belief. (This is a much-discussed matter in the literature not entered into here.[4])

If we grant the above, then it is possible to regard all personal degrees of belief as probabilities. Thus, at any given time, *A's degree of belief that* p simply becomes *the probability that p* (for person A at that time). Hence the importance of the above *coherence* or *rationality* constraint in getting us from degrees of belief to probabilities (or more strictly subjective probabilities since what we are considering are still degrees of belief of a person at a time). Once we consider rational degrees of belief that are in conformity with the probability calculus, then we can write 'prob(H)' and mean by this *the probability of hypothesis H*, that is, *the rational degree of belief that some person A has at a given time t*.

It is also quite natural to talk of *conditional* probability as in 'prob(H, E)'. This we can read as: 'the (conditional) probability of hypothesis H, given evidence E'. That is, we can speak of the (rational) degree of belief that a person has (at a time) in H given E. This is also called the *posterior* probability of H on E, the word 'posterior' indicating, quite literally, that it is the probability of H after, or following on from, the evidence E. Sometimes this is also called the *relative* probability, since it is relative to evidence. In the light of this we can say that 'prob(H)' is a *prior* probability, which is prior to, or before, any evidence. Alternatively we can say, in contrast to the word 'relative', that it is the *absolute* probability (or *unconditional* probability). The profusion of different pairs of terms should cause no confusion, since they are different, graphic ways of indicating the same difference. As will be seen, prior and posterior probability can be defined in terms of one another. The next important task is to introduce the axioms that probabilities obey, and, granted the above, that degrees of belief must obey if they are to be deemed rational or coherent.

There arises at this point a matter of importance for teachers. They might well discover that, for each of their pupils, there is a different degree of belief that each has in some hypothesis H, and a different degree of belief that each has in H, given old evidence E. What we need to consider is how the pupils react when they get new information, often in the form of new

evidence; in particular, we need to consider how each might adjust their degrees of belief in H. This is a matter which will be developed as we move through the following sections. It is possible for the teacher to consider how well each pupil behaves as a rational Bayesian! And how they might be corrected. In this respect we can agree with constructivists that a teacher needs to consider the starting point of each pupil, viz., their beliefs. But in addition, teachers also need to consider what is each pupil's initial degree of belief in H. Where we part company with constructivists is over what might be the rational way of proceeding from this starting point. There is a rational way of proceeding provided by the theory of Bayesian learning that is quite natural; we develop this in what follows.

9.1.2 The Axioms of Probability

The following are some of the relevant principles of probability that arise in scientific reasoning. Even though a small number of them are called axioms, they do not strictly represent a minimal set of axioms from which all the theorems of the probability calculus can be deduced. Rather, they are axioms in the sense that they provide a simple base from which it is easy to deduce theorems. Moreover, in stating the axioms it is convenient to use 'H' and 'E' to stand for any hypothesis and any evidence. We could replace these by any propositions 'p', 'q', etc, since the axioms are quite general. It is helpful to think in terms of hypotheses of any sort, whether in science or in police work or daily life, and any sort of evidence; this is the prime use to which they are to be put here.

Axiom 1: $0 \leq \text{prob}(H) \leq 1$

That is, the (prior or absolute) probability of a proposition, such as H, lies between 0 and 1 (including 0 and 1).

If we are dealing with conditional (posterior or relative) probabilities then this axiom can be easily transformed into: $0 \leq \text{prob}(H, E) \leq 1$.

This axiom tells us that probabilities can be assigned a unique real number between 0 and 1, including the end points. This accords with our ordinary use as when we say that the probability that it will rain today is 0.3 (i.e., prob(rain today) = 0.3), or the probability that the stock market will go down is 90%, or the prob(I am reading a book now) = 1 (since this is something each of us knows of one's self).

Axiom 2: prob(Taut.) = 1.

That is, where the proposition H is any tautology or logical truth *Taut*., then its probability is 1. This fixes the upper bound for probabilities. Given other axioms mentioned below it is easy to show from this that prob(Contrad.) = 0, where 'Contrad.' is any contradictory proposition.

Here it is convenient to mention the upper and lower bound cases of conditional probability. Thus consider prob(H, E) = 1. This will be the case just when the evidence E logically entails the hypothesis (or H is a logical

consequence of E). That is, we can establish the following important theorem (which is proved in textbooks from axioms):

Entailment Theorem: p entails q if and only if prob(q, p) = 1.

This theorem says that for any two statements p and q, if p entails q, then the conditional probability of q given p is equal to 1. Since the theorem holds for any two statements, it also holds for the case where evidence E entails a hypothesis H. In other words, E entails H if and only if prob (H, E) = 1. Though it is not common for evidence to entail hypotheses, sometimes it does. So we can say that the probability of the hypothesis given the evidence is 1. More commonly, H will entail E. That is, E will be logically deducible from H. Then prob (E, H) will be 1. This is a situation that arises in the case of the H-D method; a hypothesis, along with whatever auxiliaries A are needed, entails some evidence, prob(E, (H&A)) = 1. The significance of this will emerge later when we consider Bayes' Theorem (see Section 9.2.1).

Closely related to this is the following principle that tells us what happens when the evidence contradicts the hypothesis. In what follows we will use the negation sign '¬' in which '¬p' is to be read as 'not-p'.

If E entails ¬H then prob(¬H, E) = 1; or prob(H, E) = 0.

That is, where the evidence contradicts the hypothesis then its probability on the evidence is zero. This is a quite intuitive idea that is captured in the probability considerations being introduced.

Here we will help ourselves to more symbols from logic and write the following. Where two propositions 'p', 'q' are joined by '&' the symbol for and, or conjunction' as in '(p&q)', then we are to read this as 'p and q'. Again, where the two propositions are joined by 'v' (which is taken from the Latin 'vel' for 'either … or …'), as in '(pvq)', then this is indicates disjunction and is to be read as 'either p or q'. Now the following axioms can be stated.

Axiom 3 (or Addition Rule):

$prob(H_1 v H_2) = prob(H_1) + prob(H_2)$ (providing (H_1 and H_2) are contradictory, i.e., they mutually exclude one another).

As a special case of this axiom, consider the two hypotheses, H and ¬H, which surely satisfy the proviso of being mutually exclusive of one another. Then we have:

$prob(H \vee \neg H) = prob(H) + prob(\neg H)$

Given the above theorems, it can be shown that both sides of the equation are equal to 1. This is an important result when considering two rival hypotheses such as H and its negation ¬H. Also following from this we can show:

$prob(H) = 1 - prob(\neg H)$

That is, as one hypothesis goes up or down in value, its negation goes down or up. Note that in Section 9.1.1 we have already considered examples of violations of this principle that lead to incoherent assignments of probabilities.

Using conditional probability we can express a version of Axiom 3 as:

prob([H_1 v H_2], E) = prob(H_1, E) + prob(H_2, E) (providing E entails not both (H_1 and H_2); i.e., {E, H_1, H_2} are a contradictory set).

And we can similarly show:

prob([H v ¬H], E) = prob(H, E) + prob(¬H, E) = 1.

In the same manner, as the probability of H goes up or down on evidence E, so the probability of its negation ¬H goes down or up on evidence E.

These are often called the *Special Addition Rule* because of the proviso given at the end of each. If we drop the proviso then we have the *General Addition Rule* that says, in each case:

prob(H_1 v H_2) = prob(H_1) + prob(H_2) – prob(H_1 & H_2).

In the case of conditional probabilities we can stay with the restricted version since what we are interested in is evidence that is to adjudicate between an exhaustive and mutually inconsistent set of hypotheses, such as the sets {H_1, ¬H_1}, {H_1, H_2, ¬(H_1& H_2)}, and so on. In the second of these there are two hypotheses H_1, and H_2 that are being considered. There is no guarantee that they exhaust all the logical possibilities of hypotheses, and no guarantee that they are inconsistent rivals of one another. But if this is the case, then perhaps the correct hypothesis is neither of H_1 or H_2; and this is what the third member of the set indicates, viz., ¬(H_1& H_2). This is often called the *catch-all* hypothesis. It is not a positive hypothesis like H_1 or H_2; rather it simply states that some third hypothesis which is neither H_1 nor H_2 is the correct hypotheses – though we might not know what the third hypothesis or how to express it. If the axioms and principles being set out are to apply to H_1 and H_2, then we need a way of guaranteeing that we are considering an exhaustive and exclusive set of hypotheses. By including the *catch-all* hypothesis we then have a way of dealing with the problem of obtaining a set of exhaustive and mutually exhaustive hypotheses, and so satisfying the conditions of the axioms and principles.

Theorem: prob(H) + prob(¬H) = 1.

This expresses the idea that the probability of a pair of contradictory propositions, such as H and ¬H, must add to 1. This tells us that if the (absolute) probability of A is, say, high, then the (absolute) probability of its negation must be low. And this accords with our intuitions on the matter. Expressed as a conditional probability the axiom can be transformed into:

prob(H, E) + prob(¬H, E) = 1.

That is, if the probability of H given E is, say, high, then the probability of ¬H given E must be low. And this is as would be expected, since evidence that counts in favour of a hypothesis should count against its rival negation.

Now we can see the force of the rationality requirement on degrees of belief. Here is another illustration. Suppose I am in two minds as to what the weather will be like. I think both prob(rain today) = 0.7 and I think prob(no rain today) = 0.6. Now such an assignment of probabilities is incoherent. If a bookie were to take bets with me and I use these probabilities to arrive at my

betting odds, then I would come out a loser. In this example I have violated the requirement that the assignment of probabilities should, for rationality, be in accord with the probability calculus. If I assign prob(rain today) = 0.7 then I should assign prob(no rain today) = 0.3; and conversely if I reckon prob(no rain today) = 0.6, then I should assign prob(rain today) = 0.4. Either way my probabilities are coherent. Whether empirically I am right in the assignment of 0.7 to prob(rain today) is an empirical matter upon which the calculus of probability we are exploring cannot adjudicate directly. Its only concern is that the values of the probabilities be distributed over one's beliefs in accordance with the probability calculus.

Equivalence Axiom:

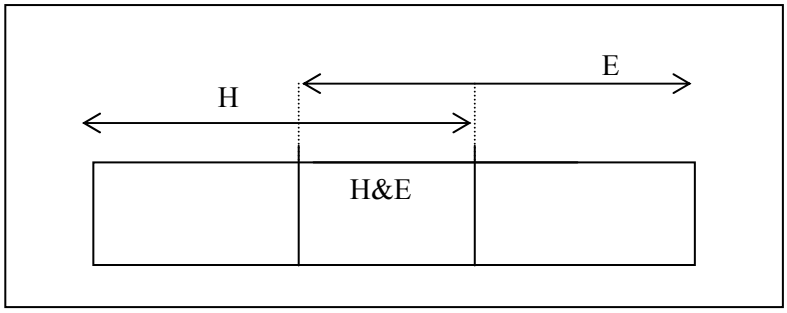
This says that for any two propositions that are logically equivalent, then their prior probabilities are the same:

If p is logically equivalent to q (symbolically {p≡q}), then prob(p) = prob(q). Similarly for conditional probability where $H_1 \equiv H_2$, and $E_1 \equiv E_2$, then prob(H_1, E_1) = prob(H_2, E_2).

Definition of Conditional Probability

$$prob(H, E) = \frac{prob(H \,\&\, E)}{prob(E)}$$

The rationale behind this definition might best be seen with the help of the diagram in this section.



Consider a rectangle that contains the entire logical space of propositions, and let the area of two inner rectangles H and E be proportional to the prior probabilities of H and E. Note also the overlap area of H and E. Suppose that one throws a dart at such a figure and hits the evidence rectangle E. Then we can ask: what is the probability that the dart also hits the hypothesis rectangle H? Note that it could miss the H rectangle entirely in hitting the E rectangle (it would land on the right-hand portion of the E rectangle). But it can also fall in the area of intersection of H and E rectangles, that is, H&E. So, what is the probability that, given that the dart hits E, that it also hits H? That is, we are asking what is the probability of H given E, i.e., prob(H, E). Looking at the diagram we can work this out. It is the intersection area (H&E) over the

total area of E. Since the areas are proportional to probabilities we can say that it is given by the ratio of prob(H&E)/prob(E). And this provides a useful diagrammatic illustration of the definition.

Axiom 4 (or Multiplication Rule)

prob(H&E) = prob(H, E) x prob(E)

We mention this axiom only for the sake of completeness and will not pursue it further – except for one important purpose. It leads directly to a form of Bayes' Theorem!

The proof is simple, and depends also on the Equivalence Axiom and the Definition of Conditional Probability. We begin the proof as follows:

(1) prob(H&E) = prob(E&H) [by Equivalence Axiom]

(2) prob(H, E) x prob(E) = prob(E, H) x prob(H)

[This is obtained by two substitutions in (1), one for the left and one for the right, of Axiom 4, observing carefully the order of H and E.]

By suitable rearrangement we get the first simple form of Bayes' Theorem:

(3) prob(H, E) = prob(E, H) x prob(H)/prob(E), providing prob(E) ≠ 0.

What all this means is the topic of the next section.

9.2 BAYES' THEOREM IN ITS VARIOUS FORMS

9.2.1 Versions of Bayes' Theorem

Bayes' Theorem comes in many forms, some more simple than others. Here we will consider only three versions, revealing different applications of the Theorem. The first version is based directly on the above Axiom 4 and is the simplest form of Bayes' Theorem.

Bayes' Theorem Version 1:

(Eq. I) $$prob(H,E) = \frac{prob(E,H)xprob(H)}{prob(E)}$$ where prob(E) ≠ 0

What does this mean? Basically it tells us something we want to know, viz., the probability of a hypothesis H given evidence E. This is the left-hand side of the equation. And it tells us that it is equivalent to the right-hand side. This has three probabilities in it. If we know the value of these then we can work out what we want to know, viz., prob(H, E). Do we ever have the right-hand side values? Sometimes, and that is helpful. But often we do not, or have only some of them. Still we can learn a lot, as we will see.

The expression on the left-hand side is the probability of a hypothesis H given some evidence E. It is called the *posterior probability*, posterior since is depends on some evidence being given. This is often just what we want to know.

We are also given three expressions on the right-hand side. The expression *prob(E, H)* is called the *likelihood*. It tells us about the probability of the *evidence* given the *hypothesis* (and not the other way around as when

we talk of the probability of hypothesis given the evidence). Now note a special case of the likelihood when it is 1. By the **Entailment Theorem**, if H entails E, then the probability of E given H must be 1. Consider the H-D method in the light of this. Let the hypothesis H&A stand for everything in the H-D method (from the hypothesis under test, initial conditions, additional theories assumed, etc, as in Section 8.3)) which entails some test implication identical to evidence E. Then, by the **Entailment Theorem**, prob(E, H&A) = 1. Thus the H-D method is subsumable as a special case of Bayes' Theorem. Bayes' Theorem also tells us something that the H-D method could not, viz., the probabilistic support that hypotheses can get from evidence. Hence the greater power and generality of the Bayesian approach over that of the H-D method.

The expression *prob(H)* is the *prior* probability of H, that is, prior to any evidence. It is also called the initial or absolute probability of H. How is this to be worked out? In general its value is not readily available and radical subjectivist Bayesians allow each person to assign whatever value they wish; more objective Bayesians resist this and try to find some objective ground for assigning a value to it. Some introduce plausibility considerations saying that the prior probability of H is a measure of the initial plausibility that a hypothesis has before evidence comes in. Different people might have different assessments of plausibility, but as the evidence comes in they will have to adjust their probabilities according to Bayes' Theorem. After a given amount of evidence they might assign very similar posterior probabilities. That is, there is convergence even though their prior assessments were very different. This is one of the matters central to a more developed version of Bayesianism, some features of which we will consider in later sections.

Finally the expression *prob(E)* is called the initial or prior probability of the evidence E. Sometimes it is called the *expectedness* of the evidence.

Putting all of this together, the first simple form of Bayes' Theorem says: The posterior probability of H given E = the likelihood of E on H, multiplied by the initial probability of H, and all divided by the initial probability of the evidence.

Bayes' Theorem Version 2:

(Eq. II) $$prob(H, E\ \&\ B) = \frac{prob(E, H\ \&\ B) x prob(H, B)}{prob(E, B)}$$

where prob(E, B) ≠ 0.

Here we have simply added to Version 1 an expression 'B' which we may call background information. If we think of this background information as old evidence and E as new evidence, then we have a way of assessing the probability of H on the total evidence, old and new. It is given by the expression on the right-hand side. Suppose we think of this in the context of the H-D method and put prob(E, H&B) = 1, i.e., the probability of new

evidence E given H and the old evidence B is 1. Then the expression simplifies to:

(Eq. III) prob(H, E&B) = prob(H, B)/prob(E, B).

This result has interesting implications. We can ignore the expression prob(H, B) since this is fixed as the posterior probability of H on the old evidence. So what happens when the new evidence E comes into consideration? Focus on the denominator expression prob(E, B), the expectedness. This is the probability of the new evidence given the old evidence. If the new evidence E is just a repetition of the old evidence, then prob(E, B) ≈ 1. So the value of the probability of H on the new and the old evidence has not moved much above what the probability of H was on just the old evidence. But if the new evidence E is genuinely new in that it is a new kind of evidence, or a new variety of evidence when compared with old evidence B, then we can expect prob(E, B) to be low. And the effect of this value, since it is in the denominator, is to push up the value of the probability of H on the new and the old evidence to move well above what it was on just the old evidence B. This explains the value of different and varied evidence, something that one would have intuitively agreed to. What is of interest here is that this falls quite naturally out of Bayes' Theorem, a point in its favour.

There is a nice historical case that illustrates this point. In the early 19th century, a young French scientist called Fresnel developed a wave theory of light. The famous French mathematician and physicist Poisson predicted on the basis of that theory that if an opaque disk is illuminated suitably from one side then, in the shadow it casts on a screen on the opposite side, a bright spot would appear in the middle of the shadow. With respect to all the background information available at the time, this was a totally unexpected and therefore a very surprising prediction. When the experiment was carried out, a bright spot was indeed observed, as predicted. Now this is genuinely new evidence when compared to the old evidence. In other words, prob(E, B) is quite low. But this means that prob(H, E&B) in equation (III) goes up considerably; E increases the degree of confirmation of H.

As can be seen, this version of Bayes' Theorem captures a strong intuition that many have about evidence. Adding new evidence that is just like the old evidence does not do all that much for confirmation. But adding new evidence that is unlike the old, or having a different variety of new evidence, is a boost to confirmation. In considering the H-D method in the previous chapter, we added a condition of this sort to the theory of confirmation that accompanied it. But it was just an add-on that had no further rationale other than it fitted with our intuitions about evidence, and so the H-D method ought to reflect that. In contrast, given this version of Bayes' Theorem we can now see that there is a rationale for this idea about evidence that flows naturally from it. Note also that we are just considering the special case of the H-D method where prob(H, E&B) = 1. But in Bayes' Theorem there is no need to make this assumption; we can consider cases where

prob(H, E&B) is less than 1 as in the case of testing statistical theories. We will not pursue this here; but it can be seen that this version of Bayes' Theorem is of wide application to all sorts of theories, statistical or not, when it comes to considering how new and old evidence plays its role.

All of the above can be seen by thinking about the following rendering of Version 2 of Bayes' Theorem:

prob(H, E&B)/ prob(H, B) = 1/prob(E, B).

The left-hand side is simply the ratio of the probability of H on new and old evidence over its probability on just the old evidence. This we would like to be greater than 1 and not equal to or less than 1. This ratio is equal to the inverse of the probability of E given B. If this is 1 (or less) then new E is not of much value. But if it is greater than 1 (i.e., prob(E, B) is low) then new E is of value for H.

So far we have only considered the background B to be old evidence. But B could be taken to be any background to H, including a theory. We will consider later a case in which B is a *background theory* which is a rival to H.

Bayes' Theorem Version 3

Before we consider this version there is a further theorem that can be established known as the **Total Probability Theorem** (in its simplest form). Suppose we are given evidence E and two rival hypotheses H and K. Then it can be shown:

prob(E) = prob(E, H) x prob(H) + prob(E, K) x prob(K).

The expression 'prob(E)' appears in the denominator of Bayes' Theorem; so it can be replaced by the **Total Probability Theorem**.

As is often the case we wish to compare several rival hypotheses. This we could not readily do with the H-D method as it deals with only one hypothesis at a time. Version 3 of Bayes' Theorem considers the case of just two rival hypotheses (but it can be generalised to n rival hypotheses). All we need do is substitute for 'prob(E)' (assuming it is not zero from now on) the result of the **Total Probability Theorem** in the Version 1 of Bayes' Theorem (one can also do the same for version 2), and we have the following two results for each hypothesis H and K:

(Eq. IV) $$prob(H,E) = \frac{prob(E,H)xprob(H)}{\text{prob(E, H) x prob(H)} + \text{prob(E, K) x prob(K)}}$$

(Eq. V) $$prob(K,E) = \frac{prob(E,K)xprob(K)}{\text{prob(E, H) x prob(H)} + \text{prob(E, K) x prob(K)}}$$

It will be left as an exercise for the reader to consider the case where the two rival hypotheses are H and ¬H; simply substitute '¬H' for 'K' in the second expression. The two equivalences provide a way of calculating the probabilities of two rival hypotheses on the same evidence E. This version of Bayes' Theorem is very useful indeed in all contexts where we need to see

just how well each of a pair of rival hypotheses are doing on the basis of the same evidence E.

Crucial experiments are of this sort. A crucial experiment involves two rival hypotheses such that one is the negation of the other. Pre-quantum mechanical hypotheses of light based on particle and wave models can be thought as such rivals. Prior to the 20th century, for example, light was considered to be either particle-like or wave-like, but not both. A crucial experiment is one in which the experiment confirms one hypothesis while disconfirming the other hypothesis. Thus crucial experiments can be well captured by equations (IV) and (V) when 'K' is replaced by '¬H' in the latter. Bayes' Theorem provides a natural understanding of crucial experiments that goes beyond what the H-D method can provide.

There is another way to understand equations (IV) and (V) that illuminates an old idea in methodology. Inductivists often speak of *eliminative induction* in which the goal is not merely to show that some hypothesis is doing well but also to show that some hypotheses can be eliminated even if there is no clear winner. It is not always clear what this might mean. Version 3 of Bayes' Theorem does give us one way of understanding this. Suppose that of a bunch of rival hypotheses some are getting very low probability, and even increasingly low probability as evidence comes in, while others are getting increasingly high probability. In such a case we can, in the light of these equations, say that some hypotheses have been eliminated as serious candidates. Eliminative induction has a simple Bayesian understanding.

There are many more versions of Bayes' Theorem that we will not state here but which can be found in most books on Bayesianism. One version is to add the background information B to E in equations (IV) and (V). Resulting equations will come in handy in Section 9.4 when the Bayesian methodology is applied to novel evidence and Kuhn's views on theory choice, respectively. Another is to consider Version 2 with the **Total Probability Theorem** substituted in the denominator. Yet other versions consider not just two rival hypotheses but n rivals such as H_1, H_2, H_3, … H_n. These rivals must be genuine rivals in that they exhaust all of the possible hypotheses; this would be the case if H_n were the *catch-all* hypothesis.

9.2.2 Bayesian Confirmation

Granted the above, what can we now say of the confirmation of a hypothesis? Bayes' Theorem tells us what the probability of a hypothesis is on given evidence. But when can we say that a hypothesis has been confirmed or disconfirmed? The following definitions are standardly suggested:

E *confirms* H if and only if prob(H, E) > prob(H)

That is, the probability of H is raised by evidence E above that which it had without E.

E *disconfirms* H if and only if prob(H, E) < prob(H)

That is, the probability of H is lowered by evidence E below that which it had without E. There is no hard-and-fast refutation but a slow progression into decreasing degrees of confirmation as evidence come in.

E *neither confirms nor disconfirms* H (or is neutral with respect to H) if and only if prob(H, E) = prob(H)

That is, the probability of H given evidence E remains the same as it was without evidence E.

What these show is that confirmation comes in degrees (as one might suspect). And so does disconfirmation. In the case of disconfirmation we have a generalisation of the idea of falsification so central to Popper's philosophy of science. But what the Bayesian approach indicates is that this is something that is not an "all-or-nothing" affair; the fortunes of a hypothesis can slide inevitably towards rejection as the degree of confirmation slips downwards.

9.3 BAYESIAN CONDITIONALIZATION: WHAT IS IT?

How does one rationally change one's beliefs? There are several ways of doing this. One of them is characteristic of Bayesianism and is known as *conditionalization*. It expresses the basic idea that we can learn from experience, but does it in Bayesian terms. Given some proposition H, let us suppose that a person assigns some (rational) degree of belief, or probability, to it, let us say $prob_{old}(H)$. Where this comes from need not be of concern at the moment (but it should not be 0). We can regard this as the rational degree of belief that a person would assign to H in the absence of any evidence, i.e., the prior probability of H. What is of concern is what *new* (rational) degree of belief the person assigns to H when they learn new evidence E? Given E it is now possible to form the new conditional probability $prob_{old}(H, E)$. What the **Rule of Conditionalization** tells us is what the new degree of belief, $prob_{new}(H)$, in relation to the old should be. It is as follows:

Conditionalization Rule:

$prob_{new}(H) = prob_{old}(H,E)$ (provided $prob_{old}(E) \neq 0$)

As an illustration consider the following. Suppose H = it will be stormy and raining by nightfall. What is the value of $prob_1(H)$ (where the suffix '1' indicates that it is some "old" or initial starting point)? For a subjectivist any value can be chosen as the initial starting point (but not 0, otherwise no learning on the basis of incoming evidence is possible). Suppose some evidence turns up such as E_1 'dark clouds are now gathering'; then one has to consider $prob_1(H, E)$, that is, prob(it will be stormy and raining by nightfall, dark clouds are now gathering). Once one considers this, what does one assign to the *new* absolute probability that it will be stormy and raining by nightfall, i.e., $prob_2(H)$? The **Rule of Conditionalization** tells us what to do. We can consider successive bits of evidence such as E_2 = 'the barometer is

falling'; E_3 = 'the clouds are even darker than before', and so on. Each time we conditionalize on the new evidence using the rule, thereby learning on the basis of new evidence, and up-dating the value of prob(H).

The **Rule** can be used alongside successive applications of Bayes' Theorem when expressed in the following way (with evidence and probabilities tagged according to a person at a given time):

$$prob_1(H, E_1) = \frac{prob_1(E_1, H) x prob_1(H)}{prob_1(E_1)}$$

Initially we assign, in whatever way, a (non-zero) value to $prob_1(H)$. Then if we are given the other values on the right-hand side, we can then work out the value of $prob_1(H, E_1)$. Having done that we can, using the **Conditionalization Rule**, set a value for the new $prob_2(H)$ (=$prob_1(H, E)$). We can use successive applications of Bayes' Theorem for new pieces of evidence E_2, E_3, etc, to get $prob_3(H)$, $prob_4(H)$, etc. But note that the Rule is not established in this way; it is merely used in conjunction with Bayes' Theorem.

In sum, what the Rule of Conditionalization tells us is that, if we start by assigning a non-zero probability to a hypothesis before evidence comes in, then when new evidence does arise, we have a way of altering our probabilities, up or down as the case may be, of the hypothesis. This is a very important result that gives substance to the idea of a Bayesian theory of knowledge. In Chapter 4 we sketched a number of theories of knowledge, all of which pay attention to the idea of evidence. In Section 4.6 we sketched a view in which knowledge did not take the pride of place that many have accorded it. Rather it is replaced by the idea of rational degree of belief. Now we have some understanding of what this might mean, given the whole apparatus of Bayesianism sketched above, including the Rule of Conditionalization and the definitions of confirmation. It is this which lies behind the Bayesian view that in science and ordinary life we do not always have knowledge in the traditional sense sought by philosophers (but we do have it when we can show that prob(H) = 1). Instead we do have rational degrees of belief in the sense set out above, and ways of rationally comparing degrees of belief in rival hypotheses to see which comes off best in the light of available evidence. The idea that there is a Bayesian epistemology was adumbrated in Section 4.6. It provides a quite radical new solution to some of the problems that have confronted theories of knowledge of the sort outlined in Chapter 4. But note that in rejecting the traditional idea of knowledge and replacing it by rational belief, no credence is given to dogmatism, scepticism, constructivism, or any other postmodern view in which the idea of knowledge is downplayed or abandoned.

At the end of Section 9.1.1 we mentioned that there can be such a thing as Bayesian learning. The Rule of Conditionalization gives substance to this. Suppose we start, like any good pedagogical constructivist, with an

individual pupil's initial degree of belief in some proposition, viz., $prob_{old}(H)$. (Note that this can differ from pupil to pupil.) Along comes evidence E which yields $prob_{old}(H, E)$ (which may be higher or lower than $prob_{old}(H)$). Now what is the pupil's new degree of belief in H, viz., $prob_{new}(H)$? If they adopt the Rule of Conditionalization, then they know what this will be. Here we depart from constructivism and inject a modicum of rationality to changes in degrees of belief determined by the Rule. But then, Just like Meno's Slave Boy in Section 3.3, there is a rational way of taking the pupil's starting point and getting them to new beliefs based on evidence and discussion. In broad outline this serves to indicate the way in which critical inquiry is involved in any learning based on taking up new information (perhaps as new evidence).

9.4 APPLICATIONS OF BAYES' THEOREM

We now have the basics of the Bayesian theory of confirmation set up. As can be seen it is a promising start to the matter of confirmation, incorporating so far some quite intuitive notions of confirmation, and also aspects of the H-D method as a special case. In this section we will explore even further some of the applications of Bayes' Theorem.

9.4.1 Novel Evidence

We have seen in Chapter 8 that a number of philosophers of science put emphasis on the confirmatory boost that a hypothesis gets by predicting novel evidence, that is, evidence which was quite unexpected. Huygens made much of this, as did Whewell. Popper has his own idea of corroboration which is the notion of a hypothesis passing a severe test. These various ideas about novel evidence flow quite naturally from Version 2 of Bayes' Theorem. This says:

(Eq. VI) $$prob(H, E\ \&\ B) = \frac{prob(E, H\ \&\ B) x prob(H, B)}{prob(E, B)} \qquad \text{prob(E)} \neq 0$$

In our previous discussion of this (see Bayes' Theorem Version 2, Section 9.2.1) we took B to be old evidence. Here we can take B to be a background theory which prevails before H is introduced. We can now ask how well new theory H is doing on new evidence E with respect to the background theory B. Again take the H-D case where H entails the new evidence E, so the above result can be re-expressed as:

Prob(H, E&B)/ prob(H, B) = 1/prob(E, B).

What this says is the following. On the left hand side is the ratio of the probability of H given new evidence E (against the background of the old theory B) over the probability of H in relation to just the background theory. What we wish to know is when evidence E makes a positive difference so that the ratio is high. If we look at the right-hand side, this would occur when prob(E, B) is low. This is equivalent to saying that the probability is low

when the evidence E is unexpected against the background theory B. Thus the increase in confirmation provided by evidence E varies as E is improbable with respect to the background theory B. Thus Bayesianism reveals the kernel of truth in the H-D method. Moreover, it endorses what Huygens, Whewell, Popper, Lakatos and others have said about the confirmatory boost that startling new evidence can give a theory when the evidence is unexpected against background beliefs B without the new theory H.

9.4.2 The Case of Kuhn

It is possible to apply Bayes' Theorem to Kuhn's later views about scientific theory choice. To see how this can be done, we shall first look at Kuhn's post-1962 views after the publication of *The Structure of Scientific Revolutions*. Most science educators are unaware that Kuhn's views have gone through a considerable transformation beginning with the 1969 'Postcript' to the second edition of his famous book, and the paper 'Objectivity, Value Judgement, and Theory Choice' which was originally delivered as a lecture in 1973 but published in *The Essential Tension* (Kuhn 1977, Chapter 13). In these and later works Kuhn takes the view that theory choice is essentially a comparative affair. What scientists typically want to know is not just how well a given body of evidence confirms a theory, but how rival theories about the same domain of phenomena fair with respect to the available evidence. In this regard Kuhn explicitly states that there are criteria for theory choice that apply across theories. (By the early seventies, Kuhn has dropped talk of paradigm altogether and reverted to the familiar term 'theory'.) The existence of such trans-theoretical criteria makes theory comparison possible, so the strong version of incommensurability as incomparability advocated earlier in *The Structure of Scientific Revolutions* is thereby given up. In Section 6.3 we mentioned these criteria briefly: accuracy, consistency, simplicity, scope, and fruitfulness. According to Kuhn, these are characteristics any good scientific theory should have. A good theory should be accurate in the sense that consequences deducible from it should sufficiently agree with the results of observations and experiments. It is also expected to be simple by bringing order to phenomena, and consistent not only internally with itself, but also with other accepted theories. A good theory should also have broad scope and therefore explain a lot, and be fruitful in the sense that it discloses new phenomena for research. Of these Kuhn wrote:

> These five characteristics – accuracy, consistency, scope, simplicity, and fruitfulness – are all standard criteria for evaluating the adequacy of a theory... I agree entirely with the traditional view that they play a vital role when scientists must choose between an established theory and an upstart competitor. Together with others of much the same sort, they provide *the* shared basis for theory choice (Kuhn 1977, p. 322; emphasis original).

These five characteristics function as criteria for choosing among rival theories in a natural way: other things being equal, choose that theory which is simpler; other things being equal, choose that theory which is more fruitful, and so on. Kuhn points out, however, that this shared basis is hardly sufficient to determine the choice of individual scientists. There are two reasons for this. First, it is possible that scientists apply them differently; they may disagree whether theory A or B is the simpler one, for instance. Second, one scientist may attach more importance to one criterion than another, and another scientist may opt for a different weighting. As a result, they may disagree despite the existence of a shared basis. In this way, Kuhn explains how *rational* disagreements can arise in theory choice. The five criteria 'function not as rules, which determine theory choice, but as values, which influence it' (*ibid*., p. 331). According to Kuhn, then, theory choice is not a matter of mechanical application of rules, but always requires judgement and deliberation.

Let us now ask how these Kuhnian insights can be incorporated into the Bayesian methodology. Begin with the first point that theory choice is always a matter of comparing rival accounts about the same domain of phenomena. Wesley Salmon shows that this insight can be captured by the Bayesian methodology in the following way.[5] For simplicity, we will assume that we are dealing with the same body of evidence E and only two rival theories or hypotheses, H and K. (The difference of terminology does not matter in this context.)

Consider Bayes' Theorem Version 2, but expressed twice, once for hypothesis H, and again for rival hypothesis K. Then put one equation over the other, and note that there is a common factor prob(E, B) that cancels out. Then one is left with the ratio

$$\text{(Eq. VII)} \qquad \frac{prob(H, E \,\&\, B)}{prob(K, E \,\&\, B)} = \frac{prob(E, H \,\&\, B)\, xprob(H, B)}{prob(E, K \,\&\, B)\, xprob(K, B)}$$

This equation looks formidable, but it is easy to read off what it says. It enables us to compare the two hypotheses for the purpose of choice. This can be done in two ways: before we have any evidence E and after we have obtained E. Before any evidence E is gathered, scientists should prefer H over K provided that the prior probability of H is greater than that of K. What happens when E becomes available? Then scientists should revise their earlier decision and choose K over H if and only if the posterior probability of K is higher than that of H. This can be seen from equation (VII) because it directly yields the relation

prob(K, E&B) >prob(H, E&B) if and only if
prob(E, K&B)/prob(E, H&B) > prob(H, B)/prob(K, B).

Salmon calls this relation a *Bayesian algorithm for theory preference* (Salmon 1990, Section 7, or 1998 Section 7). When evidence E becomes available, scientists should prefer K over H provided that the ratio of the

likelihoods is greater than the reciprocal of the ratios of the prior probabilities. In this way, therefore, Bayesian methodology can incorporate Kuhn's idea that theory choice is essentially a matter of comparing rival theories.

Kuhn also raises matters to do with rational disagreement. A Bayesian explanation of this is straightforward. Scientists, though they may agree on the same algorithm for theory preference, may disagree about their estimates concerning the prior probabilities for whatever reasons. This is very similar to Kuhn's point that scientists sharing the same five criteria can nevertheless apply them differently. But this is not all. These Kuhnian criteria for theory choice can enter into considerations for estimating the prior probabilities. This provides a further link between Bayesian methodology and Kuhn.

Turning to his five criteria, consider simplicity first. Scientists tend to prefer simpler theories over more complicated ones. A theory may be simpler than another because it postulates less entities or because it makes calculations simpler. Now, simplicity is a factor for estimating the prior probability of a theory. The simpler it is, the greater its prior probability will be.

Second is fruitfulness. Of the two theories given to us, scientists prefer the one that is more fruitful. A theory H may be fruitful if it discloses more new phenomena than K, thereby predicting novel facts that are quite unexpected with respect to background information. We have seen a case of this when we have briefly discussed the Fresnel-Poisson example in Section 9.2.1. In the language of probability, prob(H, B) will be much lower than prob(K, B). This in turn will boost the posterior probability of H more than K. Here is another bridge between Bayesian methodology and Kuhnian criteria.

A third criterion is consistency. If a theory or hypothesis is internally inconsistent in the sense that it is known to contain contradictions, then prob(H, B) will go to zero no matter what the other probabilities are, and this in turn will make prob(H, E&B) zero. This means that no matter how much evidence is gathered, it does not increase the posterior probability of H. This is as it should be. For a contradictory theory or hypothesis entails every piece of evidence, so no evidence can be really new.

The fourth Kuhnian criterion is scope. Unfortunately, scope cannot be handled so easily within the Bayesian framework. As Salmon points out, this is because having broad scope is an informational virtue, not a confirmational one like consistency and fruitfulness (Salmon 1990, p. 197). If a hypothesis H has broader scope than K, then it gives much more information about the world than K. To use Popperian language, it is more falsifiable, or, alternatively, has more empirical content, and therefore has a lower prior probability. There is no natural way for Bayesianism to capture this Popperian point. So the criterion of scope lies outside the scope of Bayesian framework.

The final Kuhnian criterion that we need to consider is accuracy. Since Kuhn defines it as quantitative agreement between the consequences of a theory with the observational or experimental results, it may well be called predictive accuracy. Clearly, other things being equal, we prefer that theory which is predictively more accurate than its rival. And this is straightforwardly captured by the Bayesian approach because the posterior probability of H will be much greater than that of K if it has more correct predictions than its rival. In that case we should naturally prefer H over K.

In short, the Bayesian framework has the resources to deal with the thorny issue of theory choice understood as comparison between rival theories, and these resources can be effectively mobilized to establish a strong bridge with views Kuhn developed after his influential book *The Structure of Scientific Revolutions*. These later views of Kuhn are more deserving of a role in science education than his earlier views.[6]

9.5 PROBLEMS FOR BAYESIANISM AND THEIR RESOLUTION

Although Bayesianism provides a powerful methodology that is clearly superior to Hypothetico-Deductivism in many respects, it is not free of certain difficulties. In this section we will mention only two of them and discuss briefly how Bayesians have responded to them.

9.5.1 The Problem of Priors

One of these difficulties concerns the interpretation and estimation of prior probabilities. The utility of Bayes' Theorem for an account of confirmation depends on the availability of prior probabilities from which posterior probability of a hypothesis can be computed. The problem in this context is not so much the near impossibility of coming up with exact values, as the very way in which they should be interpreted. Roughly speaking, there are three competing interpretations over this issue among the Bayesians: the objectivist-logical, the objectivist-empirical, and the subjective (Salmon 1967).

Since subjectivists, such as Frank Ramsey and Bruno de Finetti, interpret probabilities in general as subjective degrees of beliefs, they claim that the value of the prior probability of a hypothesis H (or a theory) is simply the actual degree of belief that a person has in that theory. Thus, subjectivists argue that there is no such thing as *the* correct value of a prior probability, so anybody can attach any value to it as an expression of their degree of belief in it. Notice that this arbitrariness will also be carried over to the posterior probability of H via the Bayes' Theorem.

One may be astonished by such an extreme subjectivism and wonder how it can be reconciled with the objective nature of science. The subjectivists respond by saying that with each bit of evidence gathered, the person will revise her earlier degree of belief according to Bayes' Theorem. Furthermore,

it can be proven under certain rather stringent conditions that as more and more evidence accumulates, the values of the posterior probabilities of H will all converge to the same value. (In the Bayesian jargon, this is called "washing out" the prior probabilities which reflect individual differences among people.) Bayesians argue that this is indeed how consensus arises in science too. Even if each scientist begins with a different subjective degree of belief in the same hypothesis, some high while others are low, as evidence accumulates they eventually all converge to the same theory using Bayes' Theorem. It seems that what was initially a weakness of subjectivist Bayesianism turns into a triumph.[7]

Though some subjectivists, or personalists, accept this "swamping" of quite divergent prior probabilities on the part of different scientists, they also propose another remedy. This leads us to what is commonly known as *tempered personalism*. It is tempered because it adds an extra principle to the theorems of the probability calculus. The extra principle seems harmless enough in that it bids us, for all seriously proposed hypotheses, not to give them extreme prior probabilities of 0 or 1 or close to these; instead they bid us to give all seriously proposed hypotheses a "fair go". This ensures that, as evidence comes in, the convergence of opinion of different scientists who assign different but not unreasonable priors may occur more rapidly. For extreme assignments by subjectivists such convergence may take a long time, so long that, as Keynes says 'in the long run we are all dead'! For extreme subjectivists adding to the principles of the probability calculus is a little like sinning in church. They see no need for such additional requirements. All we should do, says the subjectivist, is to assign what priors we like, providing that we remain coherent in our assignments. Moreover, tempered personalists have not always been able to come up with independent reasons as to why they should adopt such an extra principle, except that it removes a difficulty about how priors are to be assigned.

A second group of Bayesians, most notably Rudolf Carnap, defend an objective-logical interpretation. This interpretation tries to avoid the extreme subjectivism of the first interpretation. According to Carnap, for example, probability is an objective and logical relationship between statements, more specifically, between a body of evidence E and a given hypothesis H, where E entails H only *partially*. Carnap claims that this *partial* entailment can be cashed out in a precise and objective manner in a suitably chosen formal language in terms of the notion of state description. We shall not attempt to summarize how probability assignments can be carried out using this Carnapian strategy because it unfortunately does not work, as Carnap himself has admitted.[8]

The objective-empirical interpretation is the final one we will consider. Hans Reichenbach and his followers, in particular, Wesley Salmon, adopt this approach. According to this approach, probabilities are objective relative frequencies. A little more precisely, probability is the limit of the relative

frequency with which a property obtains in an infinite sequence of events. Salmon applies this conception of probability to the prior probability of a hypothesis H. We select a class of hypotheses, called the reference class, that are similar to H and place it among them. This reference class consists of hypotheses, the relative frequency of whose truths can be estimated by appealing to evidence that has already been gathered. The relative frequency of truths within this reference class is the probability of H. In this way an objective value based on experience is assigned to the prior probability. In the previous section we saw how some of the Kuhnian criteria can be taken into account in estimating prior probabilities.

We have briefly introduced the major approaches to the problem of priors. The dispute among subjectivist and objectivist Bayesians still goes on, leading to more insights into the Bayesian methodology. It should be noted, however, that this is an internal discussion, where all parties agree on the superiority of Bayesianism over the H-D method, or any other method.

9.5.2 The Problem of Old Evidence

This is a problem that any Bayesian, be a subjectivist or an objectivist, faces. Scientists, and let us add, common sense tell us that when a theory explains an already known fact or phenomenon, it gains some support. For example, that Galileo's law of free fall and Kepler's laws of planetary motion can be derived from Newtonian theory (under certain simplifying assumptions, of course) counts in its favour. We can say that Galileo's law of free fall and Kepler's laws of planetary motion are "old evidence" relative to the Newtonian theory in the sense that at the time Newton came up with his theory they were established facts known to him and others of the scientific community. Since these are known and established facts, they must be accepted as true, and therefore their probability should be 1 (or very close to 1, allowing for scientific fallibility). We can designate this as prob(E) = 1. But this is not all. Since 1 is the highest value for probability, prob(E, H) will also be 1, regardless of which hypothesis is used for the conditionalization. But now a problem arises for the Bayesian. Go back to equation I of Section 9.2.1.

$$prob(H,E) = \frac{prob(E,H)xprob(H)}{prob(E)}$$

Plugging these two values, we see that prob(H, E) = prob(H). That is, the prior and posterior probabilities are the same, meaning that adding evidence E does not matter at all. Thus, we cannot say, according to Bayesianism, that deriving Galileo's or Kepler's laws from Newton's theory gives support to the latter. This goes against scientists' intuitions and common sense, indeed.[9] There are several solutions to this problem in the Bayesian literature that we

cannot go into here; but they do show how Bayesianism has the resources to solve some of the problems that confront it.

9.6 IMPLICATIONS FOR SCIENCE EDUCATION

The reader may well be pleased to have reached the end of this chapter and wondering what use all this has for science education. We have already illustrated some of the applications of the Bayesian methodology for science, more specifically, for giving an account of theory choice and the confirmatory role of evidence. The idea of confirmation is central to science, and the Bayesian approach explains it very naturally. Similarly, scientists sometimes face the difficult task of choosing among rival theories over the same domain of phenomena, and again Bayesianism does a good job of capturing many of the intuitions scientists have in this matter. Indeed, these are some of the several superiorities of Bayesianism over the H-D method. Thus, to the extent that science education is concerned with the nature of science, Bayesianism should be of relevance to science educators. Importantly, we have also indicated how Bayesianism can provide a theory of rational learning in which, starting with a pupil's belief contents and their degrees of belief, they can come to learn on the basis of evidence.

We are well aware of the fact that teaching Bayesianism to science students is not easy because of its technical nature. Nevertheless, we feel it is important that science students and prospective teachers should be exposed to at least some of its central ideas such as prior and posterior probabilities and some simple versions of Bayes' Theorem. Probabilities are essential to modern science. From medicine to genetics they are ubiquitous, and as we have tried to show in this chapter, Bayes' Theorem provides valuable insight into many aspects of science, especially the question of how evidence bears on hypotheses.

Indeed, perhaps the most important implication of the Bayesian approach for science education in particular and education in general is its stress on belief revision. As indicated, it tells us how to revise our beliefs on the basis incoming information. This is true not only of observational and experimental evidence but also of evidence in ordinary situations. Bayesianism can be useful not only in science, but also in daily life. For instance, when we see a colleague's car parked in front of his office, say on a Christmas night when we normally do not expect him to be there, we infer that the colleague must be in the office. If we know the relevant probabilities, we can plug them into the Equation (I) (version 1 of Bayes' Theorem) and compute the probability that she is in her office given the evidence that her car is parked in front of it. Updating one's beliefs on the basis of evidence one obtains is one of the most important features of rationality, and rationality is essential to being critical inquirers as we noted in Part I.

There are many other simple applications of Bayes' Theorem, and some of them can have surprising results. Here is one that can be used in the science classroom. It is a well-known fact that many medical tests, such as home pregnancy tests, done for diagnostic purposes are not 100% accurate. Suppose that the probability that a woman who is really pregnant will test positive is 0.97. This can be expressed as: prob(E, H) = 0.97, where E is being tested positively and H is the hypothesis of pregnancy. But a woman can test positive even if she is not pregnant. Let us also suppose that this probability is 0.2, that is, prob(E, ¬H) = 0.2. Let us now ask the following question: what is the probability that Mary is pregnant given that she tested positively, that is, prob(H, E)? (Note that here we are asking about the probability of H given E, prob(H, E); and this is quite different from the likelihood of H, viz., prob(E, H). Note also that Bayes' Theorem tells us what is the link between these two probabilities.)

Many students will guess that the answer is 0.97; after all, they will reason, the test is 97% accurate, so there is a 0.97 chance that Mary is pregnant. Are they right? Well, to see this we need to compute prob(H, E), and this is where a version of Bayes' theorem comes in handy. Look at Equation (VII) of Section 9.4. For our purposes, we can ignore background theory B in that equation. Note also that we have only two hypotheses to consider, pregnant or not pregnant, and these are negations of one another, thus mutually disjoint and exclusive. So, hypothesis K in Equation (VII) can be taken as ¬H, i.e., not-H. Equation VII then reduces to:

$$\frac{prob(H,E)}{prob(\neg H,E)} = \frac{prob(E,H)xprob(H)}{prob(E,\neg H)xprob(\neg H)}$$

To compute the ratio on the left hand side, we only need prob(H), that is, the probability of pregnancy among the entire population of women. This is called the base rate. Clearly, this will be low. So, let us say, prob (H) = 0.05. From this we can infer that prob (¬H) = 0.95. We are now ready to compute the left hand side of the above equation, which tells us the ratio of the probability of Mary's being pregnant given that she tested positively to the probability of her being not pregnant given the same evidence. Plugging the numbers, we get:

prob(H, E) / prob(¬H, E) = (0.97)(0.05)/(0.2)(0.95) = 0.25

Thus, the odds that Mary is pregnant are only 1 to 4. It follows that prob(H, E) = 1/5 = 0.2. The probability that Mary is pregnant given that she tested positively is indeed quite low, contrary to expectations. The reason for this surprising result, of course, is that the base rate of pregnancy is quite low in the population of women to begin with, and that affects the posterior probability.

This is just one application of Bayes' Theorem, which is simple enough to be used in the classroom, provided that students have some basic

understanding of probability. Science educators can come up with more creative and informative examples of their own. We firmly believe that the Bayesian approach has much to contribute to science education, and so far as we know, the relevance of Bayesianism to education is a new area unexplored by science educators. There is therefore much that needs to be done, and here we are content simply to bring it to their attention as a worthwhile and fruitful research program and urge them to pursue it.

We would like to conclude this chapter by pointing out that Bayesianism is the frontier of current research in methodology and is the point from which those currently engaged in that research take their cue, and either work within it or look for alternatives to it. There are many working in the field of alternatives to the Bayesian methodology, but coming to grips with their work requires some knowledge of it. Much the same could be said of the debates over method in the 1960s and 70s. Popper's views were at centre-stage, and one had to master them in order to come to grips with the rival views of Lakatos, Feyerabend and Kuhn. The scene has now shifted and science education needs to take note of what probabilistic reasoning is like and what the Bayesians have to say about methodology.

These days we often hear from the postmodernists and their co-travellers like social constructivists and epistemic multiculturalists that science has no method or methods of its own, that not method but social factors such as interest and power are decisive in science, or that, more bluntly, scientific methodology after Kuhn is dead. Nothing can be further from the truth. Debates in the philosophy of science concerning scientific methodology are livelier than ever, thanks especially to the contributions of Bayesianism. The sustained attack against standard cannons of scientific inquiry by social constructivists and postmodernists is ill-founded, and, indeed, is much ado about nothing.

In the last three chapters we have considered methodology only in so far as it relates to the testing of hypotheses. Bayesianism is the culmination of this aspect of method. But there are other kinds of methodology in science. In the next chapter we consider methods for building models in science. We set this in the context of scientific realism and the extent to which the models we build fit with reality. We also show how this raises difficulties for those who proclaim the end of method and/or adopt constructivist views which are at odds with this important aspect of scientific method.

NOTES

[1] There are ways of adapting the H-D method to the crucial testing of two hypotheses. See Hempel 1966, pp. 25-8. In general, Bayesianism accounts for the comparison of many hypotheses, of which the case of crucial experiment is a special case. We mention crucial experiment in Section 9.2.1 in our discussion of the third version of Bayes' Theorem.

[2] We pass over here the solutions that Bayesianism provides for some of the long-standing problems that have faced theories of confirmation, such as Hempel's "ravens" paradox, and Goodman's "grue" problem. We also pass over the Bayesian solution to a problem that the H-D method faces that we have not mentioned; this is the irrelevant "tacking" problem. Bayesian solutions to these problems are discussed in Earman 1992, Howson and Urbach 1993, and Curd and Cover (eds.) 1998, in their 'Commentary' to Chapter 5 'Confirmation and Relevance', pp. 627-74. Solutions to these problems add support to the Bayesian theory of confirmation.

[3] As well as the texts mentioned in the previous footnote, for a simple introduction to the issues of Bayesianism also consider the excellent Gauch 2003, Chapter 6.

[4] Discussions of this important matter go under the heading of 'Dutch Book arguments' a 'Dutch Book' being a set of bets in which, if bettors do not assign values to their degrees of belief in accordance with the probability calculus, then they will always lose, or at best never win. See the books in the above two endnotes for references to these arguments.

[5] In the rest of this section we will follow the suggestion made by Salmon (1998). John Earman, a subjectivist Bayesian, has some reservations about this sort of reconciliation between Kuhn's views about theory choice and the Bayesian methodology, if by 'theory choice' is meant 'accepting it as true.' He argues that according to Bayesianism theories are never 'chosen' in this sense, but only probabilified. But if 'theory choice' is taken to mean 'decision to pursue the theory in question', 'to put one's time and energy into it', then this is acceptable to the Bayesian. For Earman's own version of reconciliation see Earman 1992, Chapter 8.

[6] Kuhn's later views can be found in Kuhn 1977, Chapter 13, throughout Kuhn 2000 and Kuhn 1993.

[7] As important as this triumph is, there is much comment in the literature on just what are the conditions that will lead to convergence of opinion when there are widely different starting points. Convergence of opinion is an important issue for Bayesians.

[8] For a lucid and readable account of the Carnapian project, see Salmon (1967, Chapter 5).

[9] There are several attempts to overcome this difficulty. See, for example, Earman (1992), Curd and Cover (1998), pp. 627- 74, and the literature cited therein.

CHAPTER 10

SCIENTIFIC REALISM AND MODELLING REALITY

The construction of models is an important activity in science. Scientists have built models of the solar system, the universe as a whole, weather systems, the DNA molecule, the atom, and many other items. Indeed, a considerable part of scientific knowledge about reality is obtained through one or another kind of model. In this chapter we focus on what models are and how they relate to reality. While scientific models receive some attention from science educators (see, for example, Matthews 2000 and Halloun 2004), we believe that how they represent reality and the related issue of scientific realism in connection with them are not fully explored. There is also much confusion in the science education literature about various forms of realism, including scientific realism. Accordingly, in the first section of this chapter we set out what scientific realism means; there is in fact no one notion here but a number of different notions that need to be distinguished. We also briefly consider some of the rivals to scientific realism. Of these instrumentalism and constructive empiricism are discussed in the first section and radical constructivism in the final section.

Despite what some say, we humans are not constructors of the external natural world explored in physical and life sciences, though we are constructors of artefacts and of our social world explored in social sciences (see Searle 1995). In this context a few brief comments are made about a plausible constructivism with respect to social reality. Though we are not constructors of the natural world (to claim so would involve much human hubris), we are constructors of theories and models about that world. Though he was not the first to construct models, Galileo made it an important part of the methodological revolution that he inaugurated in science. In this respect he was followed by Newton, and nearly all subsequent physicists. In Sections 10.2 and 10.3 we discuss Galileo's account of the role of "reason", as he puts it, rather than experience in constructing idealised models of reality. As science progresses the models can be made more concrete so that there is a better fit between them and reality. In Section 10.4 we draw a distinction between abstraction and idealisation with its correlative notion of concretisation. Section 10.5 discusses ideal laws in science that can also be made more concrete as science progresses. Theoretical models, we claim, are constructed out of ideal objects that obey idealized laws. Section 10.6 links the account of the methodology of model construction with the methodology

of test already examined in previous chapters. The final section draws the implications of what is said in the previous sections for science education.

10.1 SCIENTIFIC REALISM

10.1.1 A Metaphysical, or Ontological, Definition of Commonsense and Scientific Realism

Most of us are realists about commonsense everyday objects that surround us. But as we have seen in Chapter 5, this is not a universally adopted position; it is rejected by idealists and by constructivists. What does it mean to say that one is a realist? The core idea is of a metaphysical or ontological doctrine and in respect to some item x:

Realism with respect to item x = (1) x exists; (2) x exists independently of the mental.[1]

Like any definition the terms on the right-hand side that do the defining also need to be defined. If not either some intuitive understanding of them is presupposed, or we are to take them as primitive, or undefined and not further explicable. So, what do we mean by 'exists' in (1)? Here we could leave this as undefined hoping that all readers would have some idea of what is meant. If the items x are taken to be ordinary observable objects such as the Sun, or grains of sand on a beach, or a cat, etc, then perhaps we could add a little more and say that at least 'x exists' entails 'x is locatable in space over some period of time'; and then we can extend this notion of existence to non-observable items such as atoms or fields. What is clear is that we cannot be realists about things that do not exist, from Santa Claus to fictional characters such as Sherlock Holmes, or witches, gnomes, goblins and the like.

What does talk of 'mental' come to? Here we can spell this out by talking of human mental activity, or more broadly, of cognitive and/or sentient activity. More specifically we can talk of x existing independently of the mental as including any being's perception of x, belief or thought about x, theories or world views about x, the use of language to refer to or talk about x, and any of our cognising activities about x including those cognising activities which might be said to constitute x (this last stipulation would rule out any Kantian cognitive constitution of ordinary objects as non-realist).

Finally what of independence? Here we can spell this out counterfactually and say: x would still exist, even if no being were to perceive x, think or theorize about x or use language to refer to x, etc. That is, the dinosaurs would still have existed (and then gone out of existence) even if there were never any evolved sentient creatures such as ourselves; or if we were to come into existence (as we have) then we would have never developed language to talk of them, proposed theories about them, or even have perceived them (or their remains). In sum, what realism says can be put plainly and bluntly. There are things out there that do not need us to get on with their existence,

be the kinds of thing they are and do the things they do; they can be and do all this even if we never existed. Realism puts humans in their place and is not human chauvinistic!

The above is merely a definition of realism that applies very well to commonsense objects. If it is adequate, then it is something to which even the opponents of realism should agree; even the opponents of common sense realism (such as idealists or epistemic constructivists) have to agree as to what the doctrine is. What arguments do commonsense realists have for their claim that such a definition is not empty and something does satisfy it? And what arguments do the opponents of such realism have to show that such a definition is empty because nothing satisfies it? There is a long history in philosophy of arguments for and against the claim that the definition does capture something. Though they must accept some such definition of realism, Ancient Greek sceptics argued against our ever *knowing* that anything meets its requirements. Descartes tried to state a strong sceptical position, and then refute it thereby reinstating realism about the external world. Hume remained a quizzical sceptic. Berkeley grasped the nettle and adopted an idealist position claiming that there were really no such mind-independent objects and that all the objects that exist depended for their existence on the perceiving minds of humans, and ultimately the mind of God. Finally latter day phenomenalists, extreme empiricists, idealists and epistemic constructivists have continued to accept some such definition but deny that it is satisfied by anything. All of these provided arguments for or against realism that we discuss here (though Chapter 5 has set out some considerations against one version of the constructivist position). What we are concerned with is a definitional matter of being clear about what we mean by philosophically contentious notions such as realism, commonsense or otherwise. From here on we set aside arguments about commonsense realism.

We can also be realists not only about commonsense *objects* but every day *events* such as the changing of a traffic light, or the flight of a bird. And we can be realists with respect to commonsense *processes* such as tidal movements, the fizzing of a shaken bottle of coke or the progress of winter. For the realist, events and processes go on whether we humans are around to take notice or not. In what follows we will largely consider realism with respect to objects but also allow that realism can be extended to other categories of being. Traditionally realism was introduced with respect to *kinds* of things such as the horse, or water, and to *properties* (also called *universals*) such as redness, sphericity, equality etc. Though we can give a similar definition of what it means to be realist with respect to kinds and properties it is a further matter with a long philosophical history of debate as to whether anything satisfies the definition, that is, there are kinds and/or universals which exist independently of human (or any other) minds.

The definition of realism does not require that we *know* that any of these commonsense items exist; that would take us into epistemological matters to do with the reasons or evidence we have for or against their existence. All the definition of realism says is that, whatever x is, x exists in a mind-independent fashion. Thus our ancestors might have adopted a realist attitude to Santa Claus, witches or dragons; and some of our contemporaries might adopt a realist attitude to alien abductors or ghosts. But if no examples of these things exist, then it is not possible that realism is a doctrine that applies to them. One can be a mistaken realist. That is why in their epistemology most realists adopt a fallibilist attitude; we can be wrong about the very objects, events and processes that we take to exist. So while the definition of realism is not epistemological, most realists do have an accompanying epistemology that is a least fallibilistic, and not dogmatic, about what we can *know* to exist. Though the definition of realism given above is metaphysical or ontological and not epistemic, the epistemic and the ontological do closely accompany one another. (We will be realists about what we know to exist, and about what exists we will attempt to come to know of its existence.)

So far the item x has been particular objects such as a tree, a rock, a puddle of water, etc. But what if x is taken to be artefacts such as hammers, needles, shoes, computers, aeroplanes, etc? These are all made out of materials that exist independently of us. Would we count as hammers objects that looked like hammers and grew on "hammer trees" but were never used by any being to hammer things, or even thought of in that way? Unless there is a general intention on the part of humans (or other intentional beings) to use such items for a particular purpose, such as hitting nails (another artefact), then they would not be hammers. Here we may say that if these hammer-like items exist independently of humans and their minds, including human intentions of use, then they would not be hammers. This is not to say that the materials out of which hammers are constituted, for example the metal that is in the head or the wood of the handle, may ultimately exist independently of human minds; so realism with respect to the constituents is appropriate. But hammers themselves only exist relative to human uses and so realism is not appropriate with respect to them. This is not to say that they do not exist; artefacts do exist. Rather it is clause (2) in the definition of realism that is not satisfied.

The same thing can be said of items like money. The metal of coins exists in a mind independent way; but the coins themselves, considered as exchangeable items, though they do exist, they do not exist mind independently. Money depends very much on human intentions of use; if these were to be given up (for example, we drop the whole business of exchange and do not interact with one another in this way), then there would be no money.[2] The status of socially constructed items is of interest to the social sciences such as economics, anthropology and sociology; they deal with an ontology of items that are socially constructed. Further discussion of

these items takes us out of the realm of the natural sciences and goes beyond the scope of this book.

So far all the items that x can stand for have been observable. Can there also be realism with respect to unobservable items? Yes, say scientists of a realist persuasion. There are molecules and these are made of atomic elements, and these in turn are made of electrons, protons, neutrons and many other sub-atomic particles. There are also gravitational and electro-magnetic fields. There are genes and DNA molecules. There are tectonic plates. And there are pulsars, quasars, black holes, and the like. What we mean by unobservable can be left vague. The notion of observability is *human* observability. Since the observable is with respect to us humans and our powers to detect things with our normally functioning perceptual systems, then the unobservable is what lies beyond such powers of normal detection. But we can enhance our powers of observability using all sorts of detection apparatus. Thus Geiger Counters detect the presence of radio-activity, something that otherwise would be unnoticed by us; and our electricity meters detect the amount of electricity we use even though this is not observable by us without the help of a meter. But note that such detectability is not direct observation. We detect our gravitational field every time we travel in an elevator; but this does not count as observing what is in effect a theoretical, or unobservable (-by-us), item. Some items remain in principle unobservable by us (such as whatever is outside our light cone and so cannot get any signal to us). Yet others were unobservable at one time but observable at a later time. For example, Semmelweis was not able to detect observationally the very bacterial agents (such as *streptococci*) that he discovered caused childbed, or puerperal, fever in women who had just given birth and who subsequently died; but within a few decades these were being observed and studied with microscopes. Though the distinction is not hard and sharp, a fuzzy line can be drawn between what we humans can readily and normally observe and what we cannot. Scientific realism is realism with respect to such unobservables.

We can now extend the definition of realism from commonsense objects to scientific entities such as those just listed and define ‘scientific realism’ for this class of objects:

Scientific Realism with respect to the unobservable objects x of science and its theories = (1) x exists; (2) x exists independently of the mental.

The terms used in the right-hand side definition are to be understood as in the discussion of commonsense realism. We can expand the scope of x to include unobservable events, such as the emission of a sub-atomic particle in radioactive decay, and to unobservable processes such as cell union or division. And we can also extend the scope of x to include such properties of moving bodies as their mass, inertia, momentum, and their charge (if they are electrically charged), and so on. Here we have a broad category of items with

respect to which scientific realism can be defined, such as objects, events, processes and properties.

Again we need to distinguish matters to do with the definition of scientific realism as opposed to arguments for or against it. Such arguments will not be addressed here.[3] And again we need to associate scientific realism with a fallibilism about what exists. Scientists have been wrong about the existence of items such as impetus, phlogiston, caloric, the electromagnetic ether and the like. Even though the definition of scientific realism sets out an ontological or metaphysical doctrine about the mind-independent existence of unobservables, and so is not an epistemological notion at all, most scientific realists are also fallibilists in epistemology. Their theories about what exists can make false existence claims.

The above definitions of commonsense and scientific realism, as have been emphasised, concern ontological matters. They make no mention of our theories being *true*, or approximately true, or the terms of our theories *referring* to items, or not as the case may be. Accounts of realism that make essential mention of semantic items such as the terms of a theory *referring* (or *not referring*), or the *sentences* (or *propositions*) of a theory being *true* (or *false*), can be said to be *semantic* definitions of realism. Metaphysical or ontological realism of the sort introduced above depend heavily on the idea that it is ontological items like objects, events, etc, that exist mind-independently; and the mind independence is understood to exclude dependence on anything linguistic, such as the terms or sentences of a theory which are expressed in the vocabulary of some language.

A classic semantic conception of realism that many have adopted is given by Putnam when he says, following Richard Boyd:

> Boyd tries to spell out realism as an over-arching empirical hypothesis by means of two principles:
> (1) Terms of a mature science typically refer.
> (2) The laws of a theory belonging to a mature science are typically approximately true. (Putnam 1978, p. 20)

Clearly this is a semantic definition with its reference to terms and the statements of a theory, such as laws, being true. But it should be noted that the ontological conception lurks in the background of this definition. The first clause has as an instance: the term ‘electron’ (of mature physics) refers. But this is only the case if electrons exist (but we have to add the requirement that they exist mind independently). And again a sentence of a theory such as ‘all electrons in an atom have some energy level or other’ is true only if there exist such items as electrons, energy, and levels of energy; that is, scientific realism with respect to theses objects and properties (such as the energy of objects) do exist mind independently. We will not pursue semantic aspects of realism any further. But it should be recognised that such semantic conceptions are commonly advanced instead of metaphysical or

ontological conceptions. Here both will be recognised, with the latter being given priority.

10.1.2 Two Rivals to Scientific Realism: Instrumentalism and Constructive Empiricism

Let us now consider some of the rivals to scientific realism. One presupposition of the semantic form of realism seems quite obvious, viz., that theories have a linguistic formulation as a set of sentences or propositions, including fundamental laws. Moreover, as in the semantic definition given above, these propositions have truth-values, and may be either true or false. Or we could allow that while they are not true they are not hopelessly false in that they have some truthlikeness or verisimilitude. The important presupposition here is that theories are made up of things that can be true or false, viz., propositions. Instrumentalism denies this seemingly obvious claim. They reinterpret theories in a radical way so that they do not contain the kinds of things that can be true of false. So, what replaces propositions? Rules for calculating or inferring. According to instrumentalists, theories are nothing but sets of rules that enable one to infer from a given observational input, an observational output as a prediction. Theories are said to be like a "black box". Use some initial observational information as input – then turn the handle on the black box – then out comes some observational predictions. If these are true (the instrumentalist must allow that observational inputs and outputs can have truth-values) then the "black box" is a good predictor. If they are false then it is a bad predictor. Like the tools in a toolbox, if the "black box" does not do its work, that is, make correct predictions, then it can be discarded and replaced by another "black box" predictor that is hopefully better as a predictor.

What goes on in the "black box" when the handle is turned? The black box contains rules for calculation; from some given input "turning the handle" means employing the rules of calculation so that they will yield a given predictive output. Hence the theories we use are really to be understood as sets of rules for calculation. They are not to be understood as being about anything, as would be the propositions of a non-instrumentally interpreted theory. So for an instrumentally interpreted theory scientific realism is a non-starter. A theory is a set of rules for calculation, a recipe for getting predictions. It does not, and cannot, give us any picture of what unobservable reality is like, because it does not picture anything at all. It is clear that instrumentalism is a radical rival to realism. It is not our task here to evaluate instrumentalism; others have done that successfully (see Musgrave 1999, Chapter 4). Our task is merely to set out some rivals to scientific realism.

We will consider only one other rival to realism, the constructive empiricism of van Fraassen. He provides, first, a characterisation of scientific realism as follows: 'Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves

the belief that it is true' (van Fraassen 1980, p. 8). Since this is a definition of scientific realism by one of its opponents, realists should be wary. We can agree that we are to take our theories in a literal sense in that its component statements can have the truth-values, either true or false. This is in contrast to the instrumentalist who does not take the sentences of a theory to be statements at all; they are not capable of being true or false since they are to be understood as rules for inferring or calculating. But note that the definition of realism is of the semantic variety since it makes crucial reference to theories; it is not a metaphysical or ontological version of realism. Also cautious realists who do adopt a semantic version of realism might not want to claim that their theories are literally true. Some realists, like Popper, wish to claim that all theories are really false but some are less false than others, i.e., they have greater verisimilitude or truth-likeness. Moreover the aim of realists is to increase verisimilitude; getting the truth may not be a realisable goal at all.

Importantly the definition addresses axiological realism, that is, the values, aims or goals that realists endorse. The final clause of the definition introduces the idea of *acceptance* in contrast to *belief*. Now realists might agree that we can accept theories for whatever purpose; for example we may know that a theory is false but accept it for the purposes of working on it. And they might do this because the theory is simpler to work on than any of its more correct rivals; or it might be the only theory currently available. So for a realist, accepting a theory may not necessarily mean that they believe the theory to be true. If they are fallibilists then they might not believe this very strongly at all.

With these caveats, let us now consider van Fraassen's rival view of constructive empiricism which nicely contrasts with the above account of realism: 'Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate' (*ibid*,. p. 12). Though it is not said in this quotation, it is intended that a theory be taken literally and that all its statements, including the non-observational or theoretical statements, do have truth-values. What is also claimed is that we cannot know that any theoretical statements are true or false, even though they are one or the other. Alternatively it is claimed that what we observe underdetermines what theory might be the case. Much emphasis is placed on the idea of empirical adequacy. One way of understanding this is that we want our theories to capture all the observable phenomena, past present and future. Since constructive empiricists take their theories literally, then all their statements have a truth-value; since this also includes observational statements then these are also either true or false. What it then means for a theory to be empirically adequate is that all the (possibility infinite) observational consequences of a theory are true (and none false). Finally accepting a theory does not involve the belief that all its

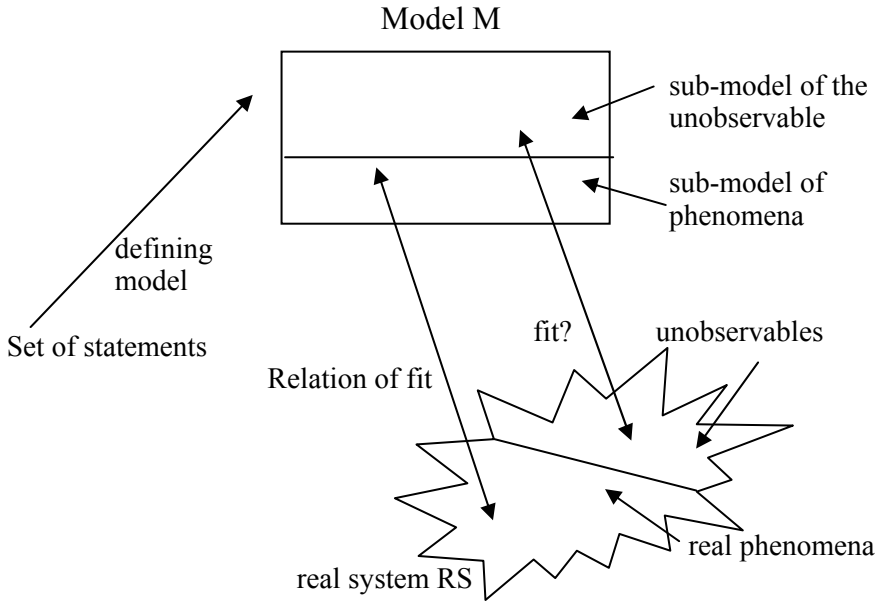
statements are true; rather it involves the belief that all and only its observational claims are true (i.e., empirically adequate).

The main contrast with realism involves replacing the realist value of truth by the value of empirical adequacy, and putting greater emphasis on the notion of acceptance. Note that there is some agreement with realists. Like constructive empiricists, realists also want their theories to be empirically adequate, i.e., true of all the observational matters to which they pertain. But they want more than this; they want their theories to be true of the unobservable world as well. Constructive empiricists say that this is hubris on the part of realists; they are asking for, and aim for, too much that is really unrealisable. So who is right about what we can legitimately obtain from our science? Are we to go along with the constructive empiricist and accept that we cannot really have knowledge of, say, electrons? Or are we to go along with realists who claim that we can have knowledge of electrons, including their existence (and much more as well)? This last claim is crucial to the metaphysical conception of scientific realism in which (with an appropriate acknowledgement of fallibilism) it is claimed that electrons do exist mind independently. Here we will not go into arguments for or against constructive empiricism. Our task here is merely definitional.[4] But it can be seen from the definition alone that constructive empiricism advocates aims for science that rival those of realism.

10.1.3 Introducing Models

In the above we have not been entirely true to van Fraassen's more complex position. He is not an advocate of the linguistic, or "syntactic", view of theories that says that they are merely collections of sentences or propositions. Rather his position is what is known as the "semantic" view of theories; theories are really a family of models. Usually for van Fraassen the models are highly mathematical, but they need not always be so. We will say more of models in subsequent sections. For van Fraassen, as for many others, the growth of science can be characterised by the way we have constructed models of the real world, and then compared these models for their "fit" with reality. However for constructive empiricists the fit cannot be with respect to all of reality, that is, both the observable world and the unobservable world (no matter whether they are objects, events processes, properties, etc). The fit can only be between a sub-model of the full model (or families of these) on the one hand, and observable reality, or the phenomena. There is no fit to be countenanced at the level of the non-observable. Here constructive empiricists are quite liberal with what models they envisage, and with which they are willing to work. There is one restriction, though: they bid us to adopt any model *providing* a sub-model of the full model fits the phenomena. This is the notion of empirical adequacy again, but within the setting of talk of theories as (families of) models, rather than sets of sentences.

Two divergent positions emerge between those who adopt the "semantic" conception of theories. Some remain non-realist, such as the constructive empiricists. But others are realist, and are known as *constructive realists*. The latter position is adopted by Giere (see Giere 1988, Chapter 3), and is best illustrated in the diagram of this sub-section. Let the regular rectangle shape represent a model, M. It is a regular shape because it is a somewhat *idealized* model (a notion which will be discussed in the following sections). If linguistic items are admitted, such as statements, then these can be said to define the model. But this is the only extent to which these linguistic items play a role. The emphasis is entirely on the models. In the diagram a (idealized) model M is intended to be a model of a real system RS (such as the solar system, a swinging pendulum, a projectile, a body in free fall, and the like). RS is indicated by an irregular figure, since real systems, as we all know, are much messier and have more diverse elements than do models of them.



Now we can ask: how similar is the model M to the real system RS? It may be similar in some respects and dissimilar in others. So we need to consider some measure of overall similarity and dissimilarity. Call this the "fit" of M to RS. Overall M can be a good, middling or bad fit to RS. Now for the

difference between constructive empiricists and constructive realists. For constructive empiricists the relation of fit can only hold between a sub-model of M, the phenomenal sub-model, and the observable phenomena of the real system RS. The relation of fit is not defined elsewhere, and in fact it is not countenanced at all. For constructive empiricists, either there is no meaningful relation of fit, because it is simply indefinable, or always two or more models can be empirically adequate and there is no way of picking a unique model to eliminate the others and have some unique measure of fit. Or if there are relations of fit we can never find out, or know, what they are because one end of the relation is, by the very nature of the case, not observable. (Here one can detect a version of the IRA argument discussed in Chapter 5.)

In contrast constructive realists claim that there is good sense to the idea of a fit holding between the non-phenomenal, or theoretical, part of the model and unobservable reality. This is depicted in the diagram by the double-headed arrow with 'fit?' next to it. Which view is right? Constructive empiricists, who allow only restricted relations of fit to the observable phenomena alone? Or constructive realists who employ an unrestricted notion of fit which allows us to say not only that we have, in our sciences, modelled the observable phenomena, but also that we have modelled the unobservable world as well? Again, this is not a matter that we can argue here. Our sole intention in this section is to set out what are the different kinds of realism, and some rival doctrines to realism.

We now turn to a consideration of the notion of modelling as introduced into science by Galileo, and the extensive revolutionary use he made of its implicit methodology that is realist, rationalist and has strong non-empiricist elements.

10.2 GALILEO AND THE SUBVERSION OF EXPERIENCE BY REASON

It is commonly agreed that Galileo has an important place in the history of science, particularly concerning his astronomical observations, the development of concepts such as momentum and theories of motion on the Earth, such as free-fall, pendulum motion, projectile motion, and the like. Just as importantly he made innovations in scientific method that Newton, and most subsequent physicists adopted: 'Galileo systematically applied the method of idealization. And that was the real meaning of the revolution in the natural sciences which was named after him.' (Nowakowa and Nowak 2000, p. 21). It is not always clear what Galileo's methodological breakthrough is. Nowak has given a useful characterization:

> The Galilean revolution consisted in making evident the misleading nature of the world image which senses produce. We only see phenomena which are the joint effect of all the relevant influences. As a result, senses do not contribute in the

> slightest to the understanding of the facts. In order to understand phenomena the work of reason is necessary which selects some features of the objects through idealization and in their idealized models recognizes some other features of the empirical originals. These models differ a great deal from their sensory prototypes; what is more, they present images of hidden relationships which could not be grasped with the aid of experience at all. Science idealizing phenomena opposes commonsense … (Nowak 1994, p. 123).

Two aspects of Nowak's remarks need highlighting. The first is the two-part claim that the senses can be misleading, and that our senses may not be able to reveal the hidden joint causes which bring about happenings we can observe with our senses. This leads to the second point concerning how idealizations are to be made in science, even when the idealizations and/or their consequences run contrary to commonsense and what we in fact experience. This raises issues about the important role of reason in constructing idealizations which are models of phenomena, such as swinging pendula, where such models are not given directly in experience, and may not be fully in accordance with experience but only approximately so. If Nowak is right about Galileo's methodology, then it provides an important contrast with any account of scientific method that is too strongly oriented to empiricism, or claims that science is a "construct" out of experience, one of the common central tenets of epistemic constructivism within science education. So a further aspect of this chapter will be devoted to noting the contrast between a methodology based on Galilean idealizations and the tenets of constructivism in science education. This latter doctrine adopts the epistemological thesis that scientific knowledge is a construct out of experience for scientists, and then extends this to a theory of learning for students of science.

A quite different kind of construction goes on in Galileo's science (and the science of others), as characterized by Nowak. If we can expand the "construction" metaphor, we can say that scientific knowledge often is a construct out of reason. The role of reason is two-fold; in the first place to "construct" idealizations or models, and then to make inferences from the models about possible observations that might only fit our experience to some degree of approximation.

Here we cannot discuss the various views that historians and philosophers of science have taken about the contrasting *a priori* or rationalist *versus* empiricist approaches that Galileo took in his science. Suffice it to say that many note the often strong "rationalism" to be found as opposed to the empiricism of commonsense or sense perception. As a single illustration from historians, consider Alexander Koyré, an eminent historian of Galilean science. He expresses a kindred contrast when he speaks of the difference between thought in Galileo and an appeal to experience that is often infused with commonsense views about the world that he wishes to challenge. This, in Koyré's view, is particularly the case in our understanding of motion. He says that for Galileo our natural ways of imagining lead us to talk of effort

and impetus. But we need to overcome these natural tendencies through thought which leads us to the more appropriate notion of momentum, a notion which is in many respects "unnatural" to us. The contrast he expresses as follows:

> Thus we must choose: either to think or to imagine. To think with Galileo, or to imagine with common sense. For it is thought, pure unadulterated thought, and not experience or sense-perception, as until then, that gives the basis for the new science of Galileo Galilei. Galileo is perfectly clear about it. (Koyré 1968, p. 13)

More dramatically Koyré sees the new approach that Galileo took in building mathematical models of motion as a victory of a more Platonic and abstract idealizing approach to science over that of Aristotle and Galileo's contemporary Aristotelians who appealed to experience: 'for the contemporaries and pupils of Galileo, as well as Galileo himself, the Galilean philosophy of Nature, appeared as a return to Plato, a victory of Plato over Aristotle' (*ibid*., p. 15). Though such contrast can mean many things, the emphasis on Plato will be understood here as one in which reason plays a major role in the construction of idealized models in science, even where such models, and what they give rise to as allegedly observable consequences, may go against commonsense.

Is Nowak (amongst many others such as Koyré) right in his characterization of Galileo's methodology? Much evidence for it can be found in Galileo's own writings, a little of which will be indicated here. Nowhere does Galileo give an explicit account of his method. But as he develops his theory of motion he makes comments aside about his methodological procedure. We will look at just a few of his comments. First, there is the famous passage in the *Two Chief World Systems* in which Galileo says, through his mouthpiece Salviati, that we should make reason conquer the senses:

> You wonder that there are so few followers of the Pythagorean opinion whereas I am astonished that there have been any …. Nor can I ever sufficiently admire the outstanding acumen of those who have taken hold of this opinion and accepted it as true; they have through sheer force of intellect done such violence to their own senses as to prefer what reason told them over that which sensible experience plainly showed them to the contrary. For the arguments against the whirling of the earth which we have already examined are very plausible, as we have seen; and the fact that the Ptolemaics and Aristotelians and all their disciples took them to be conclusive is indeed a strong argument of their effectiveness. But the experiences which overtly contradict the annual movement are indeed so much greater in their apparent force that, I repeat, there is no limit to my astonishment when I reflect that Aristarchus and Copernicus were able to make reason so conquer sense that, in defiance of the latter, the former became mistress of their belief. (Galileo 1967, p. 328)

Not only was Galileo aware that in some cases in science we would have to act and think contrary to our experiences, but he was also aware that the models we construct might not accurately fit what we experience. Thus in making the Earth move rather than the Sun, Copernicans appear to do violence to our very sensory observations as when we speak of the Sun rising

or setting, yet the moving Earth and stationary Sun are an essential part of the Copernican model of the solar system. This model is not given as some "construct" out of experience. It is still a "construct" but out of different materials, especially reason as employed in model building. As Nowak put it in the above: 'in order to understand phenomena the work of reason is necessary which selects some features of the objects through idealization and in their idealized models recognizes some other features of the empirical originals' (*loc. cit.*)

That experience can be an obstacle to model construction, especially where it is infused with allegedly "commonsense" beliefs, is a point found in Galileo and commonly commented upon by historians from Koyré to Nowak. It is also a centerpiece of Feyerabend's account of Galileo, especially his critique of the "natural interpretations" that must be exposed in reports of experience. In Chapters 6 to 9 of *Against Method* (1975) Feyerabend makes much of the point that "natural interpretations" infuse experience and our reports of experience, this being one aspect of Feyerabend's view that all observations are theory laden. In criticizing earlier theories we may also have to criticize the quite deeply hidden and embedded natural interpretations that "load" themselves onto theories, and replace them by what may appear to be quite "unnatural" interpretations employing "unnatural" concepts. Our commonsense reports of experience may, from a quite different point of view, be a quite unsuitable base from which any new theory can be constructed. On Feyerabend's understanding of Galileo's method, we need to overcome, and even contradict, the commonsense deliverances of experience, especially when the developments of new, profoundly deep theories are being established.

Feyerabend makes much of the above quotation from Galileo, and many other remarks, that show that experience, and reports of it need to be subverted. And he goes on to cite other remarks from Galileo such as 'they [the Copernicans] were confident of what reason told them' as opposed to the deliverances of experience upon which the Aristotelians relied. And again he cites approvingly Galileo saying 'with reason as his guide he [Copernicus] resolutely continued to affirm what sensible experience seemed to contradict' (Feyerabend 1975, p. 101). Particularly significant for Feyerabend's understanding of Galileo's procedure is the latter's discussion of the tower experiment in which a rock, dropped from the top of the tower, falls to the bottom. Does this show that the Earth is stationary, as many Aristotelians argued? Or does this result have to be understood anew if the Earth is taken to be rotating? On this Galileo says: 'for just as I … have never seen nor ever expect to see the rock fall any way but perpendicularly, just so do I believe that it appears to the eyes of everyone else. It is therefore better to put aside the appearance, on which we all agree, and to use the power of reason either to confirm its reality or to reveal its fallacy' (Galileo 1967, p. 257; cited in Feyerabend 1975, p. 7; Chapter 7 deals with Galileo's "tower

experiment"). Galileo uses strong words when he says that there may be a *fallacy* in experience to be overcome by reason. He gives an illustration of this when he continues, saying that 'one may learn how easily anyone may be deceived by simple appearances, or let us say by the impressions of one's senses. This event is the appearance to those who travel along a street at night of being followed by the moon, with steps equal to theirs, when they see it go gliding along the eaves of the roofs. … an appearance which, if reason did not intervene, would only too obviously deceive the senses.' (*loc. cit*.)

Galileo's point is well taken. For him, neither the deliverances of our senses, nor even our commonsense beliefs, are a sufficient basis for science; both may be called into question or even overturned if reason requires. And the converse can also be the case where what reason delivers fails to accord with experience. For Galileo neither dominates the other; instead there is a complex dialectic between the two. Of interest here are the cases where our natural presuppositions built into our experience must be called into question and replaced by what seems "unnatural" if science is to advance at all (the understanding of what we observe in the "tower experiment' being a case in point).

We must be careful about the two aspects of the points being made. The first point concerns the understanding of the role of experience in science. The very building of models may, in some examples Galileo considers such as that proposed by Copernicus for the solar system, go against the commonsense view of the world. Reports of relevant experience may contain "natural interpretations" of the world that are second nature to us, yet they must be overturned if science is to advance. For Galileo the very same lessons that the Copernican model of the solar system taught us are to be extended to our understanding of motion itself (as Koyré emphasises), and to the construction of models for particular kinds of motion such as projectiles or swinging pendula. Here the second point emerges in that particular models of phenomena we can observe may be idealizations; the observational consequences of these models will involve approximations that may be at variance with what is experienced. In what follows we will focus on this second aspect of models as idealizations. But given the account of Galileo's methodology so far, the fact that a model may well be inconsistent with what we (report of) experience shows that an empiricist account of science in which theory is somehow a "construct" out of experience is at variance with, and cannot capture, Galileo's methodological procedure. What is missing is the crucial role of "reason", as we may put it, in providing models in science, and in persisting with these models even when they go against experience.

10.3 GALILEAN METHODOLOGY AND IDEALIZATION

Let us now turn to model building. In constructing models we may make a number of idealizations; but these can be made more concrete when we drop

some of the idealizations and approach something like the real systems we are investigating. Terms (or their Italian equivalents) such as 'concrete' or 'material', which are applied to ordinary real objects, are contrasted by Galileo when he talks sometimes of 'ideal' but more commonly of 'abstract' or 'immaterial' objects. As an illustration, see the many passages (for example Galileo 1967, p. 206-8) where Galileo talks of material planes and spheres in contrast to immaterial planes and spheres and considers what might happen in concrete actual cases when they touch as compared to ideal or abstract cases. Part of the matter for debate is whether actual spheres and planes touch in one or many points in contrast with ideal planes and spheres which are said, by their very definition, to touch in only one point. For Galileo's opponents it is obvious that real, material spheres will, by their very weight, press down on a plane over many points. As a result they cannot see the rationale for adopting such an abstract and idealized model that is defined into existence and that is not true of real, material planes and spheres. Galileo's response is to say 'I grant you all these things but they are beside the point' (*ibid*., p. 206), the point being one about idealized spheres and planes and not their actual counterparts.

The above terminology of 'abstract' and 'concrete' is, for us now, a quite natural mode of discourse to adopt, and such usage introduced by Galileo has become part and parcel of the discourse of contemporary theorists when they discuss the processes of model building in science. In comparing the manner in which idealized and perfect spheres and planes touch when compared with real spheres and planes, Galileo says:

> just as the computer who wants his calculations to deal of sugar, silk, and wool must discount the boxes, bales and other packings, so the mathematical scientist (*filosofo geometra*), when he wants to recognize in the concrete the effects which he has proved in the abstract, must deduct the material hindrances, and if he is able to do so, I assure you that things are in no less agreement than arithmetical computations. (Galileo 1967, p. 207)

As a further example of idealization and model building we find Galileo responding in the *Two New Sciences* to objections to his method of idealization through Salviati when he says of projectile motion:

> All these difficulties and objections which you urge are so well founded that it is impossible to remove them: and, as for me, I am ready to admit them all, which indeed I think our Author would also do. I grant that these conclusions proved in the abstract will be different when applied in the concrete and will be fallacious to this extent, that neither were the horizontal motion be uniform nor the natural acceleration be in the ratio assumed, nor the part of the projectile a parabola etc. But, on the other hand, I ask you not to begrudge our Author that which other eminent men have assumed even if not strictly true. The authority of Archimedes alone will satisfy everybody. (Galileo 1954, p. 251)

Galileo sees in the work of Archimedes the same methods of idealization as he proposes to use in his theory of motion. In the context above, Galileo is imagining a perfectly smooth ball rolling with uniform motion along a flat frictionless surface which, upon reaching its edge, acquires a downward

motion which is to be added to its original horizontal motion. Galileo's task is to give an account of this new motion, but he recognizes that the model he has given of it is ideal and does not fully specify what happens in real systems. Other participants in the *Dialogue* tell him that he has ignored the resistance due to the medium through which the object falls, he has ignored the fact that when the body moves along the horizontal plane it will have a variable distance from the center of the earth, and so on. Galileo admits that all these are idealizing assumptions, and recognizes that any models which ignore them cannot be "strictly true", as he says. Nevertheless, he does claim that in his models he has set out the *central*, *primary* or *essential* features of what is happening in such motion. It sets aside *peripheral*, *secondary* or all *non-essential* features of the motion; but these can be taken into account by dropping idealizations when the model is made more concrete.

What is important about Galileo's methodological revolution is the construction of models by reasoning about the theory of motion which, when applied to some situation, gives the essential features of the motion; these features are not given in experience at all. Moreover, the model leaves out other features that one might envisage holding of real systems, but which are inessential to the motions being modeled. The distinction between *essential* and *inessential*, or *primary* versus *secondary*, features of models is an important aspect of Galileo's scientific method. Making this distinction is not one that can be based in experience but must be determined by reasoning, in the light of the theory, about the model being constructed. It is also important to note that the models might fit the observed facts only approximately; nevertheless they do capture the essential hidden features of the motion not given immediately in experience. In the next section a theory of idealization in models will be set out, which reflects much of Galileo's methodological procedure.

Of course Galileo was aware of the important role that experience and experimentation play in science. But in his view these do not play the *only* role in determining what theories we are to accept. Importantly experience may play no role when it comes to constructing models that get to the essentials of what is happening in ways not evident in, or even controverted by, experience. An important role must be given to reason in constructing models of real systems that are then to be compared with reality itself, or with what we observe. Galileo was aware that many contemporary Aristotelians held the view that experience was the *only* determinant of what theories we should accept. Thus Galileo has his Aristotelian mouthpiece Simplicio say the following: 'Aristotle would not give assurance from his reasoning of more than was proper, despite his great genius. He held in his philosophizing that sensible experiments were to be preferred above any argument built by human ingenuity, and he said that those who would contradict the evidence of any sense deserve to be punished by the loss of that sense.' (Galileo 1967, p. 32) For Galileo the theories of Aristotle and Aristotle's contemporaries were

already, in their science, quite close to experience; there was no need to bring them any closer to experience. The problem was, however, to find an analysis, through reason, of what we experience; what the analysis reveals is not immediately evident in experience, and may even go against it. In this respect Galileo was divided from his contemporaries over the role experience is to play in theory construction.[5] For Galileo there is a paramount role of reason in constructing idealized models of real systems. Of course there is a role for experience in comparing models, or the consequences deduced from them, with observation or experimentation to determine the extent to which the models approximate real systems. Nevertheless experience may have to be set aside when reason is applied to model construction and development.

10.4 ABSTRACTION AND IDEALIZATION

Though there is no agreed way of using the terms 'abstraction' and 'idealization' in the literature, they can be used to mark an important distinction. In this section an account is given of how they will be used. In this respect we follow, but not completely, the use of these terms as in Nowak (1994), and Nowakowa and Nowak (2000). Once these terms have been carefully defined it will be seen that the distinction drawn has considerable consequences for our characterization of theoretical models.

All familiar everyday objects, or the unfamiliar postulated in the sciences, have (intrinsic) properties such as colour, weight, charge, etc; and they have relational (or extrinsic) properties such as position, spatial relationships to one another, ownership relations, etc.[6] Now consider some real object such as a blob of metal stuck on the end of fine wire (i.e., a pendulum). From the point of view of mechanics we are not interested in some of its extrinsic properties, such as who owns it. But note that from the point of view of, say, theory in economics or sociology, whether or not it is owned, or who owns it, is of interest. We will say that the dynamicist *abstracts* away from the extrinsic or relational property of ownership in that it is irrelevant to the dynamicist whether it is owned or not. The economist does not *abstract* away from such an extrinsic property since it is part of his science to consider ownership relations; but the economist does abstract away from the energy properties of the swinging pendulum. Both the dynamicist and the economist will abstract away from its relational property of being so far from, say, the Grand Canal in Venice. Neither have an interest in their theories with this relational property of the real object. Both will also *abstract* away from intrinsic properties such as colour; what colour the pendulum has is not a matter of interest in their theories. Any real, existing, actual, pendulum will have some colour (including black or white). But what colour it has is irrelevant to both theories of dynamics, or economics (but maybe not irrelevant to some other theory to do with the optical properties of objects, or the aesthetic properties prized by an art collector).

In general, we may say that we *make an abstraction* from a real object, such as a pendulum or the Moon, when the real object has a property P but it is of no concern to, or it is irrelevant to, some theory T whether the real object has that very property P. Note the emphasis here on *real* or *actual* objects and their real and actual properties. The actual object is of interest to some theory T (of dynamics, or of economics) and so are some of its actual properties (such as respectively, mass or production cost); but other actual properties are not considered in theory T (e.g., for both dynamics and economics, the colour of the object). Finally we need to note that some abstractions may be erroneous. Thus classical dynamicists erroneously thought that they could abstract away from the frame of reference in which a body was moving and talk about, say, its absolute mass. This is an erroneous abstraction, as pointed out within Einsteinian Special Theory of Relativity. Of course, scientists might make some such abstraction for various purposes and later discover that they were wrong to make it. Dealing with such matters takes us into the territory of idealization (as defined here.)

The term 'idealization' will be used differently. In the case of abstraction an object is still a real object with property P, but we ignore property P for certain purposes such as whether it is a property with which our theory deals. In the case of idealization we do not merely ignore a property; we regard P as a property that the object *definitely does not possess*. Thus we idealize humans when we consider that they are always just, honest, loving or act with good intentions. It is not that we merely ignore or set aside their unjust behaviour, their dishonesty, their propensity for hatred, or their bad intentions. Of course, we could merely abstract from these, in the sense of allowing that we have these negative features but we simply set them aside. When we idealize humans and think of beings that lack these negative characteristics, we are not talking of real humans at all. They have features in common with humans, but they are not actual human beings; they are more strictly akin to a God, or are angels or saints. What we are talking about is best described as an *idealized* human. Here the word 'idealized' carries with it the connotation that it is not a real item to be found in the actual, real world. Nevertheless it is still an item of our scientific discourse that we can characterise as "abstract" or "ideal".

Consider now real tables, chairs, rocks, plants, and animals. These all have some colour or other. Now if we abstract from the colour of these items then we are still considering these items as real items but ignore their colour properties. What happens if we idealize in the sense above, and consider them as definitely lacking colour properties? Most would agree that no item can be real and yet not be coloured in some way. It is not that we have here a real object, but that it is colourless. Rather we do not have a real object at all. Yet we still have an object of some sort since we continue to claim that it has other properties such as mass, or volume, or inertia or the power to gravitationally attract. In so far as the object *lacks* colour we can conclude

that it cannot be a real item that we can bump into in our real spatio-temporal system. We do not find middle-size objects lacking colour in the real spatio-temporal system. Yet such "objects" lacking colour are not nothing. They are, let us say, *ideal* objects or *idealizations* of real objects. Such items are, to use the terminology of philosophers, *abstract* objects and not *concrete*, *actual* objects found in the space-time system. Note, however, that we do attribute temporal and spatial properties to ideal objects even though we do not expect to find such ideal objects in the actual space-time system we inhabit.

Another example might also help. Often in dynamical theory we consider objects to be point-like, that is, they lack any volume. Thus in simple models of the solar system the Sun, the Moon, and the planets are often considered to be point-like objects with forces either acting from, or acting upon, that point. Is there such a thing in the world as a point-Sun, point-Moon, or point-planet? No' but in dynamics they are not nothing. They are, let us say, idealized objects, or idealizations of real objects; they are not objects to be found in the real (or actual) world. We might be able to show, by means of a proof along the lines suggested by Newton, that treating an object as point-like with all its mass at its centre of gravity, is equivalent to a body with volume in three-dimensional space and with its mass evenly distributed about the centre of gravity. Such an equivalence does not necessarily undermine the main issue, viz., that we are still idealizing an object when we treat it as if it were point-like. What such an equivalence does show is that we are not making an idealization which is at a large distance from reality; in one respect the idealized and the real object share some common features, such as gravitational attraction, which are the object of investigation.

Hopefully, the illustrations should make the difference between abstraction and idealization clear without setting out necessary and sufficient general characterisations of what these terms mean in this context. We abstract from real objects when we still consider them as real objects but ignore some of their properties. We do not say they *lack* these properties; rather we set them aside for reasons to do with our theories and what properties of real objects we wish to consider. When we idealize, we are no longer considering real or actual objects, but non-real or non-actual ideal objects. This is so because we consider the object to lack some of the properties that would be necessary for it to have if it is to be a real object. The difference here is between an *epistemic* matter, as when we ignore properties while abstracting, and an *ontological* matter, as when we claim that an object lacks, does not have, certain properties when we idealize. In both cases it is humans that do the abstracting, and do the idealizing. To some extent the metaphor of humans constructing such objects can be helpful. Human activity is involved in both cases; in particular it is we who construct ideal objects since they do not actually exist outside our idealizing activities in science.

Just how far can the idealization process go? It can go quite far, but not so far as to denude the ideal object of all the properties of its real counterpart. In treating a real object as, say, a point-like object one has gone a considerable distance in that the point-like object is considered only to have, say, mass, inertia and the power of gravitational attraction, along with velocity and acceleration, viz., just those properties of dynamics. Using some notation developed by Nowak let us assume that some real object, or class of real objects, O, have real properties A, B, C, …, Q. We can represent the object as a whole by 'O{A, B, C,…, P, Q}'. In idealizing we consider O to lack properties, say, D to Q and indicate this by a negative sign '~'. Thus we can represent the idealized object as 'O{A, B, C, ~D, ~E,…, ~P, ~Q}'. Nowak then suggests that the remaining properties, A, B and C, may be linked in an idealized law L (see Section 10.5), where L is a function of the positive properties O has, such as A, B and C only. We can represent such a law as follows: L(A, B, C), where in the law A, B and C are related in some way. For example, such an idealized law might be Newton's second law, which relates only mass, force and acceleration, **F** = m**a** (where bold **F** and **a** indicate vectors). The law clearly does not relate other properties if we abstract away from real situations. Nor does it relate other properties if we idealize the objects concerned since under such idealization the objects do not have any of these other properties.

Given the above we can now easily introduce Nowak's idea of concretization. This is the reverse of the process of idealization. It is the process of making ideal objects more like real actual objects, by attributing to the ideal object more of the actual properties possessed by its real counterpart. Thus consider ideal object O{A, B, C, ~D, ~E,…, ~P, ~Q }. This might be made less ideal, or more concrete, by regarding O not as lacking properties D and E but as actually possessing them while still lacking F to Q. In so concretizing we move to an object which is still ideal, viz., O{A, B, C, D, E, ~F,…,~Q}; but we do move in the direction of greater concretization. In complete concretization we would consider an object with all its real properties.

On what grounds are some properties retained in an ideal object, and others abandoned? This is a question that can only be answered by considering what properties are postulated in a theory and what resources there are for handling them. Thus, from the point of view of certain idealizations, the properties which are retained are said to be *essential* or *primary* for the understanding of the phenomena under consideration, while the abandoned properties are, as Galileo indicated, to be *inessential* or *secondary* to that understanding. This is not to say that these inessential or secondary properties are unimportant in all respects; they can be accommodated in models which are more concrete, and so do come to play some role in understanding the phenomena being considered. But they are not essential to that understanding, as Galileo says.

Note that so far we have treated only *objects* as idealizations, and have characterized this in terms of properties they are considered to either have or lack. We could do exactly the same thing with the *properties* themselves and treat them as idealizations from real properties in much the same way; some of the features of properties are retained while others are abandoned. Thus we might idealize a property of being an ovoid, or oviform (egg-shaped). Such properties might be mathematically difficult to deal with; so initially we might treat the property as the idealized property of sphericity, and later the property of being ellipsoidal. In such a case there is an initial idealization of the property as one of sphericity; this is made progressively more concrete, but still ideal, as when we move to considering the property of being ellipsoidal. Such property idealization might be viewed as a case of object idealization, as in the case of the Earth. In dynamical theory initially the Earth is considered to be point-like. With successive concretization it was considered to be a perfect sphere, and then a sphere rotating about an axis, and then an oblate spheroid rotating about an axis. But if we view this as a case of property idealization we need to prescind from the objects in which the properties are instantiated and adopt a more Platonic view in which we consider the geometrical properties themselves, and not their instantiation in some object. In this way the sequence of properties, say, from being an ovoid, to being ellipsoidal, and then to being spherical, can be regarded as successive idealizations (in which, say, the different lengths of the axes and their different points of intersection in the case of an ellipsoid become identical and yield sphericity). Laws can also be idealized; this is the topic of the next section.

Now we can introduce the notion of a *theoretical model*. This is a set of idealized objects having idealized properties and being in idealized relations to one another and obeying idealized laws. An excellent example of this is provided by Lakatos in his discussion of scientific research programmes in which he illustrates how they might progress by considering a sequence of models of the solar system that begin in a highly idealized manner but become successively more concrete (Lakatos 1978, p. 50). He says that Newton began by considering the solar system as a collection of separate planetary systems, one for each point-like planet orbiting a point-like Sun as centre of gravity.[7] It was known, even by Newton as he worked on his models, that the centre of gravity of a single Sun-planet model cannot be where the point-like Sun is, but outside it. When Newton dropped this idealizing assumption, he worked on an even more concrete, yet still ideal, model by considering all the planets in one unified model, but only under the action of the Sun's gravitational force. Clearly the theory on which Newton was working suggests even further concretizations, such as: allowing the planets to gravitationally affect one another (this gives rise to the difficult many-body problem in dynamics); removing the assumption that the Sun and

planets are point-like objects; allowing the planets to rotate about an axis, or even wobble; and so on.

Importantly, Lakatos considers how such progressive concretization can occur even when it is known by those developing the models that they are inconsistent with what can be observed, and even always remain so. Lakatos' main point is that model building is an entirely theoretical process, or to use the vocabulary of Galileo, is a rational process. It is not a "construction out of experience", though empirical claims are an input into the process of model building. In fact Lakatos' discussion of this issue is in a section with a title that includes the phrase 'the relative autonomy of theoretical science'. Here the autonomy is of what we can construct from theory independently of what we might be able to construct from experience, a point about model construction on which Lakatos and Galileo are at one.

Such models are common in dynamics, and they also have a role in the rest of physics and chemistry. The role of models also looms large in the human sciences from psychology to economics. In economics one can find a range of models about the economic behaviour of rational humans being made more concrete by adding empirically discovered hypotheses about actual human behaviour in economic situations. (It should be noted that there are many different senses of the term 'model' at work in the sciences, and different kinds of model as well. Here we are considering only one kind of model, that defined above.[8])

10.5 IDEALIZED LAWS

Some philosophers of science would argue that all scientific laws are really idealizations of what happens in reality and that no law is strictly obeyed; however, as the laws are made more concrete they come to apply more accurately to real observed happenings. Since this will be the position adopted here, what needs to be done is to give an account of what an idealized law is like. We will find that this characterises quite closely Galileo's approach to the laws he discovered. In what follows we will adopt the view of idealized laws set out by Nowak and his school.

Let, I_1, I_2, …, I_n, I_c, be n+1 idealizing assumptions made about the objects and properties and their magnitudes in some theoretical model. For example, these might be the assumptions made in models of the Solar System that the Earth has zero volume and is a point-like object, or that it does not rotate; or that it is a perfect sphere, or that its centre of gravity is the geometrical centre of a perfect sphere, and so on. In the case of free fall the idealizing assumptions often include the claims that the body falls through a constant gravitational field, or that it experiences no drag of the air as it falls, and so on.

The final idealizing assumption has been labelled as 'I_c', where the letter 'c' stands for a 'catch-all' idealization. That is, the last idealizing assumption

says that $I_1, I_2, \ldots, I_n$ are *all* the idealizing assumptions to be made. Of course we could be wrong about this and there may be a number of matters we have not taken into account in making our idealizations about them; we might not even be aware of them. For example, early work on the solar system assumed that the only forces at work were gravitational and inertial; but we now know that this is false. If the idealized models of the solar system did not include a catch-all idealization, then the model would have failed to idealize only the dynamical forces under consideration. So there is a need for a final catch-all idealization assumption.

Now we can set out a general schema for an idealized law, and then illustrate it in some simple cases. The general schema is:

If $I_1, I_2, \ldots, I_n, I_c$, idealizing assumptions were to hold then law L would hold.

Let us fill in this schema with some particular cases, the first being Newton's First Law of motion (stated in the *Principia,* Book I, under 'Axioms, or Laws of Motion'): 'Every body continues in its state of rest, or of uniform motion in a right line, unless compelled to change that state by the forces impressed upon it'. Now as many have noted, there may be no example of a body in the entire cosmos that is under the action of no forces. It is not that the sum of the resultant forces acting on it is zero; rather there is always some resultant non-zero force due to gravitational attraction, or whatever. So there is no actual instantiation of Newton's First Law in the entire cosmos. But this does not mean that the Law is false; rather it is an idealized law that has an idealizing condition about zero resultant force in an antecedent clause. So understood the Law is unproblematic, even in an idealized universe in which there are only gravitational forces which are ubiquitous throughout space and time.[9]

Consider now Galileo's law of free fall, $s = \frac{1}{2}gt^2$, already discussed in section 2.5 in relation to the notion of explanation. Recall that this law describes a freely falling body O, where s is the distance fallen by it and t is the time of the fall from initial movement to impact on the surface of the Earth, and g is the acceleration on Earth due to gravity. The idealizing assumptions involved in this law can be described as follows.

I_1 is the assumption that O falls in a perfect vacuum and suffers no air resistance; that is, if F are the forces due to air friction then I_1 is the assumption that $F = 0$.

I_2 is the assumption that the Earth's gravitational field exerts, contrary-to-fact, a constant force at all times in the fall of O; that is, g = constant, and is not, say, a function of distance.

There is also the "catch-all" idealizing assumption that the model specifies all and only the items to be considered in the model and that none have been left out. Of course the idealizations that have been adopted do not hold in real systems; in particular this includes a further idealizing "catch-all" that only the dynamical forces mentioned in the model are the forces at work.

It is fairly evident in a case like this what are the essential or primary features of free fall motion and what are the inessential or secondary features of that motion. It is also evident that such a law will not give a correct account of free fall; in particular it ignores the fact that the falling body does not constantly accelerate and that there is a terminal velocity of free fall in the atmosphere due to its drag effect. Nevertheless, the inessential or secondary features can be incorporated into the theoretical model through a process of concretization that will give a better accord with the free fall motions that we can observe. What is important to note is that Galileo was fully aware of what his procedures were in considering free fall, and other motion; he was constructing a series of idealized models that could be concretized once one dropped idealizing assumptions. But the construction is not one out of experience; it is a construction based on reasoning about the hidden essential features of the motion under consideration.

A further example, that of the pendulum, will help illustrate assumptions about idealized objects, properties and laws. In most physics textbooks there is a proof, based on what is called the "simple" pendulum, of the period law $T = 2\pi\sqrt{(l/g)}$, where 'T' is the period of swing, 'l' the length of the pendulum, and 'g' the acceleration due to the Earth's gravity. The same textbooks usually make clear the idealizing assumptions made in the deduction of the law, all of which Galileo was one of the first to be aware.

Compare the idealized model of a simple pendulum with a real swinging device such as wire attached at one end and to the other end of which is attached a lump of lead, and the whole allowed to freely swing back and forth. In the idealized model of such a real system the top of the simple pendulum is suspended from a frictionless point; the body of the pendulum is a weightless, dimensionless, frictionless, rigid (and so idealized) line-like rod. The bob is attached to the free end; but it is treated as a point-like mass that swings in a plane. The gravitational centre of the whole system is situated at the same position as the centre of gravity of the point-like mass.

Such an idealized model fits the real system of the pendulum only to some extent. In the real system the pendulum is attached to a joint where there is a frictional force affecting the motion of the pendulum. After a short while the real pendulum will have slowed considerably while the idealized one swing indefinitely. The pendulum rod also swings in the air, which causes friction, as does the massive blob that is attached to the free end of the pendulum. The air friction is not always a linear function of the object's velocity; usually it is a non-linear function. This pertains not so much to the idealized objects in the model as to the idealized laws in which, in most cases, all frictional forces are disregarded. The bob itself is not point-like and occupies volume. And the centre of gravity of the whole system is not situated at the centre of gravity of bob, but some distance away from it.

The model also assumes that the Earth's gravitational field is uniform. This would be the case if the Earth were a perfect homogeneous sphere, but it

is not. Moreover the field is affected by the buildings, hills and mountains, not to mention the changes in the field due to the presence of the Sun, Moon and the planets. There is also a central assumption about the restoring force function at work. It is usually assumed to be linear, and is generally known as 'Hooke's Law'. The force varies with the distance x along the arc of the swing, $F = -kx$ (where k is a constant and the minus sign indicates the direction of the force is back towards the vertical). Using Hooke's Law, and the force due to gravitational attraction, one can then deduce a formula close to the period formula given above, except that there is a trigonometrical function of the angle of swing, θ, to take into account, such as $\sin\theta$. But where the angle of swing is small, one can idealise and can set $\sin\theta \approx \theta$. Granted all these idealizing assumptions, the period law, first noted by Galileo, can be deduced from the model. Of course with various kinds of concretization a better swing law can be deduced. (This is discussed more fully in other places such as Morrison (1999), Section 3.4.1 entitled 'Theoretical models meet the world').

If the swinging pendulum were to be of a different sort, such as that in a Grandfather clock, then different idealizing assumptions would have to come into play. One important assumption of the ideal pendulum model is that nothing is said of the periodic force acting on the Grandfather clock pendulum by the escapement mechanism. This abruptly starts and stops the motion. Further, through a system of falling weights or a spring, it applies a force that prevents the swings from gradually decreasing its amplitude, thereby ensuring a more even way of indicating time. The real system of the pendulum of a Grandfather clock is at a considerable distance from the idealized model of the simple pendulum. Less ideal models which move in the direction of greater concretization, and which employ more mathematics, will approach such a real system more closely.

As a final example to illustrate how a series of concretizations can lead to a succession of less ideal laws, consider the case of the Boyle-Charles Law

(1) $PV = RT$

where P is the pressure of a gas, V its volume, T its temperature and R is a constant. This law can be deduced from the (idealized) laws of Newtonian mechanics as applied to a simple model of a gas in which the gas molecules are perfectly elastic corpuscles bouncing off one another and the perfectly elastic walls of the container; it is also assumed that they are point-like and take up no room in the container. If the assumption that the gas molecules take up no room is dropped through one concretizing move, then van der Waals recognized that the above equation becomes

(2) $P(V - b) = RT$

where b is a factor related directly to the volume occupied by the molecules. He also recognised that if one takes into account the fact that the gas molecules attract one another thereby changing their force of interaction with

the container then a further equation can be developed to take into account this concretization:

(3) $(P + an^2/V^2)(V - b) = RT$

where 'a' is a constant and 'n' is the number of molecules.

This last equation is really a cubic equation in V. Yet other equations, based on models of the molecules that are quite different from those just mentioned, lead to further equations such as:

(4) *Dieterci*; $P(V - b) = RT(\exp -a/VRT)$

(5) *Bertholet*: $(P + a/T\ V^2)(V - b) + RT$

(6) *Virial Equation*: $PV/RT = 1 + B/\ V + C/V^2 + D/V^3 + \ldots$

(where A, B, etc are functions of T for which further mathematical equations are to be given, And (4) and (5) are named after physicists).[10]

Further concretizations are possible when one takes into account electrical attraction forces, quantum effects, and so on.

What the above sequence of concretizations of the first ideal gas law equation (1) shows is that from each equation lower down one can deduce the equation immediately above by setting certain values in the lower equation equal to zero. Thus, there is an important relationship between the various idealized equations as concretization takes place. More generally, this relationship can be expressed by what has been known since Bohr as the *Correspondence Principle*. Roughly, this says that a preceding law should be obtained from a subsequent law as a special case of the latter, especially when some factor is set as zero. This principle applies not only in Quantum Mechanics but also in, for example, the Special Theory of Relativity in which classical laws of motion can be obtained by letting the velocity of light go to infinity, in which case the 1/c factor tends to zero. As can be seen, the Correspondence Principle applies to the above gas law equations and is simply another expression of increasing idealization, or in reverse, and expression of increasing concretization. As such the Correspondence Principle is an important notion in science given a rationale in terms of the account of idealization developed here.[11]

10.6 THEORIES, MODELS, REALITY AND TEST

In this section a number of the above points about ideal models will be brought together with earlier chapters on methods of test.

As we will use the term here, a *theory* has two components: a non-linguistic item which is a *theoretical model* or set of such models, and a linguistic component which is broadly speaking a set of *theoretical statements,* usually laws or general principles (and which are usually idealised). A *theoretical model*, M, is a system of idealized objects, with idealized properties. The theoretical statements T will contain (idealized) laws; these are often used to "define" features of the set of models. To illustrate, we could consider an idealised model of a pendulum, that of the simple pendulum, in which there is no friction at any point in the system, the bob is point-like, the centre of gravity of the system is where the bob is situated, and so on. Newton's laws of motion, in their idealised form, help us define, or "construct", such a model, the components of the ideal model obeying the laws by fiat of the construction. This much has been set out in Diagram 1 on the left hand side.

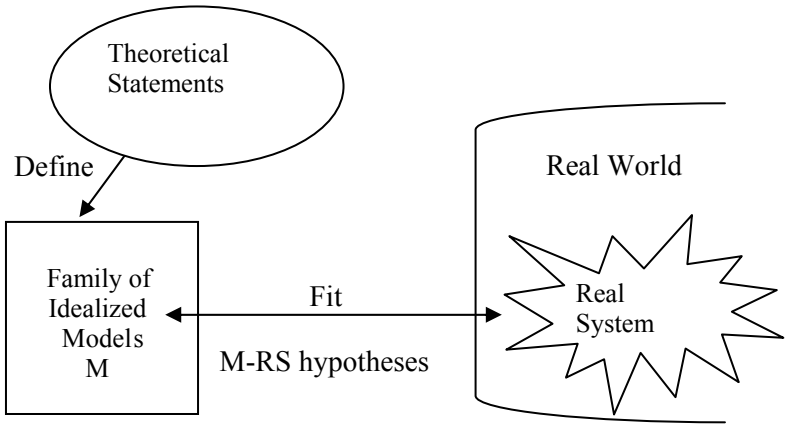


Diagram1

We can now ask just how well such a model M (of pendulum motion or ideal gases) *fit*s some real system RS (a real swinging pendulum or a real gas). The notion of *fit* has already been introduced in Section 10.1.2. Clearly

many features of real systems will be absent from idealised models; the more that are absent the less the similarity of M to RS, and so the less the measure of fit. But with successive concretizations of M so that it approaches RS more closely, the higher will be the various degrees of similarity, and so the greater will be the measure of overall fit. At the bottom of Diagram 1 the double-headed arrow indicates the relation of fit between the model M (its idealized features indicated by a regular square), and a Real System, RS (indicated by an irregular figure, since reality can be messy). It also sets out

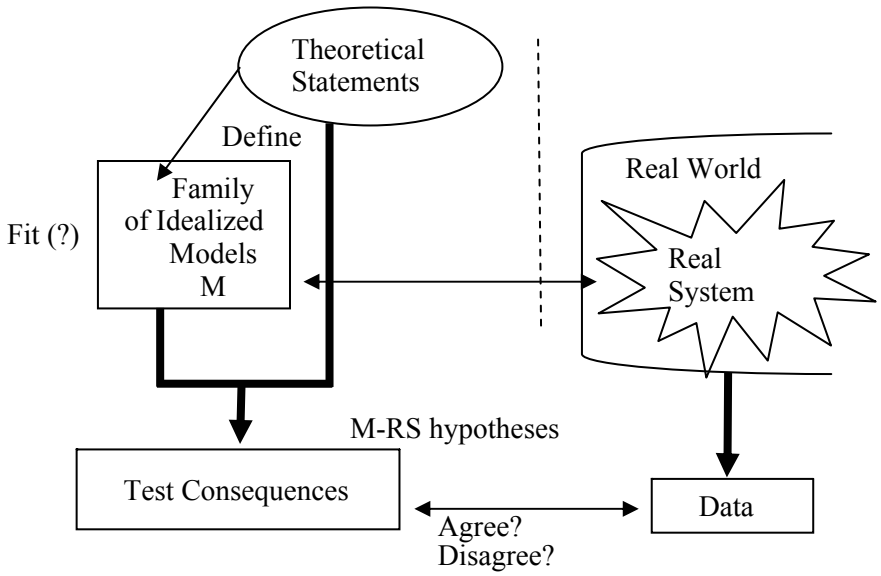


Diagram2

the relations of fit that hold between M and RS. We could, if we wish, describe the ways in which M and RS are similar to one another or are

dissimilar; that is we can describe the extent to which M does or does not fit RS. Such descriptions we can call the 'M-RS hypotheses' that set out the way in which M and RS do, or do not, differ. Here we have an analogue of the degree of truthlikeness, or of verisimilitude, possessed by a theory T and its models, when compared with the real systems that they purport to describe.[12]

The scientific realism implicit in the notion of fit, and the account of the relationship between T, M and RS is part and parcel of the *constructive realist* view of theories that departs radically from a *constructive empiricist* view of theories advocated by van Fraassen; this was described in Section 10.1.2.

Let us now turn to the matter of testing theories, understood to have the two components of theoretical statements and theoretical models.

We can observe real systems, such as the solar system, and obtain much observational data about planetary positions, their juxtapositions and opposition, their paths against the background of the stars, and the like. Or we can perform controlled experiments on some real system RS, such as a swinging pendulum, and record the values of certain observable or detectable features of RS. This is indicated in Diagram 2 in which, on the right hand side of the dotted line, a real system in the world is either observed, or experimented upon, and data collected (here there is a role for the naïve induction of Chapter 7). Also indicated in Diagram 2 is the relationship of fit, or lack of fit, between the idealized model M and what it models, viz., a real system RS.

Now we are in a position to consider the important matter of the agreement or disagreement (within some degree of error) between the test consequences and the data. There are two alternatives here. First, there might be satisfactory agreement (up to some degree of error) between the test consequences and the data. All that one can legitimately infer from this is that the conjunction of the set of statements T, and the (suitably described) model M have jointly passed the test. (For the sake of simplicity we set aside mention of any other auxiliary statements, not indicated in the diagram, that assist in the deduction of the test consequences.) With some theory of confirmation, one might even be able to ascertain what degree of confirmation the conjunction of these items has by the data, and how it can be distributed amongst T and M (see Chapter 8).

Second, there can be disagreement between the test consequences and the data which exceeds any compatibility within the allowable degree of error. So, if the test consequences are to be rejected as false, then so must the items from which they were derived. That is, either both of T and M are false, or just one. Which is the case? This raises issues of confirmation and disconfirmation, and the Quine-Duhem Thesis mentioned in Section 8.4.2. There are ways in which the blame for the disagreement can be assigned to M rather than T.

All models are idealized to some extent; and as such, when combined with relevant theoretical statements, they may not lead to test consequences that fit

the data within an acceptable degree of error. It is then possible to concretize aspects of the model to improve the values of the test consequences that flow from it so that the discrepancy between test consequences and data might be lessened or removed altogether. That is, when confronted with a conflict between test consequences and data, there is a procedure of concretization to be carried out to bring the test consequences and the data into accord. It might also be the case that successive concretizations, leading to the complete concretization of the model in which all idealizing assumptions have been removed, still do not remove all discrepancies. In such a case one would have grounds for thinking that the fault lies not with the model proposed but the theoretical statements T that are used in combination with the model.[13] And, of course, this will be the case if the theoretical laws of T are themselves idealized. But note also that the concretization of the models might not be able to proceed in a satisfactory manner if some of the idealising assumptions of the laws are themselves not abandoned. What is important here is the way in which concretization can be carried out, first with respect to the models, and then the models and their associated theoretical laws.

In summary, tests are possible for determining which of the theoretical laws, or the models to which they are applied, are to be accepted or rejected. The process of concretization plays an important role here in enabling us to improve on our models by dropping idealized assumptions. Such a procedure is an important part of the methodology of idealization that Galileo introduced and applied to a number of dynamical phenomena, such as free-fall or pendulum motion.

10.7 SOME CONSEQUENCES FOR SCIENCE EDUCATION

Some consequences of what we have said so far immediately follow for science education. First of all, students can see that theoretical models, with their abstractions and idealizations, are an essential part of science. Most natural and biological phenomena are too rich and complex to be investigated in their entirety, and even the simplest systems in the world (such as a swinging pendulum or a freely falling body) have countless properties and are under the influence of a number of factors. Thus, scientists construct models that are a simplification of the real systems to which they are intended to apply. Moreover, it is much easier to treat a simple system than a complex one, especially when one is using mathematical models, as is often the case. But it is important to note that simplification in the intended sense here is not a distortion of reality. Scientists can construct simplified, yet undistorted, models of real systems by abstraction and idealization. In this way scientists have a handle on how real systems would behave under certain idealized conditions. Once they know this, they can go back and apply a procedure of successive concretizations to narrow the gap between the model and the real system as much as possible.

Students may wonder why scientists take a roundabout way of studying reality if in the end they go back and 'reproduce' the real system by the process of concretization. The answer is that each concretization makes the model more complex, and sometimes it becomes impossible to handle this complexity (i.e., solve the equations). As long as the match between the predictions of the model and the observations or test results is sufficiently close, there is no need for further concretization.

Second, by understanding what models are and how they work students can appreciate the role of reason in science. For both abstraction and, more importantly, idealization are achieved through the faculty of reason, not through the faculties of sensing. Observations, and more generally experience, do of course play an important role in science (how else can we judge the truth or empirical adequacy of our models?). But reason plays its unique and indispensable role too. This is not just an abstract point about the role of reason in science. More importantly, students should be aware that modern science inaugurated by Galileo often goes against commonsense and plain, untutored experience. For instance, the latter suggests that heavier objects fall faster, that it is the Sun that moves not the Earth, and so on. It is only by constructing elaborate models with the help of reason do we come to realize that some of our most entrenched ordinary experiences are misleading and need to be corrected. This is one of the most important lessons of modern science that students should not miss.

This last matter leads on to our third point. The role of idealised models in science provides a corrective for excessive constructivist positions, such as that of Von Glasersfeld's, according to which all knowledge, including scientific knowledge, is a construction out of one's subjective experiences (Von Glasersfeld 2000, p. 4). Focusing on models shows how misleading such a conception of the growth of scientific knowledge is. It is just impossible for scientists, let alone pupils, to construct models that give us knowledge about the world 'out of their subjective experiences' for the simple reason, amply illustrated in Section 10.2, that the former often clashes with the latter. As Galileo has shown, our commonsense beliefs and experience are often inadequate to the task of obtaining a proper understanding of even quite simple dynamical matters such as what is really going on in the tower experiment, or when a pendulum swings. We need to expose misleading 'natural interpretations' that infuse (reports of) experience; and we may have to replace them by 'unnatural' (to the pupil) concepts that are more correct. Our central point here is that the whole idea of idealised models, so central to science, cannot be accommodated within either epistemic or pedagogical constructivism (which places an emphasis on what pupils can construct out of their experience).

Fourth, there is much that we science educators can learn from history of science, as the case of Galileo attests. From him we can learn about scientific methodology, the role models in science, and the intriguing relationship

between reason and commonsense. As is well known, Matthews has long been arguing for the relevance of history and philosophy of science for science education, and his recent book *Time for Science Education* (2000) builds an impressive case for it on the basis of pendulum motion. It is a gold mine for any science educator interested in how history of science can be a forceful resource for the teaching of science, and it also contains much about models and the role of idealizations in science from a historical perspective.

Finally, we hope that this chapter has shown that it is perfectly possible to adopt a realist position with respect to theoretical models in science and that there is no need to take refuge in sceptical or narrowly empiricist positions. Models can be compared with reality by drawing testable consequences from them, and they can be made more 'realistic' through the procedure of concretizations provided that the resulting complexity does not create insurmountable mathematical difficulties. Recently, there has been some work on the role and nature of scientific models and model building from a realist perspective in science education (See Halloun 2004). We are happy to see that more and more science educators are joining the realist camp and portraying a far more 'realistic' picture of science than those provided not only by radical constructivists, but also by social constructivists and postmodernists, as we shall see in the following Part III.

NOTES

[1] The definition of realism adopted here follows, to a large extent, the ontological or metaphysical account given in Devitt 1997, Part II. See also most of Niiniluoto 1999.

[2] The status of artefacts, money and social institutions and social properties and facts is given an excellent account in Searle 1995. The importance of Searle's analysis for our purposes is that his account of the construction of social reality is all within the framework of realism, and not outside realism or in opposition to it, as Searle makes quite clear.

[3] Arguments for realism are to be found in many places, three of which include: Devitt 1997 Chapter 7 and all of Part II which attempts to combat a range of antirealisms; Niiniluoto 1999, Chapter 6: Musgrave 1999, Part I.

[4] For a critical engagement with constructive empiricism see the following selection from a growing literature on the topic: Devitt 1997, Chapter 8; Musgrave 1999 Chapter 5; Niiniluoto 1999, Chapter 5.1.

[5] On this see the discussion in Matthews 2000, pp.100-7, of Del Monte who was a patron and associate of Galileo, but who had difficulties with Galileo's new science because of his own commitment to a strong empiricism that ran counter to Galileo's more rationalist approach to model building in physics.

[6] For the purposes of the discussion we will assume that colours are intrinsic properties of objects. There are other philosophical theories of colours that make them relational, or subjective, or whatever. These differing metaphysical views of the status of colour do not affect the discussion of the difference between abstraction and idealisation in this section.

[7] Here we should enter a caveat about models with point-like particles. It can be shown, and Newton did work in this direction, that a point-like particle with its mass all concentrated at the point acts equivalently to a massive body. Now massive bodies are much more like actual objects than point particles. But the equivalence enables one to move readily from the more ideal to the more concrete object. This also provides a ready justification for using the more ideal point-particle while other kinds of idealisation may lack such justification.

[8] For a range of different kinds of model to be found in science see Giere 1991, Chapter 2.3 who distinguishes between scale models, models as analogies, theoretical models and models as maps. Achinstein 1968, Chapters 7 and 8, considers these and other models. In the above the emphasis is on theoretical models only.

[9] There are several accounts of the idealized version of Newton's Law of Motion; for one account in line with the approach to idealization taken here see Nowakowa and Nowak 2000, Chapter 1, section III, especially pp. 52-3.

[10] The term 'virial' is Clausius' term, derived from *vis* for force, which has to do with the stresses due to inter-molecular attraction, repulsion and impact. These increasingly concretized equations are commonly given in physics textbooks; the source used here is Bromberg 1980, Chapter 2. For a further fuller discussion of the different models and equations and their simplifying assumptions see Morrison 2000, pp. 47-52.

[11] Much more could be said of the role of idealisation in science than has been said here. Important is the notion that our idealised fundamental laws of science are false of the actual world, a matter clearly indicated in Cartwright's 1983 book that has the provocative title *How the Laws of Physics Lie*.

[12] The account given here of theories and models is akin to that given in Giere (1998) and (1991) in support of his constructive realism, though it does differ in some respects.

[13] On testing idealized laws see Nowakowa and Nowak 2000, pp. 130-4.f

CHAPTER 11

SOCIOLOGY VERSUS RATIONALITY IN SCIENCE

One of the main points we wish to make in Parts III and IV of this book is that the involvement of the social in science has been exaggerated and, worse, wrongly conceived. We do not deny that there are social influences on science; we mention some of these in Chapter 14. But what we want to deny is the involvement of the social in the very content of science itself. Hence we have been at pains in Part I and II to set out a realist and rationalist account of science. Having presented our more positive view of science and knowledge, we are now in a position to critically evaluate a range of doctrines that are anti-realist and non-rationalist, or even anti-rationalist. These go under the labels of constructivism (social or individual), ethnoscience, multiculturalism in science, Foucault's "power/knowledge" doctrine, postmodernist accounts of science, and a host of others. We have already criticized a version of constructivism as it has become popular in science education circles. In the next two parts of the book we turn to the Strong Programme in the sociology of science, Foucault on power, Lyotard's postmodernist account of science and education, and the range of views that fall under multiculturalism.

This has significance for those in science education who would downplay principles of critical inquiry in the role they play in scientists' obtaining scientific knowledge and the role they play in pupil's coming to acquire scientific knowledge (as opposed to mere belief). If science educationalists place emphasis on social processes for evaluating and accepting scientific theories that are criticised in this chapter, then science education will have gone down a wrong path in ignoring the kind of critical inquiry advocated in Parts I and II. We will argue that often a wrong focus has been placed on the way politics interacts with science that has obscured what seem to us to be more urgent questions. In rectifying this wrong focus we wish to hark back to the social and political investigations into science of, say, Merton or Greenberg, and simply pass over the recent, and costly, distraction through occupation with the social at the expense of the rational.

The sociology of science is quite distinct from the sociology of scientific knowledge. Practitioners of the former, such as Robert Merton and Joseph Ben-David, have made a contribution to "old-time" sociology of science through the investigations they carried out into the organisation of science, its disputes, its ethos, and the like. We applaud this but do not wish to discuss it

here. The sociology of scientific knowledge of the sort practiced by the Strong Programme (SP) is a different matter with which we will take issue in this chapter. It attempts to carry sociological investigations into the very heart of science itself in order to show that the beliefs we entertain about the laws and theories of science are themselves socially determined. Previously this was sacred territory left to theories of rationality to explain. Strong Programmers ("SPers") wish to storm the bastion which is the refuge of those who think that the very content of our scientific theories is to be believed on the basis of methodological principles that are at the heart of critical inquiry. To illustrate the position under attack, we set out in Section 11.1 a model for the rational explanation of scientific belief that appeals essentially to methodological principles of the sort described in Part II.

Section 11.2 explores the opposite view, namely that beliefs in the very content of science are caused by social, historical or cultural factors, or interests in these, and do not arise from the application of principles of methodology. This is a tradition that finds its roots in Marx and Mannheim, and has been developed to its fullest extent in SP by Barry Barnes and David Bloor. A number of practitioners of the earlier sociology of science have expressed their doubts about the success of such a programme. Thus Joseph Ben-David in his essay 'Sociology of Scientific Knowledge', identifies the theories inaugurated by Marx and Mannheim as sustaining the sociology of scientific "knowledge" as a programme of investigation in the twentieth century. But to no avail, says Ben-David with reference to SP: 'No success can be claimed for the new Marxian-Mannheimian attempts to find a systematic (that is, permanent and regular, not just occasional) relationship among macrosocial location, ideology, and scientific theory. Indeed there is little reason to expect that there should be such relationships.' (Ben-David 1991, p. 462). In Sections 11.2 to 11.4 and in 11.6 we will make some critical points that go towards substantiating Ben-David's point.

It is not commonly realised that Michel Foucault's doctrine of "power/knowledge" shares a common form with SP. Whereas SPers look for the causes of scientific belief in more broad socio-politico-cultural factors (or interests in these), Foucault looks for their cause in the narrower, and more obscure, idea of power. His views are discussed in Section 11.5. There two aspects to his central thesis are distinguished and they are compared to similar claims advanced by Francis Bacon about 400 years ago. In our view the sensible core of Foucault's claims are to be found in Bacon while the less plausible aspects are original to him. We end with a discussion of SP in relation to scientific realism, since many take SP to support a constructivist account of "reality".

11.1 RATIONAL EXPLANATION OF SCIENTIFIC BELIEF

In Part II we considered some of the methodological norms that govern the assessment of scientific theories. We will use these in setting out what we will call a *rationality model* for the explanation of scientific beliefs. Such explanations must appeal essentially to methodological principles. To begin, consider an example. It was well known from the middle of the nineteenth century that the perihelion of Mercury (the point at which it is closest to the Sun in its orbit) is not a fixed point but moves by about 574 seconds of an arc per century. It was shown that Newtonian mechanics and the perturbatory effect of the other planets could account for 531 arc seconds per century. So there was a missing 43 arc seconds per century for which there was no explanation. A number of proposals were made in the second half of the nineteenth and the beginning of the twentieth century, such as: the postulated existence of a planet Vulcan within Mercury which orbited the Sun with the same period as the Earth so that it was not visible from Earth; dust clouds inside Mercury's orbit; an alleged oblateness of the Sun that made its gravitational field unequal; *ad hoc* changes to Newton's Law of Gravitational Attraction; and so on. All of these changes had to account for the missing 43 seconds, and some did quite well. Alas, there was an *ad hoc* feeling about some of the changes, including those that were subsequently shown to be false.

As Einstein was developing his General Theory of Relativity (GTR) in years leading up to 1915 he kept an eye on this problem. Finally, he found that he could explain the missing 43 seconds entirely from within the resources of the version of GTR he was developing. As his biographer tells us: 'That was the highpoint of his scientific life. He was so excited that for three days he could not work' (Pais 1982, p. 20 and pp. 253-6). As Pais points out it was important for Einstein that GTR, and that theory alone, could account for the missing seconds *without* the need for any other special or auxiliary hypotheses. This was in contrast to the other proposals for solving the problem of the missing 43 seconds. Here Einstein appeals to some methodological principle in coming to accept, or adopt, or believe, GTR. The principle is one that bids us to accept a theory if it explains some well known fact in a non-*ad hoc* way in contrast to other rivals which appear to be *ad hoc* in the solution they adopt. In *ad hoc* avoidance there is also an appeal to the unity and integrity of explanations that a theory provides. Let us suppose that some such methodological principle (call it 'M' for short) can reasonably be attributed to Einstein on the basis of historical research into this episode. Then what explanation can we offer of why he came to accept his theory on the occasion when he was so excited by it that he could not work?

A rationality model of explanation can be given in this case which is of the following form:

(1) Scientist S has a choice between rival theories, such as belief in theory H or belief in theory K;
(2) On the basis of methodological principle M it would be rational to prefer (or believe) K rather than H;
(3) S is guided by the rationality of methodological principle M;
(4) S accepts M and (correctly) applies M to the choice between H and K and determines on that basis that it is better to accept (believe) K rather than H
(5) So, S accepts (believes) K rather than H.

Here a methodological principle, M, enters essentially into the explanation of why S accepts one theory rather than another. Since we may assume that principles of method are rational, or at least reasonable, principles to adopt (this is something that would have to be established within a theory of scientific methodology), then it is appropriate to call this a *rationality* model for the explanation of scientific belief. Such a model does fit the story told above about Einstein (and it also fits many other episodes in the history of science). It was his application of methodological principles to the version of GTR that he was developing that led him to such excitement about the theory he accepted.

Others may also come to accept or believe GTR rather than its rival(s) on such grounds if they give consideration to the same theoretical context and employ the same methodological principles. And they might even develop further considerations using other methodological principles that, in turn, can explain their increasing confidence (or rational degree of belief) in GTR over its rivals. But not all beliefs in science are to be explained in this way. Explanations that appeal to principles of rationality clearly do not apply to every person who comes to accept (believe) some theory; they may believe a theory on grounds that have nothing to do with methodological principles. There are intelligent lay followers of science who do come to believe scientific theories on grounds much like those of the rationality model. What they do is replay for themselves much of the history of the original grounds for belief. The crucial point is that at least *some* must come to have their beliefs explained along the lines of the rationality model if there is to be any rationality about science at all. As we will see in the next section, a rival social-causal model of the explanation of scientific beliefs is proposed which appears to rule out the possibility of a rational explanation for any believer; it is replaced by a model of belief explanation that makes no mention of any principle of method. This is what the Causality Tenet of the Strong Programme (SP) claims, to be discussed next. We do not deny that *some* people may get their scientific beliefs in the way described by this quite different model. What we deny is that *all* believers get their scientific beliefs in this quite different way, if there is to be any rationality in science.

11.2 SOCIOLOGICAL EXPLANATION OF SCIENTIFIC BELIEF

11.2.1 Social-Causal Model of Explanation in the Strong Programme

Many attempts have been made during the 20th Century to give a sociological account of most aspects of science. More radically, many have focused not only on the external social relations of science but on its very scientific content, its laws and theories. In this respect they even surpass one of the early advocates of a sociology of scientific knowledge, Karl Mannheim. He claimed that belief in some of the sciences arose from the science's own "inner logic", thereby adopting something like the rationality model of explanation of the previous subsection. In contrast the Strong Programme for the sociology of scientific knowledge (SP) rejects Mannheim's plausible stance. SP is *strong* in the sense that *all* scientific beliefs held by all persons at any time are to be explained by social, historical or cultural factors.

SP is commonly spelled out in the following four tenets:

> (1) *Causality Tenet* (CT): It [i.e., SP] would be causal, that is, concerned with the conditions that bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will co-operate in bringing about belief.
> (2) *Impartiality Tenet* (IT): It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation.
> (3) *Symmetry Tenet* (ST): It would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs.
> (4) *Reflexivity Tenet* (RT): It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. Like the requirement of symmetry this is a response to the need to seek for general explanations. It is an obvious requirement of principle because otherwise sociology would be a standing refutation of its own theories. (Bloor 1991, p. 7)

These tenets call for some explanation. But first an important point. In Part I we were at pains to spell out the difference between knowledge and belief. Knowledge, it is argued, is a normative, critical notion. What determines whether we have an item of knowledge or not, are the norms of rationality; they mark the distinction between, on the one hand, *belief*, and on the other *knowledge* or *rational belief* (as we have developed these different notions in Part I and II). The above tenets talk indiscriminately about both beliefs and knowledge. Nothing is said of any norms of rationality that are to be involved in belief formation; in fact, as will be seen, they are explicitly ruled out. Rather than perpetuate the knowledge/belief confusion, in what follows we will formulate the tenets in terms of belief only.

What causes, or causally explains, a scientific belief held by some person? The first Causality Tenet makes it clear that two factors must cooperate together; the persons' social circumstance, and some other non-social social circumstance. Both of these are very broad categories and can be best spelled out by listing some specific factors. Consider the non-social factors first. Very

clearly there is no belief formation without a sufficiently developed brain to enable people to form beliefs and express them in language. Here biology and psychology, not to mention evolutionary processes and what they have bequeathed to us, make their causal contribution to the formation and maintenance of belief. All of this is common to humanity. Where people can differ is in the kind of sensory input that impinges on their nerve endings according to the environment in which they live. As social groups (and to some extent as individuals) we are differently positioned around the world as far as our sensory intake is concerned. But none of this constitutes a social difference; the factors appealed are still in the realm of those investigated in the natural sciences.

Where people differ is in their social conditions. Here a broad typology of social factors can be mentioned. The first is the language in which we express what our senses deliver to us in the way of sensory experience. The second are the systems of beliefs we already hold, from religious and non-religious beliefs, world-views and conceptual frameworks; these can differ from community to community. Third, there are the traditions and practices that are shared in a given community, including the educational practices whereby members of the community become "acculturated". Fourth, there are various social groups (or classes) with particular group interests that hold rival systems of beliefs and ideologies. Fifth, there are the social processes of negotiation and consensus formation, along with sanctions for dealing with wayward members of the community, that contribute to belief formation. And finally, there are the processes whereby the meanings of all words are established, including scientific words. This is not a matter which we can pursue here, but SPers make much of Wittgenstein's notion of rule following as a social practice with its social devices for approving or disapproving as we continue to use words to apply to objects such as 'swan', 'addition', etc.

We can now state SP more fully. It says that for any person who holds some scientific belief B, there is some non-social condition (such as those in the list above), and there is some social condition (such as those in the list above), such that the social and non-social conditions, taken in conjunction, cause the persons' belief that B. This claim underpins the *social-causal model* of explanation of scientific beliefs advocated in SP. This is close to the official expression of the Causality Tenet given above. The model for the explanation of belief for SP is:

(1) Scientist S is in social (historical, cultural) condition C;
(2) There is a causal law linking condition C with a person's having beliefs of kind B;
(3) So, Scientist S has belief B.

This is the schema suggested by the Causality Tenet that can be fleshed out in various cases (as will be illustrated in Section 11.2.2). The schema also omits

reference to the non-social conditions that also cause belief. These can be assumed to be part of the causal background against which the social conditions play the decisive role of belief formation.

But this is not the version usually found in the various case studies which apply the social-causal model. There is also an appeal to a person's *interests* in their social, political, or cultural circumstance. It is this *interest*, and not their objective circumstance, that is said to be the cause of their scientific belief (the interest being taken in conjunction with the non-social factors that are also involved in causing of belief). The *social-causal interests* model can be set out as follows:

(1) Scientist S has an interest in social (historical, cultural) condition C;
(2) There is a causal law linking the interest in C with a person's having beliefs of kind B;
(3) So, Scientist S has belief B.

In what follows when we refer to the social-causal model of explanation advocated by SP we will mean either the first model, which makes no mention of interests, or the second model which does mention interests. In both models there is an essential appeal to a causal connection or law linking kinds of social conditions, or interests in them, and kinds of belief. Note that neither social-causal model makes an appeal to methodological principles in the explanation of why scientist S has a particular scientific belief. Clearly the onus is on advocates of SP to establish the causal laws, specified in the second premise, which link kinds of social circumstance, or interests in them, with kinds of belief. It goes without saying that such laws are in short supply; hence the complaint of Ben-David cited at the beginning about the paucity of laws needed to do the requisite job of explaining.

There are a number of points to note about this expression of Causality Tenet. First, social and non-social conditions must act in conjunction to bring about belief. The non-social conditions cannot by themselves bring about belief. If they did the Causality Tenet would be falsified. This underlines one way in which the Strong Programme is alleged to be strong. Second, note that what does the causing is a conjunction of naturalistic properties postulated in the sciences such as chemistry, neurophysiology, biology, psycho-linguistics, and importantly sociology. In this respect SP takes its place within the overall philosophical framework of naturalism. The properties it appeals to in any explanation are only those properties postulated within the sciences. Third, following from this, SP is like any science in that it is purely causal. Thus SP is simply a continuation of the scientific investigation into causes, but in the case into the causes of our beliefs. Looked at in this light there are many naturalistic properties that advocates of SP can draw upon in the alleged explanations of scientific belief. It is this abundance of naturalistic properties that might give some credence to the naturalism of SP. But what undermines

it is the lack of law-like links that these naturalistic properties might have with belief states.

Fourth, as a consequence of its scientific naturalism, SP can make no appeal to anything non-naturalistic or supernatural. This raises an important point about the role of methodological norms in any explanation of scientific belief. In marked contrast, the rationality model of the previous section makes explicit mention of methodological norms in the explanation of scientific belief. This is an important difference between the rationality model for explaining belief and the social-causal model of SP. In fact SP explicitly rules out any appeal to norms of rationality or methodology because of their allegedly non-naturalistic character. This brings us to a discussion of the third symmetry tenet of SP.

The Symmetry Tenet is obscurely expressed. It tells us that the same types of causes are to explain all beliefs regardless of their truth or falsity. It remains unclear what are the types of causes that are to be admitted. We can take it that the only types of causes to be admitted in explanations of belief are those selected from the broad type *social causes*, several species of which have been mentioned above. We can also take it that since no normative principles of methodology are to be included within this broad type, then explanations of belief in terms of them are ruled out. Further evidence for this restriction comes from Bloor: 'The Symmetry requirement is meant to stop the intrusion of a non-naturalistic notion of reason into the causal story. It is not designed to exclude an appropriately naturalistic construal of reason, whether this be psychological or sociological' (Bloor 1991, p. 177).

What this tells us is the following. Norms of rationality and methodology are understood to be non-naturalistic items. As such they are not to be found within the realm of purely naturalistic items prescribed by the naturalism of SP. If they are admitted then they are said to be a 'non-naturalistic intrusion'. But this is hardly correct. It is people who use the norms in coming to hold the beliefs they do that are causally efficacious; it is a mistake to think that the norms themselves are somehow causally efficacious. This point aside, the upshot of the Symmetry Tenet is that SP, because of its naturalism, stipulates that we are to avoid any appeal whatever to norms of method of the kind found in the rationality model. The grounds of this claim are based entirely on the naturalism of SP, and the peculiar view of the status of norms of method and how they can causally affect belief.

Let us return to the last-cited remark of Bloor where he talks of a 'naturalistic construal of reason, whether this be psychological or sociological'. What does this mean? If the view is that we have a natural, or even innate, capacity to reason, then an appeal to this is admissible. But it raises some serious problems for SP. First, there are the empirically based investigations in cognitive psychology that show the extent to which we do, or

do not, employ the right principles of reasoning. It turns out that whereas most people do reasonably well on inferences using *Modus Ponens*, they do less well on *Modus Tollens* and poorly for probabilistic reasoning. People are much more ready to make fallacious inferences in the last two cases. The extent of this is surprising and is a well-established result of these studies.[1] Any appeal to our natural reasoning abilities may be to abilities that employ bad modes of inference. This does not bode well for a naturalistic account of reason and method in forming our scientific beliefs.

Second, there is no way in which SP can provide any justificatory account of why some rules of inference are good and others are bad. That must come from elsewhere. As is to be expected, SP can appeal to both the good and the bad rules of inference we use as a cause of our beliefs; but there is no account that arises out of the tenets of SP as to why the good rules are *good*. Third, much reasoning in science proceeds by quite sophisticated techniques of deductive, inductive and statistical reasoning. There is not much chance, unless we are born as geniuses, that any of us have picked these naturally in the sense that they are part of our innate equipment. We might, of course, come to learn of these as part of our general learning in mathematics, physics and elsewhere. And in so far as such learning is social, an appeal to social causes might be made. But they are generally quite distant causes of belief, in the way the oxygen we breathe is a cause of our various believings (if there were no oxygen there would be no believing). In the absence of either earlier learning or oxygen we would not acquire the scientific beliefs we do. But neither oxygen, nor the earlier event of the social acquisition of methodological principles through learning, is a direct shaper of our current scientific beliefs.

Finally, such an appeal to natural reasoning undermines its alleged strength; and it shows that SP is false. If we are to take seriously the idea of the 'naturalistic construal of reason, whether this be psychological or sociological' and treat such abilities as part of psychology only (as we appear to be invited to do – the word 'or' indicates this), then this falls foul of the Causality Tenet. It says that the non-social factors must always be in conjunction with social factors. But if we can appeal to just our psychological makeup as the basis of our ability as natural reasoners (and this seems to be a real possibility), then we can set social factors aside. What is casually efficacious here is just our inbuilt psychological abilities to reason. Our cognitive makeup alone is casually efficacious without the assistance of any social factor when it comes to reasoning (whether good or bad).

To sum up, the appeal to a psychological basis of our natural reasoning abilities is inconsistent with SP. If we also take into account social factors, then we have not eliminated the problem raised by the empirical studies of reasoning. As natural reasoners we are not very good at all. What tutoring we

do get may be social; but this in no way tells whether we are using good or bad rules. The appeal to the 'naturalistic construal of reason' does not help at all.

11.2.2 Some Examples of the Application of the Social-Causal Model

Let us now consider how the social-causal model works in some examples. Consider that of Einstein of the previous section. There we gave an explanation of Einstein's belief in GTR in terms of the rationality model which explicitly appealed to some methodological principle. But such explanations are ruled out by SP on the basis of the Symmetry Tenet. So the kind of explanation offered by his biographer, Pais, of Einstein's belief in GTR, and presumably Einstein's own grounds for his belief in GTR, are not to be admitted. Does this mean that Einstein misled us? Or was he himself grossly misled about what were his real reasons for belief in GTR? Here SP plays the unlikely role of the unmasker of the allegedly misleading grounds offered by rational explanations for belief. Advocates of SP hope to replace this by what they think are the correct causes of scientific belief regardless of what we may think they are, and the actors themselves think they are. But this alleged exposure of the misleading grounds of rationality explanations itself stands in need of unmasking. The central claim of SP that no person or scientist at any time can come to have their scientific beliefs on the basis of the application of methodological principles, and that the real causes are socio-political, or interests in these, clearly has a large number of counter-examples to be culled from the history of past and present science.

Advocates of SP have presented a large number of case studies of episodes in the history of science in which they purport to show that socio-political causes were at work in bringing about belief. But many of these are highly contested. Here we will discuss just one, that of Forman's account of the causes of belief in acausality[2] that grew up amongst a number of scientists in the period of Weimar Germany just after the First World War (see Forman 1971). Forman describes in great detail the social and intellectual milieu of scientists of the time. He emphasises the romantic reaction against science and its advocacy of causality by a number of writers including the influential Oswald Spengler. Let us accept this aspect of Forman story, and his further claim that a number of physicists also came to be influenced by this reaction; they too were caught up in the intellectual currents of the time. If we grant this we need to be careful in applying SP to this case. We cannot say, as the official version of Causality Tenet would have, that it was this milieu that caused their belief in acausality; rather, it is their *interest* in this romantic reaction which is at best the cause of their belief in acausality.

Consider some physicist who has an interest in this romantic reaction; call this interest 'I'. Also suppose that they are up-to-date in their thinking about

the issues and problems that confront the physics of their day, including its experimental, theoretical, mathematical and philosophical aspects, in particular matters to do with the causal view of physics. Call all of this 'P'. Now consider their belief in acausality; call this 'B'. The question now is: what is the cause of B? Is it I? Or is it P? Now all of I, B, and P are co-present in the mind of the physicists (we suppose they are in tune with their intellectual milieu). As is well-known, mere co-presence is not sufficient to establish causality. The common view would be that P is the cause of B and not I. What SP must show is that this is wrong and that P, even though co-present in the mind of the physicist, does not cause B; rather it is I that causes B. To establish this we must appeal to some principles of causal methodology; but unfortunately such *methodological* principles are not available to advocates of SP. This is a serious problem for SP that aims to explain all beliefs causally. There is a further difficulty. Even if such principles were available, a case can be made which shows that the investigation carried out by Forman simply does not establish that it is I rather than P that is the cause of B. (This cannot be argued here but is set out fully in Hendry 1980, and Nola 2003, Chapter 5.6). In sum, the causal approach advocated by SP is not vindicated.

This raises a further difficulty for SP. The study concerns about a dozen physicists in Weimar Germany. Let us suppose that they do come to believe in acausality (on whatever grounds) and publish their results. What can we say of other people in other parts of the world who read their publications and come to think about the issues in physics that purportedly gave rise to claims about acausality? They are clearly not part of the intellectual milieu of Weimar Germany that allegedly gave rise to the belief in acausality entertained by German physicists. Thus consider a budding Ernest Rutherford in remote New Zealand who is oblivious to the cultural milieu of German physicists but nonetheless rethinks the issues in physics and comes to the same beliefs about acausality. Is there something in the New Zealand milieu of the time, or the budding Rutherford's interest in that milieu, that causes him to come to believe in acausality? This is what advocates of SP would have to claim. But there is hardly anything akin to the intellectual milieu of Weimar Germany in 1920s New Zealand. It is hard to see what social and cultural factors could play a role in such a case. In fact for different groups of physicists around the world, advocates of SP must allege that the very different cultural milieus of the physicists (or their interests in their milieu) causally determine their belief in acausality. It would be pure happenstance that the many, very different cultural milieus gave rise to the very same belief in acausality for all of them. A much more likely explanation is the common intellectual context of the physics of the day that they all share, and not the very different cultural circumstance in which each group exists.

Clearly we are misled by SP since it invites us to look for the wrong kind of causes. The causes of belief in acausality on the part of the world community of physicists cannot be socio-cultural, but rather the very content and problem context of physics. For those who do not accept the social-causal explanatory programme of SP, the causes of belief in acausality are two-fold: the problem context of the physics of the time, and the methodological principles that physicists apply in coming to accept one kind of hypothesis rather than another, or accept that one kind of solution is better than another, and so on.

11.2.3 Reason-Giving as Social

Barnes and Bloor (sociologists of science who call themselves 'relativists') claim that the very methodological principles we employ in science, and use to give evidential support, are also believed in the way specified by SP:

> For the relativist there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such. Because he thinks that there are no context-free or super-cultural norms of rationality he does not see rationally and irrationally held beliefs as making up two distinct and qualitatively different classes of thing. (Barnes and Bloor 1982, pp. 27-8)
>
> 'Evidencing reasons', then, are a prime target for sociological inquiry and explanation. (*ibid*., p. 29)

Here, not only scientific claims, like the belief B in acausality, but also the very standards or methodological principles by which such beliefs are judged (such as those discussed in Part II), are to be treated on a par as beliefs which fall within the scope of CT, and are grist for the mill of SP. There is no rational grounding of our methodological principles; there is only their local acceptance – or non-acceptance as the case may be.

At the end of Section 11.2.1 we looked at one response that SPers have made about the normative character of the principles of scientific method. If there are such, they are a non-natural intrusion in the natural causal order. But since there can be no such intrusion, all such principles are to be treated as if they are natural features of our own reasoning. We have seen what problems there are in such a view of the norms of reasoning; there is simply no ground for distinguishing between good and bad modes of reasoning.

Advocates of SP argue for their naturalistic construal of evidencing reasons on other grounds. This has to do with the fact that such principles are locally accepted by the community; and it is their acceptance that is the final court of appeal. Thus if there is general acceptance of some principle of critical inquiry C, whatever it be, then there is no further court of appeal beyond what the community says about the rightness or wrongness of C. Wayward members of the community can be corrected if they go wrong. Here there are social sanctions imposed by the community to bring the wayward into line so that they also conform to C. But if the wayward asks 'Why should

I adopt what the community as a whole adopts, viz., C?' then there is no further answer. This is simply what the community accepts.

The issue can be put nicely in terms of the following contrasting questions: (1) 'Do members of the community (and any wayward member) accept C because C is right?' Or, (2) 'Is C right because the community adopts C (and in the long run any wayward member as well)'? If the answer to (1) is 'yes' and to (2) 'no', then there is an independent reason as to why the community adopts C, viz., it is a good or correct or right principle to adopt. And presumably there is a theory of rationality that can establish its rightness, whatever that be. (This is the line we take about the principles of critical inquiry set out in Parts I and II.) But if the answer to (1) is 'no' and to (2) 'yes', then there is no independent notion of right to which to appeal; rightness is nothing other than what the community accepts, and there is no further court of appeal. The situation is just like, say, the members of the violin section of an orchestra. If one member is playing out of tune, then the others will try to bring the wayward player into conformity. But if the wayward appeals to a score and says 'I was playing the right note – look at the score!' the other members of the strings can say that they are reading the score correctly and that the wayward person is simply misreading. What the others are playing is what is right – and there is no further court of appeal.

Here sociologists claim that the rightness of principles like C resides not in some independent theory of rationality but in what the community ultimately accepts and the sanctions it imposes for its wayward members. Here there would be an elementary theory of instruction or persuasion to bring the wayward into line. There is nothing like a further court of appeal to reason; that has been ruled out. This raises further issues about rationality and the social that cannot be pursued further here[3] (but see Section 11.4).

11.3 FURTHER TENETS OF THE STRONG PROGRAMME

We have discussed the main Causality Tenet of SP that sets out the kind of explanatory causes that it is to admit. And in the course of this the Symmetry Tenet was mentioned; this emphasises the fact that no other kind of cause is to be admitted other than those mentioned in the first tenet. In this section we will consider the other two tenets.

The Impartiality Tenet is worth reiterating and tells us: '[SP] would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation' (Bloor 1991, p. 7). There is something distinctly odd about this tenet that needs exploring.

First, it is important to note that it is merely an instance of the first Causality Tenet. That tenet says nothing about whether a belief is true or false, rational or irrational, successful or unsuccessful (for convenience, call

all of these 'epistemic properties' of a belief). What the second tenet does is highlight this feature of the first tenet. But there is a hidden agenda here that takes us to a second point. What the Impartiality Tenet is meant to rule out is an approach of the sort adopted by Mannheim with his "weak programme", and many other methodologists as well. The "weak" programme allows that scientific method can account for the true, the rational and the successful in science. The leftovers of the false, the irrational and the unsuccessful are to be given to the sociologists of science to feast upon. In ruling out this division of labour the Impartiality Tenet is supported by the Symmetry Tenet which insists that there be no asymmetry of this sort. That is, all explanations are to be of the same type, the type given by the first Causality Tenet. There are not to be different types of explanation such as those of the rationality model and the social-causal model, with the first type dealing with the true, rational and successful and the second getting the leftovers. SPers wish to feast on all the various sorts of beliefs there are, leaving nothing for rationalists to feast upon.

There is a faulty assumption in the above. This is that rationality models with their appeal to methodological principles are to explain, say, only the true, and not the false. But this is wrong. Scientific methodologies can explain both true and false claims, and belief in these. Thus consider Popper's methodology. It is a hallmark of his position that all theories are likely to be false. This is linked to his notion of verisimilitude in which our theories are most likely false, but their likeness to the truth is greater than that of their predecessors. In Popper's view, when we come to employ some methodology to adjudicate between theories, then we will adjudicate between false theories. We may find that some theory has greater corroboration than its rivals; and this may enter into an explanation as to why we accept or believe it. But according to Popper the theory we believe is still false. So it is wrong to suppose that methodology will deal with only the true, leaving sociology to deal with the false. It also follows that it is equally wrong for the Symmetry Tenet to insist that the same type of explanation should apply to the false as well as the true, and that this explanation should be only of the social-causal type advocated in the tenets of SP. Rational style explanations can do equally well for most of the false as well as the true claims of science. Much the same point can be made about the other pairs of epistemic notions such as the rational/irrational and successful/unsuccessful division; Popper's method can deal equally as well with both pairs.

Other methodologies can also be used to make much the same kind of point. Lakatos took pride in the fact that on his *Methodology of Scientific Research Programmes* a programme could grow and be acceptable even though its hard core was false. Further its growth can take place in a 'sea of anomalies' as he put it. Again it is wrong to say that scientific methodologies can only deal with the true and not the false; they deal with both. Finally

consider the Bayesian methodology of Chapter 9. In one of its versions, it bids us always to consider a set of hypotheses which are exhaustive but logically exclusive. At best only one of these can be true; the rest are contraries and must be false. So Bayesianism also deals with the false as well as the true.

In sum, there is something wrong with the claims of the Impartiality and Symmetry tenets. There is a false presupposition that methodologies can only deal with true scientific beliefs; they deal with false scientific beliefs as well. These are not leftovers for sociologists of science but are within the domain over which methodologists can adjudicate. A consequence of this is that the Symmetry Tenet is further undermined. Its requirement that there be only one type of explanation does not lead to the social-causal model but rather to the rationality model. This is not good news for SP as a whole; its basic Causality Tenet is hereby undermined, but in a way different from the criticisms made in previous sections.

Let us turn now to the final Reflexivity Tenet. This too is merely an instance of the Causality Tenet. Since it is not restricted in any way to the kinds of beliefs that fall under its scope, then the very tenets of SP itself must be special instances. What the fourth tenet claims is that SP itself has its own causes of belief. But these are not rational, that is based in principles of logic, reason or method. Rather any believer in SP has been socially caused to believe it. If there is a change in the prevailing social conditions of the right sort, then people in those changed conditions might well cease to believe SP, and perhaps start believing something like the rationality model. Of course this is not something that advocates of the rationality model would applaud; they get converts to their own doctrine in the wrong way. But it is something that advocates of SP would have to accept, as unpleasant as it may be, since it is in accord with their own doctrine!

Its advocates often justify their belief in SP by pointing to the large number of case studies that allegedly support it. We have mentioned the case of Forman and his work on the alleged causes of belief in acausality in the part of some leading Weimar Physicists. There are many other such studies as well (see the list of such studies in the paper by Barnes and Bloor (1982), footnote 7, pp. 23-25). Putting all these together we might argue by enumerative induction from the success of the case studies to the correctness of SP. But to do so would be to employ one of the central modes of inference in scientific method. Does this show that advocates of SP are inconsistent in rejecting modes of scientific inference in one place but employing them in another? It might appear so. Some advocates of SP do claim that they are at least inductivists. But do they claim this on the ground that induction is a form of natural reason? If so then they could escape this objection in the case of simple modes of inductive inference; but natural reason hardly takes us far

in dealing with more sophisticated inductive inferences of the sort found in statistical reasoning. This raises the question about whether or not advocates of SP do, in the long run, claim that while many beliefs are socially caused, some also arise on the basis of principles of evidence. This is a matter we turn to in the next Section.

11.4 DOES EVIDENCE HAVE NOTHING TO DO WITH SCIENTIFIC BELIEF?

In Part II, Chapters 7 to 9, we considered the various ways in which evidence can bear on hypotheses. Here we will consider whether advocates of SP do take evidence into consideration when they adjudicate between theories. So far it appears that they do not; what leads a person to accept or believe some theory are purely socio-cultural causal factors. No room seems to be left for evidential considerations of the sort discussed in Part II.

In recent philosophy much has been made of the claim that our theories are underdetermined not only by all the available evidence that supports them, but also by all the possible evidence that could ever support them. Thus there may be two or more theories that are equally supported by all actual or all possible evidence. So what helps fully determine the grounds on which one and only one theory is accepted in these circumstances? Contrary to this bare possibility, as a matter of fact we do tend to believe in only one theory and do not entertain any of the many other theories equally well supported by the evidence. But advocates of SP insist that underdetermination always holds and that we are always in a position in which two or more theories can fit our evidence equally well, despite the fact that we believe only one of the many equally good alternatives. If we have run out of evidence what else could determine why we believe just one of the many equally good theories? The only other factors that could decide which theory to accept are purely social. Where evidence runs out the social steps in and determines which theory we should believe. Evidence does play a role – but only up to a point. The ultimate determiner of what theory we are to believe is the social, but coming in on top of evidential considerations once they have taken us as far as they can go.

It should be pointed out that no evidence fully determines what theory we are to adopt, if we mean by 'determined' that the evidence entails the theory. There is always a gap between evidence and theory. This is clearly so in the case of the probabilistic and Bayesian account of the confirmation evidence gives a theory; also in the case of Popper there is a gap between corroborating evidence and theory. But the case of two theories equally underdetermined by the evidence raises a different issue. In both cases there is a gap between theory and evidence; but the gap is equal and it is not as if the evidence gives greater support to one theory rather than another.

The social-causal solution offered by advocates of SP is no solution, as has been argued by a number of people. Here we will mention only the difficulty discussed in Brown (2001, Chapter 7). For the sake of simplicity let us suppose that two theories H and K are underdetermined by the evidence and get equal support from it. Suppose we regard E as the compete set of all available, or even all possible, evidential considerations that could be bought to bear on the choice between H and K. Then we can say that for all the possible evidential considerations E it remains underdetermined which of H and K ought to be supported. Note that this is a general claim that applies for any pair of theories whatever.

Now suppose that some additional social factor S appears on the scene that determines that it is, say, H that is to be believed rather than K. Factor S is some socio-political-cultural circumstance of a person, or their *interest* in this socio-political-cultural circumstance. Now add S to evidence E. What the advocate of SP maintains is that it is [E + S] determines H rather than K.

Now it is important to note exactly what the underdetermination thesis says. It is a perfectly general claim that says that for any body of evidence, such as E, or even [E + S], there will always be at least two or more theories that fit each equally as well and between which they cannot adjudicate. That is, even for the new evidential considerations provided by [E + S] there will still be two or more theories, say H* and K*, that fit [E + S] equally well and which are underdetermined by [E + S]. Here a new underdetermination arises, for which a further social factor S* has to be introduced in order to remove it. But then it is clear that a regress threatens which always leaves us with some underdetermination somewhere. The upshot of this is that the sociological solution provided by advocates of SP is powerless to solve the problem of underdetermination; it does not go away but returns at a new level. For those who grant underdetermination by all possible evidential considerations (it is important to note that not all do), then the social solution is no solution at all.

11.5 FOUCAULT ON POWER AND KNOWLEDGE

11.5.1 Francis Bacon on Power and Knowledge in Science

The aphorism ‘knowledge is power’ is commonly attributed to Francis Bacon (1561-1626). A remark made about 400 years ago that partially supports this is: ‘Human knowledge and human power meet in one; for where the cause is not known the effect cannot be produced. Nature to be commanded must be obeyed …’.[4] Considered one way, the aphorism ‘knowledge is power’ invites us to explore some of the possible connections between knowledge and power. In contrast, a much stronger view is endorsed by a wide range of contemporary thinkers in the sociology of science, viz., that knowledge and power are inseparable or identical. This is the position of Michel Foucault to

be examined here. As will be seen, he often exaggerates the connections between power and knowledge. Buried within his position are the more plausible, but weaker, views of Bacon.

The Baconian aphorism is false if understood as a claim about the *identity* of power and knowledge. Power can be exercised without scientific knowledge and scientific knowledge can be possessed without the exercise of power. But often our knowledge can enhance our power and control over the world and other people; however, this does not establish the strong identity claim. The Baconian aphorism can be interpreted to say that scientific knowledge is a necessary (but not sufficient) condition for our having the power to transform the world according to human needs. For Bacon at the beginning of the "scientific era", the ultimate use of science was practical; he hoped it would improve the lot of humankind and would decrease our suffering and distress. He wished that 'the mind may exercise over the nature of things the authority which properly belongs to it'; he claimed that 'the true ends of knowledge …[are]… for the benefit and use of life; and that they perfect and govern it in charity'; and he took himself, in promoting the renovation of the methods of science, to be 'labouring to lay the foundation not of any sect or doctrine but of human utility and power'.[5]

We are the heirs of this immensely practical view of a reformed approach to knowledge-gathering that enhances our human powers every time we seek relief from toothache, obtain a cure using antibiotics, play a compact disc on our hi-fi system or take ice from the refrigerator. But the advances in science are a double-edged sword. Four hundred years after Bacon's confident claims we face the potentially disastrous perils of a science-dominated age which Bacon in a more hopeful era would not have anticipated. The boons of science have been accompanied by dangers from nuclear reactor disasters to the depletion of the ozone layer or global warming. We are, it is to be hoped, more aware of the perils as well as the advantages of the use of science than those who helped inaugurate the scientific revolution.

Bacon also insisted that if any benefit were to be obtained from science then the very preconditions that make genuine science possible would have to be realised first. Though he made little contribution to experimental science, Bacon was an early propagandist for the complete reconstruction of the methods whereby we acquire knowledge. He hoped to sweep away all previous theories of knowledge acquisition that had either hindered the development of genuine science or had produced pseudo-knowledge; these are to be replaced by principles of critical inquiry and methodology that would promote experimental science. In this respect his works became a rallying point for those who were subsequently actively engaged in the seventeenth century scientific revolution.

Bacon calls to our attention two important aspects of science – its use in enhancing human power and the methods whereby scientific knowledge is acquired. These combine both extrinsic and cognitive aims for science, the latter being the more fundamental. Realising them is a prerequisite to realising any of the extrinsic aims, a point Bacon makes in the aphorism just cited.

The Baconian 'knowledge is power' aphorism can be understood in another way. As science and technology have become allied to the military, to industry and to government, the institutions of science have themselves become important loci of power and influence from the funding they attract to the employment and economic wealth they provide, but most importantly the kind of research they support. Bacon did not overlook the role of science as an institution in its own right. In his *New Atlantis* he describes a utopian society in which scientific institutions were set up for research into the various sciences, one team in particular being devoted to research into the consequences of science for practical life. We would only need to add to Bacon's list of research teams one other. Its task would be two-fold: to research the ways in which science and technology can keep themselves flourishing and rapidly expanding, and to research ways in which the institutions of science can themselves become more powerful influences commanding not only vast sums of money and numbers of employees, but also extending the outlook and techniques of science into new fields. In this respect it is the institutions for the production of scientific knowledge that become powerful, not always knowledge itself as Bacon's aphorism suggests.

Much can be said about the social, cultural, historical and power contexts in which science exist and flourish (more is said in Chapter 14). But does this pertain to the very content of science itself, in which social factors shape scientific belief? This is a matter already addressed above. Bacon might be thought to be an early precursor of the view that it does. But not so, for two reasons. He carefully distinguished between (i) the *internal* principles of critical inquiry we use to establish the content of science independently of its power and social context, and (ii) *external* aspects of science that are contextually dependent on society, culture and history. Second, he advocated his own account of some of the principles of scientific method that would be used in adjudicating between scientific theories. Such a careful distinction is not always to be found in Foucault, to whom we turn next.

11.5.2 Foucault's Doctrine of "Power/Knowledge".

In his writings before 1970 Foucault advanced a theory about the growth and evolution of our scientific knowledge in a range of sciences, but especially the human and medical sciences, in which there were a large number of breaks, ruptures and discontinuities. In this respect his views are akin to those of Thomas Kuhn who also argued in the early 1960s that the sciences were

marked by a sequence of paradigm changes. Foucault located an even greater number of discontinuities in the sciences than Kuhn did; his views were quite extreme in this respect. Foucault's enterprise was to provide a theory of the vast variety of kinds of discontinuities that he thought he had located in the history of the sciences. One aspect of what he means by 'the *archaeology* of human knowledge' is just this descriptive theory. We will not consider here his archaeological theory.

From 1970 onwards Foucault paid greater attention to what might be the causes of such discontinuities. In the end he decided that power was *the* cause of such discontinuities; this theme dominated his writings for the next ten years. The problem, as he put it, was one of *pouvoir/savoir*, or power/knowledge. In looking for what causes maintain, produce or create our systems of beliefs, there is a shift in focus from the descriptive enterprise of archaeology to an explanatory enterprise that looks for causes. This is part of what Foucault means by calling his new enterprise *genealogy*. This can mean many things; here we will take it to be a search for explanations, largely causal, of why we come to hold the scientific beliefs we do (but it can be extended to the invention, or maintenance or change of beliefs).

Foucault was never clear about what the exact connection was between power and knowledge. Disclaimers to the contrary, sometimes he does say 'power is knowledge'. On other occasions he takes the view that there is a much weaker connection. But he never seriously questioned the idea that there might be no connection between knowledge and power; he always assumed that there was some connection, whatever it be. As argued in Part I, knowledge is a normative, critical notion in which reason, justification and/or evidence, along with truth, turns our beliefs into knowledge (sometimes rational belief). In Part II we made a case for the role of methodology in adjudicating amongst the rival theories we entertain to determine which of them is the best, or which may provide us with knowledge, or at least approach knowledge more closely. Importantly, reason and methodology have nothing to do with power. From the epistemological stance of this book, any talk of a power/knowledge nexus is mistaken. Perhaps, at best, power can to be linked to belief; but as we will see, Foucault also talks of other items, such as truth or discourses, which are also alleged to be linked to power.

What does Foucault mean by power? This is an unclear matter, but we can get some idea of aspects of his notion of power from the following remarks:

> it [power] is a way in which certain actions modify others. (Foucault (1983), p. 219)
>
> It [power] is a total structure of actions brought to bear on possible actions; it incites, it induces, it seduces, it makes easier or more difficult; in the extreme it constrains or forbids absolutely; it is nevertheless always a way of acting upon an acting subject or acting subjects by virtue of their acting or being capable of action. A set of actions upon other actions. (ibid., p. 220)

> When one defines the exercise of power as a mode of action upon the actions of others … one includes an important element: freedom. Power is exercised over free subjects, and only in so far as they are free. (ibid., p. 221)

Basically the idea is that we have power when our actions can affect the actions of others. Thus person X has power over person Y when X's actions increase the range of options that Y has (as when a benevolent person makes one a bequest so that one is enabled to do things that one would not have done otherwise); in contrast X also has power over person Y when X's actions decrease the range of options that Y has. For Foucault when X reduces Y's options to zero and forces Y to act in a particular way then X strictly does not have power over Y; rather X acts violently towards Y. For Foucault power relations are a ubiquitous aspect of the everyday interactions between people, whether it be in an interview or in loving one's partner. Clearly this is too broad a notion of power since many everyday interactions, such as the polite act of opening a door so that someone may pass through it, is an exercise of power on the above definition. We will not discuss here what other conditions must be added to the notion of power to remove such obvious counterexamples; but clearly some other conditions are needed.

For Foucault power is not only directed upon the actions of others; it is also directed upon "knowledge", another item also involved in the power nexus. Here are two passages from the many that can be found in Foucault which indicate this.

> We should admit rather that power produces knowledge (and not simply by encouraging it because it serves power or by applying it because it is useful); that power and knowledge directly imply one another; that there is no power relation without the correlative constitution of a field of knowledge, nor any knowledge that does not presuppose and constitute at the same time power relations. (Foucault 1979, p. 27).
>
> My problem is rather this: what rules of right are implemented by [a] the relations of power in the production of discourses of truth? Or alternatively, [b] what type of power is susceptible of producing discourses of truth that in a society such as ours are endowed with such potent effects? What I mean is this: in a society such as ours, but basically in any society, [c] there are manifold relations of power which permeate, characterise and constitute the social body, and these relations of power cannot themselves be established, consolidated nor implemented without the production, accumulation, circulation and functioning of a discourse. [d] There can be no possible exercise of power without a certain economy of discourses of truth which operates through and on the basis of this association. [e] We are subjected to the production of truth through power and we cannot exercise power except through the production of truth. This is the case for every society, but I believe that in ours the relationship between power right and truth is organised in a highly specific fashion. (Gordon 1980, p. 93; interpolation of [a] to [e] added)

These two passages tell us a number of things. The first tells us that power produces knowledge; but, significantly, he adds in parenthesis that it does not do so simply because power is useful, or because it serves some purpose. Here

Foucault rejects any teleological link in which the goal of power is to produce knowledge. The connection is directly causal: power *produces* knowledge. Here Foucault seems to be rejecting the more plausible position of Francis Bacon in which power might be exercised by us to produce knowledge because that knowledge is of use to us.

Two distinct theses about a power/knowledge connection can be discerned here. The first says:

(1) power produces knowledge. Alternatively in counterfactual form: there is no knowledge without the exercise of power.

The counterfactual version can be found in the second quotation in all the passages marked from [a] to [e].

A second thesis claims the converse:

(2) knowledge produces power. Alternatively in counterfactual form: there is no power without knowledge.

This thesis can be found in the first passage, and it is hinted at in [b] and [e].

The above passages also indicate that power is linked not only to knowledge but also truth, to discourses and to discourses of truth (whatever these be). It is hard to see how power could ever be a maker of truth. To establish this one would have to hold a grossly constructivist view about what makes any claim true. This also goes completely contrary to the view of truth set out in Section 2.3 which appeals to items in the world, or truthmakers, in virtue of which statements are true. Perhaps what Foucault means is that power produces what people *believe*, or *accept* or *take* to be the truth. This at least has some initial plausibility since it does not have the incoherence of the claim that power makes or produces the world's truthmakers. Also, allowing that power produces what people *take* or *accept* to be true fits with the notion that power also produces *discourses* that we believe to be true (whether they be true or not). And again the claim that power produces discourses of truth can be reinterpreted to say that power produces discourses in which we make statements about what we *take* to be true. Given the range of things that power is said to produce, from here on we will talk about "knowledge", rather than knowledge, discourses, truth, discourses of truth, or whatever. Though a better term than "knowledge" would be 'belief'; but Foucault never talks about belief.

The two theses above spell out what the power/knowledge doctrine might be plausibly taken to say. Taken together the two theses indicate that according to Foucault there is a power/knowledge nexus. Power produces "knowledge", and that "knowledge" in turn produces further kinds of power; and so on, through a cycle of interconnections between power and "knowledge". Note that in the second quotation Foucault says that this power/knowledge nexus holds not only in societies like our own but 'basically in any society'. Thus his doctrine is to be taken quite generally for

"knowledge" at any time and place. Presumably it also holds for any kind of "knowledge", though Foucault restricts his doctrine to scientific knowledge, more specifically to "knowledge" produced within the human sciences (but as will be seen, and despite what many commentators say, it is not restricted to just the human sciences).

The second thesis that "knowledge" produces power may be understood as a somewhat infelicitous way of saying what Bacon said, namely, that our knowledge can be used by us to increase our powers. But if it is understood as saying something other than this, then it borders on incoherence. It is hard to see that knowledge itself is productive of anything like power. Rather it is people using the knowledge they have to enhance their powers of action; and this is a quite different claim.

From here on we will focus on the first thesis and not the second. The first thesis says that power produces "knowledge"; or that there is no "knowledge" without the exercise of power taking place somewhere in respect of that "knowledge". Here let us take "knowledge" to be the beliefs within the various sciences. Then the first thesis can be re-expressed as: for all scientific beliefs held by any person, there are power relations that causally produce their scientific beliefs. One can also understand this to be a thesis about the causes of change in belief. In fact this is the original impetus for his interest in genealogy. It is the discontinuities, changes and ruptures in our systems of belief that call for explanation.

11.5.3 Evaluating the Claim that Power Produces "Knowledge" (Belief)

What evaluation can be made of Foucault's first thesis that power produces "knowledge"? The alert reader will see immediately that the above more explicit formulation of Foucault's first thesis is very similar to that of the Causality Tenet of the Strong Programme (SP). Both make claims about what are the social causes of scientific belief; but SP takes these more broadly to be either social, cultural or historical causal factors. (SP also makes mention of non-causal factors that also bring about belief that Foucault does not mention; but we can set this point aside.) In contrast Foucault's first thesis is narrower; only relations of power are admitted as causally efficacious. Relations of power are at best a small subclass of the social, cultural and historical causal factors said to be causally efficacious in SP.

Here the sociological approaches of Foucault and SP are closely united. Once these strong similarities are noted, then all the problems that come to infect SP also come to infect the first thesis which is an integral part of Foucault's power/knowledge doctrine. And so his doctrine falls foul of the same criticisms that beset SP. But it might also succumb to other criticisms since it allows only the more narrow relations of power as the one and only kind of cause of scientific beliefs. If we grant some of the case studies in

support of SP, they might well show that Foucault needs to avail himself of other social factors, such as cultural matters, or interests in them, as is the case in Forman's study in which it is alleged that interests in the cultural milieu of Weimar physicists caused them to embrace acausality. Foucault in fact seems to eschew any talk of cognitive notions like interests as causes of "knowledge". His causes are straightforwardly power relations.

SP explicitly includes the Impartiality and Symmetry tenets; but there is no suggestion in Foucault of counterparts to these tenets in his power/knowledge doctrine. Foucault is at least aware of the reflexivity issue, yet he has little to say about it; he is content to uneasily ride along with it, recognising that relations of power might well be taken to cause belief in the power/knowledge doctrine itself.

What is significant about Foucault's position is that he hardly ever says anything about the role that principles of critical inquiry play in the evolution of our system of scientific beliefs. But there is an appeal to them in one important passage where he considers rival genealogies such as Marxist or psychological explanations of belief:

> Has there been, from the time when anti-psychiatry or the genealogy of psychiatric institutions were launched ... a single Marxist, or a single psychiatrist, who has gone over the same ground in his own terms and shown that these genealogies that we produced were false, inadequately elaborated, poorly articulated and ill-founded? (Gordon (1980), pp. 86-87).

Here Foucault asks whether his own genealogies might be challenged by those engaged in a different Marxist or psychiatric genealogical projects, or whatever. He asks whether they might be false. Clearly what truth-value an explanation has is of crucial interest to critical inquirers. Foucault also asks whether one might be inadequately elaborated, and/or poorly articulated when compared with another. What these might mean is unclear; but they can be construed to relate to the kinds of explanation they provide. And finally he says that they might be ill-founded; we can take this to be an appeal to evidence. Overall it looks as if, when it comes to comparing one genealogical explanation with another, principles of critical inquiry come into apply. Nevertheless he is strangely silent on the role such principles might play at another level, that of evaluating the beliefs in particular sciences. Here power seems to hold sway and methodological principles of the sort found in the rationality model of explanation (see Section 11.1) have no role, when in fact they ought to have the main role.

Should we understand Foucault to be claiming that *all* bodies of "knowledge" are caught up in the causal power nexus, and that methodological principles play no role at all in the cause of belief? In interviews towards the end of his life Foucault appears to give belated acknowledgement to the fact that methodological principles do play a role in why we come to hold the scientific beliefs we do. In this respect Foucault

differs from the advocates of SP who, in their further development of the notion of rule-following taken from the work of the later Wittgenstein, give a communitarian account of why we adopt the rules we do, including methodological principles. The ultimate court of appeal is to what the community endorses as the correct rule (a matter discussed at the end of Section 11.2).

The later Foucault does not address theses issues. But he does distinguish between the adjudicating role that methodological rules can have in bringing about belief and the role that power can play. It turns out that the role of power is considerably circumscribed. In an exchange with an interviewer who says 'One of your theses is that strategies of power actually produce knowledge', Foucault replies endorsing this, but with a qualification: 'Of course you will always find psychological or sociological theories that are independent of power' ((Kritzman (1998) pp. 106). The clear import of his response is that some aspects of theories in the human sciences are outside the power network – and we would add, all the other sciences as well.

In an interview recorded at the beginning of the year of his death in 1984, Foucault makes even clearer what the scope of his "power/knowledge" doctrine might be when he separates 'relationships of power' from 'games of truth'. He says that medical knowledge has been linked to 'instances and practices of power'. This is an endorsement of thesis (2), but not thesis (1), concerning the power/"knowledge" linkage. He goes on to say that though they are linked in this way, there is also a separation, and then makes a comparison with mathematics:

> This fact [the linkage] in no way impairs the scientific validity of the therapeutic efficacy of psychiatry. It does not guarantee it but it does not cancel it out either. Let mathematics, for example, be linked – in an entirely different way from psychiatry – to structures of power; it [the separation] would be equally true, even if it were only in the way it is taught, the manner in which the consensus of mathematicians organises itself, functions in a closed circuit, has its values, determines what is good (true) and evil (false) in mathematics, and so on. That does not at all mean that mathematics is only a game of power but that the game of truth of mathematics is linked, in a certain way and without impairing its validity, to games and institutions of power. … it is clear that the relationship which can exist between the relations of power and the games of truth in mathematics is entirely different from the one you would have in psychiatry. In any case, one can in no way say that the games of truth are nothing else than games of power. (Bernauer and Rasmussen (1988), p. 16)

A number of points arise from this passage. First, it is clear that Foucault thinks that even a non-human science such as mathematics can, in some way, be subsumed under the "power/knowledge' doctrine. Many claim that this doctrine only applies to the human sciences; but this is clearly not Foucault's view. Second, it is clear that Foucault has become a little clearer in his own mind (but it still remains somewhat murky) about what aspects of mathematics (and other sciences such as psychiatry) do, and do not, fall

within the scope of the "power/knowledge" doctrine. It is quite clear that Foucault wishes to put issues to do with the validity of the claims of psychiatry and mathematics outside the scope of power relations. Strictly speaking, validity has to do with arguments. But there is a common use of the term in which it concerns the truth, or lack of it, of the claims of any science. And it is this, along with methods for determining truth or falsity, that Foucault seems to say are not impaired by power. This claim is rather negative; it simply says that some aspects of these sciences are outside power relations. He remains quite coy about how to positively characterise what these non-power relations are like and says little of them.

Later in the interview Foucault tells us: 'when I say "game" I mean the ensemble of rules for the production of the truth' (*loc. cit.*). On one reading, if there is a collection of rules for producing truths, then games of truth are like rules of inference that have as output truths (given an input of truths as premises). Understood in this way, such rules of validity are outside power relationships. For Foucault it appears that 'games of truth' are different from 'games of power', though they are still alleged to be linked in some unspecified way. On this reading there is in Foucault a glimmering of a recognition that, in mathematics and other sciences, matters of power need to be separated from matters to do with validity (methodological test?), and other matters to do with 'games of truth', in mathematics.

The issue being addressed here is one that also concerns the scope of SP. Is it to be taken in the weak sense, advocated by Mannheim, in which there is a definite role for explanations which appeal to methodological rules and principles of critical inquiry? Or is it to be taken in a strong sense in which there is no role for rational explanations at all, and that all explanations of scientific belief are to be in terms of social causes? It would appear that the final Foucault cannot be the advocate of SP that the earlier Foucault has been. He has drifted into a "weaker" Manheimian programme with its separation of matters to do with validity and truth from social matters.

Foucault needs to be asked some sharp questions at this point. Do all the sciences, from psychiatry to mathematics, have aspects that fall outside the scope of his power/knowledge doctrine? The answer appears to be 'yes'. The next question is: what aspects of theories do, and what aspects do not, fall under its scope? It might be agreed that some matters do, and others do not, fall under its scope. But what matters fall where? Here there is no clear answer. But there is a hint that matters to do with truth can fall outside the scope of the power/knowledge doctrine. With this admission Foucault has given way on his rather strong, but false, doctrine. His position ends up rather like that endorsed by many sociologists of science, but not sociologists of scientific "knowledge" who advocate SP.

11.6 THE STRONG PROGRAMME AND REALISM

In the above several criticisms of the Strong Programme for the sociology of scientific knowledge have been made; and it has been shown that, despite superficial differences, Foucault's power/"knowledge" shares many of its core difficulties. Most of these difficulties have to do with the role assigned to principles of critical inquiry; either they have been sidelined, or they are reconstrued as merely more in the way of matters to be given communal (and thus social) endorsement, or they are belatedly recognised as playing some role. Here we wish to discuss SP in connection with the realism adopted throughout this book.

Is SP inconsistent with scientific realism? It is often called 'constructivist', presumably because of the way in which social factors are said to cause, and thereby construct, people's scientific beliefs. Certainly if we are to take seriously the casual aspect of the Causality Tenet, then we need to have real casual relations obtaining. And they obtain between the effects, viz., the (scientific) beliefs that individual people hold, and the causes, which are naturalistic properties from those of biology and psychology to those of sociology. Since the social factors are meant to be omni-present, then a realist attitude towards them seems appropriate as well. Does this mean we need be realist about nothing more than the entities that sociology postulates? This fits with SP since it goes along with its naturalism. What causes scientific belief is either something social, or an interest in the social; and such items are all part of a naturalistic social science.

Consider now the Causality Tenet of SP in which naturalistic social factors, or an interest in these, are at the cause end of the cause-effect relation. It is not clear just how much of what goes on at the effect end of the causal relation is to be taken realistically. Certainly the beliefs themselves are to be taken realistically. This raises a question concerning the contents of the beliefs and what they purport to be about. And on this SP is silent. After all, in Forman's study about the causes of belief in acausality, what are we to say about causality in the world? Does it hold or not? Or is the belief in acausality just a reflex of social happenings, merely a piece of social imagery? Are the beliefs about causality merely froth on top of the nexus of social and other factors that give rise to them?

Various sciences postulate entities such as electrons, tectonic plates or quasars; and they propose laws. What are we to say about beliefs in the laws, or the existence of unobservables, of such sciences? Are such beliefs true? Are there really such entities as electrons? The matter is not clear, because as has been mentioned, SP only makes claims about the social causes of belief. It can appeal to no methodological principles, such as inference to the best explanation, which realists use to show that their theories are true, or at least partially true. So whether our beliefs are about anything, or are true or false is

a matter upon which SP cannot adjudicate. It simply tells us what are the causes of belief; and these causes are socio-cultural (or interests in the socio-cultural).

Let us suppose one adopts the stance of a realist and considers, from that stance, how people arrive at the theories they believe. Realists do allow that people can come to accept theories by using methodological principles (of the sort mentioned in Part II and in the rationality model for explaining beliefs). But they can also allow that some people are socially caused to believe in scientific theories. Realists can allow, as we commonly do, a variety of ways in which people come to believe the sciences they do; only some of these causes of belief need appeal to principles of critical inquiry. Realists are not single-minded about this, as are advocates of SP with their Symmetry Tenet.

Now take the stance of an advocate of SP and ask whether they need be realists towards the theories that they are allegedly socially caused to believe. Need they be realists? The answer is 'no!'. These advocates are merely concerned with the social factors that allegedly cause scientific beliefs. They need not even be concerned with the truth of these beliefs, as the Impartiality and Symmetry Tenets require. Nor need they be concerned with the status of the laws and theories that are the contents of the beliefs. They could take a realist interpretation of the theories they believe, but they are not constrained by anything in SP to do so. If they do adopt realism, then it is not clear on what grounds they might do so, since they do not accept any methodological principle that might determine when one ought to take a theory realistically. It is also open to advocates of SP to be non-realists such as constructive empiricists, or instrumentalists, about the theories believed. Once again their concern is merely with the socio-cultural causes of belief, and not the status of what is believed, viz., whether some theory in, say, physics is to be taken realistically or non-realistically. SP needs just enough realism to ensure that its main claims about the causation of belief have some purchase on the world; the status of the contents of the believed theories is up for grabs as far as they are concerned. Their strongly constructivist stance would indicate that they would be more happy with a non-realist construal of the rest of science.

This completes our account of the views of those who allege that social factors are the causes of scientific belief. This is a view that is best articulated with SP; but it can also be found in writers such as Foucault. And as we will argue in the next chapter, it is also to be found in Lyotard. Such writers, and many others of their ilk, have had a strong impact on accounts given by many in science education of the way in which scientific theories come to be held. We do not wish to deny that, for *some* who hold scientific beliefs, their beliefs are causes in the way SP, and others, say. What we wish to deny, and argue strongly against, is that *all* people, including scientists, come to hold their beliefs in this way, and that principles of critical inquiry play no role in the

formation of belief. If our opponents are right, then there is no scientific knowledge in the sense of 'knowledge' set out in Part I. And there are no methodological principles of the sort set out in Part II. All is just froth in the top of socio-political and cultural circumstance – including belief in SP itself! But this we have seen is a position that does collapse under critical investigation.

NOTES

[1] See the classic Kahneman, Slovic, and Tversky, (eds.) 1982, and the survey book by Manktelow and Over 1990, or the cognitive psychology book Sternberg 1999, Chapter 12. The literature on this issue is quite large, some of which can be garnered from these books.

[2] Sometimes acausality is intended to be a rejection of the principle of causality which says that all evens are caused. On other occasions it is intended that all laws are irreducibly statistical. On yet other occasions it is intended that all explanations of nature will be irreducibly statistical or probabilistic in character. Though it is taken to range over a wide variety of things, in the context above nothing turns on which account of acausality is adopted

[3] See Nola 2003, Chapter 7, for a discussion of the alleged social basis of rules and the use sociologists of science make of Wittgenstein's ideas on rule-following to give further credence to the claim that all evidential claims are merely locally accepted as such and have no further underpinning in a theory of the rationality of principles of critical inquiry.

[4] Remarks by Bacon about the connections between knowledge and power occur in several places in his works. The above quotation comes from Bacon's Book I, Aphorism III of his *Novum Organum* (any edition); see also Book II, Aphorisms I and IV.

[5] These remarks are extracted from the 'Preface' to his incomplete 'The Great Instauration'. See also *Novum Organum*, Book I, Aphorism CXXIX, for similar sentiments.

CHAPTER 12

LYOTARD, POSTMODERNISM AND EDUCATION

Postmodernism is another fashionable current of thought that found its way into the literature on education. Although it was not as popular as constructivism in education circles, it nevertheless resonated strongly in the minds of many theoreticians of education and enjoyed considerable support by them (see, for instance, Aronowitz and Giroux 1991, Doll 1993, Giroux *et al* 1996, Peters 1995 and Slattery 1995). Like constructivism, postmodernism too had a lot to say about the relationship between social conditions and knowledge as well as about the nature of science, truth and reason in the postmodern era we are supposedly living in. Both theories are strongly anti-realist and anti-rationalist, and that makes them natural allies. Although postmodernism says nothing directly about *science* education, because of what it says about the nature of science, knowledge, truth and reason and about the general role of education in today's "postmodern" societies, it has important implications for science education as well, especially in relation to curricular and policy issues.

Almost all postmodern theorizing in education follows, almost word-by-word, Lyotard's 1984 book *The Postmodern Condition: A Report on Knowledge* (referred to hereafter as *PC*), which had enormous impact in fields ranging from philosophy, cultural studies, literary criticism to education. It is therefore appropriate that we turn to a careful examination of this book before we discuss its appropriation by educationalists. Accordingly, most of this chapter will be a detailed, critical scrutiny of all the main tenets of *PC* itself. Given the enormous influence of Lyotard's views and given that, surprisingly, no such criticism exits, we believe that such pain-staking scrutiny is necessary.[1]

We begin in Section 12.1 with Lyotard's differentiation between narrative and scientific knowledge in which the former is said to legitimate the latter. This we argue is a red herring, something, as we will see, that Lyotard also came to think – despite the prominence that many give to it. In Section 12.2 we consider Lyotard's account of science. To our surprise this often turns out to be more narrowly positivistic than the account given by most positivists. Despite Lyotard's references to philosophers of science such as Kuhn, his own position is often a crude positivism. In Section 12.3 we examine his account of knowledge, its nature, and the role it allegedly plays in contemporary society. We argue that Lyotard's critical position is more often directed against information and belief than knowledge as such. In Sections 12.4 and 12.5 we turn to his 'incredulity towards metanarratives' and his

notion of legitimation. We distinguish between two different kinds of legitimation project not always carefully distinguished by Lyotard. The first is what might be called 'epistemic legitimation' (or what philosophers commonly call 'justification') of science and its methods; the second concerns the socio-political legitimation of science. We find that Lyotard's critical forays are directed more against the socio-political legitimation of science than its epistemic legitimation, but the two are often conflated, and criticisms of the former do not carry over as criticisms of the latter, once the two kinds of legitimation are distinguished. We will argue that Lyotard gives a bad argument against the possibility of epistemic legitimation. His main focus is on the socio-political legitimation of science, but we find his claims about lack of such a legitimation unconvincing (Section 12.6). In Section 12.7 we turn to his views on performativity and paralogy and conclude that there is not much substance to them. Finally, in Section 12.8 we look at the implications and the use educationalists have made of Lyotard's views for education.

12.1 THE NARRATIVE/SCIENCE DISTINCTION

Lyotard makes much of what strikes us as an unnecessary and overdrawn contrast between narratives and science. In the 'Introduction' to *PC*, Lyotard tells us: 'Science has always been in conflict with narratives. Judged by the yardstick of science, the majority have proved to be fables' (Lyotard 1984, p. xxiii). He emphasizes this further on saying: 'it [scientific knowledge] has always existed in addition to, and in competition and conflict with, another kind of knowledge, which I will call narrative . . .' (Lyotard 1984, p. 7). He even attributes a kind of superiority to narrative knowledge over scientific knowledge since the former can have a role in legitimating the latter but not conversely. This becomes clear in his discussion of Plato's narrative of the cave, which is said to legitimate the new (at the time) language game of science allegedly found in his dialogues:

> The fact is that the Platonic discourse that inaugurates science is not scientific, precisely to the extent that it attempts to legitimate science. Scientific knowledge cannot know and make known that it is the true knowledge without resorting to the other, narrative, kind of knowledge, which from its point of view is no knowledge at all.' (Lyotard 1984, p. 29)

There is much to object in this (other than the dubious Plato scholarship). If a 'narrative' means the telling of a story, perhaps with due attention to making the story amusing or engaging or edifying or whatever other criteria one might adopt for the good telling of a story, then Lyotard is wrong. Science too has its narratives. But unlike fables, which are not directly concerned with truth, the narratives of science must at least be largely true if they are to be acceptable. Thus, for example, Robert Hooke presented to the Royal Society bi-weekly descriptions (narratives) of what he viewed under a

microscope (such as the holes in cork, the eyes and hairy legs of flies, the point of a pin, the micro-contents of water, etc). Many of these reports were published in his *Micrographia* of 1665. Jane Goodall has done ground-breaking work in her many reports (narratives) of the behaviour of the chimpanzees based on her careful observations made over many years in Gombe Park, Kenya. Science is replete with such narratives. It would only come into conflict with narratives, understood as fables, if the latter purported to give descriptions of actual happenings and places which were false, or attempted to provide rival explanations of happenings in terms of supernatural powers.

Lyotard also talks of 'grand narratives' indicating a difference between them and ordinary 'narratives'. While it is not always clear what the difference might be, let us take grand narratives to involve at least the following three features: (a) a marked increase in scope of the issues covered by the 'grand narrative', (b) a greater degree of explanation of these issues (rather than mere description of the issues, since narratives are more often descriptive than explanatory), and (c) a justification (Lyotard talks of 'legitimation' here) for many of the claims that come within its scope. He also uses what seems to be a more technical term 'metanarrative', the term 'meta' indicating that there is a narrative to be told *about* another narrative (in much the same way as there are in logic and semantics meta-languages which have as their object other languages, i.e., they are *about* those languages). Are all meta-narratives also grand narratives? There is no reason why this should be so. That a language is about another language, in the sense common in logic and semantics, does not entail that it is also a grand narrative in having increased scope and explanatory and justificatory force. Conversely, not all grand narratives are meta-narratives. Thus Hegelian, or Marxian, philosophies are often invoked by Lyotard as grand narratives; but it is not clear that either is also a meta-narrative in the sense of being *about* some other subject matter.

In subsequent writings Lyotard is aware that there is something wrong with his account of knowledge and narrative in *PC*. He tells us: 'In the *Postmodern Condition* and other books of that time (at times in *Pagan Instructions*) I exaggerated the importance to be given to the narrative genre… Specifically I went too far in identifying knowledge with narrative' (Lyotard 1993, p. 19). Further he adds: 'It is inadvisable to grant the narrative genre an absolute privilege over other genres of discourse in the analysis of human, and specifically linguistic (ideological), phenomena, particularly when the approach is philosophical. Some of my earlier reflections may have succumbed to this 'transcendental appearance' (... even the *Postmodern Condition*)' (*ibid*., p. 23).

What Lyotard is more willing to recognize is the range of different uses to which language can be put, from narratives of fable and fiction to science —

to which he adds that none is privileged over any other in any way. Lyotard's reasons for revising his views on narratives are linked to his more explicit postmodernist view that there is to be no privileging of any point of view, including fabulous narratives over scientific narratives; all are on a par. The above comments do betoken Lyotard's dissatisfaction with the elevated role he had given to the notion of a narrative in *PC* (but for reasons we would not endorse). We can now take it that Lyotard has abandoned his implausible view that science, even the science alleged by him to be found in Plato's dialogues, is to be legitimated by narratives, like those of the allegory of the cave given in the *Republic*. The revised view of narratives, in which no non-scientific narrative can legitimate any scientific narrative, leads to the abandonment of one of the important, but implausible, claims of *PC*.

12.2 LYOTARD ON THE NATURE OF SCIENCE

Lyotard tells us that 'scientific knowledge requires that one language game, denotation, be retained and all others excluded. A statement's truth-value is the criterion determining its acceptability' (Lyotard 1984, p. 25). Now, what does Lyotard mean by 'denotation', or as he sometimes also says 'referent'? It turns out that these have nothing to do with Frege's notions of denotation and referent despite Lyotard's reference to him (Lyotard 1984, p. 92, fn. 86). In a footnote concerning what he calls a 'denotative utterance' 'the university is sick', he says: 'Denotation' corresponds here to 'description' in the classical usage of logicians' (Lyotard 1984, p. 88, fn. 29). And elsewhere he writes: 'Learning [*connaissance*] is the set of statements which, to the exclusion of all other statements, denote or *describe* objects and may be declared true or false' (Lyotard 1984, p. 18; our italics). This is not entirely unambiguous. But we guess that what is intended here is that a denotative utterance or sentence is *descriptive*, in contrast to being a *command*, or a *question*, or an *expression of emotion*, and so on for other illocutionary acts. Thus, it appears that 'denotative' sentences purport to *describe* facts or states of affairs if they are true, and fail to describe if they are false. The emphasis, despite the language Lyotard uses, is not so much on the Frege's notion of the *denotation* of sentences, but on their being *descriptive* (or not) of states of affairs. But at least because they are descriptive, such sentences will have one of the two truth-values.

More than this is needed to characterize science. We will explore some extra conditions that Lyotard adds to his denotation requirement and show that they have a surprisingly narrow empiricist tone despite the occasional reference to Kuhn's conception of science. In the passages to be examined in (a) to (e) it is not always clear that what Lyotard says is also what he endorses; but unless explicitly indicated we will take the following to be

views he does endorse. We will argue that all represent untenable views about science.

(a) First Lyotard tells us:

> It [science] is also composed of denotative statements, but imposes two supplementary conditions on their acceptability: the objects to which they refer must be available to repeated access, in other words, they must be accessible in explicit conditions of observation; and it must be possible to decide whether or not a given statement pertains to the language judged relevant by the experts' (Lyotard 1984, p. 18).

The second condition remains obscure, especially in the light of the vague footnoted reference to Popper (with no specific page references) who is meant to provide clarification. In fact Popper, who was opposed to the positivists' undue focus on the language of science, put emphasis on judgements about hypotheses and not the language in which they are couched. The first condition seems narrowly positivist in that any reference to objects must be to observable (accessible) objects. Such a positivistic condition would hardly have been endorsed by Popper whom Lyotard cites in the footnote.

(b) Elsewhere Lyotard tells us that 'a scientific statement … is subject to the rule that a statement must fulfil a given set of conditions in order to be accepted as scientific … in general conditions of internal consistency and experimental verification …' (Lyotard 1984, p. 8). But not all the sciences are experimental; and in any case verification has long been abandoned as a condition for being scientific since, as positivistically inclined philosophers showed, no satisfactory version of the verification principle could be expressed.

(c) Lyotard's conception of science is connected to his rejection of the correspondence theory of truth (Lyotard 1984, p. 24). It is based on the following argument: 'the rule of adequation [another name for the correspondence theory] becomes problematical. What I say is true because I prove that it is – but what proof is there that my proof is true?' (*ibid*., p. 24). This is a cryptically and poorly expressed version of the *inaccessibility of reality argument* (IRA, for short). Like many constructivists and other anti-realists, Lyotard too is relying on it to undermine the correspondence theory of truth. Since we have dealt with this argument in Chapter 5 in detail, we will say no more about it. Consider what Lyotard says right after invoking it:

> The scientific solution of this difficulty [concerning the correspondence theory of truth] consists in the observance of two rules. The first of these is…: a referent is that which is susceptible to proof and can be used as evidence in a debate. Not: I can prove something because reality is the way I say it is. But: as long as I can produce proof, it is permissible to think that reality is the way I say it is. The second rule is metaphysical; the same referent cannot supply a plurality of contradictory or inconsistent proofs. … These two rules underlie what nineteenth century science calls verification and twentieth century science, falsification (*ibid*., p. 24).

Now, as every student of philosophy knows, not even a rampant positivist thought the criterion for science was either verification, or falsification, or both. Rather, they talked of *verifiability* and/or *falsifiability*. Popper, who used a more sophisticated version of the falsifiability criterion, was forever at pains to distinguish falsifiability from falsification; moreover, he also supplemented his demarcation criterion with rules of method that went along with the goal of falsifiability (see Section 8.6 for more details). If we take Lyotard's last sentence to refer not to verification and falsification but to *conditions* of verification and falsification, then we can speak of the sentences of science as having verification and falsification conditions – and then read this as a somewhat narrowed version of the broader notion of sentences having conditions of warranted assertability. Though Lyotard says none of this, we find it the most charitable way of understanding his remarks concerning the nature of science. Otherwise, we are left with an account of science even narrower than that of the crudest positivists.

The first rule invoked by Lyotard in the above is more closely linked to the IRA argument and tells us that we are not to accept a scientific sentence as tested because of its correspondence with some antecedent reality. Rather, we are to accept it as tested because there is a 'proof' of it. It is unclear whether by 'proof' Lyotard means the more common-sense claim that, based on the *evidence* we possess, there is good reason to think that the sentence is true. In this *evidential* sense we commonly speak of a claim having a proof, even a proof beyond a reasonable doubt (as in the presentation of evidence in courts of law). But Lyotard often intends a more technical sense in which the sentence comes out as a conclusion of a proof in some formalized system of the science of which it is part. If this is the case, then we can attribute to Lyotard a much more constructivist account of science along the lines once suggested by Putnam (Putnam 1978, end of Lecture II and beginning of lecture III).[2] The difference is between the statements of a theory being true or false in respect of an independent reality (whether or not we have evidence either way), and the sentences of the theory merely being provable in some system. Though there are no explicit comments to the effect that Lyotard adopts such a constructivism, it certainly fits with his anti-realism, often explicitly stated. It also fits with the account of science discussed in (e) below. However, such a preliminary account of the status of scientific sentences takes us nowhere near the much more sophisticated accounts of the nature of science in terms of scientific method that can be found in a wide range of philosophers from the positivists to Popper, Lakatos, Kuhn and to the Bayesians.

(d) It might be thought that Lyotard does approach a more sophisticated account of science when he says: 'one is a scientist if one can produce verifiable or falsifiable statements about referents accessible to the experts' (*ibid*., p. 25). The reference to verifiable and falsifiable statements is a move

in the right direction, but it is still too narrow in ignoring theories of confirmation (such as Bayesianism discussed in Chapter 9). It is also spoiled by the claim that the verifiability or falsifiability pertains only to statements 'about referents accessible to the experts'; this vague criterion is too operationalist and leaves out referents which are deeply theoretical, such as quarks or black holes.

(e) The final attempt to supplement the above characterizations of science occurs in Section 11 of *PC*, where Lyotard sets out a very elementary and jejune account of the nature of axiomatic systems. Rather than criticize this we will pass over it and glean what we can from subsequent remarks about the nature of science from an axiomatic point of view. After his account of axiomatic systems (Lyotard 1984, pp. 42-3), Lyotard makes a link to science saying of the axiomatic approach:

> This necessitates a reformulation of the question of the legitimation of knowledge. When a denotative statement is declared true, there is a presupposition that the axiomatic system within which it is decidable and demonstrable has already been formulated, that it is known to interlocutors, and that they have accepted that it is as formally satisfactory as possible.' (Lyotard 1984, p. 43)

The grand issue of legitimation appears to be at stake because Lyotard thinks that the axiomatic approach requires a reformulation of the question of legitimation. We may suppose in this context that what is meant by legitimation is the conditions under which we can accept a statement of science as true, or what would be more normally described as the evidential support a scientific statement gets.

Within philosophy of science the heyday of axiomatisation was short-lived. Few successful axiomatisations were achieved, and more formal approaches to the sciences have tended to be model-theoretic rather than axiomatic (see, for example, Stegmüller 1976, Part I; van Fraassen 1980, Chapter 3; and Giere 1988, Chapters 3 and 4). Lyotard is overly optimistic about what axiomatics can do within empirical science. First, it is hardly ever the case that scientists know, or philosophical reconstructors of their system of beliefs know, into what axiomatic system a scientific statement is to fit. So few sciences have ever been properly axiomatised – one of the few cases is the axiomatisation of particle mechanics (see Stegmüller 1976, Chapter 6). On the whole most sciences have grown without axiomatisation. For this reason, Lyotard's talk of a presupposition that there is an axiomatised theory into which all scientific statements will fit is a gross exaggeration. In most cases it is simply not known what an even minimally adequate axiomatisation would be like.

Lyotard seems to make another important false presupposition. Even if an axiomatic system for an empirical theory is set up, this by no means shows that it is a true theory. What is needed is empirical evidence for the axioms, and the theorems, of the system. Mere proof of theorems from axioms does nothing to show that we have an empirical theory that is true of this world, or

even one that gets strong support from evidence. Does Lyotard confuse deductive proof of a hypothesis within an axiomatic system with the empirical evidence for the hypothesis? It is our conjecture that he does. Consider the following:

> The argumentation required for a scientific statement to be accepted is thus subordinated to a "first" acceptance (which is in fact constantly renewed by virtue of the principle of recursion) of the rules defining the allowable means of argumentation' (Lyotard 1984, p. 43).

We leave the reader to make his or her own sense of the parenthetical remark about recursion. What the rest of the quotation suggests is that within an axiomatic system there are rules of inference which help set the bounds for what is, and what is not, an acceptable argument within the system. Note that here Lyotard talks of the deductive rules of proof within a system and not the rules which link evidence to hypotheses (as axioms or theorems). So it looks as if he does confuse deductive proof of hypotheses with confirmatory evidence for them in his talk of the acceptance of scientific statements.

So much then for Lyotard's conception of science. Let us see if his views on knowledge are any better.

12.3 LYOTARD ON KNOWLEDGE

One of the main theses of *PC* is Lyotard's 'working hypothesis that the status of knowledge is altered as societies enter what is known as the postindustrial age' (Lyotard 1984, p. 3). Elsewhere he says: 'The nature of knowledge cannot survive unchanged within this context of general transformation' (*ibid*., p. 4). Now it is true to say that the body of human knowledge has increased over time (and perhaps we have lost some knowledge over this time as well). But in talking about a change in the *nature* of knowledge, or its *status*, is it meant that in the context 'S knows that p', the meaning of 'know', or its success and performative features, have changed? Nothing that has happened in our postindustrial society recently has impinged on this issue leading one to change any definition of 'know'. Though there are rival accounts of the 'nature' or 'status' of knowledge (see Chapter 4), nothing about processes of social change impinges on the nature or status of knowledge or the philosophical debates about this status. We need to keep separate philosophical theories within epistemology, which are about the nature or definition of knowledge, from empirical theories from sociology about alleged causal or law-like connections between 'knowledge', or more correctly belief or information, and the socio-politico-cultural circumstances of people as cognitive agents. In the previous chapter we have seen that it is part of the hubris of the sociology of 'knowledge' that it impinge on philosophical theories of knowledge in an attempted takeover.

In the light of the above, we turn now to Lyotard's extended remarks on the sender-sendee speech act relation in ordinary communication. Even if we

as senders transmit what we know to another person (the sendee), does it follow, as Lyotard suggests,[3] that sendees thereby acquire knowledge? Not so, unless sendees are also sent a sufficient justification, or provide their own, for their newly acquired belief (supposing an internalist justified true belief theory of knowledge). Otherwise, their newly acquired belief will remain just that, a belief or a piece of information, available for transmission.

All of this is a far cry from knowledge of the 'S knows that p' variety. Knowledge is normative in that it requires justification; and, furthermore, the use of the word 'know' has a performative and success character in that it indicates that its user has achieved something, that he or she cannot know incorrectly (see Ryle 1963, pp. 145-147). But these hardly ever circulate in society; rather it is knowledge's propositional content, or informational content, that gets transmitted. As a *façon de parler* we do talk of knowledge, as Lyotard and his translator do; but the more accurate term for his, and our, purposes is 'information', or 'correct information'.

Setting aside all talk of *knowledge that p* and its normativity, we can now consider the informational content of that knowledge, viz., *that p*, and ask the following. Who is the originator of it? Do they own it, or have some rights not only to the process that produced it but also the communication of the very content, that p? Is the communication of the content that p only on the condition of equivalent exchange, or an exchange based on the utility of the informational content that p to a user of it? And so on. Here one can talk, as Lyotard does, of the commodification of 'knowledge'. But this is not strictly correct because what is commodified is not the *knowledge* that p (by its very definition it cannot be commodified), but rather the *informational content* that p.

We find that Lyotard would not demur from our suggested revisions of his views because he actually says:

> Knowledge in the form of an *informational* commodity indispensable to productive power is already, and will continue to be, a major – perhaps *the* major – stake in the world-wide competition for power. It is conceivable that the nation-states will one day fight for control of *information* just as they battled in the past for control over territory ...' (Lyotard 1984, p. 5)

Even Lyotard feels the need to talk of information (using the equivalent French word 'information') rather than knowledge. In making the distinction the socio-political claims about knowledge-as-information can be separated, as they should be, from epistemological concerns about knowledge proper and its nature. In the light of this, Lyotard's book is not a contribution to the theory of knowledge, and it is a mistake to think that it is. But it does provide some suggestions about the ways in which information is transmitted in society and how this might have recently altered its character due to new telecommunication technology. But these are sociological theses about the circulation of information in society open to investigation by the canons of science, as ought to be any other claims in sociology.

Two final points. First, the issue of knowledge and power. Lyotard almost makes an identification of the two in the style of Foucault's doctrine of power/knowledge when he says of 'the current status of scientific knowledge' that it reveals 'that knowledge and power are simply two sides of the same question: who decides what knowledge is, and who knows what needs to be decided?' (*ibid*., pp. 8-9). But no one *decides* what *knowledge* is. The definition of 'S knows that p', or of 'S knows why p', for instance, is not a matter for decision or legislation; and in any such definition truth and evidence will have a role. Concerning the sociological 'knowledge'/power nexus, we have pointed out in the previous chapter that ever since the days of Francis Bacon it has been known that with an increase in knowledge about how the world works there is the possibility of an increase in human powers in virtue of the gained knowledge. Here epistemic gain makes possible enhanced powers. And, again as Bacon recognized, there is the need to make decisions about what to research and how to fund it. In this case some power might need to be exercised (but not necessarily so) in order that there be some epistemic gain. But this is by no means guaranteed even if power is exercised; power might arise from epistemic gain but it is not a sufficient condition for epistemic gain. Just as one can reject Foucault's alleged equivalence in the slogan 'power is knowledge', so we can reject Lyotard's idea that somehow they are 'two sides of the same question'. To work out the inter-relation between power and knowledge in any single case of knowledge gain is a matter of empirical investigation and not a matter of *a priori* stipulation either by Foucault or Lyotard.[4]

Second, Lyotard claims that as 'knowledge' becomes a commodity through the exchange value it has acquired, 'it loses its "use value"' (*ibid*., p. 5). But this is just wrong. Understanding use value as the utility of some thing to a user of it, the thing retains its use value and, because of this, can acquire an exchange value (thereby becoming a commodity). The terms are from Marx (1965, Chapter 1 Section 1) who said that a necessary condition of something acquiring an exchange value is that it have a use value in society in general; if an item has no use for anyone then it cannot be exchanged. That a piece of information has use value is necessary for it to have exchange value; commodified information must retain its use value and not lose it.

12.4 EPISTEMICALLY LEGITIMATING THE GAME OF SCIENCE

Lyotard characterizes what he calls 'the problem of legitimation' as follows:

> [Science] is obliged to legitimate the rules of its game. It then produces a discourse of legitimation with respect to its own status, a discourse called philosophy. I will use the term *modern* to designate any science that legitimates itself with reference to a metadiscourse of this kind making explicit appeal to some grand narrative, such as

> the dialectics of Spirit, the hermeneutics of meaning, the emancipation of the rational or working subject, or the creation of wealth. (Lyotard 1984, p. xxiii)

Since, according to Lyotard, all such grand narratives, including those that appeal to 'truth' and 'justice' *(ibid*., p. xxiv) have lost their credibility, we are faced with the problem of legitimating science. This is Lyotard's most central thesis that pervades his entire book. Indeed, he goes as far as defining 'postmodern' as 'incredulity toward metanarratives' of any kind (*loc. cit*.). Let us then give it due attention. We begin by some general remarks.

First, though it does not always emerge clearly in Lyotard's writings, there are two major sorts of legitimation to consider. The first concerns the statements of science, the observational reports, hypotheses, laws and theories of science, and the way they have been tested using various principles of scientific method. This we will call either 'justification', or, in linking it to Lyotard's terminology, the 'epistemic legitimation', of the content of science. The second kind of legitimation, left for discussion in Section 12.6, is the kind of social or political justification of science, as applied in technology or as an institution, because of the promised goals of human emancipation or increased freedom that have been alleged will arise from science. This we will call 'socio-political legitimation' to distinguish it from 'epistemic legitimation'. Keeping these two claims separate is highly significant since what is at issue is the important matter of the 'legitimation of science', a phrase ambiguous in its content. We will argue that, contrary to Lyotard, there is a case to be made for the epistemic legitimation of scientific statements; a quite different case has to be made for claims on behalf of the socio-political legitimation of science as an institution or as applied in human life.

In science we give 'proofs', that is, provide evidence for our beliefs, and that is how we justify (legitimate) them. But one of Lyotard's common responses is to say: '"How do you prove the proof? " or more generally "Who decides the conditions of truth?"' (*ibid*., p. 29). (Note that these are not strictly alternatives; confusion results from treating them as the same.) In a nutshell this is the issue of epistemic legitimation, viz., 'proving the proofs'. It may be thought that by posing Lyotard's question in this way a quick knock-down is achieved for any proof whatever, viz., for any proof at one level there must always be another proof of that proof at the next level up – so there is an infinite regress of proofs. But this is far too swift and unsatisfactory. In this section we will show that there are good grounds for thinking that there are successful ways in which the epistemic legitimation of science has been carried out. In the next section we will argue that Lyotard has no good positive case to make against the possibility of epistemic legitimation based on his considerations of language games.

Just as there are rules for conducting the game of chess, so there are rules for the game of science, which any theory of method should attempt to set

out. So, in science not only are there rules of deductive and inductive reasoning which require justification, there are also specific methodological principles for the proper conduct of empirical science, which need justification. These rules and their justification have been a matter of intense debate between inductivists, Bayesians, and the various followers of Popper, Lakatos, Kuhn and Feyerabend, not to mention a host of others such as Quine, Laudan and Rescher. To this debate Lyotard has made no contribution at all.[5]

The problem to be addressed here is the following. We need to distinguish three levels of the scientific enterprise. The first level is the history of the sequences of theories within the different sciences since their inception, whatever the science be, such as the theory of dreams, or of motion, or of the business cycle, etc. The second level concerns principles of scientific method which are used to adjudicate between, and to justify, the claims of science at the first level. Finally, there is a third level, a meta-level, at which we might ask about the justification of the principles of method at the second level. Thus, there are two epistemic legitimation problems. One is the epistemic legitimation of the claims of science using principles of method; we have devoted Part II to such a legitimation in terms of three major methodologies, inductivism, hypothetico-deductivism and Bayesianism, to show how scientific claims can be successfully justified through them, contrary to what Lyotard claims. The second is the epistemic legitimation of principles of method using whatever there may be in the way of remaining methods of justification of an *a priori* and empirical character at the third meta-level. Such a hierarchy will be familiar to anyone who has investigated the attempts of Popper, Lakatos and Laudan, inductivists, Bayesians and even the later Kuhn (just to mention a few) to justify the adoption of some principles of method, and the rejection of others, by the employment of meta-methodological principles. The enterprise of epistemic justification is an ongoing area of investigation with some successes. And it has a close match with Lyotard's talk of the 'legitimation' of principles of method from the stance of meta-methodology. It would be incumbent upon any postmodernist, incredulous of such attempts at justification, to show that none of them work. But they have hardly entered into the lists of debate with any of the philosophers of science mentioned above to show that none of the principles of method that they advocate have any legitimacy.

In the light of this, which of the four 'grand narratives' that Lyotard lists (i.e., 'the dialectics of Spirit, the hermeneutics of meaning, the emancipation of the rational or working subject, or the creation of wealth') has allegedly provided 'legitimation' for the claims (we will resist the loaded term 'discourse') of science? Neither within the positivist pre-World War II movement in Europe (which seemed to have left the French untouched), nor within subsequent developments in Anglo-American philosophy (with its

current European offshoots), have any of the four grand narratives Lyotard mentions played any role in the *epistemic* legitimation of the claims of scientific methodology, or the legitimation of the claims of the particular sciences themselves. Only some French philosophers might still think that Hegel has anything to offer the sciences, his views being long ago abandoned within positivism and Anglo-American philosophy. It is unclear what the 'hermeneutics of meaning' might mean in this context. Most philosophers would doubt that a justification for the methods of science could arise from the mere contemplation of meanings, or of interpretations.[6] Finally, it is obvious that the last two 'grand narratives' are irrelevant to the justification of methods and the sciences; they belong more with the socio-political legitimation of science.

Let us now turn to his definition of the term 'postmodern': 'Simplifying to the extreme, I define *postmodern* as incredulity toward metanarratives' (Lyotard 1984, p. xxiv). Note that this defines a descriptive predicate 'postmodern'. It says nothing about whether we *ought* to be postmodern, that is, *ought* to be incredulous towards metanarratives. The latter requires an argument from premises to the normative conclusion. Does Lyotard give us such an argument? In the next section we will show that his one attempt at an argument turns on a bad logical howler, thereby increasing our incredulity that anyone should adopt his position on postmodernity in science.

Suppose we ought to be incredulous towards any metanarrative, in particular, we ought to disbelieve any third meta-level attempt at justification. Two consequences flow from this. First, if we do not believe that there is any meta-justification, and no other non-meta-modes of justification seem apparent for Lyotardians to use, then it follows that there is no justification available for the principles methodology we adopt. Second, if there are no ways of justifying any of our methodological principles for adjudicating between rival theories or justifying our theories, then there is no justification for the very claims of science themselves. The sceptical position is extreme. Not only are we to disbelieve meta-methodology, we are also to disbelieve methodology, and in turn to disbelieve any science (since what science we do believe ought to have some justification in accordance with methodological principles – but these are to be disbelieved). Though Lyotard wishes us to be sceptical of meta-narratives, it is an unexpected consequence that we are also to be sceptical of methodological and inferential principles of reasoning, and thus of the very sciences themselves. Lyotardians need to stop this slide somewhere, but it is hard to see how they could. In throwing out the bath water of meta-legitimations or any other kind of legitimation, they have also thrown out the baby of science itself.

12.5 LANGUAGE GAMES AND LEGITIMATION

In Section 3 of *PC*, Lyotard sets out his views on language games derived from Wittgenstein's and Austin's theory of speech acts. He says that just like the rules for the game of chess, for language games 'each of the various categories of utterance can be defined in terms of the rules specifying their properties and the uses to which they can be put' (*ibid*., p. 10). On such games Lyotard makes three observations; we will focus mainly on the first of these and address the other two at the end of this section. 'The first is that their rules do not carry within themselves their own legitimation, but are the object of a contract, explicit or not, between players' (*ibid*., p. 10). It is hard to know what might constitute the 'legitimation' of such rules since what is set up is a convention amongst speakers (Lyotard uses the word 'contract') that in a given context a locutionary act is, say, a question rather than a statement, or a warning, etc. Such conventions are for agreement or adoption in a speech community. Also, the conventions hardly stand in need of any kind of legitimation in the sense of an *epistemic* justification. But we could justify them in another sense, namely that we need a range of illocutionary acts to go along with all the activities of our rich life. Not to have some, for example, not to be able to make statements or put questions, but only to issue commands or make pleas, would be a debilitating restriction on our 'form of life' (to use a Wittgensteinian locution).

Granted this, it is important to distinguish between the following: (a) the lack of a further justification for the rules of a language game which we adopt by convention; and (b) a move in the language game in which we do something which is then open to an investigation as to its legitimacy, justification or appropriateness. Using Lyotard's example, 'the University is sick' (*ibid*., p. 9), being a statement, will have a truth-value, will stand in logical relations and, if contested, will need to be justified epistemically. Importantly, the statement's truth value, logical relations and justificatory relations are not matters of mere convention; they turn on objective non-conventional matters such as what are the facts, and logical matters such as what it entails or is entailed by it, support evidence gives to the statement, and the like. The conventions whereby Lyotard makes this statement *qua* locutionary and illocutionary *act* may be a matter of an agreement between speakers of the language that is beyond legitimation of certain sorts. But the *statement made* then stands in quite objective non-conventional relations to the world, to other statements and to evidence, that are not a matter of convention. Such a distinction parallels another older act/content distinction in which there is a separation between an *act* of stating or asserting and the *content* stated or asserted. There are conventions governing the act and, once agreed to, these lack further need for legitimation. But in respect of its content, truth, logical and evidential relations are not a matter of convention

and can stand in need of scrutiny and justification. In this way we can stop a fallacious slide from claims about the conventional rules of language games that need no further legitimation, to the further claim that the products of those games such as statements (within science or outside) are also a matter of unlegitimisable convention. Does Lyotard make this slide? We will show that he does after setting out more of his views on speech acts.

Commands such as 'open the door' can be felicitous or not. They will be felicitous if the door is shut and someone can carry out the command as *per* normal; but they will be infelicitous if the door in question is already open, and the command cannot be obeyed. Here we need to qualify a remark of Lyotard when he says: 'Take, for example, a closed door. Between "The door is closed" and "Open the door", there is no relation of consequence as defined in propositional logic' (*ibid*., p. 40). No reason is given for this claim; but a conjectured reason might be that the second sentence is not a proposition and so cannot be a substitution instance for any propositional variable in a purely propositional logic. However, this is to overlook the point that there are a range of deontic logics, which can handle the logic of mixed assertoric and normative propositions (see, for example Hilpinen 1971). Further, speech act theorists, such as Austin, Grice and others, were aware that there are what they called 'conversational implicatures'[7] between different speech acts which are, broadly speaking logical relations but not logical consequences as in, say, propositional logic. Moreover, we can say that if a person performs the illocutionary act of issuing the command 'open the door' then there is a presupposition that (they and any hearer believe that) the door is shut. It is one of the fine-grained features of speech act theory that it recognizes such presuppositions and conversational implicatures that are not captured by the relation of logical consequence as defined in some calculus. So, Lyotard is right about the absence of the relation of logical consequence between some statements; but because he has too narrow a view of the range of logical relations between statements and commands he fails to notice that other relations of implicature might hold.

It is a pity that Lyotard, a devotee of speech act theory, did not follow this up because the notion of conversational implicatures, linking speech acts in allegedly different games as just described, or even deontic logic itself, conflicts with a much larger, but fallacious, consequence that Lyotard wants to draw. He goes on to say after the door example and the allegation that there are no logical links between commands and statements:

> The two statements ['The door is closed' and 'Open the door'] belong to two autonomous set of rules defining different kinds of relevance, and therefore of competence. Here, the effect of dividing reason into cognitive or theoretical reason on the one hand, and practical reason on the other, is to attack the legitimacy of the discourse of science. Not directly, but indirectly, by revealing that it is a language game with its own rules (of which the a priori conditions of knowledge in Kant provide a first glimpse) and that it has no special calling to supervise the game of

> praxis... The game of science is thus put on a par with the others. (Lyotard 1984, p.40)

This makes two claims, both of which are gross *non-sequiturs*. We have noted Lyotard's faulty premise about absence of any logical link between statements and commands; this overlooks the possibility of quasi-logical relations such as conversational implicatures, or the existence of deontic logics which deal with both statements and commands. The faulty premise is then used to infer to the much grander claim of a separation between theoretical and practical reason (of course there are differences but they do not follow logically from the above premise). But there is an even more gross *non-sequitur* when Lyotard tries to establish the grander contentions that somehow the legitimacy of science is under attack, or that it can have nothing to do with theoretical reason or *praxis* (especially any legitimating function), or that the game of science is on a par with all other games (whatever this might mean). Thus one argument that Lyotard advances for the lack of legitimacy of science based on considerations to do with speech act theory is a serious failure.

Lyotard blithely assumes that his attacks on the legitimacy of science have hit their target when he continues:

> If this "delegitimation" is pursued in the slightest and if its scope is widened (as Wittgenstein does in his own way...) the road is then open for an important current of postmodernity: science plays its own game; it is incapable of legitimating the other language games. The game of prescription, for example, escapes it. But above all it is incapable of legitimating itself.... *(ibid.)*

These remarks follow on from the previous quotation. But, again, they do not follow logically. And considered in themselves they are highly dubious. First, the game of prescription does not escape science. In the biochemist's laboratory, for example, there are special prescriptions for staining slides for viewing under microscopes, or making a DNA profile of some genetic material and displaying it in an appropriate manner for all to view. Failure to follow the correct procedures leads to an inability to see anything under a microscope, or a failure to produce the right profile for viewing. As well as such particular prescriptions for each science, there are also more general methodological prescriptions of method that any scientific investigator ought to follow in testing hypotheses (e.g., those of statistical testing).

More important is the postmodernist claim that science plays its own game; it cannot legitimate any other game, or even its own game. How does its failure to even legitimate itself follow? It does not follow from any of the above. It is just an assertion based on another *non-sequitur*, another logical slide. We can agree that there are conventions for the language game of stating (an *act*), but it does not follow that what is stated (a *content*) is conventional and a matter of agreement amongst the members of a linguistic community. Its logical relations with other statements, and its bearing on reality (its truth value) are not conventional at all. Extend this to the rules of

the game of science. We have not been told what the rules for playing the game of science are, but we have already cited the claim that 'Scientific knowledge requires that one language game, denotation, be retained and the others excluded' *(ibid*., p. 25). (Just one game for science? The denotation game? Let us grant this for the purposes of this argument.) In playing this game, which we understood in Section 12.3 to be the game of supplying warranted assertability conditions, even if it is a matter of conventional agreement that some sentence of science has the denotation (assertability condition) it has, it does not follow that the denotation (the assertability condition itself) is also a matter of convention. There is here an illegitimate slide from the correct attribution to an act of making a denotative utterance, namely that it is conventional and lacks legitimation, to the same but false attribution to the content of the act, namely that the content is also conventional and lacks legitimation. But as noted, the content of science, its propositions, do stand in non-conventional relations to reality, to other propositions and to evidence. If this is Lyotard's basis for claims about the lack of legitimation for science, then it is quite faulty.

Is this really Lyotard's argument? It would seem so, evidence for which can be obtained from elsewhere in Lyotard's constant request for a proof of the proof:

> The conditions of truth, in other words, the rules of the game of science, are immanent in that game, that they can only be established within the bonds of a debate that is already scientific in nature, and that there is no other proof that the rules are good than the consensus extended to them by experts' (*ibid*., p. 29).

That there is no 'proof of the proof' is alleged on two grounds. First, there is just the proof at the first level, which is given by rules of the game of science, there being no further levels of rules to appeal to since the rules are (allegedly) immanent in the game. Second, as to why one should accept them as good rules, this appears to be merely a matter of consensus of the experts who adopt them. How does this come about? Let us suppose we agree that there are conventionally adopted rules for playing the game of science, just as there are rules for the act of stating, to which some group agrees (for example, Popper's rule against *ad hoc* hypotheses, or Newton's rule about the parsimony of postulated causes – see Section 6.4). But from this it does not follow that the content of the rules (in parallel to the content of a statement) is also a matter of conventional adoption and not objective in some way, or not open to test or evaluation.[8] The same faulty slide seems to be at work again.

What we have been looking for in these passages is any argument that Lyotard might have to offer for the claim that science lacks legitimation. Postmodernism is defined as 'incredulity towards metanarratives'. But this is merely a reportive definition about Lyotard's use of 'postmodern'. But *ought* we to be incredulous? And on what grounds? The only considerations arise

from claims about speech acts, their conventionality and lack of further justification. Lyotard's only argument is from the claim that the speech act of stating, or the act of promulgating a rule, lacks further legitimation to the conclusion that the statement made, or the rule promulgated, is also without any justification. From which it is claimed that there is no legitimacy for science. This we have argued is a sequence of *non-sequiturs*. Lyotardian postmodernism about science turns on a logical howler.

The above is an extended but important commentary on the first of three points, indicated at the beginning of this section, which Lyotard wishes to make about language games. His second point is twofold: 'if there are no rules, there is no game, that even an infinitesimal modification of one rule alters the nature of the game' (*ibid*., p. 10). But the claim that even an infinitesimal modification of a rule alters the nature of a game ought to offend the Wittgensteinian sensibilities that Lyotard often professes. It imposes a rigid bifurcation between games, where a more family resemblance approach might be in order. Games are not made of sets of rigidly distinguished rules; rather, they are made of overlapping families of rules such that while the rules might differ from another in some respect, all are still sufficiently similar to one another to be members of the same family. A modification of a rule would not necessarily alter the *nature* of the game. Also there would be an implausible infinity of games with every infinitesimal change. Finally, 'the third remark is ... every utterance should be thought of as a "move" in a game' (*ibid*., p. 10). Let us agree to this as a matter of Lyotardian convention linking 'utterance' and 'move'.

How many language games are to be found in science? We have seen above that Lyotard claims that there is just one to the exclusion of others 'Scientific knowledge requires that one language game, denotation, be retained and the others excluded' (*ibid*., p. 25). But elsewhere he says, when considering the axiomatisation of science, that there may be different language games arising from the changed rules of inference due to changed embedding logics. But asking a question about the *number* of games leads to a prior question about the criterion for the individuation of games. Just how many rules governing a game have to change before we have a different game? As already noted, Lyotard has a quite sharp and narrow criterion which might not suit many Wittgensteinians, viz., even an infinitesimal change in a single component rule will count as a new game, thereby generating infinity of games. If this is the case, then there are many more language games than one might have thought initially. On a more relaxed Wittgensteinian criterion there will be a lot fewer games since their individuation would not turn on just one change in one aspect of one rule. Elsewhere Lyotard speaks of rules which are 'specific to each particular kind of knowledge' (*ibid*., p. 19), but this is to push the problem back to the individuation of the particular kinds of knowledge that there are. The upshot

of the above remarks seems to be that questions concerning the number of games of science that there are cannot be answered in the absence of any criterion of what is to count as a game.[9] What is implausible is Lyotard's infinity of such games.

12.6 THE SOCIO-POLITICAL LEGITIMATION OF SCIENCE

We are now in a position to formulate the central argument of Lyotard's book and evaluate its cogency. We shall focus our attention upon the role of the narrative of Enlightenment in Lyotard's argument because that is his target above everything else.

According to Lyotard, modern science legitimates itself by appealing to meta- or grand narratives such as the emancipation narrative of the Enlightenment, according to which science and scientific education will pave the way for the freedom and social progress of all by 'enlightening' their minds against religion, superstition and dogma. Thus, says Lyotard, the question of the legitimation of science is always a 'question of double legitimation' (Lyotard 1984, p. 8) because 'the problem of its legitimacy… [is] no less sociopolitical than epistemological' (*ibid*., p. 18). But, the argument goes, the 'leading' sciences and technologies like computer science, linguistics, communications, information theory, cybernetics and so on that have flourished since World War II, have altered the nature and status of knowledge.[10] Science has become commercialised, and knowledge has 'lost its "use value"' (*ibid*., p. 5), becoming a commodity to be bought and sold within the logic of market (capitalist) economy. In this context any body of knowledge that cannot be translated into computer language will be abandoned as useless. As a result, the narrative of emancipation and progress has lost its credibility. States, who used to fight over territory, raw materials and cheap labour, will now battle over control of knowledge and its production to have power over each other. Knowledge and power have become 'simply two sides of the same question: 'In the computer age, the question of knowledge is now more than ever a question of government' (*ibid*., pp. 8-9).[11]

One need not endorse the problematic Foucauldian doctrine of power/knowledge to argue that the problem of the legitimation of science also involves a socio-political dimension. All one has to do is to distinguish between *science as an institution* and *science as content,* i.e., theories, hypotheses, observational reports and the like. While the legitimation of the latter is clearly an epistemic issue, the former does involve a socio-political component at least to the extent to which scientific activity and education are publicly funded, are encouraged by the state, or are privately funded for the purposes of profit making. The support of scientific research and education through public funds obviously requires justification in a way that links

science to public interests, and that is a social legitimation issue, not an epistemic one. Similarly, the private funding of science for the purposes of private interest (e.g., profit) should not be beyond public scrutiny. We discuss these issues in more detail later in Chapter 14.

Let us then interpret Lyotard's discussion of the Enlightenment narrative accordingly. Lyotard maintains that the Enlightenment view that science frees humanity from the bonds of ignorance and superstition and contributes to social progress provides a major socio-political legitimation of science. We note that this view is not confined to the Enlightenment; a much earlier expression of it can be found in the writings of the Elizabethan Francis Bacon. And not every Enlightenment philosopher endorsed this view, an important exception being Rousseau in his 1750 *First Discourse* (*Discourse on the Science and Arts*) in which he attacked the sciences and arts as a cause of moral decay. Postmodernists' invocations of the Enlightenment are often too simplistic. Setting aside matters to do with the Enlightenment, there is still the ever-present matter of the socio-political legitimation of science. Such legitimation, Lyotard supposes, requires what he calls 'a grand metanarrative'. Presumably, it is *grand* because it justifies not this or that aspect of science, but science as a whole[12] (but note as an institution and not sciences' propositional content, this being an epistemic matter). And presumably it is a *meta*narrative because it is a story about science (which is alleged by Lyotard to be just one story among others).

Lyotard's bold claim that the narrative of emancipation has become incredible elicits several questions. Incredible in the eyes of whom? All or most of humanity? Just the Europeans? The philosophers? The intellectuals? Lyotard does not say. Moreover, Lyotard presents no evidence at all for his claim. Let us assume for the sake of argument that Lyotard is right. We can then ask why the Enlightenment narrative of emancipation has lost its credibility. Lyotard makes the astonishing claim that 'this incredulity is undoubtedly a product of progress in the sciences' *(ibid*., p. xxiv). Now, what exactly is the argument for this? It turns on Lyotard's claim that the 'leading' sciences and technologies have altered the nature and status of knowledge. But as we have argued in detail in Section 12.3, this is false; the nature or even the status of knowledge has not changed one bit because of these developments. What has changed is that, due to computerization, there are now new ways of transmitting, storing and accessing *the informational content* of knowledge.

Lyotard has not shown why the commercialisation of science and digitalisation of information undermine the narrative of Enlightenment. This is an important claim that deserves serious argument and evidence for it; but alas Lyotard presents only his convictions. Consider another claim he makes to support his argument: 'Anything in the constituted body of knowledge that is not translatable in this way will be abandoned and that the direction of new

research will be dictated by the possibility of its eventual results being translatable into computer language' (*ibid*., p. 4). If this is a point about the influence of computers on our lives, then it is almost a sociological banality. But more precisely, need all 'knowledge' be translatable into some computer language? As most constructors of computer languages know, some items are translatable into some languages while others are not; and some functions are computable while others are not. Moreover, any finite information that can be put on finite paper can be put into the medium of computers. Also, given human curiosity, it is those things not translatable into existing languages, or which are not computable, that emerge as issues which excite research interest of a deeper sort. Or consider his claim: 'Knowledge is and will be produced in order to be sold… Knowledge ceases to be an end itself, it loses its "use value"' (*(ibid*., pp. 4-5). Knowledge, as we have argued, by its very nature, is not the sort of thing that can be bought and sold, though information can be. But then information did not become a commodity with the computer age; it became an exchangeable commodity costing money long before. Sophists made money by lecturing, and so did publishing houses by selling books since the discovery of the printing machine! And, as we have pointed out at the end of Section 12.3, both knowledge and information must retain their use value in order to be commodities, not lose it.

Lyotard rightly worries about the danger that with the digitalisation of information, its access and circulation can be controlled by multinational corporations who produce and own this sort of technology – and so do we. Lyotard is concerned that scientific expertise and knowledge, especially in fields like computer science and bioengineering, is being utilized more and more for making large profits – and so are we (see Chapter 14). But we fail to see how all this justifies his claim that science can no longer contribute to the emancipation of people from ignorance, superstition and dogma and pave the way for social progress. There is no doubt in our minds that science does provide us with reliable knowledge about the world and therefore *can* help us improve our lives, but it is absurd to think that it will automatically do this under the existing regime of global capital. Certain measures need to be taken to ensure that science serves public interest more than corporate interest, that it serve world peace rather than political hegemony and that it contributes to the alleviation of poverty and suffering more than it does now. In Chapter 14 we make some suggestions in this direction.

Followers of Lyotard may say that this is indeed Lyotard's whole point! They might argue that what we have just said is not a refutation of his position; on the contrary, it is a vindication of it. Very well. But then it also shows that that point can be made without the confused fuss about the changed nature and status of knowledge, its loss of use value, incredulity toward metanarratives, the crisis of legitimation, and the like. Such fuss confuses epistemic matters with socio-political and economic ones and

masks, and often distorts, the real issues that need to be discussed in relation to the role of science in today's world.

12.7 PARALOGY TO THE RESCUE?

In Sections 11 and 12 of *PC*, Lyotard argues that in the postindustrial societies considerations of what he calls 'performativity' replace those of emancipation for the socio-political legitimation of science. In other words, Lyotard tells us that in such societies science is justified on the grounds that it contributes to their efficient working and no longer some humanitarian goal it is to realize. He then claims that performativity presupposes regular, stable and deterministic systems, but that the advent of quantum mechanics has undermined these assumptions, particularly that of determinism. Thus, argues Lyotard, legitimation based on performativity is alleged to be problematic, and so a different kind of legitimation is needed. He suggests legitimation by paralogy as an alternative and argues that it is the sort of legitimation appropriate for the postmodern condition. Before we explain what this means, let us first look at Lyotard's conception of determinism and its relation to performativity.

> Determinism is the hypothesis upon which legitimation by performativity is based: since performativity is defined by an input/output ratio, there is a presupposition that the system into which the input is entered is stable; that system must follow a regular "path", that it is possible to express as a continuous function possessing a derivative, so that an accurate prediction of the output can be made. Such is the positivistic "philosophy" of efficiency. (Lyotard 1984, pp. 53-54).

This is such a confused passage that one does not know how to correct it. Stability, predictability, and a system having a 'regular path' described by a continuous, differentiable function are all lumped together under the notion of determinism. But determinism has nothing to do with any of these. The common notion of a deterministic system is that the state of an isolated system at an earlier time uniquely determines its state at a later time. This is a purely metaphysical (or ontological) concept; in contrast, predictability is an epistemic notion. A system may be deterministic, yet unpredictable. Similarly, there is no necessity that a deterministic system must be stable. Stability has to do with equilibrium, not determinism. Finally, what all this has got to do with positivism is anybody's guess.

Given all this confusion, Lyotard's account of the supposed connection between performativity and determinism remains totally mysterious and unintelligible. But there is no reason why it should be. If a system is deterministic in that if it is in a given kind of state S_1 at an earlier time then it will always achieve some desired state S_2 at a later time, then we could say that its performativity, which we can express as an input/output ratio of S_1/S_2, is 100% guaranteed. But in that case we can talk about probabilistic performativity of indeterministic systems. Suppose a system is

indeterministic in that if it is in a given state S_1 then it will be followed by desired state S_2 70% of the time, or more often than not, or 20% of the time, etc. Then for systems which are not deterministic and achieve some desired goal state, say, x% of the time, we could have a measure of efficiency, i.e., of performativity in achieving the desired state of x%. Thus indeterminism does not mean that performativity is necessarily undermined. Rather, we merely replace the idea of a 100% performativity input/output ratio by one that is not 100% guaranteed. And there need not be anything that is postmodern about this, or applicable only to the postmodern condition.

Let us not be misunderstood. We do not endorse performativity as the right way to legitimise science socio-politically. Like many others (including Lyotard), we are also concerned by the increasing extent to which, say, university research in certain areas is more and more geared toward the goals of business or are required to yield profits, perhaps also increasing performativity in Lyotard's sense. What we criticize is, first, Lyotard's bad argument that purports to show that failure of determinism debunks legitimation by performativity, and, second, his elementary mistakes and confusions concerning determinism, predictability and stability. These obscure his more valuable insights and only perpetuate existing misconceptions about important matters.[13]

Let us now turn to Lyotard's alternative, namely, legitimation by paralogy and ask: what is paralogy, and how is science in postmodern cultures to be legitimated by it? To answer these questions, we must recall what the so-called 'postmodern' scientific developments are and what they are supposedly concerned with, according to Lyotard. For him the postmodern sciences are paradigmatically quantum mechanics, computer science, information theory, mathematical logic, catastrophe theory and the like. In addition:

> Postmodern science — by concerning itself with such things as undecidables, the limits of precise control, conflicts characterized by incomplete information, *"fracta"*, catastrophes, and pragmatic paradoxes — is theorizing its own evolution as discontinuous, catastrophic, nonrectifiable, and paradoxical. It is changing the meaning of the word *knowledge*… It is producing not the known, but the unknown. And it suggests a model of legitimation that has nothing to do with maximized performance, but has its basis difference understood as paralogy. (*ibid*., p. 60).

We find the idea of legitimation by paralogy, just introduced, problematic for two reasons. First, we disagree (again!) that 'postmodern science' changes the meaning of the term 'knowledge' (see Section 12.3). Second, we do not see how 'postmodern science theorizes its own evolution as discontinuous, catastrophic, nonrectifiable, and paradoxical'. Nor is it clear what it means to say science evolves in a 'catastrophic, nonrectifiable and paradoxical' way. What are the 'pragmatic paradoxes' Lyotard is talking about? The scientific developments with which Lyotard is concerned may have inspired in him the idea of legitimation by paralogy, but they are far from justifying it.

Turning to Lyotard's notion of paralogy, we find other formulations such as: it 'is a move (the importance of which is often not recognized until later) played in the pragmatics of knowledge' (*ibid*., p. 61). Only marginally less obscurely Lyotard tells us: 'I group under the name *paralogy*' items such as 'the study of open systems, local determinism, antimethod' (*ibid*., p. 100, fn. 211). Paralogy is also associated with what Lyotard calls an 'antimodel of a stable system': 'science is a model of an "open system", in which a statement becomes relevant if it "generates ideas", that is, if it generates other statements and other game rules' (*ibid*., p. 64). Finally paralogy is associated with the fact that

> even discussions of denotative statements [of science] need to have rules. Rules are not denotative but prescriptive utterances, which we are better off calling metaprescriptive utterances to avoid confusion (they prescribe what the moves of language games must be in order to be admissible). The function of the differential or imaginative or paralogical activity of the current pragmatics of science is to point out these metaprescriptives (science's "presuppositions") and to petition the players to accept different ones. The only legitimation that can make this kind of request admissible is that it will generate ideas, in other words, new statements. (*ibid*., p. 65)

Thus, paralogical activity consists of making the (metaprescriptive) rules of the game of science explicit, making imaginative moves (such as coming up with a new scientific 'paradigm'), adding new rules and even changing the old ones. It promotes dissensus rather than consensus, heterogeneity and plurality rather than homogeneity and universality on the grounds that new knowledge comes about by dissent, by questioning what is consensually assented to. According to Lyotard, it is this sort of activity that is in line with the spirit of 'postmodern science', and therefore only it provides an appropriate legitimation for 'postmodern science'.

Though the account above is somewhat exaggerated, for those who know of the history of science from the Ancient Greeks until now, ideas have always been proposed and challenged by others who in turn propose new ideas. But what Lyotard fails to mention is that within science there have always been 'controls', or better methodological prescriptions, on what is to be admitted as a new idea, viz., that it produces some growth of knowledge, or it has new evidential support, or it explains something new, or opens new fields for investigation, or it has an increase in verisimilitude, and so on. These are central notions of traditional scientific method which constrain the admissibility of new ideas. In the absence of any mention of methodology, Lyotard's paralogical activity of 'making it new' would hardly differ from blather, or the output of a random sentence generator. If paralogical activity involves changing the rules of the game of science so that new statements can be made, and if, as Lyotard had insisted earlier, even the slightest change in the rules of a game results in a new game, what reason is there to think that, in the absence of such 'controls' on what is admissible as new, the new statements will still belong to science rather than some new age faith? The

task of always 'making it new' is on a par with the slogan 'anything goes'. Further, it invites a close examination of the Greek origins of Lyotard's term 'paralogy', which are perilously close to 'contrary to reason'!

Finally, what sort of a legitimation is legitimation by paralogy? In the absence of any appeal to methodological principles it says nothing about the epistemic legitimation for any science, whether it be traditional forms of sciences or the sciences Lyotard likes to characterize as postmodern (such as quantum mechanics, catastrophe theory, and the like). If it is intended to be a social-political one, it is not at all clear how it does the job; why would people support an activity just because it simply generates new ideas whatever their epistemic value? Paralogical activity may be fun, but it is hard to think of a society willing to divert its substantial resources to science just for the fun of it. Either way, legitimation by paralogy is no legitimation at all.

12.8 LYOTARD AND HIS FOLLOWERS ON EDUCATION

So much then for Lyotard's views on knowledge, science and its legitimation. We hope we were able to show that all of the major theses of *PC* on these topics were seriously flawed. The reader will notice that we have avoided any discussion of them by Lyotard's followers in education. This is deliberate; for educationalists who endorse Lyotardian postmodernism merely repeat his views on knowledge, science and its legitimation without any criticism.

This is not, however, quite the case for his views on education; not because postmodernist educationalists disagree with him, but because they often advance a much more radical position with respect to the aims of teaching, the role of the teacher, curriculum and policy. So, let us first discuss what Lyotard himself says about education and then turn to the views of some of his representative followers in education.

The main point Lyotard makes concerning education is that the criterion of performativity by which the social systems in the post-industrial societies are judged has also plagued the system of education and its institutions, especially universities. In other words, today the system of education is being governed not by the goal of producing knowledge for its own sake or social emancipation, but rather by performativity. This means that it is judged by the efficiency with which it contributes to the working of the capitalist social system. For instance, he says that universities now include job retraining and continuing education programs, both of which contribute to the maintenance of the existing social order; and research that promises to yield immediate and high profits receives higher priority as well as increased funds over research that does not. In Lyotard's words,

> The question (overt or implied) now asked by the professionalist student, the State, or institutions of higher education, is no longer "Is it true?" but "What use is it?" In the context of the merchantilization of knowledge [read: information], more often

> than not this question is equivalent to: "Is it saleable?" And in the context of power-growth: "Is it efficient?" (Lyotard 1984, p. 51)

It is hard to tell here whether Lyotard feels a pang of residual nostalgia for the grand narrative of truth (towards which we have been invited to be incredulous) when the pursuit of truth is replaced by that of utility, but at any rate he is critical of the infiltration of the criterion of performativity into the system of education. He predicts that

> If our general hypothesis is correct, there will be a growth in demand for experts and high and middle management executives in the leading sectors mentioned at the beginning of this study, which is where the action will be in the years to come: any discipline with applicability to training in "telematics" (computer scientists, cyberneticists, linguists, mathematicians, logicians ...) will most likely receive high priority in education (*ibid*., p. 48).

We share Lyotard's concern that universities are more and more becoming subject to the forces of capital. He was also right in predicting that in the postindustrial countries the demand for business managers, computer scientists, programmers, and experts in information and telecommunication technologies would increase (though not about logicians and mathematicians, and he missed the revolution in genetic engineering and the great demand for genetic engineers completely). The reason for the increase in demand in these areas is that they are able to respond well to the needs of the market by producing innovations that bring generous profits. What is missing in Lyotard's analysis, however, is the impact of neo-liberal economic policies that have pushed universities to seek stronger ties with the private sector when they feared that the government funding for scientific research would be seriously cut. Despite his repeated references to the 'market', 'competition', 'capitalism', 'system', and occasionally to 'social justice', Lyotard does not give us even a crude analysis of the socio-economic world system that was emerging at the time he wrote his book. Nor does he give any clue as to how social justice can be achieved in the postmodern age. The only solution he recommends in terms of 'paralogy', as we saw, is no solution at all.

Does Lyotard's book *The Postmodern Condition* offer anything specifically for teaching and learning of science? Or more generally, does 'postmodernist education' have anything to contribute to the pedagogy of education? While this is not the kind of book to expect anything on this topic, surprisingly we find something; but unfortunately it is something very disappointing. When talking about teaching and learning, the only thing that Lyotard can think of is the much criticized 'transmission model' and 'didactics'! (see Lyotard 1984, pp. 48-52). He constantly refers to teaching as 'the transmission' of an 'established body of knowledge'. It is truly surprising to find out that he can think of no other method of teaching than an outdated didacticism. It is even more surprising that none of the literature

by his enthusiastic followers in the field of education even mentions this aspect of Lyotard's thought, let alone criticize it.

Now, since, according to Lyotard, teaching is essentially a matter of transmitting knowledge, he argues that the new information technologies (especially, what we now call the internet) will make the teacher unnecessary. He writes:

> It does not seem absolutely necessary that the medium [of instruction] be a lecture delivered in person by a teacher in front of silent students, with questions reserved for sections or "practical work" sessions run by an assistant. To the extent that learning is translatable into computer language and the traditional teacher is replacable by memory banks, didactics can be entrusted to machines…Pedagogy would not necessarily suffer. The students would still have to be taught something: not contents, but how to use the terminals (*ibid*., p. 50).

This is hailed by some as the death of professors in their traditional role (Nuyen 1995).

If teaching is merely a matter of transmission, then Lyotard and his followers are right in declaring teacher to be dead. But then teachers were long dead since the beginning of writing, when people started self-learning from books (in published or manuscript form)! Books, too, make the teacher redundant (if at all) in the same way computers and internet do. Why then the fuss about the death of the teacher specifically in the information age? Of course, there are other and better ways of teaching and learning than the transmission model, such as the Socratic model we have discussed in Chapter 3, and since it is based on a dialogue of questioning and answering between a teacher and a student, the teacher is well alive on that model.

It is also unclear why Lyotard thinks that content need no longer be taught in the age of computers. Again, although computers and new information technologies certainly have provided new ways of storing and communicating information, students still need to master content, either by themselves or with the help of teachers. In this regard, there is no essential difference between learning from a book or from a machine (though we grant that the latter can take an interactive mode).

So, Lyotard offers nothing new with respect to the method of teaching. What about the aim or aims of education? Does he have anything to say about them? Since aims such as 'truth', 'social emancipation', and 'enlightenment of minds' are part of the 'grand narratives' of Enlightenment and modernity that have lost credibility, they will not do. Lyotard told us that, as a matter of fact, performativity has replaced them as *the* aim of education in the postmodern societies, but he is not happy about that either. We are left then with paralogy as the aim of education.

Indeed, some postmodernist educators have suggested revising curricula on the basis of paralogy:

> The postmodern curriculum encourages chaos, nonrationality, and zones of uncertainty because the complex order existing here is the place where critical

> thinking, reflective intuition, and global problem solving will flourish… Curriculum models based on modern versions of Newtonian physics have attempted, like a clockwork universe, to impose uniformity. Every lesson, every goal and objective, must conform to predetermined principles, cultural forms, social structures, or curricula guides. The postmodern curriculum, on the other hand, is based on a new science: a complex, multidimensional, kaleidoscopic, relational, interdisciplinary and metaphoric system (Slattery, quoted in Phillips 1998, p. 161).

As Phillips, from whom we have quoted this passage, points out, this is a category mistake; both Newtonian physics and chaos theory are scientific theories that apply to certain physical systems and have nothing to offer as to what students must learn in school (Phillips 1998, p. 162).

William Doll's entire book (1993), which urges for a revision of curriculum based on a postmodern perspective, suffers from the same category mistake. Like Slattery, Doll claims that the modernist, transmissive pedagogy, where a fixed body of knowledge is transmitted from teachers to students, is a corollary of the Cartesian-Newtonian paradigm (p. 172); rejecting it, he then argues that 'concepts of self-organization, indeterminacy, stability across and through instability, order emerging spontaneously from chaos…', that derive from the works of Prigogine, Bohr, and Heisenberg are 'nonlinear concepts that become key as we work to develop both a new cosmology and a new set of curriculum criteria' (p. 158). A few pages later, Doll reiterates the point that the problems of teaching and learning 'need to follow the nonlinear models quantum physics and chaos mathematics set up, not the universal, all-encompassing, grand designs so prevalent in modernism' (Doll 1993, p. 162). But what has Cartesian-Newtonian paradigm got to do with the transmission model of teaching and why should *education* mimic the instability, indeterminacy and the nonlinearity found in some theories *in science*? From the fact that certain models in science have characteristics like instability, indeterminacy and so on, nothing at all follows about the nature of teaching, learning and curriculum. Similarly, Cartesian-Newtonian paradigm as a scientific paradigm implies nothing about models of teaching.

The passages quoted above from Doll contain other blunders. The last one suggests that nonlinear models in quantum physics and chaos mathematics are not universalistic or all-encompassing. But quantum theory is one of the most comprehensive theories ever developed! It is certainly more universalistic than Newtonian theory, which is taken to be part of modernist paradigm by Doll. Second, while quantum theory is essentially stochastic (and, thus non-deterministic), chaos theory is purely deterministic! Lumping the two together as if they have exactly the same characteristics is sheer postmodern ignorance.

Now, what are the implications of postmodernism for education more specifically? Begin with the curriculum content: Doll tells us that it must be general, broad, and flexible enough to leave room for student-teacher

interaction for development and change; curriculum development must be a process based on recursive reflection on past experiences, but with an orientation to future (*ibid*., p. 163). Learning is not a matter of passive reception, but active participation (*ibid*., p. 170). Students must be encouraged to ask good questions, and the evaluation of their performance must include the quality of questions asked, not just the appropriateness of their answers (*ibid*., p. 4). A postmodern education must also aim at 'developing competence – the ability to organize, combine, inquire, use something heuristically' and also emphasize dialogue between the teacher and the student, among students themselves (*ibid*., p. 178). For science education, it means that 'science – including the biological and the physical – can be seen as intuiting, developing, probing, "proving" hypotheses concerning the world in which we live' (p. 177). These are sensible suggestions, but are they novel? Does one have to be a postmodernist to accept them? One can certainly embrace them without all the fuss about nonlinearity, chaos, indeterminacy, and so on, and the attentive reader will notice that most of them flow naturally from the Socratic model of inquiry set out in Chapter 3.

But there is also much with which we disagree in Doll's suggestions, some of which we summarized above. For instance, as we have shown in Parts I and II, science education should involve a lot more than 'intuiting, developing, probing, and proving hypotheses'; it should give the student a good idea about the nature of science, its aims and methods. Also, we must exercise caution in allowing how much students should have a say in curriculum matters; after all, to make informed decisions, they should already know quite a lot about science, but presumably they are taking science courses precisely because they lack such knowledge. Dialogue, recursive reflection, and interpretation of texts should certainly be encouraged in the science classroom, but this should not be distorted to mean 'negotiating passages' in science textbooks (*ibid*., p. 156). For, although textbooks too can certainly be criticized for being sloppy, misleading or simply wrong, the term 'negotiation' loses sight of the crucial difference between objectively right and wrong answers, true and false claims.

Finally, when discussing the question of the authority in the classroom, Doll makes the important point that the teacher must develop her authority in the various classroom situations rather than merely impose it externally. We take this to mean that authority is a matter of leadership that comes with being a good teacher, not with the label 'teacher'. But then he immediately adds that 'questions of procedure, methodology, and values are not decided in the abstract, away from practicalities of life, but are always local decisions involving students, teachers, *and local mores and traditions*' (*ibid*., p. 167; our emphasis). Despite all his postmodernism, Doll forgets how wrong (not to mention suffocating and repressive) certain aspects of local mores and

traditions can be. When it comes to local beliefs, Doll's postmodernism suddenly loses all its 'critical' edge. This attitude toward the local culture is an aspect of postmodernism that it shares with epistemic multiculturalism, which we will discuss in detail in the next chapter.

Other postmodernists, who follow Jacques Derrida and Richard Rorty more than Lyotard give a deconstructionist twist to postmodernism: According to Stuart Parker (1997), for example, the point of postmodernism is to stop playing the 'game of reason and truth' 'because it has become passé… The postmodern world is more style than thing. It is a world in which old vocabularies and old oppositions of true/false, good/evil, theory/practice, heaven/hell get junked or refashioned to serve new purposes and new styles of inventing others and creating ourselves' (Parker 1997, pp. 6-7). Parker speaks of the 'postmodern impulse to give up the game of trying to produce better-because-truer theories' and argues that the major aim of education should be deconstruction (*ibid*., pp. 141-143). For him, accordingly, teachers must be deconstructionists who

> practice the kind of deconstructive manoeuvres outlined and employed in this book: …mis-reading the text; reading with the intention of causing trouble; seeing all assertions, practices and positions as textual; using the text's assumed rationality against itself; …collapsing its distinctions between the literal and the metaphorical… (Parker 1997, p. 143).

Parker's postmodernism really outdoes even Lyotard's. As if this is not scandalous enough, in a section entitled 'Educating the Postmodern Teacher', he writes:

> *Teacher education courses will need to equip students with the deconstructive manoeuvres by means of which they will be able to throw off the inhibitions of realism and engage in creative, literary writing*. The postmodern deviation from a philosophy of *demonstration* to one of *persuasion* collapses rigid distinction between reason and rhetoric, truth and consensus. Thus licensed, and in contrast to the plodding realist, the postmodernist will make extravagant leaps, cavalier gestures, resting on nothing more than the hope that she or he might entrance the sheer flourish of style. Realism's measured rationality is replaced with a hodgepodge of manoeuvres and dirty tricks which might become gold if an audience falls for them. There is no secure distinction between rational argument and rhetorical prestidigitation (*ibid*., p. 146; emphasis original).

Now, given that this is what education is all about, what room is there for curriculum? Not surprisingly, none: 'As for curriculum, get rid of it', Parker concludes (*ibid*., p. 151). For what is the point if there is no difference between truth and falsity, rightness and wrongness, reason and rhetoric; since the aim is deconstruction, any text will do. We think this is deconstruction that has gone mad and cannot be taken seriously.

In fairness, however, not all postmodernists go this far. Some embrace 'empowerment' as the major aim of education. Stanley Aronowitz and Henry Giroux, two of the leading postmodernist educators, are a case in point:

> The values that constitute postmodern education are those of empowerment in the most profound meaning of the term …. Freedom consists in the capacity of people

> and groups to transform knowledge in accordance with their own plans (Aronowitz and Giroux 1991, p. 22).

An 'empowering' education, according to them, includes the following: giving students and teachers the final authority to construct curricula as they see fit to their needs; giving no priority to the established literary, philosophical, and *scientific* canons of Western civilization; expansion of curricula to include alternative forms of belief produced by popular culture, even 'gossip' and 'folk wisdom'; emphasis on education, including science education, that has direct bearing and importance for daily life; 'affirming a politics of racial, gender, and ethnic difference'; challenging universal validity of scientific knowledge, and rejecting 'universal reason as a foundation for human affairs' because it is hegemonic (*ibid*., pp. 13-17, 20-22, and 57-61). As it is evident, postmodern education is 'a form of cultural politics' (*ibid*., p. 87):

> Rather than holding knowledge in some kind of correspondence with a self-enclosed objective reality, postmodernism views the production of knowledge in the context of power. Postmodern educational criticism insists on the intellectual equality of marginal discourses – feminism, sexuality, race, class – with those in which these discourses are occluded, when considered at all. These considerations exempt neither science nor social science from interrogation, thus removing the halo around science that has exempted it from all but internal criticism – that is, correction by other members of the scientific community (*ibid*., p. 186).

In a similar vein, Michael Peters writes:

> Historically, for instance, liberal institutions (prisons, courts, psychiatric institutions), including the school and the modern university, have legitimated themselves and their practices by reference to discourse of subject-centered reason. The project of liberal mass schooling and higher education in the late twentieth century is built around the intellectual authority inherited from the Enlightenment. It is grounded in a European universalism and rationalism heavily buttressed by highly individualistic assumptions. It is these assumptions and the authority that rests upon them that is now being called into question… "Postmodernism" is a broad cultural phenomenon of Western societies that best typifies this questioning… (Peters 1995, p. xxx).

We see that, according to these authors, postmodern education ultimately aims to transform the power relations that shape the institutions of education in the name of a more plural, democratic society and critical citizenship. They believe that while a relativism that celebrates plurality, difference, marginality, locality, dissensus and heterogeneity facilitates such an aim, universalism, rationalism and talk of objective truth hinders it. What is presupposed here is the idea that while universalism and rationalism are hegemonic and repressive, relativism is liberating and progressive.

Such politicisation of education by postmodernists bears striking resemblance to the politicisation of science by epistemic multiculturalists, and we shall show that both are equally problematic (see Chapters 13 and 14). This is not because we think that education should play no role in contributing to a more democratic society and critical citizenship. No doubt education can and should play such a role, but not by attacking established

canons of scientific inquiry and universal reason, endorsing a relativism of all intellectual discourses and inquiries, and embracing gossip into educational curriculum, which is plain silly.

One ought to be suspicious of any view of education that sees empowerment as its main aim. We do not disagree that increasing one's powers and abilities over one's life is a desirable thing, providing that such power is appropriately used. As we indicate in Section 11.5.1, Francis Bacon made the quite correct point that with our increase in knowledge there also comes the possibility of increased power (which is not identical with the Foucauldian power/knowledge doctrine that Aronowitz and Giroux seem to endorse). But all of this must come under the sway of critical inquiry as to whether or not one has knowledge (as opposed to mere belief) and to what ends power may be exercised. To make empowerment the aim of education is to have matters around the wrong way; and if this is not viewed in the light of critical inquiry it can have sinister overtones.

As we have argued throughout this book, especially in the 'Introduction' and Chapter 1, the major aim of education is critical inquiry. What postmodernist educators fail to see is that if education can turn pupils into critical inquirers, be it in science, humanities, and elsewhere, then they will be equipped with the necessary skill and knowledge to resist forms of oppression, and to contribute to a more just and democratic society as active, responsible, and critical citizens. Criticism, if it is to have any force upon those being criticized, must be based on canons of rationality that are binding for both sides. It is a great irony of postmodernist education that it can achieve the goal of producing critical citizens by downplaying transcultural or shared norms of reason and method.

Education should certainly be multicultural and also make room for marginalized people. But what this means and involves must be spelled out clearly and carefully. Multiculturalism as an educational policy must not be confused with multiculturalism as a politics of recognition. To '*affirm* a politics of racial, gender, and ethnic difference' is a very problematic version of the latter, which has been subjected to incisive criticism by many social and political philosophers. Furthermore, claiming equal intellectual worth for all perspectives is neither justifiable nor necessary for the recognition of marginalized, despised, or silenced people's discourses. Recognition in this sense does not in any way entail sealing them off from criticism. It is to these issues we now turn in the following chapter.

NOTES

[1] Merciless (but deserved) criticism of the postmodern conception of science came from the scientists. See Sokal and Bricmont 1998, which discusses Lyotard on pp. 125-128 and Gross and Levitt 1994, which makes only passing references to him.

[2] Constructivism in the sense intended here is that of a non-classical logic which offers constructive proofs. It has nothing to do with constructivism in science education.

[3] In considering the pragmatics of scientific knowledge (*ibid*., Section 7, p. 23) Lyotard says, overstrongly, that 'the sender should speak the truth about the referent' and 'be able to provide a proof of what he says' and 'be able to refute any opposing or contradictory statements concerning the same referent'. And then when the sendee 'formulates his agreement or disagreement he will be subject to the same double requirement (or proof or refutation)'. This indicates that not only does a sender know in a quite strong sense, but also that a sendee knows equally as strongly when assent is given; but this is an implausibly strong requirement on the assent of a sendee.

[4] For a case against Foucault's power/knowledge doctrine see Nola 2003, Chapters 8 and 9.

[5] For an account of the methodological principles and their justification advocated by many of the philosophers of science cited, see Nola and Sankey 2000. Lyotard shows an awareness of some aspects of the Anglo-American philosophy through the works of Wittgenstein and Austin in philosophy of language. Work in the philosophy of science germane to the issues he discusses seems to have escaped him in that he makes no critical comment on attempts to justify induction, the work of Popper, Lakatos and many others on the justification of theories of method. These last two philosophers of science were at least contemporaneous with Lyotard.

[6] See also Lyotard 1984, p. 35, where Lyotard talks of several attempts to provide legitimation, including that of 'contemporary hermeneutic discourse', and footnote 116 which takes us to works of Ricoeur and Gadamer, though no specific attempt of theirs at legitimation is discussed.

[7] For an account of conversational implicatures and their relation to the theory of speech acts, see Grice 1975.

[8] Rules of the kind proposed by Popper or Newton have a content which is open to critical examination. One such evaluation of Popper's rule is that of Feyerabend who alleges that if we were to follow Popper's rule then it would have set science back. See Feyerabend 1975, Chapter 6. A more recent attempt to test the very content of principles of method can be found in Laudan 1996, Chapter 7.

[9] Even though the number of *scientific* games might be elusive, we can find in Lyotard 1984, at least the following 'games': denotative, prescriptive, evaluative, interrogative (all on p. 20); legitimacy, research, teaching (all on p. 23); science (p. 26); speculative (p. 39); the games of scientific language (p. 45, whatever these may be (note the plural) — and how do they differ from the game of science?), and learning (p. 64)!

[10] Later, quantum mechanics, mathematical logic, fractals and catastrophe theory are added to this list (see Lyotard 1984, Sections 10 and 13).

[11] We have extracted this argument from Sections 1 and 2 of *PC*.

[12] Here we might wish to be more 'postmodernist' than Lyotard himself in taking each of the sciences separately and ask, as those concerned with the ethics of science do, whether some practice, experimentation or application has legitimation. This is distinct from asking whether science as a whole has legitimation (whatever this might mean). Thus one may consistently find a role for genetic modifications to remove a defect that causes chronic or fatal illnesses but deny that genetic manipulation be used in some programme of eugenics. Here particular

practices are "legitimated", or not, in contrast to the whole of science. Legitimating the whole of science may well, in contrast, be seen as an almost meaningless project; rather, it ought to be taken in a piecemeal fashion.

[13] In passing we would like to note that Lyotard has completely missed the revolutions in the science of agriculture before World War Two and in molecular biology afterwards. We mention these because they are areas in which high profits are at stake; here are two other disciplines that are rapidly becoming commercialized. See Kenny 1986 and Kloppenburg 1988. See also our discussion in Chapter 14.

CHAPTER 13

MULTICULTURALISM AND SCIENCE EDUCATION

Science educators and theoreticians of science education who have embraced multiculturalism proceed as if multiculturalism is a univocal and non-problematic approach that can be fruitfully applied to science education. We believe, however, that neither the existence of a distinctly multiculturalist interpretation of scientific activity nor a consensus about the meaning and the practical consequences of multiculturalism itself can be taken for granted. Multiculturalism is not a single and unproblematically formulated theory or strategy waiting to be applied to science education. A philosophically informed consideration of its implications for science education would have to address both what is valuable about it and its inner tensions and limitations.

Before we proceed any further, it will be useful to make a distinction between 'multiculturalism' and 'multicultural education'. Both of these terms have been used in so many different senses that it would be impossible to give an exhaustive list. For example, *The International Encyclopaedia of Education* defines 'multicultural education' as

> an educational process or strategy involving more than one culture, as defined by national, linguistic, ethnic or racial criteria… It is supposed to create an awareness, tolerance, understanding and knowledge regarding different cultures as well as the differences and similarities between cultures and their related world views, concepts, values, beliefs and attitudes. It is intended to provide cognitive, verbal, and non-verbal skills in coping with different cultures or cultural groups, and skills in communicating with members of these groups. It is also intended to promote academic and social achievement in intercultural settings (Ekstrand 1994, p. 3963).

While this definition emphasizes communication and understanding between cultures, the entry on multicultural education in *The Encyclopaedia of Educational Research* emphasizes equal opportunity to students with diverse social, racial and ethnic backgrounds:

> As an idea, multicultural education espouses the notion that male and female students, students from diverse racial, ethnic and social-class groups, and students with disabilities should have an equal opportunity to learn in schools, colleges, and universities… Multicultural education is also a process whose major aim is to change the social structure and culture of schools and other educational institutions, so that students from all cultural, racial, ethnic, gender, and social-class groups will have an equal opportunity to experience academic success (Banks 1992, p. 870).

It would be more accurate to say that the ideal of multicultural education incorporates both concerns above, as the following passage emphasizes:

> Education *of* diverse cultural groups, *through* a wide range of culturally impregnated experiences, *for* life in a multiracial and multiethnic society at both local and global levels. The ultimate goals are the promotion of social cohesion through critical awareness and the establishment and maintenance of a socially just society through the acceptance and celebration of diversity, the enhancement of the self-esteem of all, and the elimination of racism. (Hodson 1993, p. 689; emphasis original)

Multicultural education as described in these passages is a need that has emerged because today's modern, complex societies are multicultural societies: they consist of people with different native languages, religions, and ethnic origins. This creates the need for mutual understanding and tolerance regarding different cultural beliefs and practices among the members of a society. Moreover, those cultural groups that form a minority in a given society are often disadvantaged economically, socially and politically due to various reasons. Education is an effective way of creating conditions for equality as well as mutual tolerance and understanding among members of different groups. "Multicultural education' is the name given to the policies of education that address these issues in multicultural societies.

It seems obvious to us that education should certainly be available to all regardless of their gender, ethnicity, religious, cultural and class backgrounds, and the aim of critical inquiry which we defend in this book is certainly compatible with creating awareness and understanding regarding different cultures provided that they are not sealed off from criticism.

On the other hand, the term "multiculturalism', as it appears in theoretical literature in the social sciences, is used to refer to a certain kind of politics of recognition. What this means will be made clear below. In the science education literature, however, 'multiculturalism' has acquired a meaning of its own; it refers to a certain constellation of epistemic doctrines that form an alternative to the universalist conception of science. To distinguish it from other forms of multiculturalism, we call it *epistemic multiculturalism*.

Since there is little discussion of multiculturalism in a broader theoretical context in the literature on science education, we begin, in Section 13.1, by introducing multiculturalism as it has emerged in the social sciences in the last two decades and relate it to issues in science education. Multiculturalism in this broader context is a kind of politics of recognition. We discuss what is valuable and what is problematic in it both generally and specifically from the viewpoint of science education. In Section 13.2 we present an alternative to multiculturalism, an alternative which avoids what is problematic in multiculturalism as a politics of recognition. We then turn to epistemic multiculturalism and its conception of science in Sections 13.3 and 13.4. We argue that epistemic multiculturalism is even more problematic than multiculturalism as a politics of recognition; we reject it categorically. Many epistemic multiculturalists claim that indigenous and ecological knowledge developed by indigenous peoples (what is sometimes known as "ethnoscience')

constitute an alternative science to the standard, mainstream science practiced in the West and that therefore they should be incorporated into science education. We take up this issue in Sections 13.5 and 13.6. Despite the fact that epistemic multiculturalism is put forward as an alternative to the universalistic conception of science, the latter is poorly understood by epistemic multiculturalists. In Section 13.7 we define what we mean by universalism in science and defend it against multiculturalists' attacks. In Section 13.8 we state our view concerning the relationship between science and values, an issue epistemic multiculturalists especially bring up in comparing "ethnoscience' with mainstream science. We conclude this chapter in Section 13.9 by noting that epistemic multiculturalism loses the sight of critical inquiry as the overriding aim of education in the science classroom.

13.1 MULTICULTURALISM AS A POLITICS OF RECOGNITION

Multiculturalism, as it originally developed in the social sciences in the last twenty years or so, is a response to the demands of recognition by those minority groups or individuals in a multicultural society, who are discriminated against, marginalized, despised or disrespected because of their ethnic origin, religion, language, gender or sexual orientation. In a nutshell, multiculturalism is a kind of politics of recognition. But what this involves and how the demands for recognition must be met received different answers by different social theorists.

A particularly strong (and, therefore, controversial) version of multiculturalism as a politics of recognition has been developed and defended by Charles Taylor. According to Taylor, recognition is linked to intersubjective, communal identity in that identities of individuals are in part 'constituted dialogically' through recognition by a community of others (Taylor, 1994; see also Honneth, 1992, 2001). This means that a person's identity (i.e., how she sees herself and takes herself to be) is shaped to an important degree by how the members of her community treat her. Taylor claims that recognition is a crucial human need whose satisfaction is essential for the formation of the subjective identity of a person, for achieving self-respect, self-confidence and self-esteem as a participant in a particular community or culture:

> The thesis is that our identity is partly shaped by recognition or its absence, often by the *mis*recognition of others, and so a person or group of people can suffer real damage, real distortion, if the people or society around them mirror back to them a confining or contemptible picture of themselves. Nonrecognition or misrecognition can inflict harm…Due recognition is not just a courtesy we owe people. It is a vital human need. (Taylor 1994, pp. 25-26, emphasis original.)

In a similar vein, Axel Honneth, another influential theoretician of recognition, writes that:

> We owe our integrity … to the receipt of approval or recognition from other persons. ["Insult" or "degradation"] are related to forms of disrespect, to the denial of recognition. [They] are used to characterize a form of behavior that does not represent an injustice solely because it constrains the subjects in their freedom for action or does them harm. Rather, such behavior is injurious because it impairs these persons in their positive understanding of self – an understanding acquired by intersubjective means. (Honneth 1992, pp. 188-189)

For this reason nonrecognition or misrecognition causes serious damage to a person's identity or integrity. It should also be noted that since, according to Taylor and Honneth, identity is closely related to cultural recognition, politics of recognition turns into a politics of cultural identity.

Taylor also argues that any politics of recognition is animated by two principles: the principle of equality and the principle of difference. That is, recognition is directed to others both in regard to an essential sameness with oneself, a common humanity that is the basis of equality, and in regard to difference, the distinctive identities of individuals and groups. While the principle of equality states that all humans are equally worthy of respect, the principle of difference dictates that the unique identities of individuals be recognized.

The equality principle gives rise to a politics of universalism in that everybody is given the same rights and entitlements as citizens. Thus, the principle of equal worth or respect is meant to avoid discrimination and the existence of 'first class' and 'second class' citizens (Taylor 1994, pp. 37-39). Taylor asserts that underlying this principle is the supposition that 'all human cultures that have animated whole societies over some considerable stretch of time have something important to say to all human beings' (*ibid*., p. 66). According to Taylor, this does not, however, entail 'actual judgments of equal worth applied to the customs and creations of these cultures' (*ibid*., p. 68). Actual judgments of worth, to make sense as judgments, have to 'register something independent of our own wills and desires' (*ibid*., p. 69). He points out explicitly that this approach does not in principle invalidate 'claims to superiority in some definite respect on behalf of Western civilization, say in regard to natural science', since 'the presumption of worth imagines a universe in which different cultures complement each other with quite different kinds of contribution'. Therefore, writes Taylor, 'this picture not only is compatible with, but demands judgments of, superiority-in-a-certain-respect' (*ibid*., p. 71, fn. 19).

What emerges from Taylor's account so far is the following. First, all people must be treated equally and with respect, regardless of their colour, ethnic origin, sexual orientation, etc. This is a *universalist ethical* principle of multiculturalism; it is universalist because it applies to all people in all cultures, and it is an ethical principle which tells us how people ought to be treated, not an epistemic one. Nevertheless, it has an implication that is relevant to critical inquiry in science education. In school, teachers should pay attention to the content of what the students say, not to who is saying it.

In other words, the student's colour, ethnic origin, religion, and so on is irrelevant to the claims she makes. What matters is whether what she says is correct or incorrect, adequately justified or not, relevant to the topic discussed or not, and so on.

Second, even Taylor admits that valuing cultures and respecting their members does not mean that they cannot be judged and criticized from a certain respect. For example, if the natural sciences that were developed by Western cultures turn out to be epistemically superior to those developed by non-Western cultures, that in no way conflicts with multiculturalism. Similarly, a traditional, non-Western culture may well be superior to another with respect to something else, for example, with respect to a conception of technology which is in harmony with the environment. Making critical judgments concerning certain aspects of a culture are perfectly compatible with multiculturalism.

Finally, Taylor points out that judgments of superiority-in-a-certain-respect are not only compatible with multiculturalism but also are demanded by it. This is because underlying multiculturalism is a conception of individuals as autonomous beings, whatever their cultural identity and background. Minimally, this means that they are rational and have the free will to make their own decisions and choices. Thus, autonomous individuals have the capacity to think, reason and learn from their mistakes and, accordingly, change their beliefs and practices voluntarily by the sheer force of better argumentation. It follows that to deny that another culture may be superior to one's own in certain respects when presented with sufficient reasons is an insult to the autonomy of individuals.

In this context, it is important to note that it does not make much sense to talk of 'equal worth of cultures'. Cultures are complex wholes with diverse elements, which make overall comparison impossible. Sometimes Taylor slides into talking this way. A friendly critic of multiculturalism, Lawrence Blum, rightly points out that this is 'a meaningless notion that has crept into multiculturalist discourse (not only Taylor's), serving only to obfuscate' (Blum 1998, p. 75). He argues that multiculturalism is the recognition of equality among individuals and groups, not cultures, since equality is not a concept that can be applied meaningfully to comparisons between *entire* cultures or between *all kinds* of cultural products; a peremptory assertion of equality is not an effective strategy for countering ethnocentric claims to superiority (*ibid*., pp. 82-84).

Their disagreements notwithstanding, Taylor's and Blum's discussions are useful in revealing that the principle of equality at the basis of multiculturalism is not transferable from the level of human beings to the level of cultures and cultural products such as science or alternative conceptions of nature. Multiculturalism as a demand for equality does not require a particular epistemology for its justification as many multiculturalist science educators seem to think Nor does it provide any challenge to

universalist conceptions of science. Many defenders of multiculturalism in science education, however, assume that a multiculturalist approach must either grant the status of a science to different types of knowledge or belief systems about nature originating in different cultures, or avoid evaluative comparisons between science and these other conceptions of nature, or both. To maintain this position they are led to a relativistic conception of science and its philosophy. This is an unfortunate consequence of misconstruing the equality dimension of multiculturalism. It runs counter to the view of science developed in Part II, which is distinctly non-relativist.

The other dimension, the recognition of difference and of distinct cultural identities involves acknowledging the distinct worth of a culture or component of culture '*to the individual or group in question*' (Blum, 1998, p.79).[1] This sort of recognition usually takes the form of affirming and preserving the culture and the cultural heritage that make up the collective identity. While the value and significance a person or a group attaches to their culture certainly requires sensitive treatment, a categorical acceptance of cultural differences and the desire to assert and preserve collective identities is not without problems. First, neither culture nor identity denotes a static, closed-off, homogeneous entity. Every individual constitutes a point of intersection for several, variously defined collective identities and adopts some of a very wide range of cultural values. There is no reason to assume automatically that indigenous belief systems belonging to a particular culture always form a significant part of the self-conception of the members of that culture, or constitute a source of their sense of distinctness. For example, one may be a Westerner yet be sceptical or agnostic about Christianity. Second, even if a particular way of viewing nature and interacting with it is deemed to be a significant component of identity by the members of a certain cultural group, the recognition of its legitimacy cannot take the form of sealing it off from criticism and comparative evaluation. Such a reduction of a knowledge claim to a cultural artefact to be protected like an endangered species would be unfair both to the claim itself and to the individuals who need to be guided as to the advisability of embracing the claim. There is an analogy here to Taylor's discussion of the demand made by the French Canadians that special measures be taken and certain privileges be granted to ensure that their culture will remain distinct, 'not just now but forever' (Taylor 1994, p. 40). Unlike Taylor, we do not find this demand legitimate. Jürgen Habermas is right in stating that

> Even if we considered it a meaningful goal to protect cultures as though they were endangered species, the conditions necessary for them to be able to reproduce successfully would be incompatible with the goal of "maintain[ing] and cherish[ing] distinctness, not just now but forever'. . . Even a majority culture that does not consider itself threatened preserves its vitality only through an unrestrained revisionism, by sketching out alternatives to the status quo or by integrating alien impulses – even to the point of breaking with its own traditions. (Habermas 1994, p. 131)

Anthony Appiah also makes an important point regarding this issue when he states that trying to ensure the future survival of a culture is incompatible with 'respecting the autonomy of future individuals' (Appiah 1994, p.157). The protective measures taken for the survival of a culture are incompatible with autonomy in this sense because they can leave no choice for the individual in certain respects. This is precisely what happened when Canadian parents in Quebec were restricted by law in terms of the language of schooling their children can have. An effort to ensure the survival of traditional worldviews may be similarly incompatible with the critical autonomy of students in that it puts pressure on the students to accept a belief system without critical scrutiny.

Another warning against an uncritical affirmation of cultural difference is Appiah's concern that the lives of the bearers of collective identities may become too 'tightly scripted':

> There will be proper ways of being black and gay, there will be expectations to be met, demands will be made. It is at this point that someone who takes autonomy seriously will ask whether we have not replaced one kind of tyranny with another. (Appiah 1994, pp. 162-63)

Again, there is an analogy to be drawn between such a tyranny and unexamined expectations about the aims and scope of local knowledge. For instance, to Michael Matthews' statement that 'no ethnic science is going to adequately explain how radios work, why the moon stays in orbit, why hundreds of thousands of Africans are dying of AIDS and so on' (Matthews 1994, p. 193), the multiculturalist science educators William Stanley and Nancy Brickhouse respond by claiming that 'the problems [universalists] believe all sciences should be interested in solving are really local problems that would be of most concern to Westerners' (Stanley and Brickhouse 2001a, p. 46). This is an odd reply for several reasons. First, the matter-of-factness with which Stanley and Brickhouse assert that such problems are unlikely to be of concern to non-Westerners verges on stereotyping. How do they know that non-Westerners would not be interested in knowing why radios work or why the moon stays in orbit? Second, why do Stanley and Brickhouse think that the question of why many Africans die of AIDS is a local problem that would be 'of most concern to Westerners'? AIDS is a disease that has infected people all over the world, and surely the answer to that question is of utmost importance especially to Africans since they are affected most. Finally, we should ask in what sense a problem is local. A problem may be local in the sense that only certain people suffer from it in a certain part of the world. Thus, for example, getting rid of arsenic in drinking water in most of Bangladesh is a local problem in this sense. But the problem of how to get rid of arsenic from drinking water in general is not a local problem at all. Currently, there is no technology for removing arsenic in drinking water in vast amounts. But when a scientific solution is found, it will work not only in Bangladesh but everywhere. It is this aspect of science,

which makes it attractive for Westerners and non-Westerners alike. We will say more on this issue in Section 13.4.

What we have said so far indicates that there are no uncontroversial political and ethical reasons for multiculturalism to reject universalism and to deny the superiority of science as it is developed and practiced in the West over alternative conceptualizations of the natural world. This is a point conceded to by the very social-political theoreticians of multiculturalism such as Taylor. Whether there are any good epistemic reasons for denying universalism and the superiority of science developed in the West over "ethnosciences' or "indigenous sciences' is an issue we will discuss later in this chapter. Here we would like to draw attention to the relationship between a multiculturalist approach to science and a universalist one. Against the view that the two are incompatible (see, for example, Stanley & Brickhouse 1994 and 2001a, and Snively & Corsiglia 2001), Harvey Siegel (1997 and 1999a) has argued that multiculturalism and universalism can peacefully coexist at the moral level. His argument is simple but powerful. Multiculturalism requires a moral universal or transcultural perspective, namely, that all members of all cultures must be treated justly and with respect. This is the equality principle of multiculturalism discussed above. It is because cultural domination, hegemony and non-recognition of others is morally wrong for all cultures that multiculturalism in this moral sense must be embraced by all, even by those who do not recognize this as a moral truth. Multiculturalism as a politics of recognition, therefore, presupposes a moral universalist principle of equality.

In their response to Siegel, Stanley and Brickhouse misunderstand this point and attribute the following view to him: 'He appears to be saying that the proof of the existence of a universal moral principle is derived from demonstrating the impossibility of holding other cultures to any moral principle unless such a principle were, in fact, universal' (Stanley & Brickhouse 2001a, p. 38). But this is not at all what Siegel was arguing. As we understand it, Siegel's forceful claim is that *unless the principle of equality is a universal or transcultural imperative binding all cultures, multiculturalism loses its ground and appeal*. For if the moral principle of multiculturalism were regarded as a relative, culture-dependent principle, then 'the monoculturalist would have an obvious reply: "perhaps this domination, marginalization and oppression is wrong from the perspective of your culture, but it is fine from the perspective of mine'' (Siegel 1997, p.98). Thus, Stanley and Brickhouse interpret him wrongly when they attribute to him the view that no culture can be held accountable for any moral principle unless it is universal. Siegel's main point, on the other hand, is that there can be no *multicultural ethics* without moral universalism. We therefore agree with Siegel that multiculturalism is not only compatible with but also requires universalism at the moral level.

Stanley and Brickhouse have replied to this criticism. We quote them in full:

> Irzik's [2001] defence of Siegel's argument for multiculturalism to be a universal principle in order to have any force is simply wrong. One might pose an argument that multiculturalism should be accepted on a universal basis. One might also acknowledge that if this view is not accepted on a universal basis it is not likely to gain wide acceptance. But such arguments are always pragmatic at heart. We have no way to establish the universality of multiculturalism. At best we can support it because we believe we have more evidence (at the moment) to endorse that argument as opposed to others, not because it actually is universal (Stanley and Brickhouse 2001b, pp. 87-88)

This passage unambiguously shows that Stanley and Brickhouse have not come to terms with Irzik's and *a fortiori* Siegel's point. As we have shown above, multiculturalism as a politics of recognition is based on two principles, one of which is the principle of equality. It says that all humans are equally worthy of respect and that all humans must be treated justly and with equal respect. This is itself a universalist ethical claim of multiculturalism; since Stanley and Brickhouse are defenders of multiculturalism in science education, we do not see how they can avoid it without undermining their very own position! Moreover, we believe that the principle of equality is the acceptable core of multiculturalism as a politics of recognition even if we have doubts about its other components. Do Stanley and Brickhouse seriously doubt the rightness of this principle? Do they sincerely believe that it is right from one cultural perspective but not from another? If they do, they are moral relativists with respect to that principle, in which case they become vulnerable to Siegel's criticism above. In other words, being a moral relativist takes all the bite out of multiculturalism.

There is something else that is odd about Stanley and Brickhouse's defence which reflects their universalism phobia: 'We have no way to establish the universality of multiculturalism. At best we can support it because we believe we have more evidence (at the moment) to endorse that argument as opposed to others, not because it actually is universal.' (*ibid*.) Let us first clarify that Irzik's and our argument (and we think Siegel's too) pertains to only one component (namely, the principle of equality) of multiculturalism, not to multiculturalism in its entirety. Then let us ask why one cannot 'establish the universality of multiculturalism', more specifically in this context, the principle of equality? Stanley and Brickhouse do not give any specific argument or reason for the non-universality of this principle other than to take refuge in the general claim that 'even within the framework of Western philosophy, there is no consensus regarding the universality of moral principles' (Stanley and Brickhouse 2001a, p. 39). But we think there is a confusion here; when we say that a moral principle is universal, we do not mean that it is universally or widely *accepted*. The universal or wide acceptance is not a necessary condition for universality. To say of a moral principle that it is universal means that it is normatively binding for everyone, even by those who do not accept it. The principle of equality,

namely that all human beings are worthy of equal respect, dictates that everyone should be treated equally, that each person *qua* being a human is entitled to the same rights, and that it is morally wrong to discriminate against people because of their colour, ethnic origin, gender, and so on, not just now and here but all the time and everywhere. The fact that people may have discovered or started endorsing this principle only at a certain point and time in history does not invalidate its universality (its universal correctness) any more than a lack of consensus on it. It is unfortunately a historical fact that many societies in the past violated the principle of equality: slavery and colonialism are obvious examples, and some continue to do so even today. But it is also true that there is a growing consensus regarding it. From Locke to Kant, from Mill to Rawls, the entire modern Western social-political philosophy and thought takes the equality principle as a crucial basis for a humane society, and we do not know of any major non-Western thinker who has rejected it. More importantly, it has become one of the most important pillars of the Universal Declaration of Human Rights signed by over 150 countries. Thus, we dare claim that there is wide consensus on this particular principle, at least among social and political thinkers, and countries that are members of United Nations!

Stanley and Brickhouse think that there is another generic reason why the universality of a principle cannot be established. By 'establishing', they mean 'establishing beyond doubt' or 'with certainty' (*ibid*., pp. 40-41). Given this sense, of course we cannot 'establish' the principle of equality. But to demand this from ethics (or even science) reflects an archaic understanding of these disciplines. We must be content with good arguments and strong evidence, with justification beyond reasonable doubt. But then as Stanley and Brickhouse themselves admit, the principle of equality as interpreted as a universal principle does enjoy much more support than its relativistic interpretation, and that is excellent reason to accept it. (Isn't this what we do in empirical sciences as well?) Note also the oddity of the second occurrence of 'because' in their quotation. They write as if we have argued that we support the principle of equality because it is universal! They thus continue to misunderstand Irzik's and Siegel's point. We believe that the principle of equality is universally right because there are excellent (and, we must say, obvious) arguments in favour of it; we do not think that its sheer universality is the reason why it should be accepted.

It is really hard to understand Stanley and Brickhouse's universalism phobia. Given their commitment to multiculturalist science education, one would expect that they would also be committed to the ideals of multicultural education which we have introduced at the beginning of this chapter. Recall that one of the major aims of multicultural education is to provide equal opportunity to learn in schools for all, regardless of gender, language, race,

ethnic origin, social class and the like. Underlying this is nothing but the principle of equality applied to opportunity. We cannot see how Stanley and Brickhouse defend the ideals of multicultural education without endorsing this universal principle.

13.2 AN ALTERNATIVE APPROACH TO MULTICULTURALISM

Multiculturalism is not the only way to address the issue of recognition. A well-known social and political philosopher, Nancy Fraser, has developed a powerful alternative to multiculturalism (Fraser 1998, 2001). Without denying the equality principle, she has criticized Taylor's and Honneth's views for turning the issue of recognition into a question of cultural identity, where group or community belonging is the crucial factor that shapes an individual's identity. By contrast, Fraser claims that recognition is essentially a matter of justice, not identity. People may suffer from several forms of recognitional injustice. They may be subjected to cultural domination, may be rendered invisible by the authoritative symbolic practices of their culture, or may be devalued through stereotypical representations. Fraser points out that these symbolic (as opposed to economic or political) injustices are deeply rooted in the cultural-valuational structure of society. Thus, she points out that recognitional injustices do not simply damage one's cultural identity; they prevent a person 'from participating as a peer in social life as a consequence of institutional patterns of cultural value that constitute one as comparatively unworthy of respect or esteem' (Fraser 2001, p. 27). Since Fraser also believes that there are universal or transcultural standards, criteria or norms for judging different cultural practices in terms of recognitional justice understood as parity of participation, her approach is universalist or transculturalist.

As a remedy, Fraser urges a politics of recognition that aims to restructure such patterns so that 'parity of participation' becomes possible, that is, each person can participate in social life on a par with the rest. For example, consider one type of recognitional injustice blacks often face in today's multicultural societies: demeaning stereotypical depictions in the media such as 'Blacks are criminals, stupid, violent, primitive', etc. Such depictions not only devalue blacks, but also serve to exclude them from social life. Fraser's remedy is to transform the valuational-cultural structure of the society in such a way that 'hierarchical racial dichotomies are replaced by demassified and shifting networks of multiple intersecting differences' (Fraser 1998, p. 39). In this way, race stops being a category that constitutes a person's identity, and people are treated not with respect to their colour or racial origin but as individual human beings who are worthy of equal respect and who are peers in social participation. Thus, Fraser's suggested remedy eliminates, blurs or deconstructs group or cultural differentiation with respect

to race. In general, elimination, blurring or deconstruction of group or cultural differentiation is a transformative remedy with respect to race, gender or sexuality.

By contrast, Taylorian (communitarian) multicultural politics affirms group or cultural differentiation because it sees culture or community as something to be protected 'not just now but forever'. Recall the case of French Canadians who are required by law to send their children to French speaking schools. Such a policy not only clashes with individual freedom and autonomy, but also affirms the division of Canadian society according to linguistic differences. A better policy would be to promote bilingualism, that is, to allow families to send their children to either a French or an English speaking school but at the same time encourage that every student learn both languages.

The main difference between multiculturalist and transformative politics of recognition boils down to this: in developing policies to address the issues of cultural marginalization, non- or mis-recognition, are we going to underline, accentuate, affirm cultural differences, or are we going to try to overcome or transcend them? Taylor opts for the former; Fraser for the latter. All of this confirms the more general point with which we have begun this chapter: multiculturalism is not a single and unproblematically formulated theory or strategy waiting to be applied to science education. It is just one approach to politics of recognition among others, and it suffers from a number of serious defects. We need not be bound to multiculturalism to address the issues of non- or mis-recognition.

Note also that a transformative politics of recognition which emphasizes the parity of participation by transforming the valuational-cultural structure of society is in full accord with the aims of multicultural education as defined at the beginning of this chapter. Such a politics will contribute immensely to the ideals of giving equal opportunity of education to all students regardless of their ethnic, religious, linguistic, etc. backgrounds, of creating conditions of mutual understanding and tolerance among them, and, ultimately, of establishing and maintaining a socially more just society.

We now proceed to an assessment of the epistemic version of multiculturalism.

13.3 EPISTEMIC MULTICULTURALISM

Many multiculturalists in science education believe that their views are incompatible with universalism not only at the moral but also at the epistemic level as well. Indeed, this is where the battle between the multiculturalists and the universalists in science education is fought most fiercely. Multiculturalists typically claim that (1) sciences are disunified, (2) both content of the sciences and scientific practices are determined or shaped

by social and cultural factors, so much so that (3) nature plays little or no role in the production of scientific knowledge, (4) science is local, contextual and historically contingent, (5) every culture has its own science, and finally, (6) science is not value-free, but value-dependent (see Brickhouse and Stanley 1995, Kelly *et. al*. 1993, Ogawa 1989 and 1995, Snively & Corsiglia 2001, Stanley and Brickhouse 1994 and 2001a for endorsing some or all of them). Some go so far as claiming that (7) alternative (i.e., traditional, indigenous) belief-knowledge systems (called 'sciences' by their advocates) are as valid as mainstream science (see Pomeroy 1992, Ogawa 1995, Snively and Corsiglia 2001). We shall refer to this set as *epistemic multiculturalism*.

Theses (1) and (5) play a special role in epistemic multiculturalism, making room for alternative forms of scientific knowledge such as indigenous knowledge, traditional ecological knowledge and 'ethnosciences' more generally. Thus, to the extent to which multiculturalists endorse these theses, multiculturalism is indeed incompatible with universalism at the epistemic level. One of them must go. Our view is that it is epistemic multiculturalism that must go. For we believe that there is just science whose nature we have discussed at length in Parts I and II and not 'ethnoscience' or even 'Western science'. Science is practiced more or less in the same way wherever it is practiced, and the nature of science is more or less the same everywhere. It has the same (intrinsic) aims and uses similar methods or models; it is universal in that sense. We do not deny of course that there are many scientific disciplines such as physics, chemistry, biology, geology, and so on, but they can all be subsumed under the term 'science'. Then the term 'sciences' is understood to refer to the sum total of these disciplines. By contrast, terms like 'ethnoscience', 'Western science', 'Arabic science', or 'Maori science' are misleading because they give the impression that there are radically different kinds of sciences with little or no resemblances among them. By 'Western science' we simply mean science developed, contributed to, or practiced in the West, and by 'Arabic science' the same kind of science developed, contributed to or practiced by the Arabs. And the same goes with 'indigenous science' if there is any.

Theses (2) and (3) should be familiar to most readers; they form the core of social constructivism advocated by sociologists and philosophers of science like David Bloor, Bruno Latour, Steve Woolgar and Harry Collins. Since we have addressed them in Part III, we will not repeat them here.

Thesis (4) as stated is ambiguous between two readings. On the one hand, it could mean that science is a human construction, 'developed and judged in historico-cultural contexts and … informed and influenced by those contexts' (Siegel 1997, p. 99). Interpreted in this way, the thesis does not at all conflict with universalism because universalists do recognize the culture-dependence of science in this sense. As Harvey Siegel aptly puts it, 'universal' does not

mean 'free of all contextual influence' or 'independent of human belief, presupposition, or negotiation' (*ibid.*).

On the other hand, the thesis could also mean that there is no transcultural perspective from which we can judge the epistemic worthiness of the scientific claims put forward by the scientists. Understood in this way, the thesis is a relativistic one, which couples with thesis (7) and is not acceptable by the universalists. Indeed, it is wrong as Siegel convincingly argued (Siegel 1999b); the multiculturalists' mistake is to infer from the correct premise that we cannot transcend our historical/cultural horizon to the incorrect conclusion that therefore all our claims are relative to that historical/cultural context. This is a fallacious inference because it fails to distinguish between 'transcending all perspectives all at once' and 'transcending any particular perspective' (*ibid.*). While a 'cosmic exile' is not possible, 'if we try, we can break out of our framework at any time. Admittedly, we shall find ourselves again in a framework, but it will be a better and roomier one; we can at any moment break out of it again' (Popper 1970, p. 56; quoted in Siegel, *ibid.*). This point also invalidates Stanley and Brickhouse's claim that a universalist conception of science 'allows scientists to pretend to have a God's eye view of the world' (Stanley and Brickhouse 1994, p. 392). Universalists do not think that scientists can have an all-comprehensive view of the world independent of all theories or frameworks. They are well aware of their human limitations, but they also believe, like Popper, that it is possible to break out of their particular framework at any time if they try hard enough. Indeed, this is the whole point of critical inquiry (embodied in logic and scientific methodology); and that is the main reason why universalists value it so much.

Thesis (1) is advanced strategically by the multiculturalists to support Thesis (5). In order to justify their claim that there are as many different kinds of science as there are cultures, the multiculturalists resort to mainly two strategies, one of which is to appeal to the disunity-of-science thesis. Thesis (1) is supposed to dislodge the view that all 'sciences', if they are to count as science, must have a common core or share a family resemblance. The second strategy is to broaden the notion of science to make room for ethnosciences, traditional ecological knowledge and indigenous knowledge. While Snively and Corsiglia make use of the second strategy, Stanley and Brickhouse employ both.

The claim that sciences are disunified is by itself ambiguous. For disunity can be understood as ontological, methodological, or linguistic. To say that sciences are disunified ontologically means that what the various sciences postulate as the stuff out of which the universe is made is not of one kind but irreducibly many. Physicalism, the modern version of materalism, claims that the ontology of the world contains no more and no less than what is

postulated in a mature physics; in particular, it denies the dualistic doctrine that we need to postulate more, namely minds or the mental, as a separate and additional ontological category. Though there is a tendency towards various forms of physicalism in the twentieth century, the issue is far from being settled. In its linguistic version the disunity thesis says that there is no one language into which all scientific sentences from all scientific disciplines can be translated. Logical positivists held the hope that all sciences could be expressed in a physicalistic language, but their hopes were shattered by the ever-increasing multiplicity and specialization of scientific discourses especially with the newly emerged subdisciplines. Sciences are disunified at the level of methodology means that there is no single method for all sciences.

Stanley and Brickhouse appeal to the recent philosophy of science literature to support their contention that science is disunified methodologically. They approvingly cite Ian Hacking's statement that 'there is no set of features peculiar to all sciences, and possessed by the sciences. There is no necessary and sufficient condition for being a science' (Hacking 1996, p. 68; quoted in Stanley & Brickhouse 2001a, p. 43). They criticize their fellow multiculturalists Snively and Corsiglia for 'repeatedly using [universalist] methods of science to support a unity-of-science thesis' (*ibid*., p. 46). When, in discussing the Nisga'a fish wheel, Snively and Corsiglia suggest asking students whether the Nisga'a were observing, inferring and building models, Stanley and Brickhouse protest:

> [These methods] are not unique to science and cannot demarcate science from other endeavors. To repeat our earlier arguments, the sciences are disunified and multiple. There are no unique and necessary characteristics of science. Hence, whether or not the Nisga'a are doing science or not cannot be answered by the question Snively and Corsiglia pose (Stanley & Brickhouse 2001a, p. 46).

Admittedly, the problem of demarcating science from non-science formulated originally by Popper has turned out to be more difficult than many philosophers of science thought, and it is certainly true that Popper's own solution in terms of the notion of falsifiability did not quite work. Moreover, all attempts to characterize science in terms of a single universal method and to define it rigorously in terms of a set of necessary and sufficient conditions have also failed. Sciences seem to be more diverse and heterogeneous than we have initially thought; the idea of a scientific method understood as a set of fixed, universal and binding rules which govern scientific activity at every step and at all times seems to be elusive. Sciences do have a history, and what count as scientific problems and problem-solving strategies did evolve over time, as historians and historically oriented philosophers of science have argued. In particular, the combination of mathematics with experimental method in the 16th and 17th centuries and the development of statistical methods since the late nineteenth century are

important examples of how our scientific methodology can grow. In Part II we too have presented three methods of science to display this methodological plurality. But it is a plurality that does not entail disunity in the sense that the methods are inconsistent with one another. They can equally be applied in their proper domain, and each contributes to our overall conception of scientific method.

Despite this plurality, however, we do not think that sciences are unlike each other. The key to understanding 'the unity' of science despite its heterogeneity is the notion of *family resemblance*, a notion initially introduced by Wittgenstein and put to effective use by Hacking to characterize science in the very paper from which Stanley and Brickhouse have quoted above! Their appeal to Hacking's passage is indeed unfortunate because his very next sentence reads that 'there are a lot of family resemblances between sciences' (Hacking 1996, p. 68). Indeed, the whole point of Hacking's essay, missed by Stanley and Brickhouse, is to defend the view that there is sufficient family resemblance among various scientific disciplines to unify them, to classify them under the label "science' and that the distinctiveness of science can be preserved through the notion of family resemblance, despite its disunity.

Against this criticism Stanley and Brickhouse have responded with a manoeuvre: 'We never claimed that it was not possible to distinguish between such scholarly areas as science, history, and art when planning school curriculum. Our focus was on the disunity within and among the 'sciences' themselves' (Stanley and Brickhouse 2001b, p. 87). Now, it is true that Stanley and Brickhouse made much use of the recent arguments in philosophy of science for the disunity thesis *among* various sciences, but it is equally true that they also tried to use those arguments to downplay the difference *between* science and non-science. We simply refer the reader to their quotation, especially the first sentence, concerning the Nisga'a above again.

We submit, along with Hacking, that what holds together diverse sciences is not a single set of characteristics shared by all sciences, but rather family resemblance, that is, a multitude of relationships partially overlapping and crisscrossing. In Section 6.6 we presented a working definition of science, which contains six components: activity, aims, product, method, M-rules, and attitude. Now, all scientific disciplines share these components, but as we explained in Chapter 6, each component consists of several elements. Thus, for example, there may be no such thing as *the* scientific method, but as we have shown in Part II, there are certainly methods of scientific reasoning, which are employed by all the sciences, not all of them at the same time within all the sciences perhaps, but some of them by most of the sciences most of the time. Hacking uses the notion of 'style of reasoning' to

make a similar point. His list of styles of reasoning include 'postulation in the axiomatic mathematical sciences, experimental exploration and measurement…, hypothetical modelling, ordering of variety by comparison and taxonomy, statistical analysis of populations, historical derivation of genetic development' (Hacking 1996, p. 65). And there are many family resemblances, overlaps and mutual support among these.

Similarly, consider various aims of science: testability, truth, knowledge, prediction, etc. Physics may realize them all, but, say, earthquake science, may meet all except the aim of making quantitative predictions; chemistry may be more testable than evolutionary biology, and so on. Thus, these disciplines overlap in their aims. Similar points can be made with respect to other components of the working definition. In short, there are many family resemblances among the various sciences which give them sufficient unity.

In sum, we think that the claims for disunity in the sciences are overplayed to no clear end. As Margaret Morrison says in a recent book which argues the case for a great deal of unity in science, 'unity' has become a much-maligned word in history and philosophy of science circles…' The criticisms have included its political undesirability, its metaphysical undesirability, and its alleged misrepresentation of scientific practice. She adds: 'Although some of these arguments are extremely persuasive, the desire to banish unity altogether has resulted, I believe, in a distortion of the facts and a misunderstanding of how unity actually functions in science' (Morrison 2000, p. 1). Her positive case for varieties of unity in the sciences includes the unification by Newton of Keplerian and Galilean mechanics, Maxwell's unification of electrodynamics and optics, recent unification of theories of force, and finally unification in the biological sciences, especially Darwinian and Mendelian genetics. Her claims on behalf of various kinds of unity in these sciences undercut extreme claims of disunity in science that misrepresent many of the real relations that do hold between the various scientific disciplines.

13.4 A MULTICULTURALIST ATTEMPT TO DEFINE SCIENCE

Once the disunity thesis is endorsed as strongly as Stanley and Brickhouse do, one wonders why physics, chemistry, geology, and so on are classified as 'sciences' and why 'traditional ecological knowledge' and 'indigenous knowledge' should be taught in *science* education courses. One is then left with no choice but to inflate the notion of science, and this brings us to the second strategy employed by many multiculturalists such as Ogawa, Elkana, Kawagley *et al*., Snively, Corsiglia, Stanley and Brickhouse. Here are some examples of definitions of science, which are widely endorsed by epistemic multiculturalists.

> Here we are following [Sandra] Harding's (1998) broad definition of science as applied to a wide range of systems for understanding the natural world in different cultures. (Stanley and Brickhouse 2001a, p. 36 fn. 1)

> By science, I mean a rational (i.e., purposeful, good, directed) explanation of the physical world surrounding man. (Elkana 1981, p. 1437)

> I shall define 'science' rather simply as a 'rational perceiving of reality', where 'perceiving' means both 'the action of constructing reality and the construct of reality'. The merit of the use of the word 'perceiving' is that it gives science a dynamic nature. Caution must be taken that 'the construct of reality' involves the peculiar way in which the reality is segmented… (Ogawa 1995, p. 588).

We invite the reader to compare our working definition of science in Section 6.6 with those of the above and note how broad the latter are. *Any* systematic understanding or explanation of the physical world (and, we would also add, the biological, mental and social worlds), or *any* rational perceiving of reality, would do. They are deliberately made broad so that 'indigenous knowledge', 'traditional ecological knowledge' and 'ethnoscience' in general count as science. While explanation or understanding is certainly a feature of science, it cannot define science by itself. The qualifiers 'systematic', 'purposeful, good, directed' are not very illuminating either. The third definition by Ogawa is the poorest. The word 'perceiving' is not used in its standard sense, but in the sense of 'the action of constructing reality and the construct of reality'. So, any (rational) activity of constructing reality and what results from it becomes science! Ogawa seems to confuse science as an activity of constructing theories or models of reality with science as an activity of constructing reality itself. His definition builds idealism into science because he believes that reality is a human construction. This is a bizarre view of science which is not endorsed even by social constructivists whom we have criticized in Part III. Social constructivists argue that reality is socially constructed, but they do not claim that any social construction of reality *ipso facto* results in science. In our working definition of science we were careful to separate characteristics of science itself from philosophical positions regarding science (see Section 6.6).

One may think that the word 'rational' in the phrase 'rational perceiving' in Ogawa's definition might bring some limitation to its broadness. But, alas, Ogawa tells us that rationality should be understood relativistically in the sense that there are as many rationalities as there are cultures (*ibid*., p. 586-588). It then, of course, follows that there are as many sciences as there are cultures and that 'Western science is only one form of science among the sciences of the world' (Ogawa 1989, p. 248; see also Snively & Corsiglia 2001, p. 10). Thus, there is not only science in the standard sense developed in the West, but also 'indigenous science' or 'ethnoscience'. But this is not

all: there is also personal science! By this, Ogawa means 'a rational perceiving of reality, which is unique to each individual' (Ogawa 1995, p. 588). So, now there are also as many personal sciences as there are individuals!

The obvious problem with such a conception of science, leaving aside its idealistic nature, is that it is wildly broad; it lets in philosophy, mythology, and possibly even religion. Needless to say, a term is useful if it is precise enough for purposes at hand. As Cobern and Loving put it, 'this is anti-reductionism made absurd and the end result is that everyone loses. Diversity is lost. Meaning is lost' (Cobern and Loving 2001, p. 61). Inflating the notion of science is therefore too costly, confusing, and useless.

That the standard conception of science as currently taught in the West is too narrow is a complaint often voiced by multiculturalist science educators. Thus, Kawagley *et. al*. explicitly write that a conception of science that puts too much emphasis on mathematical formulation, controlled experimentation in the laboratory and replicability will inevitably leave out 'indigenous science' which is 'based on observation of the natural world coupled with direct experimentation in the natural setting' (Kawagley *et. al*. 1998, p. 140). Snively and Corsiglia also make the same point and add that traditional local knowledge uses the familiar methodology of 'observing, questioning, inferring, predicting, problem solving, classifying, monitoring, interpreting and adapting' (Snively and Corsiglia 2001, pp. 10-13). Naturally, they then claim that both 'indigenous knowledge' and 'traditional ecological knowledge' should be considered as science and taught as part of the science curriculum. Before we take up this issue in the next section, we would like to express our concern with terms like "indigenous knowledge' and "traditional ecological knowledge'.

Science educators who endorse epistemic multiculturalism tend to inflate not only the notion of science but also knowledge. As we have argued in Chapter 2, not all beliefs count as knowledge unless other conditions are met, which in the classical theory involve justification and truth, but in other theories involve conditions such as coherence, or reliability, etc. (see Chapter 4). Given the distinction between belief and knowledge, then, to call an entire belief system of an indigenous group or culture 'knowledge' is misleading. Indigenous belief systems may fail to meet other conditions required by various theories of knowledge, and thereby do not deserve the label 'knowledge'. In what follows, we shall use the terms 'indigenous knowledge' and 'traditional ecological knowledge' to mean that subset of an indigenous or traditional belief system that satisfies these extra conditions.

13.5 INDIGENOUS AND TRADITIONAL ECOLOGICAL KNOWLEDGE: INCLUSION OR EXCLUSION?

Do indigenous knowledge and traditional ecological knowledge in the sense just set out count as science? Should they be taught at school? Interestingly, most science educators who have explicitly written on this issue give an affirmative answer to both questions, regardless of the fact that they are multiculturalists or universalists. (There are some exceptions, such as Cobern and Loving, whom we will discuss below.) Our answer is a conditional one: it all depends on whether or not indigenous knowledge and traditional knowledge sufficiently resemble standard science. If they do, they should be included into the science education curriculum; if they do not, they should not be, in which case the complaint that they are neglected or excluded from science education is not justified (though they may well be included in other courses such as cultural studies and history for obvious reasons). To be sure, the inclusion or exclusion may be influenced by social and cultural factors to some extent, but no amount of negotiation or power exertion will make an entire scientific community accept something as science when it barely resembles science. Conversely, there is no reason why scientists should not welcome indigenous and traditional ecological knowledge if they recognize them as sufficiently similar to their practice; they may even lead to novel solutions to their problems (e.g., unknown herbal, medical remedies).

The reason why we have not given a simple 'yes' or 'no' answer, but a conditional one to the inclusion question is that we simply do not know enough about the indigenous and traditional ecological knowledge systems. Based on the information we have gathered reading some of the literature that defends inclusion of these systems under science, we can make some general comments. But first let us look at some of the examples which we summarize as follows:

- Alaska natives knew about the behaviour and nutritional habits of bowhead whale, tidal pattern of the sea, the flow of currents in rivers, migration pattern and habits of certain birds, seasonal positions of the constellations, air pressure, climatic and seasonal changes, the behaviour of ice and snow, how to make river fish traps, how to prepare and sew caribou skins for clothing purposes, how to make hunting and fishing gear, how to gather, prepare and preserve food from plants and animals, how to navigate on open seas, rivers and tundra (Kawagley *et. al*. 1998).
- Natives in America vulcanized rubber, developed food plants and techniques for managing grassland; they discovered about 500 drugs including quinine and aspirin, and Meso-American people developed calendars with base 20 (Snively and Corsiglia 2001).

Looking at these examples, an obvious remark that needs to be made is that one must exercise caution while acknowledging what traditional cultures know and what they have actually discovered or developed. For example, Montellano corrects the mistaken belief held by the defenders of indigenous knowledge and traditional ecological knowledge that Indians *discovered* vulcanized rubber. As it turns out, 'there is absolutely no evidence that Indians ever vulcanized rubber by heating it with sulfur' (Montellano 2001, p. 78). Because of its misleading language, he also criticizes claims such as 'In the Americas, traditional *scientists developed* the long staple cotton that now clothe us and developed food plants that now feed three-fifths of humanity' (Snively and Corsiglia 2001, p. 13; our emphasis). He rightly points out that both of the terms 'scientist' and 'develop' are misnomers; traditional farmers were no scientists, and what they did was to *domesticate*, not develop, wild long staple cotton and food plants.

Consider also the following example:

> Much of Yupiaq scientific knowledge is manifested most clearly in their technology. One may argue that technology is not science. However, technology does not spring from the void. *To invent technological devices, scientific observations and experimentation must be conducted.* Yupiaq inventions, which include the kayak, river fish traps, and a wide range of hunting and fishing gear, represent *technology that could not have been developed without extensive scientific study* of the flow of currents in rivers, the ebb and flow of tides in bays, and the feeding, resting, and migratory habits of fish, mammals and birds. (Kawagley *et. al*. 1998, p. 136; our emphasis)

In a nutshell, the argument here is this:

1. Yupiaq have developed many technological devices.
2. These devices could not have been developed without science.
3. Therefore, Yupiaq have science.

But this argument does not work because no independent evidence has been provided for the truth of the second premise. In fact, it is perfectly possible to have technological development without any scientific investigation, as the history of technology and science reveals. We know that until the mid-19th century science and technology were quite independent of each other, each went its own way. Furthermore, the two communities of people, natural philosophers (as they were called until that century) pursuing scientific research on the one hand and artisans and craftsmen engaged in technological work on the other hand, rarely interacted with each other. It was only in the 19th century that it became possible to use scientific theories for developing technologies (see, for example, Kuhn 1977, pp. 141-147 and McClellan and Dorn 1999). If Kawagley *et. al*. are claiming that Yupiaq practice falsifies this well-documented history of science and technology, then they should provide evidence to the effect that these natives of Alaska actually did carry out scientific investigations which made possible the invention of the aforementioned devices.

We suspect that what led Kawagley and his colleagues to their argument is not that they have discovered such evidence (otherwise, they would have documented it.) Rather, it is their use of an extremely wide notion of science and scientific experimentation. By "scientific experimentation" they seem to mean simply commonsensical trial-and-error, and science for them looks like nothing but knowledge obtained through careful, systematic observation plus trial-and-error.

If this is all there is to science and scientific knowledge, then most of our ancestors were scientists and produced scientific knowledge! Farmers, fishermen, shoemakers, carpenters, bakers, and so on, all would count as scientists. When it is claimed that Yupiaq in Alaska have extensive *scientific* knowledge of the flows of currents in the rivers, of tidal motion in the seas, seasonal changes, air pressure, etc., we can ask, in the light of Section 2.1 on the varieties of the word 'know', whether it is know *how to*, or *that* or *what*. In particular one expects that these people would have also had some know *why*, that is, some theoretical understanding of these phenomena in terms of underlying laws and causal mechanisms (especially, those that appeal to unobservable entities such as atoms, genes and fields). But again we are presented with absolutely no evidence to support this. We have no reason to believe that Alaskan or other natives who have similar knowledge had a theoretical explanation or understanding of the motion of tides in terms of the gravitational pull of the moon, of the flow of currents in terms of the principles of fluid dynamics, and so on. This is not to deny their knowledge (in the sense of know *that*) of certain natural phenomena nor belittle its importance for their culture, but to note that they seem to have inductive knowledge merely of certain observable regularities without having any idea of the underlying laws that explain them at a deeper level.

In sum, all we know is that they possess some know *that*, some descriptive knowledge of certain facts and certain regularities. Whether they have actually carried out a scientific investigation to establish the regularities or to explain them remains a moot point; for in our view people may acquire everyday knowledge of their environment without going about it scientifically. Indeed, there is nothing surprising about the fact that people who are well adapted to their environment are also well informed about it. After all, given that such knowledge has crucial survival value, they would not have continued to exist up until now. All, or almost all, of the items of indigenous and traditional ecological knowledge listed above seem to be of this kind.

As indicated, the distinctions between different kinds of knowledge which we have emphasized in Chapter 2 are highly pertinent. The constant use of the abstract term 'knowledge' by many writers, obscures the important different uses of 'know'. Alaskan natives may know, for instance, *how to*

preserve food from plants and animals, but they may have no idea *why* preserving food that way works successfully. Similarly, we know that Polynesians migrated, over a period of several thousand years, from southern Asia to occupy most of the islands across the vast, almost empty, extent of the Pacific Ocean. Such navigational know *how* is a feat the nature of which is only being re-discovered now by sailors who reproduce Polynesian methods of sailing, and not other methods of sailing. For example, Polynesians knew *how to* navigate using the stars or bird-flight patterns, but they might not know *why* doing what they do leads to success in navigation. But they might have acquired experience some knowledge *that*, say, that navigating in such-and-such a manner leads to successful landfall. As explained in Chapter 2, one may know *that* certain observable regularities hold, but not know *how* or *why* they hold.

So far we have focused on the nature of indigenous and traditional ecological knowledge. Let us now turn to the methodology that has produced it. As we have noted earlier, it is claimed that this methodology consists of 'observing, questioning, inferring, predicting, problem solving, classifying, monitoring, interpreting and adapting' (Snively and Corsiglia 2001, p. 13). Leaving aside "adapting" which we do not quite understand, all of these are perfectly legitimate aspects of scientific methodology: scientists indeed make observations, classify and interpret their data, make inferences on the basis of them, predict, and so on. There are obvious similarities between these and what we have called the naïve inductive model of science in Chapter 7. Nevertheless, one should not overlook the important differences between indigenous methodology and the scientific methodology discussed in Part II, or some of the styles of reasoning introduced early in this chapter. Indigenous methodology seems to contain nothing like bold conjecturing and severe testing. In other words, Alaskan and other natives do not have the idea of putting forward hypotheses that have high empirical content for the explicit purpose of testing them to confirm or refute them either by direct observation or controlled experimentation. They do make predictions, we are told, but they do not seem to be directed towards the testing of their hypotheses; moreover, all of their predictions are qualitative as opposed to quantitative. Indigenous people mostly rely on careful observations, common sense and a simple form of trial and error. Their knowledge of the world is to a large extent practical knowledge that enables them to survive and maintain their culture rather than aiming at knowledge for its own sake. To put it differently, their activities of obtaining knowledge are not guided by the intrinsic values of science discussed in Chapter 6, especially Section 6.3.

These are not the only significant differences between standard science and indigenous knowledge. Their modes of existence also differ. For example, Yupiaq people of Alaska preserve and transmit their knowledge

in narrative form by oral traditions, and until recently it did not exist in writing at all; 'their science', we are told, 'is interspersed with art, storytelling, hunting and craftsmanship' (Kawagley *et. al*. 1998). By contrast, today standard science exists always in written form and has become distinct from art and craftsmanship. (It is difficult for us to imagine how science can be interspersed with hunting.) As we have pointed out in discussing Lyotard's views on science in Part III, science too contains narratives such as those told by Jane Goodall about the behaviour of chimpanzees, but they must be true or approximately true if they are to be acceptable. Science and narratives would conflict only if the latter are false or appeal to supernatural powers for the explanation of natural phenomena.

This brings us to the deepest and the most striking difference between indigenous knowledge and standard science. The worldviews underlying them are radically different and incompatible. For instance, while standard science conceptualizes nature within a physicalistic, causal and non-teleological framework, some indigenous people's conception of nature is typically spiritualistic, animistic and teleological (see, for example, Aikenhead 2001, Jegede 1989, Kawagley *et. al.* 1998, Pomeroy 1992). From at least the 5th century BCE, science in the West differentiated itself from mythology and religion in the hands of Milesian thinkers in two major ways: first, scientists rejected explanation of natural phenomena in terms of spirits, Gods and other supernatural powers and admitted only those in terms of natural causes; second, they instituted rational criticism and debate. Criticizing a given view on the basis of rational argumentation and then revising it accordingly turned out to be the most effective way of improving one's account of nature. These two developments were monumental in setting the course of science and were vital for its epistemic success. In this context it is perhaps worth mentioning the simple but crucial point that what made possible such incisive critical inquiry was writing (see Lindberg 1992, chapter 1). Writing enabled a view to be expressed in a fixed form so that others can read, criticize and improve on it. By contrast, the oral tradition is not conducive to this purpose since it is very difficult to give exactly the same account twice and remember it in all its details due to the limitations of human memory, especially where there may be vast catalogues of, say, star and planetary positions as required in Ancient astronomy. As a result, views tend to get changed (and even may become elusive) as they are handed over from one generation to another. It seems to us that this is one of the major reasons why we do not find any tradition of rational criticism aimed at improving one's account of natural phenomena in Yupiaq people. (Another reason may have to do with the politico-cultural structure of Yupiaq society; since knowledge is transmitted orally from the elders to the young, the elders

have more authority, and questioning it may be perceived as both impolite and threatening to that structure.)

At any rate, these deep differences between indigenous knowledge and standard science, especially with respect to their worldviews, raise some burning questions for science education: suppose as science educators we did decide to include indigenous knowledge into science curriculum. How are we going to handle the many conflicting accounts of nature and worldviews? Will we simply present them and leave the rest to the students? Are we going to invite them to judge which is a more correct account of nature? Or are we going to simply tell them that different cultures have different sciences or scientific worldviews that are equally valid?

These are important questions not only from the viewpoint of pedagogy but also philosophy. This is where the philosophical positions will matter most. For if we tell our students that each culture has its own science which is equally valid, then we are embracing a full-blown unjustified relativism. On the other hand, if we tell them that there are trans-cultural, objective criteria for judging alternative accounts of nature and invite them to engage critically in the business of appraisal, then we are taking a universalist position. This matters also for ethics of teaching since as science educators it is our responsibility to teach our students the best science available and equip them with the necessary knowledge and skill to form their reasoned opinions concerning rival accounts of natural phenomena and conceptions of nature.

13. 6 FOUR RESPONSES TO THE INCLUSION PROBLEM

The problem of inclusion, i.e., whether indigenous and traditional knowledge systems should be included into science education curriculum, and other related questions we have raised above received, not surprisingly, very different answers from science educators and philosophers. These can be classified into at least three different groups: we will call them relativist inclusionists, non-relativist inclusionists, and exclusionists. After we discuss the views of each, we shall present our own view which we call *limited compatibilism*. Our intention is not to discuss the views of every science educator or philosopher who has contributed to this debate, but only to present a representative sample for each group.

13.6.1 Relativist inclusionists

Relativist inclusionists argue that science and scientific knowledge are cultural products in that they are heavily determined by the social and cultural environment in which they are produced. As a result, they claim that there are many sciences, perhaps as many as there are societies or cultures, and each is an equally valid (equally correct) account of the world. It is this last claim of equal validity that makes this approach relativist. Since it is only

fair to recognize the science of each culture, they all must be taught as part of science curriculum. Hence the term 'inclusionist'. Here is a particularly strong expression of this position by Sandra Harding, a scholar of social studies of science, whose ideas have resonated strongly among science educators who endorse multiculturalism:

> While the laws of nature "discovered" by modern sciences that explain how gravity and antibiotics work will have their effects on us regardless of our cultural location, they are not the only possible such universal laws of nature; there could be many universally valid but culturally distinctive sciences. (Harding 1994, pp. 318-319)

But this passage is ambiguous between two readings. On the one hand, it may mean that different, i.e., non-Western, cultures can discover a different set of laws about *the same natural phenomena*. Under this reading, the very same natural phenomena such as free fall will be described and explained by first Newtonian (or relativistic physics) and then by some other science. The obvious question to ask is: are these different descriptions and accounts are compatible or incompatible with each other? If it is the former, scientists (regardless of their culture) will naturally wonder what the exact relationship is between the two. Is one a special case of the other under certain assumptions, as in the case of the relationship between Newtonian and relativistic theories of motion? The answers to such questions do not in the least depend on cultural factors. If, on the other hand, the two are incompatible, they cannot both be true. Of course, to avoid this conclusion, one can reject either the principle of bivalence in logic or the realist-objectivist conception of truth and adopt a constructivist-relativist one. Harding opts for the latter: 'scientific "truths" no less than false beliefs, are caused by social relations as well as by nature's regularities and the cooperation of reason' (*ibid*., p. 313). Since there are different societies and cultures, and since truth is co-determined by social and cultural relations, truth is relative to those relations. This is a familiar argument dear to the constructivist-relativists, which we have criticized at length in Part III, so we will not repeat our criticisms here.

There is another reading of Harding's passage, however. Perhaps she means that non-Western cultures will discover hitherto undiscovered laws which are about aspects of nature hitherto unexplored by science developed in the West. That is of course perfectly possible, but this is no argument against the universalist. Universalists are not ethnocentrics who think that only Western people can make such discoveries. It would be, however, truly astonishing if the science practiced by non-Western cultures is nothing like the one practiced by standard, modern science. In other words, although universalists admit the (vastly remote) possibility of an alternative science, they will predict that in all likelihood it will not be a radically different science; there will be many family resemblances between them.

The view that science and scientific knowledge are determined socially or culturally is something that has permeated the discourse of even those science educators who display a more critical attitude toward multiculturalist education (see Hodson 1999):

> Once it is acknowledged that science is a human activity, driven by the aspirations and values of the society that sustains it, it is legitimate to ask whether different societies might define and organize science differently because their aspirations and values are different. Clearly, different societies have different priorities for science and identify different technological problems, for which different "criteria of success" are applicable. As a consequence, different theories and knowledge are generated, by different investigative strategies and methods. It follows that different scientific theories and different technological solutions emerge. *In other words, scientific and technological knowledge are, to a significant extent, culturally determined* and reflect the social, religious, political, economic, and environmental circumstances in which science and technology are practiced. (Hodson 1993, p. 701; our emphasis)

This is the social constructivist mythology which we come across over and over again: not only scientific and technological problems are culturally determined, but also scientific and technological criteria of success for the attempted solutions and knowledge thus produced. The only difference is that whereas social constructivists appeal to social determination, multiculturalists appeal to cultural determination. But they are equally relativist about knowledge as well as the criteria for problem solving.

In Parts II and III we have argued why knowledge in the proper sense of this term cannot be socially or culturally determined; we will further comment on the issue of relativity of criteria below. As for a scientific or technological problem, there is an innocuous sense in which it may be culture-dependent: it may be a problem which only a particular culture faces. Thus, for example, when the moas became extinct due to relentless hunting by the Maoris of New Zealand within about 150 years of their arrival in the country (and other food sources also became severely predated), starvation became a serious problem for Maori society. They were then forced to consider cultivation to an extent they had not before and to practice various forms of conservation of the remaining natural resources they did not produce. But in another sense this is not unique to the Maori culture; the problem of finding ways of producing sufficient food and sustaining a society for generations to come is obviously a transcultural problem. Different societies may solve this common problem in different ways. Similarly, malaria is mainly a climatic problem faced by certain countries. But from this it does not follow that it is an entirely local or cultural problem. Finding the causes of malaria and fighting against it are clearly transcultural, indeed, human problems not only in the sense that they are of interests to those in developed countries as well but also in the sense that the solutions to these problems are applicable to all countries, societies and cultures. Thus, malarial infection may be a problem only certain societies or countries face,

but that such-and-such a quinine drug alleviates symptoms of malaria infection is a culture-transcendent truth, even if it may be only known to be a truth by a few. There are innumerable scientific and technological problems shared by all cultures: environmental, health, agricultural, and so on. They can also share their solution when known broadly enough.

Our final example of relativist inclusionism comes from Ogawa (1995) who argues that each culture has its own "indigenous science". In this article Ogawa discusses an interview with Nepalese people in which both adults and children are asked to explain why earthquakes occur. One explanation they gave was that the earthquakes were due to the movement of the fish on the back of which the Earth was supposedly sitting. The very same people also said that it was the fire escaping to the surface of the Earth that was causing the earthquakes. Consider now Ogawa's commentary:

> My interpretation of this example … is that Nepalese people could successfully understand Western modern science as well as their own indigenous science in terms of such kinds of natural phenomena…In addition to these understandings, each individual could have a chance to ask himself: "How does the phenomenon seem to be for me?" This is the question of personal science, and I claim that it is a crucial question in the process of education (Ogawa 1995, p. 591).

Ogawa thinks that both explanations given by indigenous people are scientific. But it is a well known fact that earthquakes do not occur because of the fire escaping to the surface of the Earth; rather, there is a common cause of both fire and earthquakes. And to call archaic beliefs such as that earthquakes are caused by the movement of the fish on which the earth is supposedly sitting "indigenous" science is plain silly. Is this what multiculturalism in science education has come to? Are we back to myth and superstition disguised as "science"? Unfortunately so, and this is the price one pays when one distorts the meaning of the term "science" beyond recognition. As if this is not scandalous enough, Ogawa invites students to ask themselves what they believe without asking them which account is supported by evidence better, let alone asking them if there can be even better explanations of earthquakes, say in terms of the motion of fault lines. In our view, the implications of such views for the classroom are disastrous:

> In a science class with a "multiscience perspective" every student can act as a researcher. She or he can conduct "interviews" on classmates or even on themselves. A possible setting is the small group discussion. The group would consist of children from different cultural origins. Each member of the group can interview other members of the group and exchange results. Through the discussion process, they can be made aware that there exist different kinds of indigenous sciences as well as personal sciences (*ibid*.).

First, if such a multicultural/multiscience perspective is accepted in science education, then students will get the impression that doing scientific work is very easy: just ask other students from other cultures what they *believe*. If all share the same culture, just ask yourself what your own belief

is. Second, such a perspective is completely blind to the distinction between belief and (scientific) knowledge. It in effect says: never mind if the beliefs are true or not, if they are supported by evidence or not; never mind, as well, if these beliefs conflict with each other, since after all the major aim of science education is awareness of one's own and others' beliefs, nothing else. Finally, this perspective is totally uncritical. It never invites students to reflect critically on their own or their peers' beliefs as if they lack the cognitive capacity to appreciate criticism, discover their errors and acquire genuine knowledge about the world. It is also interesting to see in this context the striking similarity between epistemic multiculturalist and constructivist science education. Both approaches are content with making students aware of their own beliefs or constructing their own pictures about the world without any concern for truth and empirical evidence. That science education in the classroom has boiled down to this amounts, in our view, to a decisive *reductio ad absurdum* of these approaches.

13.6.2 Non-relativist Inclusionists

As the name suggests, non-relativist inclusionists believe that indigenous and traditional "sciences" must be incorporated into science education along with standard science but refrain from assigning equal validity to them. This is clearly a more sensible position than relativist inclusionism, given how problematic the latter is. We are happy to see that more and more multiculturalists in science education are distancing themselves from such relativism. For instance, Snively and Corsiglia withdrew their earlier claim that mainstream science is 'just one among a number of equally valid and truthful sciences'.[2] But, nevertheless, the five-step procedure in their (2001) article, which aims to investigate both what the modern science and the traditional ecological knowledge perspectives would say on a given topic, fails to ask the crucial question: which is the correct or more correct perspective? By contrast, Stanley and Brickhouse do invite students to compare them and to ask which one is more likely to be true (Stanley and Brickhouse 2001a, p. 47). Such a call for comparative evaluation is important since it promotes critical thinking in students.

Unfortunately, however, we do not come across this attitude among science educators very often. Many of them are content with presenting both standard science and "indigenous science" with an eye for integrating the two. Thus, Kawagley *et. al*. (1998) urge 'infusing' the two, and the worldviews underlying them, without asking to compare and evaluate them critically. Similarly, Aikenhead (2001) talks about cultural border crossing and employs the metaphor of teacher as a culture-broker who helps students with passing from one science and worldview to the other. The idea is that science is a subculture of a larger unit, the culture of a society, and

is embedded in it. So, standard science is a subculture of Western culture and "indigenous science" is a subculture of the indigenous culture. Since each is embedded in its respective culture, it contains the constitutive characteristics of that culture. For "indigenous science" these are spiritualism, animism, anthropomorphism, respect for environment, storytelling, etc.; for science practiced in the West, they include naturalism, physicalism, objectivism, universalism, rationalism and so on.

Now, infusion of, or border crossing between, the two sciences may not present a problem as long as the two complement each other. For example, we are told that Nisga'a fishermen were able to detect, from the unusual behaviour of the crabs, that the waters in which they lived were contaminated by the newly built molybdenum mine, and this was also confirmed by standard scientific investigation (see Snively and Corsiglia 2001, p. 19). Here, we have a nice example of cooperation between what natives observed and what scientists discovered by analyzing the water. But imagine that the two did not converge to the same conclusion; suppose, for example, that scientists came up with strong evidence that the cause of the crab's unusual behaviour was not the toxic heavy metal coming from the mine but something else. What then? What should the teacher tell her students? That each account is equally valid? This would be disastrous for not only epistemological, but also practical reasons. For if the real cause is the toxic metal, closing down the mine will be an effective strategy for restoring the conditions for clean water and normal crab behaviour; otherwise, not. This is important for the well being of the natives as well as the environment; toxic heavy metals pollute the environment, tend to accumulate in sea animals and pose health dangers for those who consume them. The point is that in the imagined circumstances the two explanations – scientists' and natives' – cannot both be true, and as science educators we have a responsibility to address such conflicts. What Aikenhead recommends is that 'as a culture broker, the teacher … helps students *negotiate* cultural conflicts that might arise' (Aikenhead 2001, p. 346; our emphasis). But we find the talk of 'negotiating' utterly misleading; for it gives the impression that it is entirely up to the dialogical relationship between the teacher and the students in the classroom to decide the matter without paying any attention to the role of evidence. This impression becomes stronger when coupled with Aikenhead's further contention that 'effective culture brokers substantiate and build on the *validity* of students' personally and culturally constructed ways of knowing' (Aikenhead 2001, p. 341; our emphasis). Here we find the infusion of multiculturalist and constructivist conceptions of knowing and teaching that we have criticized in Parts I and III. Once the teacher (a culture broker) bases the class discussion on the validity of students' personal and cultural ways of knowing, then not much room is left for scientific reasoning and evidence to

play a role. In such cases of conflict, we believe that our job as science educators is to make sure that students understand the two accounts thoroughly, to present all the existing evidence for each account and then invite them to reason through their way to decide which explanation is better supported and thus more likely to be true. It is only in this way that students will develop their critical intellectual abilities and at the same time be exposed to the nature of science.

When we turn to border-crossing between the worldviews underlying standard science and "indigenous science", the situation is worse; for the conflict between the two is both categorical and conspicuous. In one there are spirits, in the other none; both cannot be true. Thus, we cannot understand how the two worldviews can be 'integrated' or 'infused' without falling into contradiction. It would be naïve to think that students will not worry about this situation when they are presented with the two worldviews even if the teacher does not require them to adopt one or the other. The contradiction is out in the open for all to see. We are surprised that many science educators have not come to terms with this obvious situation which should be extremely worrisome for teachers and students alike. We worry that border-crossing that does not deal with this issue is a recommendation for schizophrenia.

13.6.3 Exclusionists

By "exclusionism" we mean the position that indigenous and traditional ecological belief systems should not be seen as science. Cobern and Loving defend exclusionism. Their argument is that the conception of science as developed and practiced in the West leaves out most forms of indigenous and traditional ecological knowledge (more strictly belief). They believe that broadening the notion of science to incorporate these alternative forms of knowledge is not desirable. The reason is that standard science is so successful in obtaining knowledge about natural phenomena that it will end up 'co-opting and dominating' them. They conclude that

> Therefore, indigenous knowledge is better off as a different kind of knowledge that can be valued for its own merits, play a vital role in science education, and maintain a position of independence from which it can critique the practices of science. (Cobern and Loving 2001, p. 51)

They nevertheless argue for bringing these alternative forms of knowledge into the science classroom because it gives an opportunity to students to benefit from the insights provided by them (*ibid*., p. 63). For example, students may learn that indigenous knowledge is a form of knowledge that values the environment, that gives the message that technology should be low impact and that allows people to live harmoniously with nature. Cobern and Loving's approach does not shy away from using science to criticize indigenous knowledge when necessary (for example, when it gives a

wrong account of natural phenomena, say, in terms of supernatural powers) much as indigenous knowledge can be used to criticize certain practices of science (for example, when these practices have the potential to damage the environment).

In summary, Cobern and Loving's exclusionism is a highly nuanced position that rejects the widespread multiculturalist view that indigenous and traditional ecological "knowledge" are forms of science, but nevertheless argues that they should be brought into science classroom for reasons indicated above. In opposition to many multiculturalists, Cobern and Loving acknowledge the epistemic superiority of science in producing reliable knowledge about nature and underline the importance of a critical attitude toward any conception of science.

13.6.4 Limited Compatibilism

Limited compatibilism, a position which we have briefly introduced at the beginning of Section 13.5, is what we endorse. It is the view that whether indigenous knowledge and traditional ecological knowledge count as science depends on whether or not there are sufficient similarities between them. At the moment we are not knowledgeable enough about these alternative forms of knowledge to make a judgment about *overall* similarity or dissimilarity, though we did comment about certain resemblances and divergences, as far as we could tell. Thus, for example, we have noted that both rely on systematic, careful observation, classification, and comparison; both make inferences, predictions and provide explanations. On the other hand, we have also drawn attention to several deep differences. Indigenous and traditional ecological belief systems do not employ any form of sophisticated methodology such as hypothetico-deductivism or Bayesianism; they are mostly descriptive, not theoretical; the explanations lack any appeal to underlying laws or causal mechanisms; their predictions are not quantitative; moreover, they embody spiritualistic, animistic thinking; and, finally, there seems to be no institutionalization of rational criticism and debate which is so essential for scientific inquiry.

Instead of trying to pass an overall judgment of inclusion or exclusion on the basis of these similarities and dissimilarities, we shall try to draw the implications of such a judgment and specify some of the parameters of a rational and, hopefully, fruitful debate concerning the status and value of alternative forms of knowledge. The problem of inclusion goes well beyond the boundaries of a discussion about the nature of science and encompasses issues having to do with politics of recognition as well as pedagogy and ethics of teaching. Only after we set the terms of the debate and disentangle the epistemological, political, ethical, curricular and instructional-pedagogical dimensions of the problem can we hope to resolve the issue.

First, there is the question of whether indigenous and traditional ecological knowledge systems should be counted as science or not. We have already indicated our position and *a fortiori* the direction in which further discussion and research should be carried. Also our distinctions between the different uses of 'know' (Section 2.1) finesse different kinds of "knowledge".

Second, there is the question of whether these systems of knowledge should be taught at schools or not. If they do count as science, then the answer should be in the affirmative. In this way the issue of recognition is also addressed, and the demand for recognition is met. It is only fair that the contributions of *all* cultures to science be recognized. This will certainly help boost the self-esteem of the members (especially students) of those cultures whose contributions have been ignored.

Third, even when the indigenous and other alternative forms of knowledge are incorporated into science curriculum as part of science, there is still the question of how they should be taught *in relation to contemporary science*. This question arises because, as we have seen, both the content of these knowledge forms and the worldview behind them may conflict with those of science. If two accounts about the same domain of phenomena conflict, they cannot both be true. If we teach our students both of these accounts by ignoring this elementary logical truth, then we are not doing our job as educators; indeed we are doing a disservice to our students by concealing the truth. Neither ought we teach what is false. When there is absolutely no scientific evidence for the existence of spirits, we cannot pretend that accounts of natural phenomena based on them are correct. Furthermore, we think that it is equally wrong to deprive students of the kind of knowledge that would enable them to critically adjudicate between standard science and indigenous knowledge, whenever they conflict. If we want them to be knowledgeable about the nature of science as well, we must address the issue of comparison and talk about the criteria for evaluating different accounts of natural phenomena (Southerland 2000). There are such criteria which we have talked about earlier, and we will say more about them in the next section when we discuss universalism.

Fourth, it seems to us that many multiculturalist science educators assume (sometimes explicitly, but often implicitly) that in case such a critical comparison shows that standard science is epistemically superior to indigenous and traditional ecological belief systems, the cultural identity of those students who are members of communities that have produced these systems will be disrespected, their self-esteem will be damaged, and their culture will be devalued or weakened. For example, Kawagley *et. al*. write:

> Scientists and educators today often present textbook/laboratory science as the true science. Such a narrow view of science not only diminishes the legitimacy of knowledge derived through generations of naturalistic observation and insight, it simultaneously *devalues those cultures* which traditionally rely heavily on

> naturalistic observation and insight. (Kawagley et al. p. 133-134; our emphasis; see also Aikenhead 2001)

We suspect that this assumption is one of the major reasons why they either embrace relativism or shy away from critical evaluation (or both). But the assumption is unfounded, based on a misunderstanding of multiculturalism as a politics of recognition. As we have argued in Section 13.1, multiculturalism is compatible with, and even demands, judgments of superiority-in-certain-respects. No disrespect occurs to indigenous peoples and their cultures if it is shown and said that science developed in the West is epistemically better than indigenous knowledge (in terms of having more predictive and explanatory power, broader scope, etc.) any more than disrespect occurs to French people when it is shown and said that the Cartesian account of planetary motion based on vortices is inferior to the Newtonian account. Similarly, why should this damage the cultural identity and the self-esteem of students? On the contrary, we think that when they learn an epistemically more powerful way of understanding the workings of nature, they will themselves feel empowered by this sort of knowledge. And to the extent to which multiculturalist education aims to equip students with cognitive, verbal and other related skills to understand different cultures, communicate with their members and promote academic and social success in multicultural societies, this sort of knowledge should be encouraged.

If because of this they begin to value the corresponding component of their culture less, the result may be a weakening of that component of culture, but there is nothing wrong with this. Cultures are dynamic, not static, entities that constantly change and evolve due to interaction with other cultures, and they should not be seen as 'endangered species' that needs to be preserved at all costs.

Fifth, suppose now that an in-depth study reveals that indigenous knowledge does not qualify as science. What then? An obvious implication, of course, is that it cannot be taught as science. Does it then follow that it should not be taught in the science classroom at all? Recall that Cobern and Loving's reply to this question was that such a conclusion does not follow. They have argued that it can still be taught in science classroom as a different kind of "knowledge" that has its own merits. We believe that teaching of indigenous knowledge should be done in history or cultural studies courses, not in the science courses. In Turkey, for example, the cultural, scientific and technological contributions of many cultures, societies, and civilizations are discussed in history textbooks and classes; but in science textbooks and classrooms only the most developed sciences are included. In this way, we believe, the demand for recognition of the contributions of non-Western cultures to the pool of knowledge is met.

Finally, replacing standard science with indigenous, traditional ecological or similar forms of knowledge would be disastrous, and any attempt to do so should be strongly resisted (see Matthews 1994, p. 181 for such attempts). This is because standard science is far superior epistemically than its rivals and, therefore, it is our obligation as educators to teach the very best to our students whatever their cultural backgrounds are. But this is not the only reason. Science and science-guided technology is so important in today's world that to deprive students of their knowledge would prevent them from getting (better) jobs, making informed decisions concerning science and technology policies that would affect them and their fellow citizens – in short, from participating in social life on equal terms with others. One might claim that knowledge of standard science is not of much use for some traditional cultures because they do not need it, or that they may even be better off without it given its corrosive effect on their culture or environment. Against the objection that there are cultures that do not need standard science at all, we would point out that while this may be true now, it may not be true in the future. Indeed, given the fact that globalization (in its economic, social, political and cultural senses) is a phenomenon that is affecting every corner of the world, it is hard to imagine such isolated cultures can continue to exist as they are. Members of traditional cultures, therefore, have an obligation to their future generations to equip them with knowledge that would enable them to adapt to a rapidly changing world.

13.7 UNIVERSALISM IN SCIENCE

Earlier in this chapter we said that epistemic multiculturalism is a position that has been developed to replace the universalist conception of science. Despite the fact that universalism has been the main target of multiculturalists in science education, it has been poorly understood and often misunderstood by them. Because of this, most of their criticisms are simply off the mark. But before we tackle their criticisms, we would like to clarify what we mean by universalism.

The core idea of universalism that we defend in this book has been eloquently formulated by Michael Matthews:

> Universalists regard science as an intellectual activity whose truth-finding goal is not, in principle, affected by national, class, racial or other differences: science transcends human differences…This universalist view recognizes that while aspects of culture do influence science, nevertheless cultural considerations do not determine the truth claims of science… The core universalist idea is that the material world ultimately judges the adequacy of our accounts of it. Scientists propose, but ultimately, after debate, negotiation and all the rest, it is the world that disposes. The character of the world is unrelated to human interest, culture, religion, race or sex…Just as volcanic eruptions are indifferent to the race or sex of those in the vicinity, and lava kills whites, blacks, men, women, believers, nonbelievers equally, so also the sciences of lava flows will be the same for all. For the universalist, our science of volcanoes is

> assured by a human construction with negotiated rules of evidence and justification, but it is the behaviour of volcanoes that finally judges the adequacy of our vulcanology, not the reverse (Matthews 1994, p. 182).

Accordingly, universalists claim first there is a reality, a world out there, which exists independently of minds, cultures, languages, theories or worldviews. This we call *minimal realism* (see Section 10.1.1 on varieties of realism). It is minimal because it does not say anything about the nature of this reality; it merely asserts there is an independent reality. This is a minimal realism that only the most rabid of idealists would deny. Often, however, it is the only realism many multiculturalists in science education will assent to. Following Devitt (1997, chapter 2), a less minimal realism claims that most of the following objects exist independently of us human beings and our minds: planets, mountains, atoms, genes, magnetic fields, electrons, entropy, and so on. This more substantial realism about everyday and scientific objects claims that we are largely right about such existence claims; the qualification 'most of' allows for fallibility with respect to some of the items in the list. Expressed this way the more substantial realism is a contingent thesis about our world that we can come to know in an *a posteriori* fashion. This thesis of realism does not say that everything exists in this way. Obviously, there are many things (such as universities, courts, money, etc.) that would not have existed if there were no human beings.

Second, universalists also claim that this reality that has independent existence is knowable, in particular, by scientific means. We call this, for obvious reasons, *epistemic realism*. We know, for example, that the circumference of the Earth is about 40,000 km, that planets in our solar system have (roughly) elliptical orbits. Thus, universalists claim that we have knowledge of both particular and general facts which are expressed in laws of nature. We also know that electrons are negatively charged, that organisms, except viruses, are made of cells, that water is a collection of H_20 molecules, and so on. As these examples indicate, universalists believe that we have knowledge of not only the observables such as planets, but also of unobservable entities such as electrons, cells, viruses and the like. Knowledge of laws (in particular, causal laws) enables us to have explanatory and predictive knowledge about the world. In sum, universalists believe that we have a variety of knowledge about the world, including know *that*, know *why*, etc.

Third, universalists believe that what makes such claims to knowledge true are the features or aspects of reality, not interests, gender, race, ethnic background and the like. Thus, universalists hold an objective, realist theory of truth (see Section 2.3). On this theory of truth if a claim is true, it is *universally* true regardless of time, place and culture. To the extent to which science has hit upon truths using scientific methods and techniques, scientific truths are universally true and scientific knowledge is universally valid. This is not to say that science never errs. Sometimes it does for a variety of

reasons, and that is why universalists are at the same time fallibilists. But it is equally important to note that science has the means to detect its errors and correct them sooner or later. Following Popper, we take the view that many of our theories may only be approximately true, or have some degree of verisimilitude or truth-likeness. This is especially the case where our theories are based on ideal models which only fit the world to some extent, improved models making a better fit (see Chapter 10).

Fourth, universalists claim that there are objective and universal or, at the very least transcultural, criteria (standards) to judge the merits of alternative theories about nature. These criteria can be read off from the intrinsic aims of science discussed in Sections 2.5 and 6.3. An intrinsic aim, when realized, also serves as a criterion for evaluating rival theories. Thus, for example, other things being equal, we prefer a theory over its rival if it has greater explanatory power; other things being equal, we prefer a theory over its rival if it has greater predictive power, and so on. We have already said a lot about predictive and explanatory power, truth, and fruitfulness in the previous chapters, so here we will only mention consistency. A theory is internally consistent if it is free of contradictions. It is externally consistent if it does not contradict the other theories in other fields. For instance, Darwinian evolutionary theory is remarkably consistent both in itself and with other theories in geology and cosmology with respect to the age of the Earth and the Universe. Cosmology estimates the age of the Universe to be 12 billion years old, geology estimates the age of the Earth to be 4.5 billion years old, and the theory of evolution tells us the process of evolution took in the order of hundreds of million years. These all fit beautifully. In short, we can say that a theory is to be preferred over another if it is consistent, more fruitful, yields more predictions (especially novel ones) and has greater explanatory power, and so on. The fact that these criteria have been formulated and employed mainly within the context of Western culture does not rule out their efficacy for evaluating different types of knowledge claims about nature. Furthermore, whether or not a theory exhibits the characteristics of consistency, fruitfulness, etc. better than its rival is an objective matter which in no way changes from person to person or from culture to culture.

Fifth, universalists hold that science is a rational activity. This means that a scientific hypothesis or theory is held when there are good reasons for it. Having high empirical content (being highly testable), passing severe tests, having explanatory power, being fruitful, etc. are all good reasons for accepting a hypothesis or theory. The same point can be made with respect to the rationality of theory choice. All else being equal, it is rational to choose that theory that has more explanatory power than its rival; all else being equal, it is rational to choose that theory that is more fruitful than its rival, etc. The existence of such criteria make rational theory choice possible.

Universalists are well aware that there is no algorithmic procedure for theory choice. In other words, they do not think that theory choice is mechanically determined by the step-by-step application of methodological rules. If this were so, then there would have been no need for human deliberation and decision; theory choice could have in principle been carried out by machines. However, this is not the case at least for two reasons. The first is that in practice a theory rarely satisfies all of the criteria for being a good theory better than its rival. One theory may explain phenomena better, but be less fruitful or simple. Thus, the criteria might pull in opposite directions in a particular situation. Another reason is that scientists may attach different weights of significance to each criterion. Some may value fruitfulness more than explanatory power. Scientists must weigh each, compare and decide. For these reasons, theory choice always involves human deliberation and judgment, and it is here that personal taste or sheer idiosyncrasy may find a modest place in science (see Kuhn 1977, Chapter 13). Despite the widespread belief to the contrary, universalists do not deny the human factor in such scientific decisions and accept that there is room for rational disagreement in science at any given moment, a disagreement that can be settled in time by further research.

Finally, universalists believe, largely as a result of the above, that objective scientific inquiry and knowledge about the world is possible. Objectivity of science should be understood in several senses. First, to say that scientific knowledge claims are objective is to say they are publicly testable and can be subjected to public, critical scrutiny by anyone who understands them. Second, scientific knowledge, *qua* being knowledge, is also objective in the sense that its truth does not change from culture to culture or according to personal taste. Finally, science is also objective in the sense that there are universal or transcultural criteria that enable us to appraise scientific theories. It should be obvious to anyone that objectivity in these senses is an ideal that science strives for and often, though not always, succeeds in achieving.

There is one respect in which we would demur from the characterization of universalism given by Matthews when he speaks of ‘negotiated rules of evidence and justification’. The view we have adopted, especially in Part II, is that principles of scientific method are just as objective as the facts about the world. We take the principles of logic to have objective force and not to be a matter of negotiation. The same applies to the principles of method in the domains to which they apply. And again the same applies to the theory of statistical inference that has come to dominate much of science. The logic and methodology of science is not a matter for cultural variation (though we do allow for an evolution in our ‘styles of reasoning’ as set out in Section 13.3 above).

After having explained what universalism is, let us now turn to multiculturalists' understanding and criticism of universalism. One of their favourite arguments against universalism is that because our knowledge of reality is always meditated, shaped or determined by language, culture or personal interest, we cannot determine if our beliefs are in accordance with mind-independent reality, we can never know reality as it really is (see, for example, Stanley and Brickhouse 1994, p. 389 and 2001a, pp. 41-42). This is a variant of the famous inaccessibility of reality argument. Since we have criticized it in detail in Chapter 5, we will just note here that multiculturalists and constructivists are united in endorsing this fallacious argument and in rejecting the realist conception of truth. But we would like to make one point clear. Universalists do not and need not deny that our access to reality is mediated. They accept for, example, that our knowledge of reality is mediated by our language, by the theories we employ, and so on. But they deny that we cannot know reality as it is objectively. Similarly, universalists are aware that scientific activity is influenced by social, economic, political factors and even personal bias. They know, for example, that what problems and projects one pursues may depend on personal interest or funding; they also know how political factors may influence the formulation of theories and the development of science. The Lyssenko affair is a case in point (see Chapter 14). But they also claim that science has the means, through its methodology, to detect and eliminate such distorting influences. To put it differently, universalists distinguish between *mediation and influence* on the one hand and *determination* on the other hand. While they accept the former, they deny that scientific inquiry is determined by non-epistemic factors.

Multiculturalists sometimes also reject the minimal realism endorsed by the universalists. As we have seen earlier in this chapter, Ogawa is a case in point. Recall his definition of science: 'I shall define "science" rather simply as a "rational perceiving of reality", where "perceiving" means both "the action of constructing reality and the construct of reality" (Ogawa 1995, p. 588). According to this definition, science is a rational construction of reality. Since science is a human activity, reality becomes a human construction. This is a denial of minimal realism. Even those multiculturalists who do not deny minimal realism waver on this issue. For example, Stanley and Brickhouse explicitly endorse minimal realism when they write that 'they assume a material reality that exists independently of what we may think or know of it' (Stanley and Brickhouse 1994, p. 389; see also 2001a, p. 41). But in an another article they say that 'we reject, as another false dichotomy, the simplistic view that either one is a realist or an antirealist' (Stanley and Brickhouse 2001a, p. 42). We find the same wavering attitude in multiculturalist scholars of social studies of science as well. A good

example is Sandra Harding. Consider the following passages from her book *Is Science Multicultural*?:

> It should not need to be said, but probably does, that the choice between absolutist forms of realism and constructivism offers only inadequate options from the perspective of this study. Of course 'there is a world out there', 'reality exists', and successful, useful sciences and technologies, modern or not, have to be good at grasping a great deal about the realities of the parts of the world with which they interact. (Harding 1998, pp. 19-20)

So, Harding concedes the two points universalists make: that there is an independent reality and that it, or at least certain aspects of it, can be known by science. She is, in other words, conceding to both minimal and epistemic realism, respectively. But later in her book she takes the minimal realist claim back in the footnote to the passage below:

> Even in the 'same' environment different cultures have different interests and choices. These lead cultures to pose distinctive questions about 'the same' part of the world (*ibid*., p. 64).

While this passage is compatible with minimal realism, the footnote to it is not:

> 'Same' is in scare quotes since, as we shall see in the next sections, different cultures conceptualize 'the world' in ways that are shaped by their distinctive interests, discursive resources, and ways of organizing the production of knowledge about nature. *In important senses, different interests, discursive resources, and ways of organizing knowledge production create different worlds, different patterns 'in nature'* that are the object of systematic scrutiny. (*ibid*., p. 205, fn. 19; our emphasis)

This is very typical of multiculturalists. They first pay lip service to minimal realism and even to epistemic realism, and then they turn around and say that the world is *created* (in other words, constructed) by interests, discourses and so on. Once again we see how multiculturalism and constructivism merge on this issue. Note also that once such a constructivism is endorsed, it becomes impossible to talk about *the* world or *nature* (which, incidentally, explains the use of scare quotes *also* around 'the world' and 'in nature'); since there are different interests, different discourses and so on, there is not just one, but many worlds. In the hands of multiculturalism denial of minimal realism leads to ontological relativism. One is either a minimal realist or an antirealist; one cannot be both. Either there is an independent reality or there isn't. There is no third alternative. The sooner the multiculturalists learn to live with this and become realists the better they can serve the purposes of science education.

Another criticism made by the multiculturalists against the universalists is that the latter's conception of science 'retains a significant positivist influence' (Stanley and Brickhouse 1994, p. 391). By this they mean the following:

> The universalist view of science claims that the ontological world itself judges the validity of a scientific account of that world, and this account is unrelated to such things as human interest, culture, gender, race, class, ethnicity, or sexual orientation.

> Such a view ignores the role of scientific community in mediating knowledge claims and retains some of the more problematic tenets of positivism. (*ibid*., p. 390).
>
> Although the natural world constrains what would be a reasonable accounting of reality (e.g., apples fall to the earth), it is the community of scientists who ultimately decide what sense to make of such observations. It is true that such groups will work to determine methods for making judgments, but such methods are always the result of community deliberation. In other words, what we choose to study and the methods we use are determined by human dialogue and interpretation. Thus, there is nothing universal about such methods as they are subject to ongoing reconsideration and reconstruction as the group of scientific inquirers constantly changes. (*ibid*., pp. 391-392)

Although there are many objections we could raise about these passages, we will focus on just two matters. First, the conclusion, expressed in the last sentence does not at all follow from what is said before them. There is absolutely nothing in the universalist picture of science of the kind that we defend here which prevents us from acknowledging the role of scientific communities in science. As Kuhn has taught us, one of the features that distinguishes mature (modern) science from its earlier forms is precisely the formation of relatively autonomous communities of scientists who all go through a similar kind of education in their respective fields, are exposed to the same examplars, publish in similar journals, attend similar meetings and get organized around similar associations. What the universalist rejects is that it is the scientific community that determines a claim to be true. This is the point of Matthews' statement that 'it is the behaviour of volcanoes that finally judges the adequacy of our vulcanology, not the reverse' (Matthews 1994, p. 182). Most likely, multiculturalists are confusing two things: truth and acceptance as true. It is the latter that is decided by the scientific community, not the former. In other words, the scientific community decides to *accept* (or reject) *a claim to be true*, but it cannot make it true. In a similar vein, the universalist does not deny that scientific theories and methods are human constructions and that the interpretation of data, what problems deserve more attention, and which methods are appropriate for the solution of which problems involve discussion and deliberation. These are truisms, trivial consequences of the obvious fact that science is a human activity.

But from these it does not follow that scientific methods are not universally applicable and have only local validity. Take, for example, naïve inductivism and the hypothetico-deductive (H-D) method which we have discussed in Part II in detail. Both have been known, and the former has been in use, from early on since the time of Ancient Greek philosophy. True, the H-D method began to be employed in science in the West from the 16th century onward, but this does not invalidate its transcultural validity. In other words, its application to natural phenomena transcends all cultural, racial, ethnic, sexual, and personal boundaries. Moreover, it is employed in one

form or another by European, Middle Eastern, Asian and African scientists all over the world. Furthermore, H-D method has certain objective characteristics that do not change from culture to culture or according to gender or personal taste. It is an objective, transcultural feature of the H-D method, for instance, that when there is a prediction failure, the conjunction "Theory *and* Auxiliary Assumptions *and* Initial Conditions' from which the prediction is derived cannot be true. There is nothing local about the H-D method in this sense. And the fact that it was discovered in a certain part of the world during a certain period of history does not in any way invalidate its applicability to natural phenomena provided that scientists are ingenious enough to come up with hypotheses and draw testable consequences from them.

Even the arch-universalist Popper who has been the target of much multiculturalists' critique grants that the objectivity of science and scientific method are because of, rather than in spite of, cooperation among scientists. Popper has always been critical of the view that objectivity consists in the impartiality or open-mindedness of the individual scientist on the grounds that objectivity in this sense is a myth, an impossibility because one can never completely free oneself from one's own 'system of prejudices' or 'expurgate his ideological follies' (Popper 1971, p. 217). But, nevertheless, he argued, scientific objectivity is possible in virtue of scientific method and critical inquiry more generally:

> Objectivity is closely bound up with the social aspect of scientific method, with the fact that science and scientific objectivity do not (and cannot) result from the attempts of an individual scientist to be objective, but from the friendly-hostile cooperation of many scientists. Scientific objectivity can be described as the intersubjectivity of scientific method. (*ibid*.)

In this regard it may also be worth bringing Kuhn's latest views concerning theory choice to the attention of the multiculturalists. Multiculturalists frequently appeal to Kuhn's philosophy in order to support their view that science lacks universal standards and that there is no court of appeal higher than the decision of the scientific community. To be sure, some of the passages in Kuhn's earlier 1962 work *The Structure of Scientific Revolutions* do lend support to this. But later he began changing his views. For instance, in his paper entitled 'Objectivity, Value Judgment, and Theory Choice' in *The Essential Tension* (Kuhn 1977) concerning criteria for theory choice (such as quantitative accuracy, simplicity, consistency, broad scope and fruitfulness—his five values) he wrote:

> Throughout this paper I have implicitly assumed that, whatever their initial source, the criteria or values deployed in theory choice are fixed once and for all... Roughly speaking, but only very roughly, I take that to be the case. If the list of relevant values is kept short and if their specification is left vague, then such values as accuracy, scope, and fruitfulness are permanent attributes of science (Kuhn 1977, p. 335).

Thus, to the question whether such criteria are universal or not Kuhn gave a very qualified answer. But in post-1980 writings, he dropped the qualification. In a late statement of his views in 'Afterwords', he says:

> As the developmental process continues, the examples from which practitioners learn to recognize accuracy, scope, simplicity, and so on, change both within and between fields. But the criteria that these examples illustrate are themselves permanent, for abandoning them would be abandoning science together with the knowledge which scientific development brings (Kuhn 1993, p. 338).

Kuhn's position concerning the universality of criteria for theory choice can be understood in the following way. As a historian, Kuhn saw that some of the criteria did change, albeit very slowly. For example, he noted that in ancient times *quantitative* accuracy was demanded only from astronomy, not from other disciplines, certainly not from physics. But one major result of the scientific revolution in the 16th and 17th centuries was the mathematization of many scientific disciplines. Thus, that a theory should make quantitatively accurate predictions became a routine demand from physics, chemistry, and so on. The demand for accuracy turned into the demand for quantitative accuracy. Kuhn seems to think that the five values became permanent features of science after the scientific revolution. New features may be added in the future, but Kuhn thought that the old ones are very unlikely to disappear. It is for this reason that Kuhn writes that 'abandoning them would be abandoning science altogether'.

13.8 SCIENCE AND VALUES

Multiculturalists also draw our attention to the question of the relation between science and ethics/wisdom, and it is for this reason that they emphasize the value-ladenness of science. We also agree that science is a value-laden activity, but this must be spelled out carefully. In Chapter 6 we have distinguished between aims intrinsic to science and aims extrinsic to it. Truth, testability, predictive and explanatory power, consistency, simplicity, fruitfulness and wide scope are all aims intrinsic to science; they are aims in science pursued for their own sake. Such aims are epistemic or cognitive since when they are realized they facilitate giving us knowledge about the world. Intrinsic aims can also be called values because they are valued highly in science; they are values that a good scientific theory must have or aim at. Science is value-laden in this sense, though some values may be more highly valued than others. For example, many scientists value novel predictions over simplicity. These intrinsic (epistemic, cognitive) values or aims have so far functioned effectively in producing knowledge and evaluating scientific theories.

Science may also be value-laden in a second, extrinsic, sense, namely 'what people judge to be good' from the viewpoint of social, political, environmental and ethical values (Graham 1981). Thus, for example,

discovering a new and cheap form of energy that does not pollute the environment is value-laden in this second sense. But such values are non-epistemic in the sense that their realization does not facilitate the production of knowledge; science would continue to be what it is and continue to do what it does best even without them. When multiculturalists in science education talk about the relationship between science and values, they have this broader, non-epistemic and extrinsic sense in mind. The relationship between values and science in general is a very complex one on which whole books have been devoted, and we cannot do justice to the issue in this book (see, for example, Graham 1981 and Lacey 1999). We will be content to make just a few points.

First, some non-epistemic values and concerns have direct bearing on scientific activity, especially scientific research because of their relevance to what we have called scientific attitudes (see Chapter 6). Reporting the results of a study without distortion, getting the consent of people who will be subjects in an experiment, being vigilant against racist, sexist and ethnic prejudices that may affect the formulation of hypotheses or the designing of experiments are obvious examples that mostly constitute what Robert Merton has called 'the ethos of science' (Merton 1973, especially chapter 13). These values should be respected not only because they are desirable in themselves, but also because they help scientists produce more reliable knowledge. Other non-epistemic values such as social justice, equality, a safe and clean working and living environment, etc., though desirable in themselves and for social and political reasons, do not play a direct role in facilitating knowledge production. Nevertheless, *the relationship* between science and extrinsic values can and should be raised and discussed, whenever appropriate, in teaching science: is nuclear energy too risky for people and the environment? Should human cloning be allowed or not? These and similar questions are an important part of science education.

Second, the intrinsic values of science can neither be reduced to nor replaced by extrinsic, non-epistemic ones. As we argued in Section 6.2, realizing our extrinsic values for science depends on realizing the intrinsic values of science, and not conversely. No activity, no matter how valuable from a moral, political, or social-cultural perspective, will bear any family resemblance to scientific activity if it is deprived of its epistemic/cognitive values. If it is to deserve its name, science education cannot afford to teach non-epistemic values at the expense of epistemic ones.

Third, we nevertheless believe that scientists cannot escape from the responsibility they have to society which makes their scientific activity possible. The middle position we advocate, therefore, emphasizes the social, political and ethical responsibility of the scientist without giving up the relative epistemic/cognitive autonomy of science. Recall again the distinction

we have drawn between the aims of science and those of scientists in Chapter 6. As a responsible *person*, the scientist must continually try to strike a balance between the two kinds of aims or values, that is, between creating theories that are true, explanatory, fruitful etc., and at the same time producing useful knowledge or, at the very least, avoiding damage to people, society, and the natural environment. This is no easy task, but each scientist *qua* being *both* a scientist *and* a citizen of her country and of the world must address this issue at every step of her activities.

Finally, we do not believe that science alone can solve all of the problems human beings face in this world, especially the moral ones. Wherever appropriate, science can and should provide us with the relevant facts that might help us with our moral choices, but it cannot determine those choices. We should therefore clearly articulate what science can and cannot achieve, carefully draw its limits and thus cut it down to size (Chalmers 1990, Rose 1986). It seems to us that perhaps the most valuable lesson we can learn from indigenous and traditional ecological belief systems is that cultures must continually strive for a harmony between their scientific practices and the ideal of a good life. But we do not believe that we can serve this purpose well by embracing moral relativism or epistemic multiculturalism.

13. 9 MULTICULTURALISM, SCIENCE EDUCATION AND CRITICAL INQUIRY

We have argued that multiculturalism cannot be imported into science education without critical assessment. There is much in multiculturalism that is problematic, to say the least. We have also distinguished, on the one hand, between multicultural education and multiculturalism, and, on the other hand, between multiculturalism as a politics of recognition and multiculturalism as an epistemic position. We think that multicultural education in today's complex societies is a must, but there is nothing that we can recommend in epistemic multiculturalism. While multiculturalism as a politics of recognition has several negative aspects, it does have a valuable core. This core, which consists of the principle of equality, is perfectly compatible with a universalistic conception of science and at the same time constitutes the foundation of multicultural education: every citizen has a right to best education available and should have an equal opportunity for it; all students must be treated justly and with respect; no student should face discrimination because of their colour, ethnic origin, class, sexual orientation, and so on. But this in no way means that their views, or the worldviews of the communities or cultures they belong to, are noncriticizable or equally valid. The curricula must recognize and include the scientific contributions of all people, of all cultures. These are truisms beyond dispute.

We believe that adopting an epistemic multiculturalist perspective in the science classroom is at odds with the aim of critical inquiry. The affirmation and celebration of what is local, indigenous and traditional, though it can make some students feel good, does not help them become critical inquirers in any way. By embracing the relativist view that all belief systems are equally valid, epistemic multiculturalists immunize any belief against criticism. One then wonders why students who are members of indigenous cultures are taught the science of the non-indigenous cultures, and vice versa, at all.

Science is in essence a critical activity. Even the most entrenched beliefs (such as determinism, the absoluteness of space and time, and so on) are open to critical scientific scrutiny and can be revised when necessary. Science, therefore, can be a valuable and powerful tool that can be used to pinpoint and eliminate what is false, unjustified or simply superstitious in local culture. Epistemic multiculturalism deprives students of enjoying the critical power of science. It should have no room in the science classroom.

With currents such as postmodernism and multiculturalism standard science has increasingly come under fire, not just from an epistemic perspective but also a political one. Whether science excludes minority groups, whether it ignores their contributions, and whether it serves for a just and egalitarian society are all political concerns voiced by these currents. Indeed, it would not be an exaggeration to say that under their influence science and science studies have become politicised to an unprecedented degree. The next chapter examines this politicisation of science.

NOTES

[1] It may be worth noting that there are countries that have endorsed multiculturalism as an official policy. Canada is a notable example: 'Canadian multiculturalism is fundamental to our belief that all citizens are equal. Multiculturalism ensures that all citizens can keep their identities, can take pride in their ancestry and have a sense of belonging. Acceptance gives Canadians a feeling of security and self-confidence…The Canadian experience has shown that multiculturalism encourages racial and ethnic harmony and cross-cultural understanding, and discourages ghettoization, hatred, discrimination and violence. Mutual respect helps develop common attitudes.' (http//www.pch.gc.ca/multi/what-multi_e.shtml) Note the discourse of identity as well as the principles of equality and difference, echoing Taylor's formulation.

[2] Compare the later version of their (2001) paper with the one presented at the Annual Meeting of the National Association for Research in Science Teaching, San Diego, CA, April 1998: Snively. G. and Corsiglia, J. (1998) 'Discovering Indigenous Science: Implications for Science Education'.

CHAPTER 14

POLITICS OF SCIENCE AND SCIENCE EDUCATION

That there is a constant interplay between science and politics is not a new thesis; it is a fact well known and recognized by those who advocate a realist, rationalist and universalist conception of science. Just to give a few examples, Bacon observed long ago that the use of scientific knowledge can enhance human powers (see Section 11.5.1). Centuries later, especially with two world wars in the 20th century, applied science became an enormous source of military and therefore political power for governments. Conversely, after turning into a costly enterprise, the direction of scientific research began to be shaped considerably by the availability and amount of funds, the distribution of which is open to all kinds of political negotiation. The Lyssenko affair attested to the degree to which scientists may be influenced by ideological factors in pursuing a theory. Nevertheless, according to the realist, rationalist and universalist conception of science that we advocate in this book, the influence of political or social factors either stays at the institutional level and therefore does not penetrate into the content of science, or even if it does, it can be detected, eliminated or at the very least bracketed by scientific means.

Science education was not blind to this set of complex interrelationships between science and politics, and since at least the mid-fifties many developed countries including USA and Britain stressed the need to include in school science the social, political and economic dimensions of science (Bybee and DeBoer 1994; Matthews 1994, Chapters 1 to 3). The call for the inclusion of the history and philosophy of science into science education was, in part, an expression of this need.

By and large, the image of science in science education circles until around late 1970s was universalist. In the previous chapters we saw that with the rise of currents like constructivism, postmodernism and epistemic multiculturalism this image of science came under fierce attack.[1] What is perhaps more interesting is that at the same time, under the influence of these movements and the social studies of science more generally,[2] science began to be perceived, in unprecedented ways and degrees, as a political enterprise, and politics of education became one of the primary concerns of many educationalists. If, as postmodernists argue, the main aim of education is empowerment or deconstruction, if mainstream science education is culturally hegemonic and imperialistic because it includes the teaching of only 'Western science' at the expense of 'indigenous science' as multiculturalists charge, if

social constructivists are right in pressing that science is socially constructed in its very content and, finally, if science is revealed to be 'ideological, masculine, elitist, competitive, exploitative, impersonal, and violent' by the social studies as it is often claimed (Aikenhead 1997, p. 220), then this is expected and not surprising. Under these conditions science and science education cannot but be political in the strongest sense, through and through.

We believe it is not a coincidence that such politicisation goes hand-in-hand with the constructivist-postmodernist-multiculturalist attack on the realist-rationalist-universalist image of science; for in the eyes of the attackers, there is an *intrinsic* connection between the two. Surprisingly, however, to the best of our knowledge, this relationship has not been explored in detail, if at all.

As far as we can see, the attack and the politicisation are related in the following way. Many postmodernists, epistemic multiculturalists and constructivists claim that talk of universal science and reason, objective truth and knowledge is repressive and reactionary because it is imperialistic, homogenizing and unifying. That is why they attack it; that is, they attack it in the name of a progressive and emancipatory education and politics. And they embrace relativism for the same reason because they think that relativism is tolerant of alternative (local, indigenous) forms of science, knowledge and rationality. In this way conceptions of science become related to political concerns. This is indeed a radical politicisation of conceptions of science, knowledge and reason.

In the next section we document and spell out this argument in more detail and then subject it to a critical scrutiny. Our conceptual analysis and criticism reveals that there is nothing in postmodernism and epistemic multiculturalism that is intrinsically liberating or progressive. While we believe it is a mistake to think that a political position can be derived solely from epistemological considerations, we nevertheless argue that the two can be allied with each other under certain historical conditions. In Sections 14.2 and 14.3 we present two case studies, one from India and another from Turkey, that show how conservative and/or reactionary movements in various parts of the world today are nourished by postmodern and epistemic multiculturalist ideas about 'local knowledges', 'alternative sciences' and so on. These case studies support our contention that, although epistemological positions such as universalism and relativism are politically neutral, under certain conditions they can be exploited to serve certain political purposes, indeed, sometimes very conservative or reactionary ones, nullifying the argument that postmodernism and epistemic multiculturalism are intrinsically emancipatory and progressive.

We believe that it is important to address the interplay between science and politics as part of science education in today's globalized world, but this

must be done with care. In our opinion, most of the problems associated with science today have to do more with *the institution of science* as it operates and sustains itself within the larger context of real politics and economics policies than with science as an activity and thought (see Section 6.6). As is well known, in the twentieth century both science and science-driven technology have become sources of wealth and power. Forces of the free market economy, and the interests of states and governments for gaining more power, are exerting an enormous (negative) influence on the direction of scientific and technological research, forcing universities to enter into unhealthy relationships with industry. To blame science *tout court* or to blame the universalist conception of it does nothing but to muddle these real issues. We highlight this point in Section 14.4.

14.1 IS UNIVERSALISM REPRESSIVE? ARE POSTMODERNISM AND EPISTEMIC MULTICULTURALISM EMANCIPATORY?

What exactly is the argument for the claim that universalism is repressive, and postmodernism and epistemic multiculturalism are emancipatory? Since nowhere is it stated clearly, we shall quote some passages at length.

> Does the ideal of a single, universally valid science decrease global democracy?…For many feminist, multiculturalist, and post-Kuhnian science studies theorists, however, the universality ideal increasingly appears as a force for maintaining inequality and obstructing democratic tendencies and for obstructing the growth of knowledge. For these groups, claims to the transcultural truth of modern sciences' representations of nature, and of only those of modern science, function to mask ways that modern sciences and their representations of nature's order tend to distribute the cognitive and social benefits of scientific and technological changes disproportionately to those already positioned to take advantage of them, and the costs primarily to those least able to resist them. Moreover, universality claims legitimate the devaluation and even destruction of knowledge traditions that have enabled women, the poor, and less powerful cultures to interact effectively with their environments. (Harding 1998, p. 168).

> There are at least two consequences of a universalist conception of scientific knowledge: (1) it allows scientists to pretend to have a God's eye view of the world, giving them permission to "tell the truth" without being held responsible for these truths; and (2) it rationalizes the destruction of knowledge systems deemed inferior by Western standards. (Stanley and Brickhouse 1994, p. 392)

> Thus, in many educational settings where Western modern science is taught, it is taught at the expense of indigenous science, which may precipitate charges of epistemological hegemony and cultural imperialism. (Snively and Corsiglia 2001, p. 7).

> Once it is acknowledged that the West does not have a monopoly on all the good scientific ideas in the world or that reason, divorced from value, is not everywhere and always a productive human principle, then we should expect to see some self-modification of the universalist claims maintained on behalf of empirical rationality.

> Only then can we begin to talk about different ways, of doing science, ways that downgrade methodology, experiment, and manufacturing in favour of local environments, cultural values, and social justice. This is the way that leads from relativism to diversity. (Ross 1996, p. 5).

> Is legitimacy to be found in consensus obtained through discussion as Habermas thinks? Such consensus does violence to the heterogeneity of language games. And invention is always born of dissension. [Postmodern knowledge] refines our sensitivity to differences and reinforces our ability to tolerate the incommensurable. Its principle is not the expert's homology, but the inventor's paralogy. (Lyotard 1984, p. xxv).

From these we can glean the following argument. There is no truth or rationality that is not historically and culturally embedded. Modern science, too, is a cultural product that emerged in the West in the 16th and 17th centuries. Like all cultural products, science is shaped by social, political, economic or ideological factors as well as legitimated by such considerations. Talk of universal or objective truth, knowledge and rationality masks ideological hegemony. Such a discourse is repressive and reactionary because it is an expression of Eurocentrism and imperialism; it not only homogenizes differences but also silences the local and the indigenous voices. It implies that there is only one truth, one kind of science and one kind of rationality for all cultures. Those who claim to have privileged access to truth, science and rationality naturally acquire epistemic authority; it empowers them to accuse others of being superstitious, irrational and backward, and to silence or repress them if they resist. It is precisely this kind of authority that Western powers have utilized for colonizing other countries, for their imperialistic purposes. Postmodernism and epistemic multiculturalism undermine the authority of universalism by embracing epistemic relativism. Thus, if there is no objective truth and no transcultural rationality, every culture is free to form its own belief system and live by it. In other words, relativism is tolerant of diversity and plurality of (even conflicting or incommensurable) perspectives, belief systems, and methods; it does not impose any one perspective, belief-system, method etc. as the only correct or rational one. It is in this sense that postmodernism and epistemic multiculturalism are supposed to be liberating and politically progressive.

It is hard to exaggerate the pervasiveness and the rhetorical appeal of this argument in the postmodernist and multiculturalist literature. But once it is formulated clearly, it is easy to see what is wrong with it. First of all, it is based on a false premise. Universalists do not claim that science gives us privileged access to (or a 'God's eye view' of) truth or reality. They have emphasized the fallibility and revisability of its claims repeatedly (see Section 6.3.1). Of course, universalists do claim that science gives us more reliable knowledge about the natural world than magic, religion and indigenous belief systems, but scientific theories and methods are not in the possession of any

one group or culture. Thus, any culture can use them to gain fallible but reliable knowledge about natural phenomena.

Second, having universal truth in no way entitles anyone to silence, repress or dominate anyone else in the name of truth. In the past certain Western countries may have *exploited* the discourse of universal truth, reason and progress in order to colonize other people and degrade them, but this implicates the rulers, not the notions of truth, reason and progress. Scientific knowledge, like any other kind of knowledge, can be used for all sorts of purposes, some good, some bad, so it is absurd to blame the universality of knowledge rather than its exploiter.

Third, if there is no objective truth and no transcultural or common rationality, the oppressed are deprived of any means of criticizing the oppressor by appealing to it:

> Forsaking the ideal of truth would result in not subverting but further legitimising the authority of those already in power. Individuals or groups who would like to challenge established forms of authority would be deprived of the philosophical, logical arguments that would lend support to such challenge in the name of truth… In the realm of scientific or literary interpretation, forsaking objectivity and the ideal of correctness would remove all rational obstacles against putting knowledge into the service of dominant political and economic interests. (Irzik, S. 1990, p. 5).

Postmodernists and epistemic multiculturalists forget that objective truth is often the only "weapon" of the powerless against the powerful. A transcultural or shared rationality is the only alternative to brute power. If Lyotard's own slogan 'to speak is to fight' is to have any force against dominant ideology, there must be transcultural standards to which one can appeal for criticizing it. To deny their existence is to deny 'the force of the better argument'. Astonishingly, some postmodern science educators do just that:

> Lyotard clearly rejects Habermas' vision of an evolutionary social leap into a new type of rational society defined as the communication community that reaches consensus based on the better arguments. This, Lyotard argues, is the unacceptable remnant of a 'totalizing' philosophical tradition in which conformists are valorised and anti-conformists are 'terrorists' of the ideals of consensus. How is this idea best illustrated in a science classroom? *One way is by rejecting our persistent faith in the 'force of the better argument'* in that it leaves critically unquestioned the very context of argumentation which is always marked by the effects of status, power, and influence. (Zembylas 2000, p. 165; our emphasis.)

But denying the force of the better argument leaves dominant ideology or, worse, brute power as the only court of appeal. It is then too obvious who benefits from this!

Fourth, postmodernists and epistemic multiculturalists must also come to terms with the criticism that relativism with respect to truth and knowledge is simply untenable. Since we have argued against relativism in Section 4.3 at length, we will not discuss it further.[3]

Last but not least, postmodernists and epistemic multiculturalists are simply wrong to think that epistemic positions have intrinsically political

implications (see Kensur 1988; Irzik, S. 1990, pp. 5-7). It is just impossible to derive any political positions from purely epistemological ones. Rather, what happens is that the two often get allied in different ways in different contexts. The intertwining of epistemological and political views is contingent, not logical and can take a variety of forms, depending on the circumstances. Just as the discourse of universalism had liberating effects against tradition, superstition and religious dogma during the age of Enlightenment, but was also exploited by certain Western countries to colonize and degrade other people during the 19th and the early 20th centuries, relativism can also serve the status quo or reactionary politics as well as fostering tolerance and diversity. Indeed, in the last several decades what we witness in many parts of the world is that postmodernism and epistemic multiculturalism have provided the philosophical grounds for extremely conservative and reactionary politics in stark contrast to the intentions of its proponents. In the next section we present two cases to illustrate this alliance.

14.2 CASE STUDY 1: INDIA

In a series of eye-opening essays and books Meera Nanda (2002, 2003), an Indian biologist and a scholar of science studies, has shown how Hindu nationalists used the postmodern and multiculturalist celebration of 'difference', 'the marginal', 'the local' and 'the indigenous' for 'Hinduization of science' and for supporting a reactionary form of modernism. In 1998 India carried out her first nuclear bomb tests. Three years later the Indian Space Research Organization put a satellite into orbit successfully. The ideologues of Hindu nationalism, including the ruling Hindu nationalist government, immediately packaged these successes as a result, not of modern science and technology, but of their Hindu religion! They argued that sacred Hindu texts such as *Bhagavad Gita* already foretold the making of the nuclear bomb. The government started supporting projects to develop biological and chemical warfare based upon ancient religious texts. The University Grants Commission, which oversees the funding of higher education, declared that it plans to offer courses in Vedic astrology, Vedic mathematics, meditation, telepathy and other spiritual 'mind sciences'. Such traditional beliefs, Nanda writes, are quickly finding their way into school curricula under 'Vedic science', which uses its own "methods" such as meditation and intuition and is said to be superior to modern science because it is holist and organicist as opposed to being reductionist, materialist, and mechanistic.

As Nanda points out, such claims serve the ideological purpose of Hindu supremacy and the glory of Hindu nation (Nanda 2002, chapter 1). According to her, Hindu nationalists are systematically mythologizing modern science. By clothing modern science with the discourse of religious myth and

worldview, they rob scientific knowledge and inquiry of its potential critical power. As a result, the Hinduization of science will

> further entrench the holistic, organismic worldview as our national ethos, and a source of public morality. Moreover, absorbing science into myth and rituals –the hallmark of reactionary modernism– makes the defence of religion appear like a defence of reason and modernity and brings out the mobs in the streets who want to become modern without losing their traditional identities. (Nanda 2002, pp. 22-23)

What is interesting for our purposes is that Hindu nationalist ideologues have mobilized many of the postmodern and epistemic multiculturalist ideas to push their agenda summarized above. The main intellectual mechanism behind Hinduization of science, Nanda argues, is the relativist view that each culture has its own rationality, its own science and that modern science is a product of the culture of the West, which is no more valid than 'the local' and 'the marginalized' Vedic 'knowledge'. Thus, Hindu nationalists present Vedic 'knowledge' and Hindu ideology more generally as an 'alternative science' that would enable them to resist the imperialist hegemonic culture of the West and to 'decolonize' the minds of the people. Indeed, it would not be an exaggeration to say that Hindu nationalists were following Andrew Ross' advice in the previous section almost verbatim; they were developing 'different ways of doing science, ways that downgrade methodology, experiment, and manufacturing in favour of local environments, cultural values'.

Nanda observes that Hindu nationalists are 'obsessed with science, in the same way and for the same reasons as the "creation scientists" are obsessed with science' (Nanda 2003, p. xiv). That is, they want to show that modern science either supports, or already contains, the main metaphysical elements of their sacred texts. But this does not restrain them from criticising modern science as being reductionist, materialist, mechanistic, and dualist, much as postmodernists and epistemic multiculturalists do. They further claim that 'Vedic science' is, by contrast, non-reductionist, spiritualistic, organist and holist. Nanda argues forcefully that such a 'science' is anything but liberating for the masses because it serves for the legitimation of the existing *status quo*, perpetuating the caste system (*ibid*. Chapter 3).

Similarly, she points out that an uncritical affirmation of 'the local' and 'the marginal' is not always politically progressive:

> The problem, however, is that what appears as marginal from the point of view of the modern West, is not marginal at all in non-Western societies which have not yet experienced a significant secularization of their cultures. Local knowledges that Western critics assume to be standpoints of the "oppressed" are in fact, deeply embedded in the dominant religious/cultural idiom of non-Western societies. Using local knowledges to challenge Western science may, however dubiously, illuminate the blindspots of modern science in the West. But in non-Western societies themselves, such affirmation of the scientificity of local knowledges ends up affirming the power of the dominant cultural-religious institutions (*ibid*. p. 155).

In short, postmodernist and epistemic multiculturalist ideas provide a convenient ground for the agenda of the Hindu nationalism: the attack on the universalism of science, objective knowledge and truth; the celebration of relativism, 'indigenous science' and traditional beliefs; the view that modern science is 'Western' and as much local as any other belief system; and the idea that every culture has its own science—all contribute to the justification of a nationalist, anti-secular, and illiberal project of 'Hinduization of science'. This is hardly progressive and emancipatory politics.[4]

14.3 CASE STUDY 2: TURKEY

While Hindu nationalists are trying to absorb modern science and technology into their own mythologies and religion, Islamists in many parts of the world such as Pakistan, Malaysia, and Turkey are busy with 'Islamization of science and knowledge'. To see what this means, and how it relates to postmodern and multiculturalist ideas, we shall present the Turkish case in detail.

In the last two decades a number of Turkish Islamist intellectuals began criticizing modernity, especially secularism and industrialization (see Bulaç 1995a, 1995b, 2001; Özdenören 1995, and Özel 1992). They argued that modernity deprived humans of their spiritual/religious values and enslaved them to the greed for material wealth. Science and technology also received their share of sweeping criticism because they are seen as the driving motor behind industrialization and the ever-increasing rationalization of all aspects of social and economic life. Modern science and technology, the Islamist intellectuals argued, helped secularise modern life, but utterly failed to establish any moral order. With the age of Enlightenment science itself became a new religion, but one without ethics. They claimed that modernity, which is a Western and therefore a Christian phenomenon, destroys not only people's traditional cultures, lifestyles and characters, but also the environment. As Ali Bulaç, one of the most distinguished Moslem intellectuals boldly put it, 'Modernism, which promised a "paradise on Earth" turned the entire planet into hell… It is impossible to unify Religion and Modernism over a common denominator' (Bulaç 1995a, pp. 7-8).

These radical Islamist intellectuals situate science and technology as crucial elements within modernity; modernity provides both the content and the context for a certain conception of science and technology, which in turn serves to perpetuate modernity. A critique of the former, therefore, is *ipso facto* a critique of the latter. Conversely, we can say that radical intellectual Islamists' anti-science and anti-technology discourse is also directed against modernity's system of values that lie behind modern science and technology. It would be useful to document this 'value matrix' from the viewpoint of Islamist intellectuals. The elements of this matrix involve the following: a dualist, mechanist and reductionist philosophy of being; the constitution of

the human mind (reason in particular) as the source and foundation of both knowledge and morality; secularism; progressivism; rationalization; materialism; individualism; and capitalism. We summarize these below.

(a) *Dualism and mechanism*. As is well known, the rise of modern science in the sixteenth and seventeenth centuries is based on a new conception of being, namely, Cartesian dualism and mechanism. Cartesian dualism destroys the unity of being by declaring matter and mind to be two distinct and independent substances. While the essential property of matter is extension, that of mind is thinking. Since only human beings think, everything else – the world, stars, and even the human body and animals – is just extended matter. The entire universe is nothing but a huge machine subject to mechanical laws. According to Islamists, it is precisely such a conception of nature devoid of any divine meaning and order, which legitimises the scientific-technological control, domination and exploitation of nature and thus paves the way for ecological destruction (Kutluer 1985, pp. 70-75; Bulaç 1995a, pp. 18-21).

(b) *Reductionism*. Modern science is also reductionist in the sense that it aims to understand the structure and function of the whole in terms of the structure and function of its parts. Reductionism is allied to a version of materialism when it adopts the view, implicit in Cartesian philosophy, that all physical phenomena can be explained by the size, shape and motion of little particles out of which material objects are constituted. Reductivist materialism gives support to the view that only that which can be measured or quantified can be explained and understood (Bulaç 1995a, p. 15).

(c) *Worship of human reason, humanism and secularism*. According to modern thought, the only source and foundation of knowledge is human reason broadly construed. Thus, although modern rationalists and empiricists are divided over the question of whether it is sense experience or innate concepts and principles that provide the ultimate source and ground of knowledge, they agree that it cannot be anything other than the faculties of human mind. Reason replaces revelation, faith, custom and habit; and science, as the embodiment of reason par excellence, becomes the only activity capable of producing knowledge about nature, society, and the individual (Özdenören 1995, pp. 139-140; Özel 1992, p. 150). Similarly, the human mind and reason also become the sole source and ground of legal and political rights, moral values and principles. Religion no longer regulates the domain of law and moral conduct. Man becomes the measure and the ultimate end of man (Özel 1992, p. 80).

(d) *Instrumentalization of reason and rationalization*. Modern reason is identified with instrumental reason. In so far as rationality is attributable to human actions it is means-ends rationality, any action is rational so long as it achieves a pre-given goal in the most efficient manner possible (Bulaç 1995a, pp. 21-22 and 314). The instrumentalization of reason goes along with a

rationalization of the domains of a state's economy, its institutions (bureaucratisation), as well as its public and individual life. Efficiency becomes the regulating principle of virtually everything; accordingly, social sciences (especially, economics, sociology, and social psychology), which are the products of modernity, tend to be mere instruments for the efficient and productive organization of various social spheres and for rendering them more calculable (*loc. cit*.).

(e) *Progressivism*. The idea of social, economic, industrial, technological and scientific progress – all to the benefit of humanity – is one of the most important aspects of modernity (Bulaç 1995a, pp. 31-32; Kutluer 1985, pp. 28-29; Özel 1992, pp. 153-159). The narrative of progress as the emancipation of peoples serves conveniently to legitimate modernity as the latter is seen as an inevitable stage in the historical development of mankind. The idea of scientific-technological progress reinforces this narrative because, according to modernists, progress in other domains is made possible by the advances in science and technology; indeed, without them modern consumer society would simply stop existing.

(f) *Individualism and materialism*. The modern individual is an atomised, materialist, selfish, greedy, and alienated individual (Özdenören 1995, pp. 10-19; Özel 1992, pp. 68-81). Each has lost his or her unity and 'spiritual, intellectual, metaphysical and cosmic reality' (Bulaç 1995a, p. 27). For that reason, the modern individual's humanity is an impoverished one. According to Islamist intellectuals, it is no coincidence that while secular science and technology become more and more mechanistic, reductionist and instrumental, man becomes less and less human. For, modern science and technology have reduced man to a functional body that consists of a measurable, calculable swarm of atoms and genes.

(g) Finally, modern societies, Islamist intellectuals observe, are capitalist societies. Capitalism both nourishes and is at the same time nourished by secularism, instrumental reason, rationalization, materialism and individualism. To the extent to which Western science and technology serve these values, they serve capitalism (Bulaç 1995b, pp. 234-243).

This completes our documentation, in the Islamist intellectuals' writings, of the value matrix of modern science and technology against which their critique is directed. It is worth pointing out, however, that occasionally, Islamists' critique extends over the very products of technology and the content of science. For example, according to Ali Bulaç, people can live, and indeed have lived, for centuries without TV's, refrigerators, computers, detergents, artificial food, clothes, fertilizers, plastic, cement and asphalt. These products are not at all necessary for survival; they are promoted by Western powers in order to encourage consumerism in traditional societies

and thus make them dependent on Western economy and technology (Bulaç 1995a, pp. 39-42).

It is not our purpose here to evaluate the validity and accurateness of this sweeping critique of the value matrix of modernity. That critique is contained in Part I and II. Rather, our aim here is, first, to draw attention to the striking similarity between the Islamists' critique and that of the postmodernists and the epistemic multiculturalists, and, second, to show that such a critique of modernity leads, in the hands of radical intellectual Islamists, to a very conservative politics. Let us begin with the former.

Indeed, postmodernist thought provides a convenient source for the Islamists' critique of modernity. Writings of J. F. Lyotard, J. Baudrillard, G. Deleuze, F. Guattari, J. Derrida, and I. Illich are cited approvingly. For example, Bulaç (1995a, pp. 231-232) agrees with Lyotard that knowledge should not be identified with scientific knowledge and that scientific knowledge has become a commodity like everything else. Bulaç praises Lyotard for accurately describing the new (postmodern) nature of knowledge in the post-industrial societies in which knowledge and power have become one, and also for formulating the double problem of its legitimation: 'Who decides what knowledge is, and who knows what needs to be decided?'(Lyotard 1984, p. 9). Since science is seen as the greatest threat to religion, undermining the authority of science and the scientific worldview is of crucial importance for the radical intellectual Islamists.

Following Ivan Illich, Bulaç also claims that the entire system of modern education from elementary schools to universities is an effective agent of modernity and constantly reproduces existing social and economic relations together with the modern lifestyle. He concurs with Deleuze and Guattari that the state has besieged the individual's private life from all sides and penetrated deeply into the roots of his consciousness with the help of modern science. To resist this is to risk being schizophrenic (Bulaç 1995a, pp. 116-121). Again, one is struck by the affinity between this and postmodernist critique of contemporary education. (See, for example, Peters 1995, Kanpol and McLaren 1995, and Giroux *et. al*. 1996).

Although Islamist intellectuals are themselves no postmodernists, this does not prevent them from appealing to postmodern critique of science (see Bulaç 1995a, pp. 238-239). It is not difficult to see why: partly because such critique comes from within, and partly because they create a 'liberating' effect. Modern science can no longer claim universal validity. Mechanistic and reductionist explanations of natural phenomena are bad and should be replaced by holistic, spiritualistic and even mystical ones; modern technology is destructive of natural order and harmony of things; since secularism is a 'Western' phenomenon, it does not apply to other cultures; and so on. All this, of course, makes it possible to counterpose the notion of an Islamic science

against modern science and to present it as a new and culturally authentic alternative, just as postmodernists and epistemic multiculturalists recommend.

It is also worth pointing out that the philosophical affinity between postmodernists, epistemic multiculturalists and radical Islamists in Turkey is further strengthened through an intellectual alliance with the post-positivist philosophy of science, in particular, Thomas Kuhn's and Paul Feyerabend's works. They are widely, approvingly, but also selectively read, by postmodernists, epistemic multiculturalists and Islamist intellectuals for obvious reasons. With his colourful metaphors like gestalt switches, political conversions and puzzle solving, Kuhn is interpreted as destroying the universal conception of scientific rationality, progress and truth, and as narrowing the gap between science and non-science, and *a fortiori*, between science and religion. If it is not possible to speak of a linear progress in science, it should be even more impossible to talk of a social progress as envisaged by modern thought (Kutluer 1985, pp. 174-180). (But these Islamist intellectuals are totally silent about Kuhn's later clarifications and revisions of his views, which provide little support for their agenda.)

Similarly, Feyerabend's provocative attack on method and science resonates in radical Intellectual Islam with full force. Feyerabend (1987, 1988) argues that there is no such thing as *the* universal scientific method if by 'method' one means a set of fixed and supra-historical rules that are valid for and applicable to all kinds of problems, in every scientific context; every rule is context-bound and thus has its limits. Just as there is a plurality of 'methods' in science, Feyerabend claims, (Western) science itself is just one tradition among many, one ideology among others. It is in no way superior to such practices as religion, magic and astrology. Feyerabend believes that since in the past numerous cultures have survived for thousands of years without the help of science, it is outrageous to impose science upon them at the expense of their traditions. In his preface to the Turkish translation of *Against Method*, comparing Moslem intellectuals with the proponents of the Japanese Enlightenment who adopted not only the practice of science but also its ideology, he says 'Moslem radicals have a more reasonable attitude. They use the products of first world science but despise its ideology' (Feyerabend 1989, p. 12). Feyerabend's pluralistic relativism finds its most succinct expression in his claim: 'Every culture, every nation can build a science that fits its own particular needs' (*ibid*., p. 11; see also Feyerabend 1991, pp. 11-12). There are as many kinds of science as there are societies, and no objective grounds exist for establishing the cognitive superiority of one over another.

Feyerabend's philosophy claims to unmask the 'ideology' behind science by exposing its roots, i.e., the materialist Western civilization. Thereby it affirms the Islamists' conviction that science, as it is commonly practiced

today, is a hegemonic Western tradition and paves the way for the conception of an alternative science based on Islamic values:

> Who could ignore the contributions anarchist methodologists made to philosophy of science and epistemology, courageously displaying modern science's despotic, reductionist, mechanist and monist real face? (Bulaç 1995a, p. 142).

No wonder, then, that a Turkish translation of Feyerabend's *Science in a Free Society* was also published under the title 'The Church of Science' (Bilim Kilisesi) and received enthusiastic reviews like 'Scientific Despotism on Trial' in Islamist journals.[5]

In short, radical intellectual Islamists in Turkey use the post-positivist, postmodernist and epistemic multiculturalist critique to undermine modern science and the scientific worldview, and to open up space for an 'Islamic science'. By 'Islamic science' they mean a new science (yet to be invented) informed by Islamic values and principles. Here are the most important values and principles, and we advise postmodernists and epistemic multiculturalists to pay careful attention to them.

(1) *The Ontological Dimension*: God (Allah) is One, and he is the creator of everything. All being originates from a single *arche*, a single source, i.e., God. This is called the principle of unity (*tawheed*). Since the universe is the result of a divine creation, it is an organic whole that reflects God's unity. This means that every object, every event and process, in the universe points to a divine Order and Reality. Thus, the principle of unity extends far beyond the unity of God and incorporates both the unity of soul and body and the unity of man and nature.

(2) *The Epistemological Dimension*: The ultimate source of our knowledge of the universe is *Quran*, which consists of God's words, revealed directly to the Prophet Mohammed. Therefore, revelation is the highest form of knowledge. Scientific knowledge is not only inferior to revelation, but also subject to it in the sense that the aim of science is to reinforce man's belief in God and to help him become His servant by discovering the divine order. Understood in this way, the pursuit of knowledge (*el-ilm*) itself is a form of worship.

(3) *The Ethical Dimension*: From these ontological and epistemological considerations follow man's epistemic and ethical humility. Because the universe is a divine creation, its mysteries transcend human reason. Our knowledge, including scientific knowledge, then, is bound to be limited. We should not naively believe that scientific and technological developments will solve all of our problems. Similarly, man should not disturb the order of the universe as this would be "arrogance". Accordingly, science and technology must be restrained so as not to destroy the natural environment; they must serve the individual and the community in harmony with the natural order of things.[6]

Let us now ask postmodernists and epistemic multiculturalists: "How can a 'scientific' activity based on unquestionable authority be progressive and emancipatory? Would they seriously encourage the Islamists and others to pursue their project of building a novel science? Is such a project even remotely possible?" It is not just that such a task is Herculean. One does not even have the slightest idea how to begin. We have every reason to believe that such an attempt is futile and that it is a total waste of time and energy. Ironically, when the Islamists are busy with their impossible task, the scientific and technological gap between them and the West will widen even more, and all the while postmodernists and epistemic multiculturalists living in the developed Western countries will continue to enjoy the fruits of modern science and technology. One cannot help think that postmodernists and epistemic multiculturalists are conspiring against the underdeveloped, the oppressed, and the colonized!

Let us take some solace in the fact that such an idea of alternative science is not taken seriously by any Turkish governments or by people in general. It has remained as an unfulfilled dream of the Islamist intellectuals. In fairness to them, however, we should point out that some of them were sensible enough to express some doubts as to the realisability of their dreams. Kutluer, for example, cautioned his readers against hasty solutions which come under the label of 'Islamization of knowledge': 'We cannot now reach a final decision concerning the necessary preconditions for an Islamic model of science, because we are not sure yet whether such a model is possible or not' (Kutluer 1985, p. 186). In a similar vein, Mustafa Armağan, who had recently translated and edited a collection of articles on Islamic science by non-Turkish Moslem authors, asserted in his introductory essay that the notion of Islamic science represents a movement against modernity, but then asks 'will the Islamic wave be able to produce a genuine alternative?' (Armağan 1990, p. 21).

But, unfortunately, the idea of 'Islamic science' was taken very seriously in Pakistan so much so that it became a state-sponsored movement following the coup of 1977, which brought General Zia-ul-Haq to power. The military government introduced a number of changes in the education system. Some of these included the following: afternoon prayers during school hours, reading *Quran* as a matriculation requirement, the recognition of religious madrasah certificates as equivalent to MA degrees, giving an extra 20 points for memorizing *Quran* to students who are applying to engineering schools, requiring religious knowledge for science teachers, and, finally, revising standard school curricula to include Islamic values (Hoodbhoy 1991, p. 37). Not surprisingly, however, no Islamic science is yet in sight, and the effects of 'Islamization of science' on science and science education are, not surprisingly, disastrous (*ibid*.).

Earlier, we stated that in presenting the views of radical intellectual Islamists in Turkey our aim was two-fold. First, to draw attention to the parallel between the Islamists' critique of modernity, modern science and its universalist image and that of the postmodernists and the epistemic multiculturalists; and to point out how this relates to the idea of an 'Islamic science'. Second, to show that such a critique leads, in the hands of radical intellectual Islamists, to a very conservative politics. The two tasks are of course interrelated, and we have already suggested that even the very idea of an 'Islamic science' contains authoritarian elements. Now let us turn to the second task.

For the radical Islamists, the construction of 'Islamic science' is just one aspect of the grand project of establishing an Islamic societal culture. A societal culture is one which 'provides its members with meaningful ways of life across the full range of human activities, including social, educational, religious, recreational, and economic life, encompassing both public and private spheres' (Kymlicka 1996, p. 76). Virtually all of the intellectual Islamists emphasize that Islam is not just a system of faith that prepares the believer for another world, but it is at the same time a practical worldview that encompasses all – economic, social, political, cultural and personal – dimensions of life. Their aim, therefore, is a complete overhauling of society along Islamic principles.

According to radical intellectual Islamists, a truly Islamic society cannot be secular (see Bulaç 1995a , pp. 234-243; Özdenören 1995, pp. 15-17; Özel 1998, pp. 118-125). The reason is simple: all sovereignty belongs to God, not to people; and, Islam is also a way of governing the state and society, not just a doctrine of faith. These intellectuals argue that secularism is foreign to Islam; because secularism is no less a Western phenomenon than modern science and technology, it has an alienating effect on Islamic culture. To argue that countries are better off with secularism is an invalid generalization from the experience of the West; to require that all countries be governed by secular political regimes is a Eurocentric imposition that should be resisted.

Notice the identical form of the following two arguments:

(1) Modern science is a Western cultural phenomenon.
Therefore, it is not universally valid.
Therefore, every culture is free to develop its own science.

(2) Secularism is a Western cultural phenomenon.
Therefore, it is not universally valid.
Therefore, every culture is free to develop its own regime.

The first argument is that of postmodernists and epistemic multiculturalists; the second argument is that of radical intellectual Islamisists. The reader can see how easy it is to move from a viewpoint that uncritically celebrates culture in (1) to the argument of (2). If even science has only local validity, it

goes without saying that the same should also be true of secularism. After all, secularism is foreign to Islamic culture. Thus, the relativistic logic leads to the conclusion that if the first one is a good argument, the second one should be even better.

In the previous chapter we have seen what is wrong with the first argument. From the fact that modern science has originated in a certain part of the world in a particular period of history, it does not at all follow that its claims do not have universal validity. It is of course wrong for any culture to *impose* anything on another culture, so in that sense every culture is free to develop its own science of nature, but, as we seen, it is extremely unlikely that it would be much different from mainstream science. As for the second argument, we will be content to point out that the 'tolerance and diversity' its conclusion expresses is hardly progressive and liberating; indeed, to paraphrase Nanda, it is a gift, or rather a charity, of 'ethnopolitics' we would simply like to return to postmodernists and multiculturalists.[7] True, secularism has emerged in the West, but it is a gain for all peoples in the world – a gain that has been earned as a result of long and hard struggles.

In short, given the larger agenda of the radical intellectual Islamists' to establish an Islamic societal culture, it is perverse to think that an emphatically religious politics of science would be progressive and emancipatory, that the rejection of secularism can be the basis of a tolerant and pluralistic society. Postmodernists and epistemic multiculturalists should rethink what their philosophy is serving for in others' hands.

14.4 BETWEEN SCIENCE AND POLITICS

So far we have criticized the politicisation of conceptions of science, knowledge and reason in the hands of postmodernists and epistemic multiculturalists. But to deny that there is an intrinsic relationship between conceptions of science and political stances is not to say that there is no relationship between them at all, as the previous two sections amply demonstrate. Nor is it to say that political, economic, and personal factors have no influence on science. This is a huge topic to which we cannot do justice in this book, and there are many excellent studies that investigated the influence of such factors on science.[8] What we would like to do in this section is to clarify what it means for *science* to come under social, political, and economic influence and to outline, in a general fashion, various ways in which science is or can be so influenced. Thus, our focus is conceptual rather than empirical, indicating the direction social studies of science should take to achieve their goal of emancipatory and progressive politics better.

The locution 'social, political and economic factors influencing science' is too vague. As we noted in Chapter 6, the term 'science' could mean a system of thought and activity, but it could also mean an institution. In order to have

a firm grasp on what it means for social, political and economic factors to influence science, these two senses must be distinguished analytically from each other. Science as an institution comes under such influence a lot more than science as thought and activity. Let us start with the latter.

In Section 6.6 we gave a working definition of science in terms of six components: activity, aims, product, method, M-rule, and attitude. Thus, when we talk about the influence of political and economic factors on science, we are talking about the ways in which these components are influenced. Clearly, not all of these are open to such influence, and those that are admit varying degrees. Consider first the aims that are intrinsic to science: truth, knowledge, testability, explanation, prediction, and so on. We have been at pains throughout this book to show why especially truth and knowledge (both as aims and products) by their very nature are not the sort of things that can be influenced by political and economic factors. The same point applies to other intrinsic aims, M-rules (i.e., methodological rules like 'avoid making *ad-hoc* assumptions' – see Section 6.6) and scientific methods such as naïve inductivism, hypothetico-deductivism, and Bayesianism discussed in Chapters 7 to 9. By contrast, certain aspects of scientific activity and attitudes are indeed vulnerable to social, political and economic influence. More specifically, the direction of scientific research (for example which problems are pursued more rigorously than others), does get strongly influenced by considerations of profit and political power. There is strong evidence that such considerations also tend to distort openness, collaboration and free critical inquiry, thereby undermining the ethos of science.

It is also worth noting that social, political and economic factors exert their influence on the direction of research and the ethos of science typically through the institution of science, that is, through the governing bodies of universities, research centres and the like. For it is these bodies that receive enormous amounts of funds both from public and private sources, sign special agreements with the industry, and allocate them to the relevant entities like departments and research groups. Similarly, political influence exerts itself on the institution of science often indirectly through science policy by such mechanisms as prioritizing certain research projects and areas rather than others.

Recent sociological studies on the institution of science, which are not plagued by the postmodernist, social constructivist and epistemic multiculturalist hubris, do an excellent job of showing where the real problems today lie. Works by such scholars as Greenberg (2001), Kenny (1986), Krimsky (2003) and Bok (2003) clearly indicate that in the last two decades or so universities are being encouraged to establish new forms of cooperation with the industry. As a result, they argue, research agendas are increasingly being shaped by commercial and corporate interests rather than

by scientific value or public good, that unprecedented conflicts of interest arise because many scientists, while serving for business firms in various capacities at the same time hold their positions as professors in their universities, and that all this seriously damages the ethos of science.

Needless to say, this is not all there is to this issue. The impact of various social, political and economic factors on science is an immensely complex matter to which, as we just saw, entire books have been devoted. Here we are content to underline the general point that a social critique of science of the kind mentioned above is not only more sound, but is also more substantial and thereby serves the purpose of a just society much better than an obscure and unfounded critique of science typically given by postmodernists and epistemic multiculturalists.

Moreover, sweeping claims that implicate science as a whole – science is ideological, masculine, elitist, exploitative, and violent (recall the quotation by Aikenhead at the beginning of this chapter or similar charges by Harding, Ross and others in Chapter 13) – without distinguishing between science as an institution and science as thought and activity, between scientific knowledge, methods, instruments, technological products *and the uses they are put to by governments and corporations* has several grave consequences. First, it muddles crucial issues summarized above. Second, it creates an unnecessary polarization between the practitioners of social studies of science and the scientists (think of the recent 'science wars'), thereby obstructing a meaningful dialogue in addressing these issues. Finally, it gives such a distorted image of scientific activity and thought that it de-motivates students to study it; why would anyone study something that is 'ideological, masculine, exploitative and violent'? Such sweeping criticism undermines the very purpose of science education. In short, the politicisation of science in the hands of postmodernists and their allies is not only fruitless, but damaging as well.

Some of this damage has been carried over into, and perpetuated within, science education. Hopefully with the groundwork of Parts I and II, there will emerge greater clarity about the issues discussed in Part III and IV that will free science education from the negative influences of some of our current intellectual movements.

NOTES

[1] To be more precise, the post-positivist turn effected by Kuhn, Lakatos, Feyerabend, Hanson and Toulmin *as it is perceived by* the community of educators must also be noted in this context, but this requires a separate study of its own.

[2] Although feminism was also very influential in this respect, we have deliberately left aside any discussion of it in our book for reasons of scope. For a critique of feminist standpoint epistemology informed by constructivist and postmodernist ideas, see Koertge (1998) and Pinnick, Koertge and Almeder (2003).

[3] For a thorough discussion of relativism, see Siegel (1987).

[4] That the postmodernists and multiculturalists did not intend this is irrelevant. Their views do give support to such undesired results despite their intentions to the contrary.

[5] See *Bilim Kilisesi-Özgür Bir Toplumda Bilim*, Pınar Yayınevi, İstanbul, 1991; M. Bilici, 'Scientific Despotism on Trial', *Matbuat* Vol. 8-9, Dec. 1994-Jan. 1995, p. 35.

[6] For these ontological, epistemological, and ethical considerations see Bulaç (1995a, pp. 311, 314, 322-326 and 1995b, pp. 266-274, 306-309). A similar conception can also be found in non-Turkish Moslem authors; see, for example, Sardar (1984).

[7] The idea that the doctrines of postmodernists and their ilk is a piece of epistemic charity that ought to be returned to the givers, perhaps without thanks, is a theme of Nanda 2003, especially Chapter 5. On the contrary, emancipation requires more thought, reason and rationality of the sort we can all universally exercise.

[8] See Greenberg (2001), Kenny (1986), Krimsky (2003), and Merton (1973).

EPILOGUE

In this book we undertook both a constructive and a critical task. The constructive task was to give a positive theory of education and apply it to the teaching of science. Constructing a theory of education required both a conceptual-analytical and an epistemological inquiry. We had to analyse and distinguish between different senses of learning and knowing, inquire into the relationship between them and sort out a normative account from its empirical counterpart.

The critical task was to scrutinize the most fashionable currents of thought that impinged on science education. These included constructivism, postmodernism and multiculturalism. None of these currents come in only one version, so we took up radical constructivism and social constructivism separately. Similarly, we distinguished on the one hand between multicultural education and multiculturalism, and on the other hand, between multiculturalism as a politics of recognition and as an epistemic view. Postmodernism as well comes in either in Lyotardian or deconstructive version, though it is the former that is the most dominant in education. We tried to see the connections among them if there were any, criticized each and salvaged whatever is valuable in them.

Our conclusions, both positive and negative, can be summarized succinctly as follows:

Positive conclusions:

- Normatively speaking, the core aim of education is critical inquiry. There is no teaching or learning worthy of its name, which is not based on norms of critical inquiry.
- A theory of education which places critical inquiry at its centre cannot be constructed without a proper epistemology.
- In our view such an epistemology should distinguish between knowledge and mere belief, adopt a realist conception of truth and a rationalist-universalist (or rationalist-transcultural) approach to justification.
- Science, understood as a system of thought and activity, is critical inquiry *par excellence*.
- It exemplifies critical inquiry through its various aspects: its aims such as testability, its ultimate product knowledge or rational belief, its methods such as hypothetico-deductivism or Bayesianism, its methodological rules and, finally, its ethos.
- Scientific methodology is alive, well and thriving despite the rumors to the contrary. It is the critical core of science.

- There is nothing wrong with a realist, rationalist, and universalist theory of knowledge and science. It survives the anti-realist, scepticist, dogmatist, and relativist attacks.
- Model building *via* abstraction and idealization is an important part of scientific activity. Despite the fact that models are human constructions, they give us reliable and objective knowledge about the world.
- There is a model of teaching and learning, which is perfectly compatible with such a conception of science. This is the Socratic model. The Socratic model is critical inquiry applied to teaching and learning.

Negative conclusions:

- Despite being the most popular epistemological and pedagogical theory in science education, radical constructivism is neither novel nor tenable. As a theory of knowledge, it is a crude form of empiricism and instrumentalism familiar to the philosophers, who have abandoned it long ago. As a pedagogical theory, it contains both truths and falsities and is no contender to the Socratic model. For the Socratic model captures its truths and avoids its falsities.
- Other fashionable currents such as postmodernism and epistemic multiculturalism, both of which produced a sizable literature in education and a considerable literature in science education, have nothing to offer as a theory of knowledge and as a theory of science. They both suffer from the disease of relativism and both have lost sight of critical inquiry.
- As a pedagogical theory, both have similar, but disastrous consequences. They should be kept outside of the science classroom.

There is a deep irony lurking behind much constructivist, postmodernist and multiculturalist thinking. Their proponents often endorse their views for a more critical education, for a more democratic and progressive politics, but they end up serving *status quo*, conservative, and even reactionary politics. Unreflective and unreflexive criticism, that is, criticism unguided by norms of rationality and method that are adopted *after* long and arduous critical inquiry that turns also upon itself, can be perverse and self-defeating. Such is the fate of these fashionable currents.

Fashions can be tolerated and even enjoyable in attire. But intellectual fashions are useless and, moreover, can be damaging if their only virtue is newness and difference to dazzle the mind without a concern for truth and to merely attract attention. We believe that the most popular intellectual movements of our times taken up in this book are of this sort. And glamorous

but empty novelty, both in jargon and substance, certainly seems to be a sign of our "postmodern" era. In this postmodern theorists like Lyotard were right. But, alas, it is no more than a phyrric victory.

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