Chapter 2. Science and Education : Notions of Reality, Theory, and Model[*]

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Introduction

It was argued in Chapter 1 that science education and technology education should both be as 'authentic' as possible and that modelling and models, for which a typology was proposed, can form a bridge between the two. However, modelling and models must be seen within a broader context, that of the relationship between notions of 'reality', 'theory' and 'model', for two reasons. First, science education, which aspires to be authentic, must be based on an historically and philosophically valid view of the nature of science, in which these three notions play important parts. Second, it can be argued that perhaps, to some extent and in some way, the development of ideas by an individual parallels (or can be seen as a metaphor for) the development of ideas in science. The treatment of the reality/theory/model relationship given in this Chapter, which is of importance in its own right, is set within the second of these two reasons because it subsumes the first.

Science, Science Education, and Constructivism

In the last twenty years or so, a very large body of research data has been accumulated into the nature of students' understanding of specific elements of the content and processes with which science is concerned (Pfund and Duit, 1988). This output, known as 'alternative conceptions' or 'alternative frameworks' or 'naive understandings' or 'children's science', may be summarised as follows:

1. From an early age, and prior to any formal teaching and learning of science, children develop both meanings for many words used in science teaching and views of the world;

2. Children's ideas are often strongly held and significantly different from the current views of scientists;

3. Children make sense of many new experiences by constructing meanings based on their existing ideas;

4. Students retain, modify, or change, their existing understandings when they are taught, as well as acquiring ideas.

That spectrum of ideas known as 'constructivism', broadly definable as 'using existing ideas to construct meaning from new experiences whilst using acquired experience for producing new ideas', has

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achieved wide support as being the best available explanatory psychological framework within which to set these conclusions. It has also been seen by many as providing the best approximation available to the conditions for teaching and learning needed to achieve 'authenticity' (Tobin,Tippins,Gallard, 1994). Inevitably, the constructivist 'movement' has acquired its critics, the strongest of whom is Matthews (1994, 1998). As he puts it:

> "For many, constructivism has ceased being just a learning theory, or even an educational theory, but rather it constitutes a worldview or *Weltanschuung* ..…constructivism is committed to certain epistemological positions that are very contentious and, given the widespread educational influence of the doctrine, deserve close scrutiny." (Matthews, 1994, p.139)

Osborne (1996) has analysed the achievements, strengths, and weaknesses, of constructivism, with an emphasis on the epistemological assumptions which underpin the pedagogical actions that are most commonly taken by teachers in 'the constructivist classroom'. He points out that an identification of the difficulties that students experience in learning has led to practical measures to help them become more aware of their own understandings, to the rejection of the *tabula rasa* assumption in teaching and to improved teacher skills of formative assessment. However, he suggests that constructivism misrepresents the nature of science, by failing to accept the notion of realism which underpins scientific practice, and confuses the contexts of knowledge making and knowledge learning. The consequences of these weaknesses are, in his view, that it offers no guidance on how students might adjudicate between competing theories, offers no guidance to teachers on the selection and sequencing of content in curriculum, and rejects didacticism as an approach to teaching. The major criticism he levels against the 'radical constructivist' perspective is based on its alleged vehement rejection of realism. The criticism he levels against the 'social constructivist' perspective is the failure of schools to create the social context for such scientific learning to take place, whilst he sees 'personal construct psychology' to have failed to produce testable predictions about learning.

Just the existence of these criticisms suggests that those who wish to base the teaching of science on constructivist principles must do so with approaches which are both well-founded epistemologically and well-developed pedagogically. The assertion underlying this book is that modelling and models should and can have a major impact on the curriculum, teaching, and learning, of science in schools in the movement for authenticity. As this impact is likely to made within constructivist assumptions, it follows that there must be the greatest possible ontological and epistemological clarity over what might be done, why, and how. A number of key questions must in addressed in an attempt to obtain such clarity. Is 'realist' an acceptable epistemological and ontological basis for science education? What meanings and

roles have modelling and models within realism? How do notions of model and theory, which are intertwined within most discussions of science and science education, relate to each other within an acceptable realist assumption? How can modelling and models contribute to a well-developed pedagogy for science education? We start with the notion of realism.

Notions of the world-as-experienced

The first key element of a realist view is the assertion that the world-as-experienced actually exists independently of humanity, being composed of entities of a fixed nature. Ogborn (1995), concerned with the relation between science and science education, believes that it is the only sure foundation on which science, and hence authentic science education, can be based:

> " ---knowledge in the natural sciences is made by human beings, is never - because nothing
>
> could ever be - absolutely certain, and yet provides solidly reliable accounts of the material
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> world, upon which we can certainly act." (p.6)

The second key element of a realist view is the assertion that science can gradually approach a complete knowledge of reality. This has been explored by Bhaskar (1978). He argues that, although knowledge is produced by the application of a social product (scientific methodology) leading to the modification of other antecedent social products (theories and models), a realist view requires assumption of two 'dimensions' of understanding and two kinds of 'objects' of knowledge:

a) The 'intransitive dimension' in which the object is the real structure or mechanism that exists and acts quite independent of human beings and the conditions which allow them access to it;

b) The 'transitive dimension' of ideas about the nature of the entities of which the world is thought to exist. These are produced, communicated, and changed, are historically situated and contingent, and are thus a human achievement.

Using this terminology, practising scientists can be said to adopt a policy of viewing the ideas within the transitive dimension as provisionally real (intransitive), such that suitably informed individuals anywhere can use them to act upon the world (Harré, 1986). Action in the form of experiments, using these ideas as tools for enquiry, is needed to test the validity of the assumption of intransivity. If this such action is always successful and the ideas [do not] infer facts which are not yet found in the world then they gradually come to be viewed provisionally as true, as factual, as permanently part of the assumed intransitive dimension. However, if the world actually exists independently of what we know of it, such ideas always remain fallible and open to modification, even to refutation.

Every type of entity of which the world-as-experienced is intransitively composed, in the realist view, has a distinctive nature, can do only specific things, and can only have certain things done to it. An entity shows all of its range of behaviour in 'open systems' (those unaltered by human action), although systematic, scientific, enquiry usually involves the construction of limited, hence artificial, conditions, or 'closed systems', to prevent other entities intruding into the behaviour of those on which enquiry is focused (Bhaskar, 1978). The gap between the nature of a closed system in which a candidate entity is explored and an open system in which it usually exists in the natural world places an inherent limitation on a readiness to accept it as 'real'. The most complete acceptance of an entity as real, as intransitive, comes about when it is very successful in providing explanations of open systems.

There is, as one would expect, an anti-realist view of the world-as-experienced. This asserts that it is never possible to conclude that the world-as-experienced is actually composed of particular types of entities if they are not directly observable. Such entities must, on this argument, remain entirely the products of the human imagination. For some anti-realists of positivist inclination, theories are just useful summaries of data, which are collected by experiment, and better theories incorporate greater quantities of more accurate data.

Which is the more suitable basis for science education: realism or anti-realism? As has already been said, practising scientists tend to adopt a provisionally realist stance: so it is, perhaps, up to anti-realists to justify their views. They do so by attacking realism. How is this done and to what effect? Ogborn (1995) argues that anti-realists start from the assumption that science is conducted in accordance with fixed rules of rationally using a fixed empirical methodology based on a realist (intransitive) view of entities. Anti-realists, according to Ogborn (1995), then show that the actual practice of science deviates from this representation of it. They then conclude *both* that scientific knowledge is socially constructed (because scientific methodology is not context-independent) *and* that the assumption of realism is unfounded. The anti-realist argument thus denies the differentiation between the intransitive and the transitive dimensions, believing the world-as-experienced to be entirely transitive. The weakness in the argument, for Ogborn(1995), is that, although science-as-practised is demonstrably not conducted on strictly rationalist lines and by the mechanical application of an algorithmically-applied empiricist methodology, it cannot be inferred from this that reality does not exist. As the assumptions of the anti-realists are, for Ogborn (1995), false, so must be the anti-realist view itself. What emerges from these arguments is that it would be unsafe to base an authentic science education on an anti-realist view of science.

On what interpretation of the realist view should science education then be based? Should a 'strong' interpretation be used, where it is assumed both that the world-as-experienced exists and that

science can gradually approach a complete understanding of it? Or should a 'weak' interpretation be used, where only the realism of the world-as-experienced is assumed, with the question of whether science can progressively get closer to an understanding of it being left to one side. Both would allow for the assumption of the transitive dimension.

In the next three sections we set out the ideas on the nature of 'theory' and 'model' expounded by Thomas Kuhn, Nancy Nersessian, and Mario Bunge. These three are all realists, but they differ in the way that their philosophical positions would seem to relate to the 'strong' and 'weak' interpretations of realism . Kuhn, we will argue, takes a 'weak' view, Bunge a 'strong' view, with Nersessian, who is mainly concerned with processes of model change in science, being capable of supporting either view. In the last section of this Chapter, we look at the implications of the 'weak' and 'strong' views of realism for Osborne's(1996) criticisms of constructivism. Finally, we suggest an approach to modelling and models which could be the basis for an authentic approach to science education within a constructivist perspective.

Thomas Kuhn on reality, theory, and model.

Kuhn (1970a) was concerned with the representation of change at the macro level in science. He introduced the notion of 'paradigm', in which the set of problems to be addressed in a field of enquiry, the theories and models adopted, the experimental techniques used, the criteria applied in the evaluation of results obtained, are fixed. Work in a new field of scientific enquiry shows no clear agreement between participating scientists on such matters: it is 'pre-paradigmatic'. This is then followed by a period of 'normal science', in which the operating paradigm seems to be agreed by scientists and can be identified by an observer. Dissatisfaction with some aspect of the explanations produced during a normal science period leads to a chaotic 'revolutionary science' period, as new problems, theories and models, methodologies etc, are tried out. This settles down into a different paradigm in a new period of normal science.

Kuhn on the nature of theories

In his work, Kuhn says very little about theories as such. His representation of science does not include an explicitly developed theory of theories (Giere 1988, p.35-36), a remark that can also be made about his treatment of models (Abrantes 1998). The reason for this is that he was concerned with the processes by which scientific knowledge changes at the macro level, rather than with the logical structure of the detailed products of research (Kuhn 1970b, p.1). In 'Postscript-1969', a section added in the second edition (Kuhn 1970a, p. 174-210) of his most famous book, he points out the main source of confusion which arose from his original treatment of his ideas (Kuhn, 1962). It is that the concept of paradigm was

used in Kuhn(1962) both in a general and in a restricted sense. In the general sense, 'paradigm' was employed to mean the entire constellation of group commitments shared by the members of a scientific community. It was to denote this meaning more clearly that he later suggested the expression 'disciplinary matrix' in Kuhn (1970a):

> "Scientists themselves would say that they share a theory or a set of theories, and I shall be glad if the term can be ultimately recaptured for this use. As currently used in philosophy of science, however, 'theory' connotes a structure far more limited in nature and scope than the one required here. Until the term can be freed from its current implications, it would avoid confusion to adopt another. For present purposes I suggest 'disciplinary matrix': 'disciplinary' because it refers to common possession of the practitioners of a particular discipline; 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification." (p.182).

The main components of the disciplinary matrix were identified by Kuhn (1970a) as being: symbolic generalisations (expressions, either in mathematical or verbal form, deployed without question or dissent by group members), models, shared values, and the exemplars (concrete solutions to problems, that serve as models for the solution to similar problems). The last component represented the restricted sense in which the word paradigm was originally used by Kuhn (1962), and one which he considered to be of the uttermost importance both for the education and practice of members of a scientific community.

Kuhn on the nature of models

Two distinct senses in which the notion of model is used can be found in Kuhn's treatment of the notion of 'disciplinary matrix'. One sense has to do with the role played by the 'exemplars'. This is concerned with the processes of learning to become a scientist and later of actually doing science as an independent scientist. In both cases what is involved are problem-solving activities modelled on solutions already accepted within the paradigm:

> "As the student proceeds from his freshman course to and through his doctoral dissertation, the problems assigned to him become more complex and less completely precedented. But they continue to be closely modelled on previous achievements as are the problems that normally occupy him during his subsequently independent career." (Kuhn 1970a, p. 47).

These exemplary problem solutions were regarded by Kuhn as one of the essential vehicles for learning the cognitive content of a theory, which he saw as consisting, among other things, of verbal and symbolic generalisations together with examples of their function in use (Kuhn 1977a, p.501). For him, 'normal science' research is mostly guided by a direct modelling of these exemplary problem solutions, as opposed to the application of abstracted rules (Kuhn 1970a, p.47).

The second sense in which the notion of model is used is concerned with beliefs in particular types of models. In the 'Postscript-1969' (Kuhn 1970a), he refers to a spectrum of types ranging from 'ontological' to 'heuristic' models. Ontological models were regarded by Kuhn as objects of metaphysical commitment, deeply held by scientists, about what actually exists in the universe and about what their main features are, perhaps corresponding to Bhaskar's(1978) intransitive dimension. In this category Kuhn (1970a) included beliefs such as "heat is a constituent property of bodies" and "all perceptible phenomena are due to qualitatively neutral atoms in a vacuum, or alternatively, to the interaction of matter and force, or to fields".

Heuristic models were seen as analogies, which enable one object of study to be fruitfully considered as if it was another, which is known to be completely different in nature, perhaps corresponding to Bhaskar's(1978) transitive dimension. Scientists are not committed to them in any permanent way as objects of belief and they are viewed and used in pragmatically and instrumentally. Examples of this variety that he gives are "an electric circuit may be regarded as a steady-state hydrodynamic system" and "a gas behaves like tiny elastic billiard balls in random motion".

In spite of the difference in commitment to the two varieties of models by scientists, in 'Postcript-1969' (Kuhn 1970a) stressed the similar functions that they serve for a group, a community, of scientists :

"Though the strength of such commitments varies, with non-trivial consequences, along the spectrum from heuristic to ontological models, all models have similar functions. Among other things they supply the group with preferred or permissible analogies and metaphors. By doing so they help to determine what will be accepted as an explanation and as a puzzle-solution; conversely, they assist in the determination of the roster of unsolved puzzles and in the evaluation of the importance of each." (Kuhn 1970a, p.184).

Models, for Kuhn, perform these functions by virtue of being a source of similarity relations which can be either external (between essentially different objects and situations) as in the case of heuristic models, or internal (between objects and situations essentially of the same type) as in the case of ontological models (Hoyningen-Huene, 1993).

The status of heuristic models is quite clear in Kuhn's work. They are used to demonstrate a formal similarity between laws and theories in different domains (Abrantes 1998), so that those from one can help to explain or to investigate the other. The case is not so clear, however, when ontological models are considered. Kuhn used expressions like "metaphysical models" (Kuhn 1970c, p. 271), "objects of metaphysical commitment" (Kuhn, 1977b, p.463) and "ontological models" (Kuhn 1970a, p.184) interchangeably. In doing so, he introduced a confusion, by equating 'ontological' models with 'metaphysical' models. Since ontology refers to assertions about the nature of reality and metaphysics

does not, what did Kuhn means by 'metaphysical'? His work does not (at our reading) include a definition, so we must assume that he was using 'metaphysical' as a synonym for 'philosophical', as opposed to 'scientific' in the realist sense of the latter term. This interpretation is supported by his statement:

> "And as the problems change, so, often does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play." (Kuhn 1970a, p.103)

Kuhn on the nature of reality

Why did he refer to scientists' commitments to ontological models as being 'metaphysical'? This can be answered by considering Kuhn's views of the relation between theory and reality as given in 'Postscript-1969' (Kuhn, 1970a). For him a theory was better than its predecessors only in the sense of being a better instrument for discovering and solving puzzles. He was not a 'strong' realist, in the sense defined earlier in this Chapter, despite realism being the prevalent perspective on nature adopted by both philosophers of science and lay people at the time of his major publication (1962):

> "There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its 'real' counterpart in nature now seems to me illusive in principle. Besides, as a historian, I am impressed with the implausibility of the view. I do not doubt, for example, that Newton's mechanics improves on Aristotle's and that Einstein's improves on Newton's as instruments for problem solving. But I can see in their succession no coherent direction of ontological development. On the contrary, in some important respects, though by no means in all, Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's. Though the temptation to describe that position as relativistic is understandable, the description seems to me wrong. Conversely, if the position be relativism, I cannot see that the relativist loses anything needed to account for the nature and the development of the sciences." (Kuhn 1970a, p.206).

In not accepting an ontological approximation to reality in the historical development of theories, it was only natural for Kuhn to consider the beliefs held by scientists about what exists in nature as metaphysical. He thus saw no harm in broadening the usage of the word 'model' to include objects of belief such as atoms, fields, or forces acting at a distance (Kuhn 1977b, p.463, Note 9). For him, assertions about what exists in nature were to be seen as being a model of it and never as a claim about what is really there. For him, a scientist may fully believe that there is a match between theoretical entities and their real counterparts, but those ontological beliefs are, at the end of the day, no closer to reality than the electric circuit is to the steady-state hydrodynamic system, or a molecule is to a billiard ball.

On our analysis then, Kuhn was a 'weak' realist. He was prepared to accept that the world-of-experience actually existed, if only for the sake of argument. His use of 'model' to refer to the 'model solutions to problems within a paradigm' is relevant, as we shall see later, to the concerns of this Chapter and needed to be clarified. However, his recognition of heuristic models as helpful analogies and the his ambivalence over the status of ontological models suggests that, whilst he was willing to accept what Bhaskar (1978) subsequently called the transitive dimension, he was not willing to accept that the progress of science shifts entities in the transitive dimension to the intransitive dimension. Scientific change is not necessarily moving closer to an understanding of the intransitive, but rather to a different presentation of the transitive, made for different purposes.

Nancy Nersessian on reality, theory, model

Nersessian (1992a) emphasises the importance of overcoming the traditional separation of the analysis of the context of discovery from that of the context of justification in philosophy of science if we are to address the status of Bhaskar's(1978) intransitive/transitive distinction. She pointed out that Kuhn did not fully clarify the discovery/justification issue within his notion of 'paradigms of science'. In her own words (Nersessian1992a, p.7) says that:

> "[Kuhn] identifies conceptual change as the 'last act' (in paradigm change), when 'the pieces fall together'. Thus portrayed, conceptual change appears to be something that happens to scientists, rather than the outcome of an extended period of construction by scientists. A change of 'gestalt' may be an apt way of characterising this last point in the process, but focusing exclusively on this last point has - contrary to Kuhn's aim - provided a misleading portrayal of conceptual change; has reinforced the widespread view that the processes of change are mysterious and unanalyzable; and has blocked the very possibility of investigating how precisely the new gestalt is related to its predecessors".

In order to grasp the process of conceptual change in science, Nersessian (1992a) developed a system of cognitive-historical analysis, which sought to investigate the context of development in which

> "A vague speculation gets articulated into a new scientific theory, gets communicated to other scientists, and comes to replace existing representations of a domain" (op. cit., p 6).

In the cognitive- historical perspective, developing scientific theories is a problem solving process that consists of modelling activities which involve generating new conceptual representations from existing ones (Nersessian 1992a, p.12). The modelling capabilities of the mind are exercised through a set of abstracting techniques, which include imagistic reasoning, analogical reasoning (See Chapter 5) thought experiments (see Chapter 8) and limiting case analysis.

In her 1992 (a) study Nersessian analyses the specific mechanisms by which scientific theories are developed. Her thorough examination of Maxwell's studies of the electromagnetic field, based on Faraday's ideas, shows how the use of analogical and imagistic reasoning supported Maxwell's development of a new theory. Her sketch analysis of Galileo's and Einstein's studies focuses on thought experiments and limiting case analysis as tools for modelling new theories. In the case of Galileo, for instance, she analyses the establishment of the law of falling bodies by means of considering the fall of a body in a medium and exploring the consequences of reifying the medium down to the limit of a vacuum. In her analysis, Nersessian emphasised some features of Galileo's approach. These are: (i) the assumption, for the first time, of an Archimedian model for framing the issue of falling bodies, an approach which was already then used in hydrostatics; (ii) modelling the consequences of the initial assumptions via thought experiments and limiting case analysis, thus redeveloping for the context of falling bodies some features which were already known in the context of hydrostatics.

An important feature of Nersessian's work on modelling is her use of theoretical tools e.g. abstraction techniques, which both deal with the particularities of the specific scientific domain under investigation and also which represent some degree of generalisability in approach. In other words, she bridged the gap between context independent and context dependent approaches accounts of the processes of knowledge building.

By comparing the work of Kuhn and Nersessian, it is possible to establish two distinct but complementary patterns for the relationship between theories and models. According to Kuhn, models are a constitutive component of an already established disciplinary matrix. Such models, which are constrained by the intransitive nature of the phenomena studied within an existing paradigm, offer analogies on the basis of which the phenomena might be conceived within that paradigm. Complementarily, Nersessian points out that models are important as a starting point for the development of theories. She also stresses that modelling activities are carried out by scientists using abstracting techniques. The creation of new ideas leads the scientific enterprise to results that go beyond the model which was the starting point.

Nersessian says nothing which leads to the conclusion that she doubts the existence of a human-independent reality. However, she seems ambivalent over the intransitive/transitive issue. She has clarified the nature, status, and mode of operation, of models as a key element in the transitive domain. However, her position is not so clear on whether or not the change between paradigms, produced by modelling in the transitive dimension, does or does not lead scientists nearer to a complete understanding

of reality. Her position could be said to be capable of supporting both a 'strong' and a 'weak' view of realism.

Mario Bunge on reality, theory, and model

As we have seen, Kuhn was primarily concerned with periods of 'normal science', in which theories and models play a stable but ill-defined yet mutually supportive role, interspersed with periods of 'revolutionary science'. As we have also seen, Nersessian is concerned both with the role that models have within paradigms and in the formation of new paradigms. Bunge is concerned with the relationship between theory and model at any time in the development of a field of scientific enquiry. He views the development of theoretical knowledge as the main purpose of science . Thus:

> "To convert concrete things into richer and deeper conceptual images and to expand them
> into progressively complex theoretical models, increasingly faithful to the facts, is the only
> effective method of apprehending reality by thought." (Bunge, 1974,p.12)

He is a realist (Cupani, 1991) who both accepts the existence of the intransitive dimension and sees science as capable of providing, eventually, a full understanding it. This, as we shall see, involves him in using modelling and models within the transitive dimension **and being the way to reach reality**. These views qualify him as a 'strong' realist.

His scheme of analysis has three components:

Generic Theories. These are abstractions produced by reason and intuition, which are potentially capable of applying to any part of reality.

Model-Objects. These represent the common properties of a group of real objects.

Theoretical Models (otherwise called *Specific Theories*). These, what in this book we would call 'models' (whether expressed, scientific, or historical-see Chapter 1), are produced by applying a generic theory to a model object, interpreting the latter in terms of the former.

He summarises the relationship as follows:

> "When suppositions and special data referring to a particular body (a model-object) are
> associated with classical mechanics and classical gravitation theory (generic theories), a
> specific theory is produced (a theoretical model) about that body. In this way we have Lunar
> theories, theories about Mars, theories about Venus, and so on." (Bunge, 1973,p.54)

The Table below contains the outlines of a number of Bunge's actual examples (Bunge, 1973,p.53). Thus:

SYSTEM	MODEL OBJECT	THEORETICAL MODEL	GENERIC THEORY

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Moon	Spherical solid rotating about its axis, in rotation about a fixed point, etc..	Lunar Theory	Classical Mechanics & gravitation theory
Moonlight	Plane polarized electromagnetic wave	Maxwell equations for the void	Classical electromagnetism
Piece of ice	Linear casual chain of beads	Statistical mechanics of casual chains	Statistical mechanics
Crystal	Grid plus cloud of electrons	Bloch's Theory	Quantum Mechanics

He has most to say about model-objects and theoretical models.

A model-object is an idealisation, a generalised object produced by the simplification of a number of real objects so as to emphasise their commonalties. It is an arbitrary idealisation, being the product of what Bunge (1974 [,p.16]) refers to as 'fictional materialism', which must be evaluated in terms of 'fitness for purpose' rather then in terms of being right or wrong. Such objects are treated temporarily as if they were the reality from which they were abstracted- Harre's (1986) 'policy realism'. Doing so enables scientists to focus on specific aspects of a complex reality. Bunge (1977) believes it to be unimportant if the model-object is constructed by the use of analogy: the issue is the quality of the insight gained when it is combined with a generic theory. For example, in the early years of the study of heat and electricity, model-objects based on the idea of an 'incompressible fluid', derived by analogy from the well-developed science of fluid mechanics, enabled considerable progress to be made in those fields of enquiry.

A theoretical model (in this book, a 'model'-whether expressed, scientific, or hstorical) occupies a scientifically vital intermediary position between a model-object, which being an idealised empirical object cannot yield knowledge by the direct application of logic, and a generic theory, which being entirely the product of imagination cannot be directly applied to reality. A theoretical model includes a representation of the properties and behaviour of the model-object and of the entities of which it is constructed. This enables the application of hypothetico-deductive reasoning to produce predictions, which can be subsequently tested. The main attribute of any theoretical model is that it can represent a domain of reality. Indeed, for Bunge (1974[,p.22]) it can simulate the real, thus enabling the internal mechanisms which support the relationships between the entities of which it is thought to be composed to be defined. He differentiates between theoretical models, in which internal mechanisms are postulated

and 'black box' models where they are not. These mechanisms, within a realistic perspective, are not accessible to perception, but are merely inferred. He calls these 'hidden mechanisms'

> "A hypothesis of hidden mechanisms can only be considered as confirmed if it satisfies the following conditions: to explain observed operations; to foresee new facts other than the ones foreseen by black-box models; and to be compatible with known laws."(Bunge, 1974,p.22)

The significance of a generic theory is evaluated by considering its success, when used to interpret a model-object so as to yield a theoretical model, in leading to predictions which are empirically confirmed (Bunge,1974 **[,p.19]**).

The relationship between these three ideas can be shown through an example given in the Table included above. The phenomenon which is called *moonlight* was simplified and abstracted into the model-object of *a plane-polarised electromagnetic wave* and interpreted through the generic theory of *classical electromagnetism* to yield the theoretical model known as *Maxwell's Equations*. The latter enable predictions to be made and tested e.g. the effect of a polarise on the brightness of moonlight, the effect on the plane of polarisation of a magnetic field. Confirmation of the anticipated outcomes validated the worth of the model-object, the theoretical model, and most importantly of all, the generic model. Bunge's contribution, within the concerns of this book, has been to show the role of models in forging a link between reality-as-perceived and reality-as-idealised. He is a 'strong' realist, subscribing not only to the notion of reality but also to the view that science can, in due course, provide a full understanding of that reality.

Nature of science, individuals's learning, and constructivism

The Nature of Science

Authentic science education must be based, as far as possible, on an acceptable view of the nature of science i.e. one which is received as being reasonably valid by historians and philosophers of science and also by practising scientists. So how do the chosen three philosophers stand up to this test? Such a decision is especially important if it is decided both to base the science curriculum on the view of science presented by one of them and to make students overtly aware of that basis.

When the ideas of Kuhn first appeared in 1962, they were a radical alternative to the logical positivist approach which had dominated thinking for many years beforehand. In his own terms, they represented a new paradigm and fostered extensive work to exploit its potential. Inevitably, with its application to the history of specific fields, the cracks began to appear in the system. It seemed that change was not so coherent as Kuhn's system suggested. Moreover, as Nersessian (1992) points out, the context of the justification of scientific knowledge was treated adequately by Kuhn but the context of the

discovery of scientific knowledge was not, although the notion of 'normal science' does provide a framework within which the two can reside. Kuhn's scheme was succeeded by those proposed by Lakatos (1974) (see Chapter 11) and by Laudan (1977). Nevertheless, although the explanation for change in science given by Kuhn is rather course-grained, it could be a valuable first step into the field of philosophy of science at school level. Kuhn's idea of 'exemplars' as 'models *of* problems' in science is a novel way of looking at the idea of models, although his treatment of models *in* science is neither well-developed nor clearly. distinguished from that of theories, they being conflated together under the heading of 'disciplinary matrix'.

The ideas of Nersessian (1984, 1987, 1989, 1992 a,b) are too recent to have been explored to the same extent as those of Kuhn. Her own application of the cognitive-historical approach to the ideas of Faraday, Maxwell, Lorentz, and Einstein, have provided valuable insights into the 'context of discovery' of scientific knowledge. However, she pays little attention to the context of justification. A practising scientist, upon reading her case studies, should come to the conclusion that they are a valid reconstruction of scientific discovery/invention. Able students should be able to follow the cognitive processes involved and all students will empathise with the demonstration of scientific thinking as an example of human creativity. However, the approach is limited to cases where detailed documentary evidence is available. Although she does not seem (at our reading) to take position on the issue of 'realism', she does propose that models are used in the development of new theories.

The work of Bunge (1973,1974,1977) is very helpful in that it deals with the relationship between the notions of 'model' and 'theory' in some detail. The scheme would seem to be applicable to scientific enquiry at any stage in the process of change, from the situation (in Kuhn's terms) of 'normal science' and 'revolutionary science'. With suitable examples, it should be intelligible to students.

The nature of science and of learning by an individual

The question to be addressed here is: to what extent are the work of the three philosophers discussed above an adequate basis on which to view the learning of science by an individual? This is a complex question, to which only a preliminary treatment can be given in the space available.

There is no general agreement about the existence of a relationship between change in science and change in the cognition of an individual: see Schwitzgebel (1999) and Gilbert(1999) for recent discussion of the issues. Researchers have taken widely differing positions on the matter. Piaget and Garcia (1989) saw the processes to be identical, with the mechanisms of equilibration, assimilation and accomodation being at the heart of both. Whilst Nersessian (1992a) sees a strong analogy between the two, other researchers have just pointed out the parallels between them whilst maintaining that the social

psychological circumstances of science and of science education are very different (e.g. McClelland 1984, Lythcott 1985).

Ten years after the initial publication of their ideas (Posner, Strike, Hewson, Gertzog, 1982) in which they developed a strong analogy between Thomas Kuhn's representation change in science and change in the cognition of an individual, Strike and Posner (1992) revised those ideas. In the intervening years, numerous studies had taken place to test their basic premises: that conceptual change should take place if an individual is dissatisfied with a current conception and if an alternative is both intelligible, plausible, and fruitful. Although the results of those studies had been very mixed, Strike and Posner (1992) maintained the credibility of their scheme, only seeing the need to pay closer attention to the learner's 'conceptual ecology'. The pattern of results may be taken to indicate that the original analogy is not, in fact, a strong one. However, it may just reflect the weaknesses in Kuhn's scheme, not least the relative ambiguity of the roles of 'model' and 'theory' in it. It may be that indicators of having changed a conception bear little relationship to the processes involved in undertaking that change.

If this last point is of any merit, then it might point to greater success if Nersessian's ideas are used as the basis of the analogy. She is herself cautiously optimistic about the possibilities:

> "Conceptual change as it has occurred in the history of science provides a valuable resource
> for gaining an understanding of the general issues of restructuring and, in some cases, may
> even aid the formation of hypotheses about the dimensions along which to probe students
> representations." (Nersessian, 1989, p.164)

Although little work seems to have been done to test these possibilities, the clear and central role for models in her scheme leads us to expect success, if only because model formation and use is a key element in the development of understanding (Johnson-Laird, 1983). Similar arguments also apply to Bunge's scheme, although it has not yet been even suggested that it be used as the basis for an analogy to an individual's cognition.

The nature of science and an acceptable version of constructivism

One of the central questions addressed in this Chapter has been: to what extent are the views of the nature of science of the three philosophers, particularly in respect of the reality/theory/model relationship, an epistemologically and ontologically adequate basis for an acceptable pedagogy based on constructivist principles? Providing a direct answer might infer support for the fairly common practice of using these principles as a template for the design and conduct of classroom teaching and learning. On the basis of evidence of a lack of widespread success of such a use, Tobin and Tippins (1993) cast doubt on the wisdom of such an approach. Nevertheless, the key issues of adequacy remain, for Tobin and Tippins (1993) see a well-founded view of constructivism as a valuable critical referent against which to evaluate

a wide variety of classroom practices, a variety far wider than that normally encompassed within 'the constructivist classroom'.

Whether used for planning classroom activity or as a critical referential scheme, constructivism in science education must be responsive to Osborne's (1996) five issues:

• the need for a basis in realism.

All three philosophers accept the first element of a realist view: the world-as-experienced exists independently of humanity. However, the differ over the second element: whether or not science can eventually provide a full understand of the world-as-experienced. As we have shown, Kuhn may be termed a 'soft' realist, accepting the first element but not the second. Bunge is a 'hard' realist, accepting both elements, whilst Nersessian seems somewhat ambivalent on the issue. If one wanted to portray science as a way leading to ultimate truths about the world, as do many senior academic scientists, then one would base

curricula on Bunge's ideas: more limited ambitions might be met within a Kuhnian framework.

There is one set of issues being more consciously addressed in nations which are alert to the ethnic diversity within themselves. These issues concern potential tension or even conflict between the cultural base of formal science, which may be termed 'White, Western. and Male', and that of other communities. Whether viewed as matters of cultural hegemony (Cobern 1998), or more pragmatically as the problems that ethnic minorities have in believing the conclusions of WWM science (Aikenhead 1996), the question of 'whose reality?' arises. A sensitivity to diverse 'voices', amongst which must be those of women of all ethnic groupings, is called for (Gilbert, Boulter,Rutherford, 1998b). Nersessian's approach, with its emphasis on a recognition for how individuals think, seems important in this context.

• the need for an effective treatment of the contexts of discovery and of justification;

Kuhn deals well with the context of justification (the operation of normal science) , but is not so successful with the context of discovery (seen it as an undifferentiated element in an inchoate period of 'revolutionary science'). Bunge is also effective in dealing with justification (the production of 'theoretical models' from theories prior to experimental testing) but less successful over discovery (he has apparently little to say about how new theories are produced). Nersessian, on the other hand, is very convincing over the context of discovery, but does not address the context of justification, seems to address both effectively.

• the provision of guidelines for theory adjudication;

Kuhn gives a clear indication of where theory adjudication has taken place, at least in the case where it is failing/has failed, heralding a period of revolutionary science. However, he gives little treatment of how this takes place psychologically, concentrating instead on its sociological manifestation. Bunge is the

opposite: he shows how theoretical models are produced and tested as a theory is to be evaluated, but says little of the sociology involved. Nersessian is effectively silent on the subject.

- the provision of clear guidelines for the selection and sequencing of content;

A consideration of the ideas of all three philosophers suggests that students might be introduced to the evolving theories and models in a given area of enquiry in the order of their historical sequence. However, if this is to be done, then close attention must be paid to providing a historically valid representation i.e. one in which the circumstances of change, the manner of change, and the consequences of change, are discussed not only from the vantage point of the present day but, much more importantly, also as these processes were seen as they actually took place in the past. Nersessian has a lot of invaluable detailed methodology to contribute to this approach, perhaps viewed (at least to a first approximation) within the framework provided by Kuhn.The treatment of this theme through the medium of Bunge's ideas has apparently not yet taken place.

- the placing of a suitable value on didactic approaches to teaching;

This is only an issue if constructivism is seen as the direct basis for classroom activity. If, as is suggested by Tobin and Tippins (1993), it is seen as the basis for the critical review of pedagogic practice, then the issue is not significant and didactic approaches to teaching can have their place e.g in defining the curriculum and in the teaching of ideas which students are unlikely to have come across in everyday life

Looking back over the discussion of the above issues identified by Osborne (1996), it does seem that all three philosophers have something to contribute to several of those issues. More might have been said if closer consideration had already been given to the educational implications of the more recent philosophers i.e. Nersessian, Bunge. It may be tempting to educationalists to pick individual aspects of the models of science presented by several philosophers and to combine them into a model constructed especially for pedagogic purposes. As has been shown elsewhere (Justi, this volume; Justi and Gilbert, 1998a), that whilst such hybrid models can be useful in solving particular educational problems, they are not open to rational (as opposed to expedient) replacement as they have no origin and hence no status in the philosophy of science. To close this section, we would observe that the assertion by Osborne(1996) and others (e.g. Matthews,1994, 1998) that 'radical constructivism' rejects realism, has been refuted (Hardy and Taylor, 1997). It does seem to be true that, within the current dispensation for the teaching of science throughout the world, conditions for the true social construction of knowledge are rarely, if ever met. Lastly, although personal construct psychology is sufficiently precisely formulated as to permit the construction of testable hypotheses, relatively little such work has taken place in science education.

Models, theories, and constructivist pedagogy

In this last section we will attempt to draw together what has been said and to focus it on the central concern of this book: models.

The role of models in realism

Models are important in respect of the second element of realism: whether or not science is capable of eventually providing a full understanding of the world-as-experienced. If, like Bunge, one adopts the 'strong' view of realism, then models can acquire one of two statuses. Those that are believed to fully represent the world-as-experienced become incorporated in Bhaskar's(1978) intransitive dimension: they are thought to be the truth. However, refutation is still possible and the fact that a model has gone unchallenged may just be due to inertia on the part of the science community. One has only to look at the apparently unassailable position of the 'inert gas configuration model' in the late 1950s and that of the 'only two allotropes of carbon model' until the mid 1990s to appreciate that 'the hubris of models ' is always possible. The other, much more common, status of models is as part of Bhaskar's(1978) transitive dimension. They are overly constructed by analogy, initially for some specific purpose, and survive in active scientific enquiry just so long as they are useful. Thereafter they become 'historical models' (see Chapter 1) and are condemned to be used only for routine enquiries and to that graveyard of all science, the school (and university?) curriculum. In the 'weak' view of realism, all models forever remain part of the transitive dimension.

Models and theories

It would have been nice to have produced a definitive relationship between theories and models, but we have not. After all, philosophers have kept this ball in play for some hundreds of years: fame has eluded us (for the moment). What we have done is to bring the theory/model relation in the work of our chosen three philosophers to the fore. Kuhn, as we have seen, has little to say in detail about either. They are lumped together in the notion of 'paradigm'. 'Theory' is left undefined within 'disciplinary matrix'. Ontological models have an indetirminate status. Heuristic models, however, are more clearly perceived: they are the pragmatic analogs which form the body of Bhaskar's transitive dimension. Nersessian sees the formation of models by analogy as a key element in the formation of theories: the fog between the two is somewhat dispelled. Bunge offers a route from phenomena to (in his terms) theoretical models through the construction of 'model objects'. However, this allows us to see how theories may be applied to phenomena through the medium of theoretical ical models, but has little to say about how theories themselves are constructed. The general view of the relationship between a theory and a model may be summarised in the following way: a model is a readily perceptible entity by means of which the abstractions of a theory may be brought ot bear on some aspect of the world-as-experienced in an attempt to understand it.

Models and science education

Chapter 1 outlined the range of entities of which models may be composed (objects, ideas, systems, events,processes), a taxonomy of ontological status for models (mental, expressed, scientific, historical, curricular, teaching, hybrid), the range of modes in which (other than mental) models may be represented (concrete, verbal, visual, mathematical, gestural). It also showed that modelling and models can contribute to the three main purposes for the study of science according to Hodson(1993) i.e. in learning science, in learning to do science, in learning about science. In this, the final sub-section of Chapter 2, we would like to sketch some of the implications of these ideas for a epstemologically/ontologically defendable view of constructivism if it is used as a critical referent in the design of the science curriculum and in classroom teaching and learning.

Whether or not one bases science education on the ideas of a philosopher with a 'strong' or a 'weak' view of realism, it may be pragmatic to adopt a 'policy weak realism' (*pace* Harre) basis for constructivism. Science will be assumed to not yet have reached ultimate truth in the areas being studied. This adoption will have attractions in terms of two of the three purposes for studying science.

Consider first the case of 'learning science'. Such a policy will give the individual scientific and historical models to be studied a higher status than may be thought justified by present-day scientists. However, it will have a motivational effect on students. Individual models, and perhaps more importantly a sequence of models in a field of enquiry, can then studied in terms of their strengths and weaknesses, rather than as 'failed attempts to perceive the truth'. Representing existing mental models in a variety of modes, one of the most readily defendable tenets of 'the constructivist classroom', followed by the use of the outcomes to explain known facts, will provide a route to an understanding of specific historical or scientific models.

In the case of 'learning to do science', such a policy would encourage the creation of curriculum requirements and classroom conditions in which phenomena could be framed, questions for research identified, models constructed, predictions made, experiments conducted, and the outcomes evaluated. In respect of this 'sequence', collaborative working would facilitate the social construction of knowledge. The effects of the 'weak realism' policy would be to make the students efforts seem more worthwhile, for it could not be said that 'the truth was already known' a view demotivating of genuine enquiry.

Lastly, the case of 'learning about science'. This is the most demanding for both teachers and students of the three purposes for science education. One successful approach might be based on the use of case

studied on the conduct of science written within frameworks provided by a range of philosophers, both 'srong' and 'weak' realists.

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