The role of analogy, model and metaphor in science

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THE ROLE OF ANALOGY, MODEL AND METAPHOR IN SCIENCE

by

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Summary

The central topic of this thesis is a critical consideration of the role of metaphor in science and the metaphorical status of science, preceded by a discussion of analogy and the role of models in science.

A distinction is first made between 'imported analogy' and 'manifest analogy'. It is argued that it is 'manifest analogy' which is usually discussed in the philosophy of science whereas it is 'imported analogy' which really plays a more critical role in science, and it is 'analogical acts' rather than arguments by analogy which lead to scientific progress. It is analogical acts which are the main factor in scientific intuition and creativity, and which issue in the use of models and metaphors in science.

The main claims for models are then analysed in terms of explanation, meaning, ontology, insight, logic, prediction, deployment and simplicity, and a summary is given of the modellist position.

A preliminary consideration is then given to metaphor with a view to dissociating it from purely literary associations, and discussing its cognitive, ontological and semantic status and its relation to the literal.

A variety of current views of the role of metaphor in science and of the metaphorical status of science are then described in some detail. In the following chapter the main difficulties of and objections to metaphorical views of science are
considered. Ways of overcoming or countering these difficulties and objections are then canvassed and a summarizing case for the metaphorical view is presented on the grounds that: (i) it explains the prevalence of certain scientific terminology, especially in scientific theory, (ii) it gives a satisfactory account of scientific discovery and progress, (iii) it provides a synoptic view of, and at least some answers to, a number of problems about the relation between theory and observation, induction, explanation, meaning variance, and so on, in the philosophy of science, (iv) it accords well with the facts of scientific history, (v) it explains scientific ability, (vi) it is pragmatically commendable, (vii) it promises to be capable of development into a more satisfactory 'logic' of scientific discovery than has previously been put forward.

In a final chapter, the relation of ideas about metaphor and philosophy of language to the emergence of modern concepts of science in the seventeenth century is discussed in detail in order to establish why, in general, metaphorical views of science have not been usual in the modern period, and to show that scientific practice has been at variance with scientific attitudes on this matter.
O Aristotle! If you had had the advantage of being 'the freshest modern', instead of the greatest ancient, would you not have mingled your praise of metaphorical speech, as a sign of high intelligence, with a lamentation that intelligence so rarely shows itself in speech without metaphor — that we can so seldom declare what a thing is, except by saying it is something else?

George Eliot — *The Mill on the Floss*

But knowledge that is delivered to others as a thread to be spun on ought to be insinuated (if it were possible) in the same method wherein it were originally invented....Yet certainly it is possible for a man in greater or less degree to revisit his own knowledge, and trace over again the footsteps both of his cognition and consent; and by that means to transplant it into another mind just as it grew in his own.

Francis Bacon — *De Augmentis Scientiarum*
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Introduction

The main purpose of this thesis is to consider the relation of metaphor to science and to discuss the view that science is in some sense metaphorical or characteristically employs metaphors. This involves (1) an examination of various claims about metaphor in relation to science, (2) an attempt to evaluate these claims, mainly in the light of a detailed analysis of the nature of metaphor, (3) a consideration of some major difficulties for the claim that science is metaphorical, (4) a discussion of the main implications of a metaphorical view of science.

Since analogy, model and metaphor are intimately related concepts in relation to science it is necessary to give some consideration to analogy and models as an essential preliminary to discussing views about metaphor, and this consideration has needed to be sufficiently detailed to warrant regarding the thesis as being about analogy and model as much as about metaphor. For this reason the title mentions all three topics. Nevertheless the central focus of the thesis, to which the rest is contributory, is a consideration of metaphor in science. It is for this reason that the analysis of the concept of metaphor is attempted in a more detailed and complete way than is usual within the confines of writings on the history and philosophy of science, and I have drawn heavily on discussions of the nature of metaphor within a literary context and on general discussions, which usually have a strong bias towards examination of the literary use.

There are several reasons why I have thought it worthwhile to devote a thesis to this topic. First, the metaphorical view of science, which is of fairly recent origin, seems original and interesting enough to merit a more detailed consideration than it has hitherto received. Second, there are in fact a variety of views about metaphor in science, which need to be sorted out.

*The footnotes are given together, by chapters, at the end of the thesis.
Third, the existence of a body of sophisticated analysis of metaphor of a general or literary kind makes an attempt to relate this analysis to a discussion of metaphor in science possible and worthwhile. Fourth, the general implications of a metaphorical view of science are important and need examining. And fifth, the metaphorical view of science is one which goes some way to bridging the gap between science and the arts, and deserves critical consideration on that account.

Some indication of my general plan of procedure has already been made, but it remains to make this a little more explicit. First, I shall examine the concept of analogy in relation to science, since I believe that an understanding of analogy is fundamental to any understanding of the role of metaphor. I shall go on to consider the concept and role of model in science. It will then be possible to give an exposition of the main claims about metaphor in science, arising as they do out of opinions about the functions of analogy and model in science. Then follows a detailed analysis of the concept of metaphor and an evaluation of the claims about metaphor in the light of this analysis. This evaluation will naturally lead on to a consideration of difficulties for the metaphorical view. In conclusion, I shall try to justify careful consideration of the metaphorical view by showing that it has some important implications for our understanding of the history and philosophy of science.
Chapter 1

Both the concept of metaphor and the concept of model include within their sense the concept of analogy. As far back as Aristotle one form of metaphor is described as 'giving the thing a name that belongs to something else...on the grounds of analogy'. More generally a modern description of metaphor says: 'In metaphor the mind sees and expresses an analogy'. Analogy and metaphor are frequently coupled together as integrated topics. Metaphor and analogy are even sometimes used as if they were synonymous terms. Pepper, for example, speaks of 'basic analogy or root metaphor'. Any discussion of the cognitive aspects of metaphor usually refers explicitly to analogy, and Hester distinguishes cognitive from emotive theories on the ground that in the former metaphors are regarded as basically analogical.

There is a similar close connection between model and analogy. Both the title and content of Hesse's book Models and Analogies in Science indicate such a connection. In general much of the literature on models would agree in its purport with the succinct statement of Hutten: 'The model works by means of analogy'. Even where analogies are distinguished from models, for example by Duhem, the distinction is not made on the ground that the model does not exhibit an analogy. In Duhem's case, he regards the model as being based on an analogy of a crude or limited kind.

It is implicit in much of the writing on metaphor and model that analogy is a more fundamental and simple concept than metaphor or model. One talks, as above, of metaphor 'expressing an analogy' or being 'grounded on analogy'. Similarly, a model works 'by analogy' or exhibits an an analogy with what it is a model of, or for.

The word 'analogy' and the concept it names were put into general use by the Greeks, notably Plato and Aristotle, and it is perhaps not surprising that 'analogy' has been used in a variety of ways and contexts in the period since then. One confusion in its usage which tends to persist is in the use of 'analogy' to signify
both a relation and the things related by that relation. Thus, we may say that a planetary system has an analogy (i.e. the relation of analogy) to the (Bohr) atom or that the planetary system is an analogy of the (Bohr) atom. The word 'analogue' is fortunately in common usage as a synonym for the latter sense of 'analogy', so I shall reserve 'analogy' to signify the relation, unless citing or commenting on other people's writings which use the word to mean the thing related. A further ambiguity about 'analogy' is that it is sometimes used in the sense of 'likeness with difference' (of any degree or kind) and sometimes with the narrower sense of 'likeness or resemblance of relations'. In Webster's Collegiate Dictionary the ordinary definition is given as:

resemblance in some particulars between things otherwise unlike: SIMILARITY.

On the other hand, Jevons says:

It has been said, indeed, that analogy denotes a resemblance not between things, but between the relations of things.

While Mill contends that:

It is on the whole more usual, however, to extend the name of analogical evidence to arguments from any sort of resemblance provided they do not amount to a complete induction: without peculiarly distinguishing resemblance of relations.

As both kinds of analogy enter into scientific thinking it seems best to employ 'analogy' in both senses, while bearing in mind that analogy in the sense of 'resemblance of relations', or, if it is preferred, the special case of 'resemblance of relations', is undoubtedly the more common and important kind of analogy for science of any degree of sophistication.

If we consider the kinds of analogy that are possible, apart from analogy of relation, there seem to be just two. First, there is the case of general unanalysed or even unanalysable resemblance between two things. Unanalysable resemblance does not enter much
into scientific thinking, for, as Dorolle says, 'it supplies nothing....to the intellectual process (for one thinks only by comparison and analysis)'. On the other hand hitherto unanalysed, but nevertheless potentially analysable, resemblance may well be fundamental to science either in its early stages or else as part of an original insight at more sophisticated levels. For example, we can see how the intuitive perception of general unanalysed resemblances and 'affinities' enters into classification by use of the so-called 'natural method' in Botany in the eighteenth century in the following passage from Guyenot:

These classes of plants did not result from a methodical analysis of characters and did not rest on any precise definitions. Nevertheless, guided by a remarkable intuition, instinctively seizing upon general resemblances, men like Lobel and above all Bauhin prepared, in grouping plants by their affinities, for the establishment of natural classification at the end of the eighteenth century. It is remarkable that the recognition of those natural groups which we call families preceded the precise study of species.

The second sense which might be attached to 'analogy', apart from 'analogy of relation' is 'resemblance in an ensemble of qualities, or of properties or attributes (not relations) given in immediate sense experience'. In practice, however, it is difficult to draw the line between qualities and relations. Colours, tastes, sounds and tactile sensations seem to qualify unambiguously as qualities, but already with such properties as size and shape one seems to be entering into considerations of spatial relation, and it is a moot point whether even the most elementary organization of our experience takes place on the basis of analogies between ensembles of (non-relational) properties without any admixture of relations among those properties.
However, if we interpret 'properties' in a more liberal sense to include such attributes as size, shape, state of motion and number, etc., and in general such properties (including relations) as might be attributed to things in ordinary perception by any adult in command of the ordinary language of description (and recognizing that there is likely to be some disagreement in marginal cases about the limits of 'ordinary' language and 'ordinary' perception) then we may differentiate between analogy based on such properties and analogy based on the formulation of novel or more esoteric relations between such properties. I shall use the terms manifest analogy to signify analogy based upon properties given in immediate sense experience or in ordinary perception and imported analogy to signify analogy based upon more abstract or esoteric relations.

This distinction is, I believe, more important than at first appears. Much of the discussion of analogy in relation to logic and induction is concerned with manifest analogy, which is more readily reducible to a formal symbolism than imported analogy. There has also been a tendency in the British empiricist tradition to concentrate on manifest analogy either to the exclusion of imported analogy or with a resulting confusion of the two kinds and an assimilation of imported analogy to an analysis and discussion more appropriate to manifest analogy.

This concentration on manifest analogy originates with Bacon and is, I believe, the root of much that vitiates Bacon's scientific methodology and the derivative methodologies of Herschel and Mill.

Several commentators have drawn attention to the fact that Bacon's scientific method consists of a systematic tabulation or analysis. The most detailed and well-known account of his method is given by Bacon in the Novum Organum.

Bacon clearly recognizes the tendency of the mind to proceed
by means of analogies. He says:

The human understanding is moved by those things most which strike and enter the mind simultaneously and suddenly and so fill the imagination; and then it feigns and supposes all things to be somehow, though it cannot see how, similar to those things by which it is surrounded. He recognizes the danger of an uncritical and unmethodical use of analogy:

The human understanding is of its own nature prone to suppose the existence of more regularity in the world than it finds. And though there be many things in nature which are singular and unmatched, yet it devises further parallels and conjugates and relatives which do not exist....

Elsewhere he uses the term 'analogy' synonymously with 'parallels' and speaks of instances of analogy as representing 'the resemblances and conjugations of things' so that it is clear that, while he does not specifically mention analogy, he is speaking of analogy in this passage. On the other hand, he does not regard the use of analogy as wrong. On the contrary he regards it as essential to scientific discovery:

Men's labour therefore should be turned to the investigation and observation of the resemblances and analogies of things, as well in wholes as in parts. For these it is which detect the unity of nature, and lay a foundation for the constitution of the sciences.

To avoid the pitfalls of overhasty generalization and the assumption of 'more regularity in the world than it finds' the human understanding must exhaustively list and compare 'instances' (analogous phenomena) in order to discover the invariable concomitances which will reveal the true 'forms' or 'nature-engendering' 'natures'. It is not necessary for us to
undertake the difficult task of elucidating what Bacon means by 'forms' or 'natures' in order to understand his treatment of analogy, and the fundamentals of his method. This method is succinctly stated in the following passage:

But the induction which is to be available for the discovery and demonstration of sciences and arts must analyse nature by proper rejections and exclusions, and then after a sufficient number of negatives come to a conclusion on the affirmative instances, which has not yet been done or even attempted, save only by Plato, who does indeed employ this form of induction to a certain extent for the purposes of discussing definitions and ideas.26

A more detailed account of how this is to be done is given in the second book of the Novum Organum where Bacon advocates a comprehensive survey (or 'presentation to the understanding') of all analogous examples, or 'instances' as he calls them, of a particular phenomenon under investigation. This is done in a positive way by the table of essence and presence ('all known instances which agree in the same nature, though in substances the most unlike')27 and the table of degrees or table of comparison ('in which the nature under inquiry is found in different degrees, more or less'), but there is also an important negative table - the table of deviation or of absence in proximity ('instances in which the given nature is wanting' and which are 'most akin to the others in which it is present and forthcoming').29

Although Bacon sometimes speaks of 'laws' as synonymous with forms, it is also implicit in his whole discussion that the 'forms of simple natures' which he regards as the goal of scientific discovery are describable in terms of qualities and properties found in ordinary experience and describable in the ordinary language of description. His conception of these simple natures has been well described as a 'qualitative atomism'.31
The important point is that Bacon does not seek for a causal explanation in terms of an extraneous mechanism or relation, nor in terms of mathematical relations, but rather by taking some of the instances themselves as the source of explanation, by a process of qualitative analysis. One might say that such 'instances' are in a sense 'privileged' in terms of the insight they give into the 'forms of simple natures'. He recognizes that 'no one successfully investigates the nature of a thing in the thing itself; the enquiry must be enlarged so as to become general, but this is not because the explanation is extraneous to the phenomena, for he says elsewhere, in an amplified version of the same claim:

Whereas it is most unskilful to investigate the nature of anything in the thing itself, seeing that the same nature which appears in some things to be latent and hidden is in others manifest and palpable.

Bacon's intention can be clearly made out in the one detailed example which he gives of the use of his method: the investigation of the nature of heat. Here he uses some of the instances of heat phenomena as a guide to interpret the others. He uses those instances where heat is associated with rapid motion and this supplies a demonstration of the true form of heat, viz. 'heat is a motion expansive, restrained and acting in its strife upon the smaller parts of bodies'. He prefaces his analysis of the form of heat with the following remarks, which indicate that, in practice, he is dealing with properties manifest in the instances:

It is to be observed that the form of a thing is to be found (as plainly appears from what has been said) in each and all of the instances in which the thing itself is to be found; otherwise it would not be the form. It follows therefore there can be no contradictory instance.
At the same time the form is found much more conspicuous and evident in some instances than in others, namely in those wherein the nature of the form is less restrained and obstructed and kept within bounds by other natures. Instances of this kind I call Shining or Striking Instances.\textsuperscript{37}

Bacon's dependence on manifest analogy is evident from the fact that he treats of more remote and abstract analogies, and analogues of relation, under 'Instances Conformable, or of Analogy' he regards these as 'of little use for the discovery of forms' although they 'often lead us along to sublime and noble axioms, especially those which relate to the configuration of the world'.\textsuperscript{38} He also deals with analogy other than manifest under 'Supplementary or Substitutive Instances' but he suggests that it is only to be used as a last resource 'in the absence of instances proper' or for corroboration of proper instances.\textsuperscript{39} In general he says of this use of analogy:

Substitution by analogy is doubtless useful, but is less certain, and should therefore be applied with some judgement. It is employed when things not directly perceptible are brought within reach of the sense, not by perceptible operations of the imperceptible body itself, but by observation of some cognate body which is perceptible.\textsuperscript{40}

Keynes correctly emphasizes and summarizes the analogical nature of Bacon's method in the following passage:

Bacon's great achievement, in the history of logical theory, lay in his being the first logician to recognize the importance of methodical analogy to scientific argument and the dependence upon it of most well-established conclusions. The \textit{Novum Organum} is mainly concerned with explaining methodical ways of increasing what I have termed the Positive and Negative analogies,\textsuperscript{41} and of avoiding false Analogies. The use of exclusions and rejections, to
which Bacon attached supreme importance, and which he
held to constitute the essential superiority of his method
over those which preceded it, entirely consists in the
determination of what characters (or natures as he would
call them) belong to the positive and negative analogies
respectively....The doctrine of prerogative instances is
concerned no less plainly with the methodical determination
of Analogy. And the doctrine of idols is expounded for
the avoidance of falsa analogies, standing, he says, in
the same relation to the interpretation of Nature, as the
document of fallacies to ordinary logic.  

Keynes is essentially correct here in relating Bacon's method
to logic and to scientific arguments rather than to scientific
discovery and invention. Unfortunately seemed to regard his method
as one of discovery as well as validation, and consequently he
thought of science as a careful and comprehensive systematic analysis
of what is given (though confusedly) in analogous instances of
experience.

Keynes does not make the point that Bacon is concerned mainly
with manifest analogy, but this is the obvious purport of the
following remarks which he makes elsewhere:

Bacon's error was double and lay in supposing, first, that
these distinct elements lie upon the surface and consist
in visible characters, and second, that their natures are,
or easily can be, known to us.....

A similar assumption that the use of analogy in scientific
method relates to phenomena which are explicit in the sense of
being unambiguously identifiable and analysable into elements and
parts for the purposes of comparison and enumeration underlies
Mill's treatment of analogy and induction and his discussion of
'natural' or 'real' kinds. It is, I believe, at the basis of
Mill's disagreement with Whewell about the nature of induction.
This is evident in the following criticism by Whewell of Mill's
methods of experimental enquiry:

Upon these methods, the obvious thing to remark is, that they take for granted the very thing which is most difficult to discover, the reduction of the phenomena to formulae such as are here presented to us. When we have any set of complex facts offered to us; for instance those which were offered in the cases of discovery which I have mentioned - the facts of the planetary paths, of falling bodies, of refracted rays, of cosmical motions, of chemical analysis; and when, in any of these cases, we would discover the law of nature which governs them, or, if anyone chooses so to term it, the feature in which all the cases agree, where are we to look for our A, B, C, and a, b, c? Nature does not present to us the cases in this form, and how are we to reduce them to this form? You say when we find the combination of A B C with a b c and A B D with a b d, then we may draw our inference. Granted; but when and where are we to find such combinations? Even now that the discoveries are made, who will point out to us what are the A, B, C, and a, b, c, elements of the cases which have just been enumerated?

If Bacon's method is appropriate to scientific discovery at all it is to science in its early stages. Perhaps the nearest one gets to the use of manifest analogy is in taxonomy and classification, rather than in the discovery of 'natures' or 'forms'. Here, for example, is Adanson's account of his method of classification in botany:

I first make a complete description of each plant, putting in as many separate items, each of the parts, in all its details; and in so far as they represent new species, which have relations to those already described, I describe them side by side, suppressing the resemblances and noting only the differences. It was by a grouping of these comparative
descriptions that I perceived that the plants were ranged in classes or families, which were neither 'systematic' nor arbitrary, not being founded on one or several parts but on all parts. 47

However, even such apparently analytic methods as Adanson's depend on intuitive judgements of what constitutes the same or a similar character, and such characters are often far from being identical from one plant to another.

Another example of what seems to be the use of manifest analogy is in Cuvier's classification of animals and palaeontological researches. 48 In palaeontology, for example, he often had recourse to analogies drawn from the structure of living creatures to assist him in the reconstruction of past life forms from the evidence of chaotic and incomplete fossil remains 49. These analogies often depended upon the concomitance of properties and characteristics which were merely observed to go together and were not morphologically or causally explicable. As Coleman says, in his account of Cuvier's scientific work:

...what Cuvier acknowledged was his very real dependence upon observed correlations, to which he added an admission that the 'sufficient causes' for certain correlations were still unknown (he did not admit that the causes did not exist). 50

It is significant, however, that Cuvier regarded such analogies as supplementary to his true method - a supposedly rational analysis of the 'conditions of existence' of an animal and of the subordination and correlation of parts in its organization, based on functional and causal analogies of a more sophisticated and esoteric kind 51.

Some writers on analogy have recognized that it is only manifest analogy which is readily reducible to logical treatment. Hesse says, for example, that it is only where we have 'an analysis of similarity into relations of identity and difference' that we
can 'assimilate arguments from similarities...to what traditional logic has called "argument from analogy"'.

While manifest analogy is no doubt frequently used in science in a way which approximates to the supposed logical norm, i.e.:

a, b, c, d all have the properties P and Q.

a, b, c all have the property R.

Therefore d has the property R.  

this use is mainly at the commonsense level appropriate to our ordinary handling of experience, whether scientific or otherwise, and straightforward analogical arguments of this kind do not appear to have played a very critical or important role in the history of science.

We may take as a fairly straightforward use of analogy in science Galileo's arguments to show that the earth is a planet. In his argument the Moon played the important role of a sort of middle term in the analogical argument which was sometimes directly made from planet to Earth but sometimes of the form - Earth is analogous to Moon; Moon is analogous to planet; therefore Earth is analogous to planet. Jupiter provided the most direct and powerful analogy by turning out, under telescopic observation, to have satellites like the Earth. (Even here the Moon was involved in the argument. Medicean planets: Jupiter :: Moon: Earth. Jupiter and Medicean planets move, therefore Earth and Moon can move.) Under the telescope planets had a visible disc, unlike the stars which remained points of light under the magnification of the telescope. Moreover the planets have 'phases', hence by analogy they are like the Moon in being opaque non-luminous bodies shining by reflected light. The Moon is revealed by the telescope to be like the Earth because it has mountains which throw shadows and its phases reveal (the argument is abbreviated) that it is not self-luminous. The matter of the Moon is dense and solid like the Earth because otherwise the surface would not be uneven but like the smooth surface of the seas on Earth which find their own
even horizontal level. Furthermore the Moon cannot be a smooth mirror-like surface but must be of a similar constitution to non-reflective substances such as the Earth (here Galileo resorts to internal terrestrial analogies drawn from the diffuse way a wall reflects light compared to the selective (indirection) defined image reflected by a mirror). Since the planets have phases like the Moon they are also diffusely illuminated and so are similar in constitution to the Moon (and so to the Earth). Venus, in particular, has phases of the same sequence as the Moon and hence by analogy must circle the Sun. (Here the argument from the formal geometric structure of the Copernican system and the analogy of Earth and planet fuse with great effect.) Again, the Earth is like the Moon (and hence the planets) in itself reflecting light and hence having phases viewed from the Moon (evidence from earth-shine on the Moon). And so the argument, conducted with superb skill, goes on in the Starry Messenger, The Assayer, and the Dialogue concerning Two Chief World Systems. The key part played by the Moon as a middle term in Galileo's arguments is underlined by the long discussion in the first day of the Dialogues concerning the Two Chief World Systems of the seven analogies between the Moon and the Earth.

It can be seen that although there are elements of this argument which are close to manifest analogy and to the logical norm there are many extraneous causal and theoretical considerations (for example those relating to the sequence of Venus's phases) in which an interpretative perception going well beyond the ordinary is involved.

The course of scientific history has made it obvious that methods such as Bacon's and Mill's which depend on manifest analogy are not sufficient to lead to scientific progress and they have frequently been criticized adversely for their shortcomings as methods of discovery. The salient fact which stands against these methods is that if they were sufficient there would be no place for scientific genius. Acton makes the point
in the following passage about Maine de Biran:

Condillac, like Bacon, believed that the correct method of enquiry would serve for a 'levelling of wits'.....Maine de Biran's objection is that no amount of analytic or linguistic skill can lead to those discoveries which consist in looking at the facts in a totally new light, or in noticing connections and analogies which have hitherto been overlooked. I quote this statement of his. 'The strength with which, in an indivisible instant, the mind simultaneously conceives a host of things, all this seems to me quite independent of the artifice of signs and it is just in this instantaneous forming of thoughts in a certain order rather than in their analytic development that the pre-eminence of certain minds consists'.

This passage from Acton draws attention to a number of factors that seem to be characteristic of scientific discovery and which bear further examination. First, that some special ability going beyond routine method is required. Second, that this involves novel perceptions. Third, that such perceptions are instantaneous and comprehensive. As to the first of these factors, the need for some special ability, this seems hard to deny, in view of the universal and popular recognition of leading scientists as having some abilities out of the ordinary. Even Bacon acknowledges this, for among the requisites of scientific ability he includes:

A mind nimble and versatile enough to catch the resemblances of things which is the chief point and yet at the same time steady enough to fix and distinguish their subtler differences.

We may add to Bacon's judgement those of some of the scientists themselves. Sir Humphry Davy, for example, says:

Imagination, as well as reason, is necessary to perfection in the philosophical mind. A rapidity of combination, a power of perceiving analogies, and of comparing them by
facts, is the creative source of discovery. G.K. Gilbert, a noted American geologist, has this to say:
To explain the origin of hypotheses I have a hypothesis to present. It is that hypotheses are always suggested through analogy. Consequential relations of nature are infinite in variety and he who is acquainted with the largest number has the broadest basis for the analogic suggestion of hypotheses. From a distinguished modern physicist we have:
Science is an immensely creative enriching experience; and it is full of novelty and exploration; and it is in order to get to these that analogy is an indispensable instrument.

William James, a psychologist of genius himself, and therefore peculiarly fitted to have an insight into the psychology and processes of scientific discovery, says:
Why cannot anyone reason as well as anyone else? Why does it need a Newton to notice the law of squares, a Darwin to notice the survival of the fittest?

He goes on to answer the question:
The flash of similarity between an apple and the moon, between the rivalry for food in nature and the rivalry for man's selection, was too recondite any but exceptional minds. Genius, then, as has already been said, is identical with the possession of similar association to an extreme degree.

It will be noted that many of these passages specifically associate analogy with scientific ability and scientific discovery (Acton speaks of 'noticing analogies', Davy of 'perceiving analogies', Gilbert of suggestion 'through analogy', and Oppenheimer of analogy as 'an indispensable instrument') while the others mention 'resemblances and 'similar association' - terms which suggest analogy. Other scientists who have recognized the creative role of analogy in
scientific discovery and progress are Hooke, Kepler, Mach, Maxwell, and Poincaré.

It is quite evident that the analogy referred to is not manifest analogy, nor is it employed in any routine inductive method. Moreover none of these passages refers to analogical argument, but rather to 'noticing', 'perception', 'suggestion' or 'association'. I shall call any act of noticing, perceiving or attending to analogy in this original way an analogical act. It cannot be stressed enough that an analogical act is not in any sense an argument, even though it may well provide a basis for one. Even where it does provide the basis of an argument, it cannot be a purely formal argument on the lines of the logical paradigm of an argument from analogy mentioned above.

Perhaps the best way of indicating the nature of an analogical act is by comparing it initially to an act of recognition. When we strive to recognize a familiar face, a quotation, or a fragment of melody, there is a conscious apprehension of an effort of synthesis - of the present consciousness putting out feelers to the past and, when successful in recall, drawing complexes of past experience into the present as an element of the present perception or judgement. Sometimes, either spontaneously or in response to an effort of comprehension, the importation of complexes from the past can then be said to formulate the present experience, perception or judgement. This is to say that before the importation of a complex from past experience the formulation exists in present, only potentially or as a possibility, like the hidden pictures in children's puzzles. An analogical act is rather like an act of recognition of this kind, except that it does not concern identification, except in a special sense of identifying something as similar in certain important respects to something else. It is closer, for example, to identifying some unknown person as some known person's son, on the basis of strong resemblance, or of recognizing a fragment of melody as something by Beethoven because
of its style, without perhaps actually being familiar with the melody in question.

An analogical act/identification - novel not only for the individual but in the history of thought. A scientist, let us say, is interested in a certain field of phenomena. His mind ranges over a mass of data, together with certain laws or theories which are relevant. He also makes certain assumptions about the entities involved and perhaps also about mathematical techniques and calculi thought to be relevant. Obviously all this cannot be in the mind's eye at once. It rather comprises a reasonably well-defined area of concepts and their relations which the spotlight of attention ranges discursively over, an area any part of which can be brought into focus at will while the rest of the field remains constantly accessible - a sort of hinterland from which one can readily import perceptions and judgements. Let us suppose this field of phenomena comes to be seen in a new way, as being related by relations not previously thought of, or as comprising different kinds of entities than were previously assumed or as susceptible of new mathematical techniques or amenable to new calculi. Any such reformulation is, I would claim, the result of an analogical act, and the reformulation is accomplished by importing the new structural or interpretative frameworks from some other area of our experience. Of course the field of phenomena may be quite limited and may, at least in the early stages of science, consist of everyday experiences or straightforward observations without any of the apparatus of laws, theories and mathematical calculi. But, whatever its complexity, the field of phenomena under consideration for some sort of ordering or explanation I shall call the *topia* analogue. The other element of an analogical act which is brought into the mind either spontaneously or by an effort which may be stimulated by a vague awareness of familiarity I shall call the imported analogue.
It is fair to claim that even a peremptory glance at the history of science is sufficient to reveal analogical acts thickly strewn along the advancing path of science. Indeed, sometimes analogical acts within the realm of scientific discovery have been epoch-making, turning points in the history of individual sciences, great strokes of synthesis which provide a whole new basis for scientific advance.

One thinks immediately of Galileo recognizing an analogy between the motion of an object dropped on a moving ship and projectile motion, of Newton seeing the moon, a heavenly body, as analogous to a terrestrial projectile, or of Darwin recognizing that competition for survival in nature is analogous in its effects to selective breeding under domestication. In these cases an analogy has been an essential and central instrument in bringing whole continents of facts under law and order. But rather than of these more salient contributions to scientific progress it is perhaps more instructive to think of the continual small advances which bring now this, now that, small area or region under control. It is a dramatic, exciting and significant event when a Descartes brings geometry and algebra together in an analogical act of great potential, but the mechanism is not intrinsically different from that which enabled Wells to see the formation of dew as the same process as the condensation of water on a glass fresh from the well or on the inside of windows when the external air is chilled. The essential analogical act is the same but the complexity of the topic analogue is much less.

It would be a quite false simplification of this conception of the role of an analogical act in scientific discovery to suppose that it is necessarily accomplished at one stroke and within a short period of time. It is true that the analogical act in so far as it is an illuminating perception of resemblance is momentary and instantaneous. However,
considered in all its fullness, it should be connected with a whole complex of antecedent behaviour and knowledge and a similar complex of consequent implication and association. Before the illuminating perception, the topic analogue is ranged over, and sometimes one, sometimes another, part is brought into focus or juxtaposed with this or that other part. The topic analogue is itself an amorphous entity, ever growing and changing, sometimes crystallizing out only to dissolve again under the pressure of discordant facts drawn from other areas of the topic analogue. Imported analogues breed and interbreed with each other.

The whole process is admirably illustrated in Kepler’s accounts of the discovery of the three laws of planetary motion \(^{75}\), one of the few cases where we have anything like an adequate record of the processes of scientific creativity. In the case of Kepler’s first law it was the coincidence between the ratio of Mars’s distance at aphelion and perihelion and the secant of the ‘optical equation’ which issued in the analogical act, but this was preceded by what I have called analogical interbreeding, e.g. in the move from the circle (with one focus) via the ovoid (with one focus) to the ellipse (two foci) as the shape of the planetary orbit \(^{76}\). In the case of the third law it was by way of dense thickets of musical analogies (harmony, dissonance, assonance, octave, semitone, tone, tune, etc.) that Kepler was finally able to find his path. \(^{77}\)

The successful analogical act may be accomplished in a moment of insight, but its full implementation, elaboration and qualification may take one or several lifetimes, and is not restricted to the first discoverer. The analogical act in fact probably usually achieves a strategic formulation of some key laws and/or facts into an ordered relation, like an invading army subjugating capitals and major towns and leaving the subjugation of the provinces until later.
For example, when Lavoisier recognized the analogy between the combustion of carbon (with the production of heat and a quantity of 'fixed air') and the process of respiration (with the maintenance of animal heat and the production of 'fixed air') it was largely left to scientific posterity to complete the formulation and analysis of the topic analogue in physics, physiology and biochemistry.\(^{78}\)

An analogical act may bring together under one formulation previously discrete areas of thought. For example, Newton's analogy, when integrated with other ideas such as inertial motion, brought whole masses of terrestrial and celestial observations, ranging from the tides to the precession of the equinoxes, into connection. Similarly two sciences were brought together when Van't Hoff perceived the analogy between physical equilibrium, in terms of physical states and variations of temperature and pressure, and chemical equilibrium, in terms of chemical combinations and variations of temperature and pressure.\(^{79}\)

Sometimes the topic analogue can hardly be said to exist at all before it is, as it were, pulled together by the imported analogue. For example, when Guettard recognized the Auvergne landscape as (extinct) volcanic, it was only the fortuitous noticing of the resemblance between building stone and volcanic rock which led Guettard to the illuminating analogy.\(^{80}\)

The imported analogue may be from an adjacent or from a remote area of experience in relation to the topic analogue. For example, the analogies which served to guide the analysis of rotational motion about an axis were drawn from the near domain of previously established theories and laws about translatory motion.\(^{81}\) On the other hand there are notable examples, such as the one mentioned above from Van't Hoff, of what Kelvin 'cross-fertilization' between the sciences.
However it does not matter, provided the imported analogue contains the necessary relations or structure, how remote an imported analogue is from a topic analogue. When Mayer bled a sailor who fell ill on the ship on which he was a medical officer, it was the analogy between the brighter redness than usual of the venous blood in the tropics and the usual colour of arterial blood that led him, via the idea that metabolism drew less oxygen from the blood in hot climates because maintenance of body temperature required less heat, to a lifelong interest in the conservation of energy, of which concept he was a pioneer\textsuperscript{82}. Again the analogy which led to Bradley's explanation of the aberration of light depended upon the imported analogue of the movement of a flag on the mast of a sailing ship as it changed tack; a very far cry from the astronomical problems which Bradley was trying to solve\textsuperscript{83}.

As the examples from Mayer and Bradley show the reservoir from which analogues can be imported is as wide as the experience of the individual. All is grist to the analogical mill and any scientist might say with Molière, 'Je prends mon bien ou je le trouve'. Even though imported analogues may be remote or recondite, this is perhaps not the rule. Analogues may be imported, and probably usually are, from domains which have a familiar and established relevance to the topic analogue. Just as poets can sometimes be said to draw on a stock diction, so for example scientists in a certain period or situation turn readily to, say, mechanistic analogues or certain differential equations.

I have stressed the difference between manifest analogy and imported analogy and dealt with the analogical act at some length since I believe that a number of important consequences follow from the fact that it is imported analogy and analogical acts which play a creative part in scientific progress.

But first it might be as well to support the claims of imported analogy and analogical acts as the instruments of
scientific discovery by mentioning two features of scientific discovery which have frequently been remarked upon. One is the frequently sudden nature of scientific discovery. Often problems are solved and discoveries made by a sudden insight, sometimes after a long period of impasse (remember James's phrase - 'flash of similarity') Garrett's book on scientific and technological discovery, which consists largely of quotations from accounts of actual scientific discoveries and inventions, often by the discoverers and inventors themselves, is appropriately entitled, in view of the purport of those quotations, The Flash of Genius. There is a similar theme in Hadamard's book on the psychology of mathematical invention. He quotes Gauss, for example, as follows:

Finally two days ago I succeeded not on account of my painful efforts, but by the Grace of God. Like a flash of lightning the riddle happened to be solved.  
Poincaré uses such phrases as: 'the idea came to me without anything in my former thoughts seeming to have paved the way for it', and 'brevity, suddenness and immediate certainty'. Presumably the feeling of the irrelevance of former efforts and ideas is illusory, since such a sudden insight only seems to come after a great deal of preceding effort and play of analogies has taken place. That mathematical invention is concerned with analogies is abundantly clear from various other phrases of Poincare, such as 'the analogy with elliptical functions guided me', 'the idea came to me that the transformations I had used to define the Fuchsian functions were identical with those of non-Euclidean geometry'.

Indeed, the dramatic suddenness of scientific discovery is a familiar theme of anecdotes from the lives of great scientists, whether they concern Archimedes in his bath, Newton and the apple, or Darwin's solution of the problem of divergence in his carriage. We may take as representative of such episodes Kekulé's
celebrated account of his discovery of the benzene ring:

During my stay in Ghent, Belgium, I lived in a fine room on the main street. I sat in this room and wrote on my textbook, but could make no progress — my mind was on other things. I turned my chair to the fire and sank into a doze. Again the atoms were gambolling before my eyes. Little groups kept modestly in the background. My mind's eye, trained by the observation of similar forms, could now distinguish more complex structures of various kinds. Long chains here and there more firmly joined; all winding and turning with snake-like motion. Suddenly one of the serpents caught its own tail and the ring thus formed whirled exasperatingly before my eyes. I woke as by lightning, and spent the rest of the night working out the logical consequences of my hypothesis.89

Such introspective accounts by scientists seem to lend strong support to the description of scientific invention and discovery as an analogical act.

The second feature of accounts which have been given of scientific discovery and observations which have been made on the nature of such discovery, and which accords with the remarks I have made about imported analogy and analogical acts, is the attention they give to the rather nebulous but essential imaginative factor of intuition. A discussion of the role of intuition in science is given in a paper by Nyman90. Nyman cites Whewell in particular as the originator of modern views that scientific discovery relies on a sort of intuitive insight out of reach of method91, but he also claims that there has been among philosophers a traditional recognition of intuition going back to Spinoza's doctrine of scientia intuitiva and also claims support from the writings of scientists such as Bernard, Driesch, Mach and Planck92.

Nyman expressly repudiates the view that the construction of hypotheses or theories is the result of 'the methodical, not to say
mechanical process of induction". He goes on to say:

...it is intuition which by virtue of the simultaneity of its glance can track down the ultimate relations hidden among the elements, the members, the measurements and the protocol statements... And it is precisely because the intuitive act has the faculty of maintaining in a single view, in a single field of ideas, a greater number of elements of reality than can our usual discursive and analytical understanding... that it is able... to find much more surely resemblances and discover relations of dependence that must necessarily escape our attention working in a more discursive and analytical fashion...

Another writer who emphasizes the importance of intuition is Poincaré. He describes the situation in relation to discovery unequivocally when he says: 'Logic, which alone can give certainty, is the instrument of demonstration; intuition is the instrument of invention'. Later on, while discussing what serves as a guide for mathematical invention, he says: 'This guide is, first, analogy'.

In the passage previously quoted from Acton, Maine de Biran talks of the mind simultaneously conceiving 'a host of things', and in the passage just quoted from Nyman there is a similar stress on the simultaneous perception of a number of elements in a single act. Again, Kekulé mentions spending the rest of the night working out the logical consequences of his vision of the snake. Nyman also quotes the following passage from the Duke of Wellington to illustrate this aspect of intuition:

There is a strange thing that one sometimes notices: when one is in the process of considering a question, a whole series of ideas comes to the mind in a flash: you perceive them all but it might take two hours to put on paper all that crossed your mind in an instant. You have before you all the sides of the thing, all the relations between the different elements, all the consequences.
It is this rich potential which especially distinguishes imported analogy and an analogical act from the operation of reason, in some quasi-formal inference of the form mentioned earlier, on manifest analogy. In a critique of Faraday's work in *The Edge of Objectivity* Gillispie deplores Faraday's lack of mathematical ability and his consequent reliance on analogy. Gillispie says:

Analogical thinking is a kind of linear transfer of ideas from one area to another, while abstraction frees ideas from the physical and poises the mind for the thought experiment. 98

This rather perversely states what is the opposite of the truth and denies to analogy the very virtue which distinguishes analogical thinking - its non-linearity. The origin of Gillispie's judgement seems to be a confusion of the two types of analogy I have been trying to distinguish, and the assimilation of analogical thinking to a step-by-step quasi-deductive process.

What imported analogy and the analogical act lead to is not a limited inference to such-and-such a property or properties (d, e, f, etc. in the logical paradigm) but a multi-dimensional gestalt-like insight into new ways of looking at phenomena. At the very least the analogical act will present the phenomena in a novel aspect or relation, but the potentiality of the analogical act is by no means limited to this. One reason for the critical place which I assign to the analogical act in science is that it does have potentially a multi-dimensional simultaneous scope which embraces the fields of both the topic and imported analogues. How much or how little is contributed by the imported analogue to the formulation can and does vary. It may be merely, in the case of a topic analogue which already has a settled system of concepts, a purely abstract new relation (perhaps mathematical) among the existing concepts that the imported analogue is, as it were, called upon to provide. Sometimes more extensive demands can be made upon it and it is able to introduce descriptive and causal detail into
the imported analogue. Sometimes, as in the case of a scientific revolution, the imported analogue comes trailing metaphysical strings, and so on.

Looked at in this way, the analogical act and the imported provide analogue/something more than a limited inference, more even than a new relation or set of relations among phenomena. They provide a new perspective, new possibilities of description, new horizons to explore to, novel inferences to be followed up.

Several writers have recognized this aspect of scientific discovery. It is a major theme of Toulmin's book, The Philosophy of Science. He says:

The heart of all major discoveries in the physical sciences is the discovery of novel methods of representation, and so of fresh techniques by which inferences can be drawn – and drawn in ways which fit the phenomena under investigation.99

Elsewhere he says:

...it is...unfortunate that logicians have come to speak of scientific discovery as 'inductive inference': where no rule of inference could ever be given, the very notion of inference loses its point. Discovery is, rather, a prerequisite of inference, since it includes the introduction of novel techniques of inference drawing.100

Other writers who have stressed the important role that seeing things in a new perspective plays in scientific discovery are Hanson101, Kuhn102 and Farre103. Farre says, for example:

The discovery of a way of looking at the world is the necessary prelude of a science, not a constituent of it...The perspective is thus fundamental to the whole scientific enterprise, which is inscribed within it and which is unrealizable without it.104

Moles, in his examination of the heuristic methods associated with scientific creativity, lists:
The method of transfer, one of the most valuable, universally recognized, based on an often superficial analogy, applies a concept in a new domain and draws from it in this domain new results.  

Earlier in his book he is at pains to point out that such methods are not logical in the ordinary sense, and he includes analogy under what he calls the 'infralogical'.

The dynamic use of analogy which we have been discussing may be illustrated by the way that a ship in motion was used as an analogue during the Copernican Revolution to establish a number of points about the motion of other terrestrial objects, such as projectiles, and also about the motion of heavenly bodies. Although the imported analogue is in a domain adjacent to the domain of the topic analogue in this case, the example is used because it demonstrates how even what is a comparatively simple and familiar analogue can give rise to a number of novel insights and inferences.

Buridan used the analogue of a ship's motion in a negative way to refute the Aristotelian (but not Aristotle's) idea that a projectile continues to move because of the pursuing air, but he also used it in a positive way to establish the idea of the idea of the visual relativity of the heavenly motions (and hence the implied indifference from a phenomenal point of view of the Ptolemaic and Copernican systems), as did Oresme and Cusa. Oresme made the further and important point (as it became in Galileo's hands) that a hand drawn downwards on a ship moves quite differently in relation to the shore. Copernicus used the analogue to establish both visual relativity and that objects in similar motion may be mutually at rest.

With Galileo (in *Letters on Sunspots* and *Dialogues concerning Two World Systems*) the ship analogue is used to make a variety of points: that true motion is relative to whatever does not move; that observed motion is relative to the observer and is not absolute; that in the absence of wind or waves a ship once in motion
continues to move forever ('circular' inertial motion); that the contents of a ship share its motion and behave in relation to the ship as if there were no motion (inertial frame, laws of dynamics and kinetic behaviour indifferent to uniform motion of inertial frame); that change in motion of the ship affects dynamic behaviour of its contents (accelerated inertial frames result in changes in dynamic behaviour); that motion of objects in a ship relative to shore can be analysed into two components: one due to ship's motion, one to own motion (vectorial analysis, parabolic projectile motion); that objects dropped from the mast always take the same time to fall irrespective of ship's motion (independence of components of motion); that motion relative to shore should be added to motion within the ship to obtain total motion relative to shore (Galilean transformations)\textsuperscript{111}.

Using this example, a number of points need to be made. First, Galileo is not arguing from parallel cases or from manifest analogy. He is not arguing from the known analysis of ship's motion to the known analysis of the motion of balls rolling along planes, pendula, cannon balls and arrows, and so on. He assumes the analogy of a ship's motion to all other motions and uses the ship analogue as a means of analysis. Second, he is not using the ship analogue merely to illustrate points which he has previously and mysteriously intuited by some other occult method. The ship analogue is not a pedagogic or expository device which decorates his thought. It is integral to his thought and understanding since it is for him as well as his readers the route to understanding. For, as well as the facts about ship motion being familiar and salient, any inferences from Galileo's analysis could be easily checked by experiments in the ship situation (although it is possible that Galileo never indulged in anything other than 'gedanken' experiments in this connection\textsuperscript{112}). Moreover and more importantly still the ship situation has unique features from a phenomenal point of view which command attention.
The last point which needs to be made is that, nevertheless, it required the talents of a Galileo to perceive, for example, the analogy between ship and projectile motion. Obviously, Galileo's analysis is not in terms of any ordinary properties or relations. The only resemblances between the ship and any other objects which Galileo concerns himself with are resemblances of motion and these in turn are analysed into resemblances of change of motion and relative directions in space, and even more abstractly still into number relations or geometric ratios.

When one remembers that imported analogy may well bring together remote and heterogeneous domains of experience, and that analogical acts by their nature are not likely to be concerned with obvious resemblances, it is not surprising that analogies of relation are the most important for science, and that in many scientific contexts therefore analogy may be regarded as a resemblance of relations.

As it turns out the more obvious analogies between things, e.g. numerological analogies such as the fact that the sages of Greece, the planets (as known to the Greeks) and the gates of Thebes were all seven in number, and analogies of colour or shape have no particular significance and lead to no useful inferences although mankind for long thought they did. It is the more complex and abstract analogies that are the basis of science.

As Aohinstein says of the scientific use of analogy:

Two points should be noted about the similarities mentioned. First they are somewhat abstract by contrast, say, with similarities of colour, shape, or size. And, indeed, typically, at least in science, similarities of the latter sort do not generate analogies, or do so only insofar as they are relevant for the more abstract similarities mentioned.

Höffding, in the following passage, makes the stronger point that it is only through analogy of relation that any sophisticated ordering of phenomena is possible. Having said that 'Analogy is likeness of the relations of different objects, not likeness of
single qualities', he goes on later to say:

As there are important differences between the domains of experience, the facts not being homogeneous but constituting many groups, every one with its peculiarities, our thinking must enlighten one group or domain by the means of another, especially so, that the experiences which arrange themselves for thought in the simplest and most fertile way, are made use of by the understanding of the other. This would not be necessary, if existence did not manifest qualitative differences. But the parts of experience as they are known through experience, are not homogeneous, and analogy is therefore a necessary way to understanding.\textsuperscript{115}

Moles, in his work on scientific creativity, argues that it is a generalizing abstraction which empties concepts of their sensual content and leads to formal analogy and he refers to 'the classic liaison between analogy and abstraction' citing the authority of William James\textsuperscript{116}. From the point of view that we have been discussing, this puts the thing round the wrong way, for I have argued that, in effect, it is the analogical act which brings otherwise discrete areas of experience together and enables one to see, for example, that two things are alike in exhibiting a relation. However, it can easily be seen how an analogical act can lead on to an abstraction of this relation, and, indeed the analogical act may be said to be a necessary prelude to abstraction. As Buchanan says, 'argument by analogy is the fundamental technique in the process of abstraction', and it is clear from the context that he does not mean 'argument' in the sense of the normal 'logical' paradigm\textsuperscript{117}.

It is this connection of analogy with abstraction which accounts for the extraordinary power and fertility of mathematics, and which underlines the importance of the mathematization of science which took place during the Scientific Revolution\textsuperscript{118}. 
For in mathematics we have an extremely abstract system of relations capable of providing analogies of relation for the most heterogeneous domains of experience. Moreover mathematics has the important advantage that it is concerned with relata whose properties are determined solely by the system of relations into which they enter and that it is in a sense the science of purely formal relations. The relations concerned are basically few but very precisely and unambiguously determined, and as it happens endless more complex series of relations can be freely constructed out of them.

There are some implied or assumed 'descriptive' features of mathematical symbolism. For example, some symbols refer to variables, some to constants, some to relata, some to relations, and some to relations of relations, and in addition there is the 'proper name' system of cardinal numbers. But these contingent features of the symbolism are fixed and unambiguous, and in fact a function of the relations into which the entities referred to by the symbols enter. By manipulating these systems experimentally, rather like a child exploring the constructional limitations and possibilities of the way he can fit together the pieces in his Meccano set, the mathematician can accumulate a reservoir of complex formal analogues and a mastery of techniques for exploring and understanding their complex relations. David Hawkins is, I think, recognizing this role of mathematics when he says:

The law of gravity becomes a Gestalt.....in proportion as its implications and analogies are pursued and become familiar; and in the same proportion its prosaic meaning as an algebraic formula recedes into the background.

and later he says:

So it is that mathematics changes its position in the world of intellectual culture, from that of a tool of economic and technical interests to that also of an indispensable tool of intellectual growth to be prized and advanced and therefore for some to be regarded as an
end in itself. 120

One can see at once the abstract, gestalt-like and analogical character of mathematics in the way, for example, that one set of equations (Laplace's) serves as analogue for the phenomena of change in such diverse fields as gravitation, electrostatics, electricity, elasticity, and the permanent flow of liquids.

The peculiarly precise way in which we can make sense of phenomena by means of a mathematical analogue, and the immensely complex and fertile way in which the resulting modes of description enable otherwise completely unassimilable qualitative phenomena to be integrated into relations with one another by reduction to one mode of description (i.e. one which is homogeneous and qualitatively neutral or identical - the numerical and mathematical) accounts for our tendency to overlook the analogy and look upon the analogue as not one possible description of the phenomena but as the most real, most essential, correct, or just the only literal description 121. The following account puts this is an admirably clear perspective:

When relations of number can be pointed out it is possible through them to read the course of events and the place of elements in the world of experience. But this applicability depends itself on an analogy: an analogy between the place of numbers in the numerical series and the place of events in the temporal series or of elements in the spatial series. The numerical series is the most exact order we can form, and it is natural that all other orders and series be brought into the most intimate connexion with it.....Science then conceives qualitative alterations as if they were only quantitative, and the alterations are thought of as going on in something which only presents quantitative differences. In this way such concepts as matter, atom, ether are produced. They are images or schemes which can be determined wholly by numbers - which can be applied
consequently and which can lead to secure conclusions concerning new experiences. This analogy has shown itself so fertile and important, that it has been regarded as an identity, or at least as the only possible analogy. 122 Hoffding possibly overstates the reduction wholly to number or number relations but he does describe a common aspiration of science to be mathematical in form 123.

The connection of analogy and mathematics is the more clear if we go back to the very origins of the use of the concept of analogy in the sense that we now understand it, in Aristotle's thought 124. Aristotle only treats manifest analogy and 'argument by analogy' (according to the usual logical paradigm) briefly in his consideration of what has been variously translated as 'example' and 'paradigm' 125, which he rejects as valid forms of inference, but recognizes as having a rhetorical or persuasive value, indicating probabilities or at the very least some positive grounds for a conclusion. Lloyd, in his comprehensive survey of the part played by analogy in Greek thought, says of Aristotle:

In the Organon, Aristotle is preoccupied with the method of the syllogism and the attainment of certainty in reasoning, and assessing the paradigm from this point of view he rejects it and all forms of induction other than complete enumeration of particulars, with his clarification of the different modes of opposition. This examination marked a notable advance in logic: in this case Aristotle clearly revealed the weakness of analogical argument as a mode of inference. Yet his discussion of analogy is, one might say, still incomplete, for while he analyses it successfully from the point of view of demonstration, he devotes far less attention, in the Organon, to the heuristic function of analogy and to the question of its role in scientific method as a preliminary source of hypotheses, although in practice
analogies figure prominently in this role both in Aristotle himself and throughout early Greek science.\textsuperscript{126}

What Lloyd calls here the heuristic use of analogy is, I think the same as imported analogy and the associated analogical act. In practice, 'Aristotle uses analogies far more often than syllogistic arguments to infer facts, and as a means of establishing his theories and explanations', although there is also an implicit critique of such an heuristic use in his tendency to qualify his own and other people's analogies by indicating the differences which exist in spite of similarity, by submitting resemblances to analysis, and by treating analogies as tentative and hypothetical\textsuperscript{127}. Advocacy of the heuristic use of analogy is evident in the following passage. It should be noted that it concerns analogies in different genera and not things of the same genus or species which exhibit manifest analogies:

One should examine the resemblances between things which belong to different genera, to find out how the relation of one thing to a second thing is found again in the relation of some other thing to something else. Again, for example, the relation of knowledge to what we know in the relation of sensation to the sensible.\textsuperscript{128}

Gomperz says of Aristotle: 'the penetration (of his mind) reveals itself nowhere to a greater degree than in the discovery of hidden resemblances.'\textsuperscript{129}

The passage just quoted from the \textit{Topica} is obviously concerned with what is sometimes called 'analogy of proportion' in Scholastic philosophy\textsuperscript{130}. This analogy of proportion has its origins in and is a generalization of the original mathematical concept of analogy, and it was Aristotle who seems to have been responsible for the critical development of and broadening of the very concept of analogy.

Dorolle quotes a number of passages to show how the idea expressed by the term 'analogy' changes significantly in Aristotle's
hands. From its original mathematical use to mean what we should call the same 'ratio' (i.e. \( A:B :: C:D; \) or \( \frac{A}{B} = \frac{C}{D} \)), where \( A, B, C, D \) are mathematical entities, e.g. numbers, or, in Geometry, lines. Aristotle first generalizes the concept to more indefinite quantitative relationships, e.g. to express the relation between speeds of an object in different media, but subject to the same 'force', as proportional to the relative 'densities' of the media. Here \( A, B, C, D \) could not in Aristotle's day be assigned any definite numerical value but the relationship \( \frac{A}{B} = \frac{C}{D} \), where \( A \) and \( B \) are speeds and \( C \) and \( D \) densities, was still assumed to hold. From this it was no long step to conceiving of analogy as expressing simply resemblance of relations, i.e. \( A:B :: C:D \), where the relation \( A:B \) (or \( C:D \)) can be of any kind.

The scope of analogy is immensely widened by this step, and the relation of analogy is rendered even further capable of deployment if the relations \( A:B \) on the one hand and \( C:D \) on the other are conceived of not as identical but as similar. Thus we get the analogy relation in all its generality: \( A:B :: C:D \), where \( A, B, C, D \) may be any entities whatever and ' :: ' expresses the relation between \( r \) and \( s \) of similarity and not identity. Of course, such a loosening up and generalization of the analogy relation results in a corresponding looseness in the certainty or rigour with which analogical inferences are made, but it enables us to recognize, as essentially similar, acts of thought of all degrees of specificity and exactness which occur as part of scientific discovery and explanation. It was, in fact, just such a broadening of the concept of analogy which enabled Aristotle to interpret some forms of metaphor as essentially analogical and to regard such metaphors as the particular hallmark of creative genius in poetry.

Daudin, in the following passage, goes so far as to claim that this broadening of the concept of analogy is the key to the development of scientific thought in antiquity:
One knows that 'analogy' according to its primary and fundamental meaning is the equality of relations which unite respectively two terms $a \sim b \sim c \sim d$. It is, we think, difficult to exaggerate the importance of the role that this scheme of proportion, so insignificant as it appears to us, has played in the interpretation of experience accepted and developed by the great conceptual philosophies of antiquity, a role comparable it seems to the idea of law in modern thought. 134

Enough has been said, I hope, to establish the importance of imported analogy and the analogical act for science, and to dissociate the fruitful use of analogy in science from quasi-formal inferences about the possession of manifest properties. By contrast, I have argued that the basis of progress in science is not an analogical argument in the ordinary sense but an analogical perception which involves the importation of analogues from discrete areas of experience into the areas of experience under investigation, with a resultant reformulation or reordering of the area under investigation (the topic analogue) so that hitherto unremarked analogies are seen and novel inferences suggested. I have suggested this use of imported analogy is the key to what is often described as 'intuition', that the richness or fertility of analogical acts varies greatly, that they may result in importations from esoteric or from familiar domains, that imported analogy is likely to be concerned with analogy of relation, and hence with comparatively abstract properties and relations, and with mathematics in particular.

I do not, of course, wish to suggest that scientific thought and discovery are not also concerned with induction and deduction, or that analogical acts do not sometimes lead on to analogical arguments in the ordinary sense. However I believe that it is only if the essential preliminary steps of imported analogy and the analogical act are recognized that a proper understanding
of the claims that have been made about the role of models in science and about the metaphorical nature of science is possible.

In particular no question of the metaphorical use of language in science can arise unless terms are employed in new domains in ways which involve novel insights and interpretations and the extension of familiar vocabulary to describe these novelties. And it seems to me that only imported analogy and analogical acts involve or necessitate such a use of language.
Chapter 2

There is an extensive modern literature on the use of models in science. The bibliography of this thesis contains some fifty items which deal mainly with models and at least ten other items have sections on models. This literature is only marginally concerned with models in the sense of physical artefacts, although this sense (i.e. the sense in which we speak of 'scale models', constructed, for example, by engineers or schoolboys) is sometimes used to explicate other senses of the word. The currency of the word 'model' in a different sense to 'scale model' seems to have begun in scientific contexts in the work of Kelvin and Maxwell in the nineteenth century. Maxwell used and advocated a method which he called 'physical analogy' which he describes as follows:

By physical analogy I mean that partial resemblance between the laws of a science and the laws of another science which makes one of the two sciences serve to illustrate the other.

His method served not only to facilitate understanding, but also to enable the transfer of mathematical equations and mathematical solutions from one branch of physics to another, and to suggest new lines of enquiry. Kelvin frequently used descriptions of imaginary mechanical models designed to 'fulfill the conditions required in the physical phenomena that we are considering, whatever they may be' in order to develop, illustrate and communicate his scientific theories. The French physicist Duhem made a well-known attack on the use of these methods, which he lumped together. The substance of Duhem's attack was that models or physical analogies are superfluous to the scientific enterprise, except for a minor and probably dispensable heuristic value. However, he did admit some use for a more formal analogy between well-formulated theories expressed in abstract mathematical form, where 'experimental intuition quite naturally poses a problem and suggests a solution for it' for one but not the other theory.
Associated with Duhem's ideas was his formal deductive conception of physical theory. For him a physical theory consisted of a system of hypotheses ('a very small number of extremely general judgements, referring to some very abstract ideas') and of laws which can be strictly derived from these hypotheses by 'deduction that is very lengthy perhaps, but very sure.'

In turn Campbell, an English physicist, provided a critique of Duhem's view of a physical theory (although he does not specifically mention Duhem) by arguing that the formal, logical, deductive criterion of scientific theories is not satisfactory, since, in addition to meeting such logical criteria, theories must also display an analogy with already established laws. Campbell does not use the word 'model', and Duhem uses it only in the restricted sense of 'imaginary mechanical model'. However, later writers have interpreted their views, according to a more recent terminology which does use the word 'model', in such a way as to regard Duhem and Campbell as being the precursors of two current opposed views of the role of models in science. One modern writer, on the other hand, has denied that Duhem's and Campbell's views are opposed in this way.

A further development in the usage of the word 'model' occurred in physics with the tendency to call certain theories 'models', a notable example being the so-called 'Bohr-Rutherford model' of the atom. It will be noted that our discussion so far has been confined to physics, and this development of ideas about models within the science of physics is reflected in the fact that by far the majority of the books and articles about models in the bibliography of this thesis concern the use of models in physics. However the word is now used widely in relation to other sciences than physics, although sometimes with slightly different meanings or emphases.

From the point of view of this thesis it is not necessary
and analysis of the
to make an exhaustive survey and/literature on models, but simply
to try to clarify those ideas about models which relate to
theories about metaphor in science. It will certainly be
necessary to distinguish among several different applications
of the word 'model'. In order to do so it will be helpful to use a
terminology which originates with Keynes and which has gained
some currency, although it should be pointed out that Keynes
uses this terminology in a restricted and formal sense in
relation to induction and in relation to what I have called
manifest analogy. The terms are positive and negative analogy.
With Keynes a positive analogy is 'the set of propositional functions
which are satisfied by both of two objects' while the negative
analogy is 'the set of functions, such that each is satisfied
by one and not the other of two objects'. Hesse has adapted
this terminology simply to mean the properties common to two things
being compared (two analogues) and negative analogy to describe
properties belonging to one and not the other. She has added to
this terminology the term neutral analogy to describe the
unascertained or unexplored area of resemblance or difference
between things related by some positive analogy.

It is unfortunate that the literature on 'models' displays a
bewildering lack of agreement about what exactly is meant by the
word 'model' in relation to science. As Harris says in a review
of a symposium on models and analogues in biology: 'the terms
model and analogue cover a broad and continuous spectrum of
examples'. Brodbeck remarks: '....what exactly is a model and
what purposes does it serve? I venture to suggest that ten model
builders will give at least five different or, at least,
apparently different answers to this question.'

Some attempt must be made to resolve this confusion, and it seems
to me that there are, among the varieties of usage, four main
senses to be attached to the word 'model'. Inevitably some of the
disagreements about models spring more from different usages
of the term than from substantive points of disagreement, but there are nevertheless points of disagreement which do not arise from differences of connotation. Before distinguishing various senses of the word 'model' it will be as well to notice in what way the various usages of the word agree. To some extent this has already been touched upon in the first few paragraphs of Chapter 1, where it was noted that a model displays an analogy. In brief, what is common to all usages of the term is that the model is taken to be a species of analogue. In many cases it seems to be implicit that models are fairly complex and analogues which form the subject matter of or are otherwise comprised within an established science. But, since there is no clear-cut criterion by which to specify the necessary degree of complexity or system, and since some writers specifically include simple analogues drawn from ordinary experience in their discussion of models there seems to be no special reason to insist on models being systematic analogues drawn from some existing science. In general, when it is obviously implicit that such analogues are being referred to, there is nothing which would in principle exclude simple analogues drawn from ordinary experience being subjected to the same analysis or discussion. Moreover, such an inclusion would create a discontinuity between early and later stages of a science, and further would preclude consideration of at least some of the examples which are generally regarded as models in the literature.

Hesse distinguishes two of the main senses in which 'model' is used in Models and Analogies in Science. She calls them model and model. Model corresponds to Maxwell's physical analogy or Kelvin's mechanical model which are used as external analogues for the purposes of formulating or developing laws and theories but which contain disanalogies to those laws and theories, i.e. exhibit a negative analogy in relation to them. Model corresponds to a theory of the kind referred to as a model (what kind of theories these are remains to be discussed), or a
part of a theory or an auxiliary of a theory. In the terminology I have previously used model, refers to an imported analogue and model (with some qualifications to be noted) to the formulated topic analogue.

In more general terms it might be said that these two uses of the word 'model' correspond to the conceptions of a model for a theory or law and the model of or belonging to a theory. At least some of the disagreements about models spring from the confusion of these two. I have referred above to four senses of the word 'model'. These result from there being two variants of each of the two senses already mentioned, which depend on the formality or otherwise of the conception of model. In the case of either sort of model the model may be conceived of as either a set of assumptions about, or actual, entities, processes, structures, or causes and their relations, or, alternatively, as a set of propositions or statements connected together in a deductive system.

These different usages of the word model may be illustrated from the writings of various philosophers of science.

Brodbeck, for example, conceives of a model as having a formal version of model. Having said that a theory is 'a deductively connected set of laws' and that laws have a specific logical or mathematical form, she goes on to say:

Two theories whose laws have the same form are isomorphic or structurally similar to each other. If the laws of one theory have the same form as the laws of another theory, then one may be said to be a model for the other.

Another formal version of model, which has been much criticized and discussed is Braithwaite's:

A theory and a model for it have the same formal structure since theory and model are both represented by the same calculus.

A more extended definition is given by Braithwaite in the following passage:

... a model for a theory T is another theory M which
corresponds to the theory \( T \) in respect of deductive structure. By **correspondence in deductive structure** between \( M \) and \( T \) is meant that there is a one-one correlation between the concepts of \( T \) and those of \( M \) which is such that if a proposition in \( T \) logically follows from a set of propositions in \( T \), the correlate in \( M \) of the first proposition in \( T \), the correlate in \( M \) of the first proposition in \( T \), logically follows from the set of correlates in \( M \) of the propositions of the set in \( T \).\(^\text{25}\)

It is difficult to find a precise and brief definition of a model in the sense of model\(^\text{2} \) which is not conceived formally, since the conception is usually implicit rather than categorically stated. However, the implication is clear in the following passage from Theobald, where he is discussing **models for scientific theories**:

Models in fact contribute to more inclusive systems of explanation, in the sense that a theory which is expressed in terms of a model will resemble in some ways the theory and laws associated with behaviour of the model itself.\(^\text{26}\)

Presumably a set of propositions cannot exhibit behaviour, so that it is evident Theobald is referring to an entity or process. It is moreover clear that Theobald is not using the word 'model' in the sense of model\(^\text{1} \), since he later expressly says that 'care must be taken not to confuse the model with the theory for which it is a model, despite the close connection'.\(^\text{27}\)

This non-formal concept of model\(^\text{2} \) is usually evident from the examples of models which are given. In the following quotation from Hutten it is obvious that he cannot be referring to a system of propositions:

Let us take a specific example. In nearly all theories of physics we employ the model of an **oscillator**. The motion of a simple mass point acted upon by a
force proportional to its displacement is easy to visualise. Moreover the oscillator can be made more complicated by introducing friction and impressed forces, and so a variety of physical situations can be represented by it.\textsuperscript{28}

An example of the formal model use of the word 'model' is the following from Achinstein:

\textit{...the propositions comprising a model of an $x$ are the same ones as those constituting what may be called a theory of an $x$ (the Bohr theory of the hydrogen atom, the free electron theory of metals, the Ising theory of ferromagnetism, etc.)...}\textsuperscript{29}

The non-formal concept of a model is made very explicit in the following two passages from McMullin:

- The model is frequently evoked by an analogy from some quite different domain. But it is not itself an analogue.....
- The model is the postulated structure, whereas the theory is the set of statements in terms of which this structure is provisionally described. The theory is thus a linguistic and mathematical entity: the model is not.\textsuperscript{30}

McMullin's account of a model, in its explicit rejection of any claim that the model is an analogue seems to give the lie to my claim that, in all usages, the model is a species of analogue. In a strict sense this is true. However, I think that what is being rejected is the identification of the model with an imported analogue. It is, I think, evident that the model is an analogue (in the general sense) of whatever it is from a different domain that evokes it 'by analogy'.

An interesting variant on the non-formal use of model is that of Nash, who explicitly separates the deductive systematic features of a theory from the model aspect:

\textit{Always a scientific theory is the aggregate of a formalism}.
and a model. The formalism constitutes the deductive machinery required for the theory's function as a correlative device; the model gives rise to the multitude of semantic rules and, as we shall see, much more besides.31

We have by no means exhausted the possible meanings to be attached to the word 'model'. For example Deutsch discusses what he calls a 'generalized formal model' where it is evident he intends to refer to general conceptual schemes which are fundamental to the whole enterprise of science at various times32.

Of course a writer's conception of the role or status of a model in science will vary according to which of the senses he attaches to the word 'model'. Most of the writings on model will, I believe, be found to assume one of these four senses. Sometimes, one sense of the word 'model' is used, while model in another sense is still assigned a role, but under another name. For example, Harre, in Theories and Things, uses model in the sense of model1 but also refers to model2 under the title of 'parent situation':

"The simple kinetic theory of gases uses a model based on molecules whose properties are derived by analogy from the mechanical properties of common material objects, these constituting the parent situation...."33

Achinstein uses the word 'analogy' and sometimes 'analogue' in place of model2,34 while recognizing that model is sometimes used in this sense35.

Disregarding these differences in terminology and various details of interpretation which vary from one writer to another, it is fair to claim that two opposing claims about theories are the real point at issue in most discussions of the nature and function of models in science. On the one hand are those writers who believe that theoretical statements and the theoretical entities to which they refer are to be understood and derive their meaning solely by reference to their logical connection within a deductive system of propositions with other statements
which refer only to empirical observations and experimental procedures (sometimes referred to as 'phenomenal statements'). To this group belong (a) those who believe, with Bertrand Russell (at one period), that one should prefer 'logical constructs' to theoretical entities, and should substitute the former for the latter, (b) operationalists who believe that all extra-logical terms are definable in terms of operations, i.e. actual observations or experimental procedures, (c) positivists, who believe that all theories and laws are merely predictive or summarizing formulas which serve to link phenomenal elements derived from direct experience, without having any independent meaning or ontological significance. It is not necessarily denied by such thinkers that models of one kind or another in fact play a part in scientific thinking, and in scientific research and discovery. Bridgman, for example, the leading operationalist, says:

...the model is a useful and indeed inescapable tool of thought, in that it enables us to think about the unfamiliar in terms of the familiar.

Braithwaite, who describes himself as a contextualist, says that: ...an understanding of a theoretical concept in a scientific theory is an understanding of the role which the theoretical term representing it plays in the calculus expressing the theory, and the empirical nature of the theoretical concept is based upon the empirical interpretation of the final theorems of the calculus. If such a contextualist account of the meaning of theoretical terms is adequate, thinking of a model of a theory is quite unnecessary for a full understanding of the theory. but he also allows a 'psychological advantage' to the use of models for those who cannot digest an uninterpreted calculus.
By contrast to thinkers like Braithwaite, the modellists, as those who take the opposing view may usefully be called, believe that scientific theories are more than calculi, or predictive or summarizing devices, or deductive systems which generate theorems, which, suitably interpreted or translated by means of a 'dictionary', or correspondence rules, state empirical laws.

The modellists believe moreover that the surplus content or significance of scientific theories is connected with the role and function of models in science. (I include under 'modellists' in this description some, e.g. like Campbell, who do not actually talk of models but whose theories approximate to the modellist account under another terminology.)

The essential modellist claim may be put in a variety of ways. It may be made, as for example by Harré, in the form of a claim that all theories are concerned in a broad sense with the mechanisms of nature and that models are an essential tool in discerning such mechanisms (he uses 'mechanisms' in a wide sense of 'how nature works')\footnote{41}. The modellist claim may be made in the form of Campbell's claim that a theory must have 'meaning' and that it has meaning by virtue of the analogy it displays with established scientific laws\footnote{42}. Achinstein sees the role of a model as attributing inner structures, compositions, or mechanisms to objects or systems so as to explain various properties they have; moreover it is supposed to provide at least some approximation to the actual situation (i.e. it makes ontological claims)\footnote{43}. Hesse claims that models supply to theories a neutral analogy which provides a programme for the extension and development of a theory\footnote{44}. Spector claims that a model gives semantic rules by which we may attribute observational predicates to theoretical terms and hence to unobservable objects\footnote{45}. Gottlind sees models as providing 'intermediary links' between mathematical equations of a high degree of generality (and which are also compressed and employ few symbols) and empirical
phenomena. Nash makes a similar but stronger claim that a model is an essential link between a mathematical theory and experiment, without which the mathematics is not a scientific theory at all. He also claims that models have explanatory functions, and are almost essential heuristically for scientific discovery. Hutten also regards the function of a model as to connect theory with experiment (he does not specify that the theory has to be mathematical), but he also regards one of the chief functions of a model as being to supply a descriptive terminology for theories.

Some of the claims that are made for models see models as essential to theories and theory making, whatever view one might have of what a theory is and how it functions. For example, presumably, whatever one's concept of a theory, it will be regarded as essential that theory must be linked with experiment and observation. Other claims about models depend, however, on what one requires of a theory. If it is not regarded as necessary that a theory be meaningful, or descriptive of structures or mechanisms or capable of extension, development or deployment, then, even accepting that models do have some of the functions mentioned, it will not be thought that they are essential to scientific theorizing.

Probably the chief claim about theories which the modellists' claims are designed to satisfy is that theories should be explanatory, where explanation is regarded as something more than a deductive connection between explanans and explanandum. In turn, whether theories are meaningful in the required sense, or whether it makes sense to talk of explanation in relation to theories in some sense other than the deductive one mentioned will depend on views about the ontological status of hypothetical or theoretical entities, epistemological beliefs, and other metaphysical beliefs, as well as the resolution of difficult problems about the relation of theoretical to observational
language or about the precise limits of the theoretical/observational distinction.\textsuperscript{51}

As regards ontological commitment, there is at least some evidence that scientists, as opposed to philosophers, are for the most part naive realists and suppose that their theories are meaningful in a direct way and are concerned with and descriptive of some objective physical reality. Scientists do not usually discuss the question much, but the famous physicist Max Born makes the following claim in an article called 'Physical Reality':

\begin{quote}
All great discoveries in experimental physics have been due to the intuition of men who have made free use of models, which were for them not products of the imagination, but representatives of real things.\textsuperscript{52}
\end{quote}

This not only makes his ontology clear but explicitly connects it with the use of models. He goes on to claim that Nils Bohr, another great physicist, 'has repeatedly and emphatically said that it is impossible to describe any actual experiment without using ordinary language and the concepts of naive realism'.\textsuperscript{53}

Since it is not possible to argue out such questions within the scope of this thesis, I shall simply observe that views about models and related views about metaphor do have some dependency on certain other beliefs about the structure and significance of scientific knowledge, and that modellist views tend to be offered as alternatives to e.g. positivist or operationalist views. Indeed certain lines of argument, such as that models are essential to connect theory and experiment obviously explicitly reject the claims of e.g. logical constructionists. On the other hand there is nothing intrinsically incompatible with positivist views in such a modellist claim, unless it is coupled with beliefs about the ontological commitment involved, which run counter to positivist claims.
It now remains to give as general an account as is possible of modelist views and to relate modelist accounts to my previous discussion of analogy and analogical acts.

The problem of giving an account of models which is sufficiently general to comprehend the wide variety of writings on the subject has already been mentioned in discussing the different senses to be attached to the word 'model'. The complexity of the situation is however greater than that analysis would indicate. Even disregarding the complex taxonomy of models proposed by Harré in *The Principles of Scientific Thinking*, where he distinguishes eight or nine different kinds of model (giving them such names as paramorphs, homeomorphs, teleiomorphs, etc.), there are a large number of different kinds of model mentioned and discussed in the literature on models; as the following sample indicates: 'logical', 'analogue' (used as an adjective), 'functional', 'mathematical', 'theoretical', 'physical', 'formal', 'material', 'archaic', 'auxiliary', 'main', 'post hoc', 'complementary', 'phenomenological', 'scale', 'simplifying', 'abstractive', 'structural'.

From the point of view of this thesis, the two most important kinds which need to be discussed are the mathematical and the theoretical. It will not materially affect our discussion if we regard other names encountered in the literature as near-synonyms for these two. For example, 'functional', 'arithmetical', and 'phenomenological' are near synonyms for 'mathematical'. There is a problem in relation to the use of the word 'formal'. In practice most formal models discussed are mathematical; 'formal' is therefore sometimes used as synonymous with 'mathematical'. On the other hand 'formal' is frequently used to distinguished the syntactic, or logical, part of a theoretical model from its semantic non-logical detail. The problem is further complicated by the tendency of some writers
to regard 'mathematical' models as a special type of 'theoretical' model. By contrast Hutten denies that there is such a thing as a wholly mathematical model. Brodbeck has an extensive discussion of the variety of possible interpretations to be found of 'mathematical'. Some of these points will arise in the subsequent discussion, but, in general I shall use 'mathematical' in a straightforward way either to refer to a model\textsubscript{1} or a model\textsubscript{2} which is wholly or predominantly mathematical, or else to refer to the mathematical part or structure of a model\textsubscript{1} or a model\textsubscript{2}; I shall assume that the context will indicate sufficiently whether I am referring to the model as a whole or to only the mathematical part. I shall regard 'mathematical' models as typical or representative of 'formal' or 'logical' models, or of the formal or logical part of them, while recognizing that 'formal' and 'logical' have a wider comprehension than the mathematical.

In general, also, the subsequent discussion will concern what many writers treat under 'theoretical' models. However I shall use the term 'model' in a wider sense than some writers on theoretical models so as to include any imported analogue which serves to formulate a topic analogue.

Before proceeding to a discussion of modellist claims I shall indicate how the modellist account may be assimilated to my account of analogical acts and imported analogy. This may be done, I think, along the following lines:

1. An imported analogue which formulates or reformulates some data of science, whether these are phenomena, or previously established laws or theories (or combinations of these) is a model\textsubscript{2}. Models\textsubscript{2} range from simple concepts or complexes of concepts drawn from ordinary experience, with connotations which are not normally precisely, exhaustively or formally stated, to extensive formal systems of propositions. At its most formal and skeletal a model\textsubscript{2} may be a calculus, comprising
relata defined only by and related purely by logical or mathematical relations. Or it may be a calculus embodied in some interpretation. In any case a model must be conceptually amenable and manipulable in the sense that the contingent semantic features of its relata (if any) and the logical and semantic features of its relations are well known and handled with facility, and that these relations and relata are describable in an already understood vocabulary or symbolism.

2. The role of the model is importantly not limited to formulating the topic analogue within the limits of the immediately evident positive analogy, which provides the ground for the analogical act. Both the model and the topic analogue commonly have contingent semantic and syntactic associations which permit the further deployment of the model in relation to the topic analogue, once the model has a foothold in the topic analogue.

3. Fundamentally the same principle is involved in the exploratory procedure of deployment whether it is done by reason, observation or experiment, and whether it is fairly immediate and is done terms of what is already known about either analogue or by more protracted procedures of discovery.

4. In so far as such a process is capable of successfully formulating the as yet unexplored and therefore neutral analogy between the model and the topic analogue it is capable of providing argument by and, potentially, explanations by analogy. But such a procedure is so immensely contingently dependent on the contextual details of both the model and the topic analogue that it seems both odd and unnecessary to evaluate any such specific argument by purely logical criteria.

5. Again it is a fairly complex pragmatic problem how far (in the face of any negative analogies which are already known or which the process of deployment turns up) one is prepared to
adopt and qualify the terms and symbolism of the model \(_2\) in describing the total resulting formulation of the topic analogue.

6. In any case one of the following is necessary if the formulation of the topic analogue is to be made comprehensible:

(i) the formulation of the topic analogue is described in terms drawn from the vocabulary and symbolism of the model \(_2\) but these terms are qualified either by the addition of qualifying expressions or by being given new definitions qualifying previous meanings of the terms.

(ii) equivalences are stated between the terms and symbolism of the model \(_2\) and the topic analogue.

(iii) the descriptive vocabulary of the model \(_2\) is appropriated by the topic analogue without explicit qualification, although such a qualification may be implicit in the different context of semantic and syntactic associations of the topic analogue.

7. In any case the significance of the symbolism and relations and the meaningfulness of the descriptive terms of the model \(_2\) are extended to the topic analogue.

8. The extension of the symbolism or descriptive vocabulary of the model \(_2\) sometimes results in novel explanatory assumptions about the structure, processes, configuration or mechanisms of the phenomena involved in the topic analogue and the laws and experimental or observational data are differently understood and/or reformulated in the light of these assumptions. The qualified symbolism and vocabulary of the model \(_2\) which comprise these novel explanatory assumptions are the model \(_1\) or part of the model \(_1\) of the theory of the phenomena of the reformulated topic analogue.

It will be noted that there is nothing in the preceding account which serves to distinguish in a *prima facie* way models which have provided a satisfactory and acceptable basis
for scientific progress from models such as that of the microcosm/macrococosm which have been misleading. It is a favourite theme of the critics of models and analogies that they are often dangerously misleading, and this cannot be denied. However, this relates to my previous claim that analogical acts are not, initially, analogical arguments, and whether they lead to scientific progress is a separate problem, relating to evaluation. But consideration of this will be given later. I mention it at this time to make the point that although the literature on models usually deals with examples which have been at least partially successful, examples may equally well be given from the numerous examples in the history of science which have turned out to be mistakes or to lead to dead ends.

Examples which are commonly given from the history of science of models are:

<table>
<thead>
<tr>
<th>Model₁</th>
<th>Model₂</th>
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<tbody>
<tr>
<td>Planetary systems</td>
<td>Bohr–Rutherford atom</td>
</tr>
<tr>
<td>Container of billiard</td>
<td>Kinetic theory of gases</td>
</tr>
<tr>
<td>balls in motion</td>
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<tr>
<td>Water waves</td>
<td>Light waves</td>
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<tr>
<td>Division of a liquid</td>
<td>Nuclear fission</td>
</tr>
<tr>
<td>drop</td>
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<tr>
<td>Incompressible fluid</td>
<td>Electric Field</td>
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<td>flowing through tubes</td>
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<td>of variable section</td>
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<tr>
<td>Society</td>
<td>Organism</td>
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<td>Organism</td>
<td>Society</td>
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<tr>
<td>Malthusian struggle in</td>
<td>Natural selection</td>
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<td>society</td>
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<tr>
<td>Machine</td>
<td>Animal and human body</td>
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<td>Ocean</td>
<td>Atmosphere</td>
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<td>Computer</td>
<td>Brain</td>
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Some of these are examples of successful relatively permanent models, such as the wave model for light and the kinetic theory of gases. Some, like the vortex theory of atoms, have undergone eclipse. Others, like the evolution of society on the biological model, have had varying fortunes.

It is also evident that where the model has formed a relatively permanent contribution to scientific thinking, there has sometimes been a transfer of terminology from model$_2$ to model$_1$, as in the case of light and electric current, and sometimes the meanings and significance of the terminology of the model$_2$ has been retained by means of equivalences, as in the kinetic theory of gases.

A principal point of disagreement between modellists and others is on the question of whether models are only heuristic or play a permanent role in scientific understanding. A variety of arguments have been put by modellists to support their contention that the model plays a permanent role.

These arguments are to some extent interconnected, but nevertheless are discrete enough in emphasis or point to be stated separately. They relate to the following points:

1. **Explanation.** Scientific theories are required to provide explanations as well as serve as predictive or correlating formulas. Only models provide explanations.

2. **Meaning.** Theoretical terms and theoretical propositions have meaning apart from the meaning they acquire indirectly by virtue of their relations to other terms and propositions which latter have meaning because they are descriptive.
directly of observations and experiments. Models supply meaning to theoretical terms and propositions directly.

3. **Ontology.** It is only by means of models that physical properties may be attributed to the subject matter of theories. Similarly it is only through models that questions about reality in relation to theoretical systems arise.

4. **Insight.** The insight or disclosure which a model affords, while it may be an essential part of the heuristics of scientific discovery, is not for this reason dispensable, but forms a permanent link in scientific understanding.

5. **Logic.** Models play an essential logical role by providing uneliminable links in the deductive structure of a theory, particularly in linking theory to observation and experiment.

6. **Prediction and Deployability.** Models enable theories to be more widely or strongly predictive than they would otherwise be. They are also essential to the extension or development of theories, or at least permit a greater development or extension of theories than is possible if the theories are comprised solely of a deductive system.

7. **Simplicity.** Models play an essential role in reducing the multiplicity of phenomena to conceptually amenable systems.

We shall now consider these points in more detail:

1. **Explanation.** There does not seem to be much doubt that science is expected or required to be *explanatory* in some stronger sense than providing only predictive or correlational formulas, or providing formulas from which predictive or correlational formulas are deducible. Although there may be areas of science, e.g. in Quantum Mechanics, where some scientists have renounced or denied the possibility of consistent explanations which are plausible in any sense stronger than that of providing statistical predictions or correlations, this takes place in a framework of physical ideas which are generally taken to have explanatory value in a stronger sense, and in any
case Quantum Mechanics is atypical in this respect of science generally.

In *Foresight and Understanding* Toulmin presents a good case for regarding the aspiration to explain in a stronger sense than to predict, correlate or provide logical premises, as the hallmark of science. He argues that science seeks to make sense of regularities by assimilating these regularities to what he calls 'ideals of natural order'. It is in conformity with convictions of this kind about the function of science that modellists see models as essential because they have the required kind of explanatory value. Workman, for example, in an article on explanation, claims that the purpose of explanation is to make phenomena seem 'natural' (in Toulmin's sense). That is, they are shown to be explicable in terms of principles and laws which are themselves acceptable as basic and not requiring a further regression of explanation. Another, and related, way of conferring naturalness, Workman argues, is by 'transfer of naturalness from something already accepted as natural'. In relation to this he says:

**Explanations involve an inescapable use of analogy.**

This is partly because the unobserved part of the description in an explanation, being unobserved, cannot be directly described. It must be verbalized and conceptualized in terms of other experiences. The unobserved element of an explanation must be described as being like such and such states of affairs.

.....The analogy first gives a package of characteristics to a concept and then one by one they are subtracted.

He goes on to say that, provided this process does not go beyond certain unspecifiable limits, the unobserved will retain 'accidental properties', automatically provided by analogy, which will confer naturalness upon it. We may I think read 'model' here for 'analogy' especially since Workman
uses the example of the kinetic theory of gases.

A similar emphasis on analogy and model in relation to explanation is given by Boden in an article entitled 'The Paradox of Explanation'. After arguing that a seeming paradox is created by the fact that an explanation of $X$ cannot be not-$X$, since one cannot deduce $X$ from not-$X$, nor can the explanation be $X$, since no explanation is offered by showing that a thing is itself, she goes on to resolve the paradox by claiming that $X$ can be explained by $Y$ if it is shown that $X$ is both like and unlike $Y$:

\[ \ldots\text{making a phenomenon or concept more intelligible will involve showing its relationships to those things which are rather unlike it, as well as showing how it fits into its own logical category or stratum}.\]

Elsewhere she says:

\begin{quote}
Explanation works by analogy, but the analogy may be more or less marked, and may be analogy of content as well as analogy of structure. When the analogy is well marked in terms of content, or observable characteristics, we speak of a model. \cite{68}
\end{quote}

The supposed explanatory value of a model is frequently stated or assumed in the literature about models, and is often central to the account that is given \cite{69}.

2. Meaning. Campbell puts the point that a theory must have meaning very unequivocally in the following:

\begin{quote}
A proposition or set of propositions is not the same thing as another set to which they are logically equivalent and which are implied by them. They may differ in meaning. By the meaning of a proposition I mean (the repetition of the word is useful) the ideas which are called to mind when it is asserted \ldots A theory is valuable, and is a theory in any sense important for science, only if it evokes ideas which are not contained in the laws which it explains. \cite{70}
\end{quote}
Earlier Campbell has argued that a theory consists of a 'hypothesis' and a 'dictionary' and that the 'propositions of the hypothesis must be analogous to some known laws'. He goes on to say, after the passage quoted above:

The hypothesis gives the real meaning of the theory and involves the analogy which confers on it value; and the dictionary uses the analogy, and the propositions contained in it are usually suggested by the analogy, but it adds nothing to it.

From these statements it is evident that Campbell meaning is necessary for a proper scientific theory and that meaning is conferred by analogy with known laws. However it is also evident in a later discussion of the properties of theories and the explanations they afford that when he is referring to explanation he is referring only to prediction and deduction. Moreover, in the last sentence of the first passage quoted from Campbell above, he obviously does not regard the ideas evoked (i.e. the 'meaning') as identical with or constituting the explanation. For this reason it seems correct to distinguish Campbell's 'meaning' criterion of the value of a model (or 'analogy' as he calls it) from the explanatory one.

A more closely argued statement of how models give meaning to theoretical terms is given by Spector. Spector is concerned to refute a description of the role of models and a definition of model given by Braithwaite. In the course of this refutation he outlines his own concept of a model and its function in science. It is for him a central function of a model to give direct interpretations to theoretical terms. It not only can do this, it must do this, to allow for certain types of physical reasoning. He says:

If a semantical rule is to be successful in giving meaning to a theoretical term in a calculus, it must be stated in a metalanguage which is 'already understood'. That is, it must supply a designatum which is already under-
stood independently of the theory being constructed. 74

An important vehicle for doing this, he argues elsewhere, is the model:

...analogical reasoning from substantive similarities in the designata of terms in the derived formulas of the model and in the theory to substantive similarities in the theoretical (primitive) properties amounts to a direct interpretation of the theoretical terms in the theory. Thus in the case of an identity of derived properties, the completion of the analogy is tantamount to giving semantic rules for the theoretical terms. 75

However, Spector's argument obviously relies on the fact that the terms of the model which correspond to the theoretical terms of the theory, and hence give meaning to them, themselves have a direct meaning. Since he also requires that a semantic rule which gives meaning to theoretical terms must be stated in a 'metalanguage which is "already understood"' 76 and that the metalanguage must be a 'theory-uninfected observation language', 77 he seems to require that the model itself must be describable in terms of such a 'theory-uninfected observation language'.

3. **Ontology.** This has already been mentioned in relation to Born. It is quite possible to hold a modellist view of a sort and deny that models are concerned with physical reality. The position expounded by Vaihinger and which he calls critical positivism is essentially one which admits the necessity for or dependence of science on models and analogies, but claims that they are heuristic fictions. 78 However, Vaihinger interprets 'heuristic' in a wide sense to mean 'necessary for understanding' rather than 'useful for discovery'. He is quite unequivocal about the ontological status of fictions of this kind:

It must be remembered that the object of the world of ideas as a whole is not the portrayal of reality - this would be an entirely impossible task - but rather to provide us with an instrument for finding our way
about more easily in the world. 79

...true fiction, formulated in a strictly scientific manner, is always accompanied by the consciousness that the fictional idea, the fictional assumption, has no real validity. 80

Similarly Tricker, after discussing the role of models in science, comes to the following conclusion:

Looking upon a scientific theory as an analogy also offers no unwarranted belief in the ultimate truth of its picture or of the objective existence — whatever may be meant by that term — of the entities it postulates. Science has no necessity to insist upon the fundamental veridity of the pictures it proposes, whether concrete, as in the fashion of the last century, or in the mathematical mode of the present. 81

However, modellist claims are more commonly associated with a recognition that models are concerned with reality (physical reality) and that at least some of them make existence or ontological claims. Black observes that both Kelvin and Maxwell sometimes advanced to an 'ontological commitment' in their use of models 82. Both Spector and Achinstein criticize Braithwaite's concept of a model adversely on the grounds that his formal concept of model does not admit the possibility of a model making any claims to represent reality. Spector, e.g., says: 'However.....these questions do legitimately arise for some systems which physicists would recognize as models; and since Braithwaite's explication of the concept of model cannot allow for this, it is inadequate' 83. He goes on to include in his analysis of models the following two kinds: a model which has some 'substantive similarity' to the theoretical objects of the theory 84, and a model where the 'question of the reality of the model.....is not only significant but is answered in the affirmative' 85. Achinstein also attacks Braithwaite along
somewhat similar lines.

McMullin bases his argument for the reality of a model on the 'surplus content' of a model. He says:

The good model has a surplus content which enables the theory based on it to survive challenge and extend in all sorts of unexpected ways. This is why it is false to say that science is no more than a summary of previous experience. The presence of this surplus content is our assurance that the model-structure has some sort of basis in the 'real world'. For what is reality if not the reservoir from which such a surplus is drawn.

Harré's whole account of models in Theories and Things is designed to refute positivist claims and to substitute for them a doctrine of 'ontological depth'. As part of his argument he states that models are 'candidates for reality' and that this implies the following possibilities:

(i) some models are rejected as real mechanisms;
(ii) for some, their existential status is as yet undetermined;
(iii) for some the question of their reality has been settled favourably.

The essence of Harré's claims is that things 'exist' in various universes of discourse and that models sometimes give rise to existence claims for theoretical objects which satisfy the following criterion of existence:

A thing of a certain kind exists if it fulfils our set of stable expectation for things of that kind.

As we shall see later the full explication of Harré's ideas relates to his ideas about metaphor in science.

As is evident from Harré's discussion, to some extent the problem of ontology turns upon what one counts as real or existent. Ramsey takes the rather radical position of claiming that a successful model (or metaphor) in a sense creates its own
ontology:
Models are neither mnemonics nor jingles; neither black sheep nor skeletons in the cupboard. The English physicists of the nineteenth century were right in wanting some 'ontological commitment', some 'real existence'; they were only wrong, but badly wrong, in thinking this could be given descriptively. It is this error which the contemporary use of models makes evident and spotlights and is determined to avoid. The ontological commitment arises in a disclosure, and the model provides us with its own understanding, and its own inroad into, what the disclosure discloses.

From our discussion it is clear that, at least for some writers, the ontological problem is of central importance in relation to models.

4. Insight. The question of insight relates directly to my description of an analogical act. In general not much attention or stress is given to the origination of models in the literature on models. As a result the relation between model and theory or topic analogue is often discussed as if it were a matter of straightforward comparison, which takes for granted the relevance of the model and the existence of points of resemblance between the model and the data (whether these be phenomena, laws, theories or combinations of these). However the whole point of my previous prolonged consideration of imported analogy is to stress the creative aspect of imported analogy and the novel formulations it achieves. Paradoxically it is often those who deny any essential role to models and analogies in science who stress the heuristic role of models in the processes of invention and discovery, but they do so in order to make the point that models are no more than heuristic. Nevertheless, the creative act involved in analogical thinking would perhaps not be denied by most modellists. Dambska begins an article on models
by saying:

The employment of analogy seems to be a characteristic form of all creative activity of the human spirit and in cognitive operations a natural, almost instinctive, means of enlarging and transforming our knowledge.\(^{93}\)

Ramsey stresses the function of the model in giving insight. For this reason he centres his discussion around what he calls 'disclosure models', of which he says:

\[\ldots\text{the model must somehow or other chime in with and echo the phenomena. In this way the universe itself authenticates a model. The model arises in a moment of insight when the universe discloses itself in the points where the phenomena and the model meet. In this sense there must be at the heart of every model a 'disclosure'.}\] \(^{94}\)

Later he says:

The contemporary use of models in science or theology - models which are not picturing models - points us back, then, to that moment of insight where along with a model there is disclosed to the scientist or theologian that about which each is to be, in his characteristically different way, articulate.\(^{95}\)

In several places Ramsey says that the model enables us 'to be articulate'.\(^{96}\)

It seems to me that it is the retention of insight which is an essential part of scientific understanding and enables other people beside the scientific discoverer himself to understand the discovery. It is equally implicit in the claims of Spector that the model permits meaning to be given to theoretical terms, in the claims that models provide explanation, in the claims that models provide essential links between theory and observation or experiment, and in the claims about the ontological significance of models that the insight provided by the model is more than an heuristic device and becomes a permanent element in scientific understanding, without which
we could not see why the advanced formulas and calculi and the abstract theories of science have any relevance to phenomena.

Although he would no doubt not subscribe to normal modellist claims, Mach makes a relevant and correct point when he connects the communication of scientific ideas with the mode of scientific growth. About growth he says: 'like all other sciences physics lives and grows by comparison.'\(^7\) He then goes on to say that phenomena may be described in terms of such comparisons:

What a simplification it involves if we can say, the fact A now comports itself, not in one, but in many or in all its features like an old and well-known fact B. The moon comports itself as a heavy body does with respect to the earth; light like a wave-motion or an electric vibration; a magnet as if were laden with gravitating fluids, and so on. We call such a description an indirect description... We thus see, without difficulty, that what is called a theory or a theoretical idea, falls under the category of what is here termed indirect description.\(^8\)

Schön, in his book *Invention and Evolution of Ideas*, has some remarks on metaphor which may equally be applied to model (indeed Schön uses 'metaphor' in a wide sense to include what we should ordinarily call 'model'):

...we sometimes use metaphors as devices for explaining theory when they were actually essential to the formation of the theory. In discussing a theory of genes, the lecturer may say, 'Think of it, if you will, as a kind of code,' when in fact he has no other way of thinking of it.\(^9\)

Nash makes the same point in a discussion of analogy:

To wield the theory effectively, clearly we must somehow grasp its postulates - and this we always seek to do by relating them analogically to things or situations that
Today we employ a diversified array of physical models that, in some cases, may figure only implicitly in a theory constituted by little more than a set of equations. However vague, distant or incomplete may be the analogy—however deeply hidden in the mathematical formalism may be the physical model—always they are indispensable to our grasp of the theory.

This connection between the mode of discovery and the structure of belief is stated in two equally striking similes by Hutten and Poincaré. Hutten says:

Physical theory developed by gradually overcoming the limitations of the original model; but this does not mean that we no longer use models in physics, nor even that the original model is completely abandoned. Some connexion with the original model is retained; the model is re-designed and, thus, refined in order to agree better with our latest experiments. What remains is the wave equation, together with a minimum interpretation in terms of experience upon which the application of the equation rests. It is like the grin and the Cheshire cat; the picture of the cat has receded into the background, but knowing that there was once a cat we understand that the residual phenomenon may be interpreted as a grin.

Poincaré says:

Perhaps you may think I use too many comparisons; yet pardon still another. You have doubtless seen those delicate assemblages of silicious needles which form the skeleton of certain sponges. When the organic matter has disappeared, there only remains a frail and elegant lacework. True, nothing is there except silica, but what is interesting is the form this silica has taken,
and we could not understand it if we did not know the living sponge which has given it precisely this form. Thus it is that the old intuitive notions of our fathers, even when we have abandoned them, still imprint their form upon the logical constructions we have put in their place.

I have discussed this point and illustrated it at some length since I believe it is of vital importance in understanding the modellist and metaphorical claims that are made in relation to science. To put it in a somewhat aphoristic way so as at once to call to mind and vie with a similar-sounding positivist aphorism, it might be said that in scientific theory and scientific explanation an essential part of the meaning and of the logic of the theory or explanation is the formulating analogy which leads to its discovery. Against such a claim it might be alleged that it makes the most elementary mistake of confusing and conflating two quite separate and distinct things: the means of discovery of a theory, explanation or law, and the proof or justification of that theory, explanation or law. A counter-claim to this argument would, for the modellist, I think run somewhat as follows. Except in the sterile or arbitrary way of supporting a theory or law by any axiomatized logical or mathematical system which takes our fancy, scientific proof or justification always involves the integration of any new scientific theory or discovery into what we know or accept as known already. And in this sense of proof or justification, which involves an inference carrying conviction rather than just a logically respectable (or valid) deduction from some premises or other (no holds — or rather premises— barred) it is an essential factor that what brings conviction to the discoverer is also what will bring conviction to everyone else, so that the discoverer's analogical acts must in some essentials be repeated by those who seek to understand the
purport of such discoveries or theories.

It is interesting that, in spite of his supposed anti-modellist stance, and his advocacy of formal logical criteria for scientific theories, Duhem makes the following statement:

To give the history of a physical principle is at the same time to make a logical analysis of it.

It seems to me that we may very well make sense of this dark saying in terms of the aphorism stated above.

5. **Logic.** There are two ways in which modellists attribute logical importance to models in relation to theories: (i) the logical structure of the theory, it is claimed, is determined by or derived from the model, (ii) it is by means of models that logical or quasi-logical links are established between theory and empirical propositions.

There are three ways of presenting the first type of argument: (a) by arguing that in many cases there are an infinity of mathematico-logical structures which would yield theorems corresponding to the empirical data, and it is by means of models that a choice is made among these, (b) by showing that theories are required to conform to other criteria than the formal logical one of yielding theorems which correspond to empirical data, since this formal logical criterion will not distinguish between what is by general consent a genuine example of a scientific theory and a 'theory' about the same phenomena which is by general consent either trivial, absurd or metaphysical, (c) by showing from historical examples that logical structure is as a matter of fact derived from models.

(i) (a). This type of argument is not usually very explicit in relation to models, although in essence it is an argument frequently used to provide a logical justification for argument by analogy in general. Justification of argument by analogy on purely logical grounds seeks to show that any
hypothesis which displays an analogy with established laws is \textit{ab initio} more probable than one which does not.\textsuperscript{108} While such demonstrations are no doubt interesting on purely logical grounds they hardly appear of much importance to science since science is never content to rest its case on the formal logical probability of a hypothesis, and in any case a mere principle of the superior probability of any analogically-based hypothesis does not help in choosing a particular hypothesis. Attempts to choose between various analogically-based hypotheses on logical grounds have been made. Hesse, for example, discusses this question in relation to material analogies (analogies which involve analogy between observables), on the grounds of inductive support, probability, falsifiability and simplicity, but concludes that although it is in general possible to justify the use of models rather than hypotheses which are not analogically based on these grounds, it is not possible to choose between such models on these grounds.

It seems to be implicit in some writings on models that models are in fact used to distinguish between hypotheses, or at least to reduce the infinity of possible hypotheses. Rosenblueth and Weiner, for example, discuss the solution of 'closed box' problems in science and describe the solution of such problems as involving the use of theoretical models, which leads to what they call 'progressive concretization'.\textsuperscript{109} In a commentary on this Turner says:

\begin{quote}
\ldots Rosenblueth and Weiner, without mentioning Maxwell, generalised his observation (see below) to the observation that any kind of explanation permits of an infinite number of solutions.\textsuperscript{110}
\end{quote}

Here Turner is referring to the claims of Maxwell, usually attributed to Poincaré as their originator\textsuperscript{111}, that problems
such as that of the dynamical explanation of electro-magnetism permit an infinity of possible solutions. In order to distinguish among these one must have independent evidence about the hypothesis put forward. Such independent evidence is, however, furnished for Maxwell by putting forward physical hypotheses involving the postulation of material systems whose properties can be investigated independently. Maxwell also suggests that more general solutions to such problems can be found which do not commit one to any particular physical hypothesis, but Turner shows that this generalized solution nevertheless rests on certain dynamic analogies from mechanics. It is evident, then, that in both cases Maxwell relies on models to reduce the infinity of possible solutions.

In those areas of physics where claims are often made for a purely mathematico-logical theory to account for phenomena, e.g. in quantum theory, it still seems to be the case that models of some sort are employed. In an article by Bunge which sets out to refute the claims of analogy to more than heuristic importance in quantum theory, he nevertheless admits that to prove a theorem in quantum theory one 'adds a definite model to the general assumptions'. Bunge's point seems to be that such models are not part of the general theory, and are only used 'heuristically'. However he uses 'heuristically' in the unusual and wide sense of 'permitting the application of general theory to specific physical theory'. Moreover the revised terminology he suggests for quantum theory in order to avoid analogical associations covertly refers to concepts such as 'quantum' and 'field' which seem to rely on some sort of model of physical reality, no matter how abstract. Also in his discussion of the 'literal' interpretation of a physical theory he refers to the mapping of the symbols of the theory into the set of their physical partners, giving as examples of the latter set an 'atom', 'atom angular momentum' and 'a jump in the atom
angular momentum value. Since these 'physical objects' certainly depend on that ubiquitous model the atom and the very possibility of mapping in the way required would hardly be possible without some correspondence between the theory's symbol and this model, it looks as if a model of some kind is implicit in the theory. Hesse in 'Operational Definition and Analogy in Physical Theories' argues that even where quantum theory is apparently based purely on mathematics, this mathematics functions as a sort of model, and depends upon underlying analogies. In an article 'Models in Physics' in which she discusses the use of mathematical formalisms in physics, Hesse concludes that such formalisms may function like models 'without having any mechanical or other physical interpretation' but the examples she gives of such formalisms nevertheless do rest on some physical assumptions (although very general) and these it might be argued constitute a model of the physical system concerned.

(i) (b). The originator of this argument seems to have been Campbell, who says, of a 'theory' he made up to 'explain' (i.e. by the purely logical criterion of 'supply a deductive premise for') the variation of the resistance of a metal with the temperature:

Any fool can invent a logically satisfactory theory to explain any law. There is as a matter of fact no satisfactory physical theory which explains the variation of the resistance of a metal with the temperature. It took me about a quarter of an hour to elaborate the theory given....and yet it is, I maintain, as formally satisfactory as any theory in physics. If nothing but this were required we should never lack theories to explain our laws; a schoolboy in a day's work could solve the problems at which generations have laboured in vain by the most trivial process of trial and error.
He goes on to say that the theory is useless because no analogy has been used in its development. The same problem is touched upon by Hesse, but the most detailed discussion is in Achinstein's *Concepts of Science*. Achinstein's account is given in a section called 'On Theory Construction' (but the discussion in this section is part of a wider discussion, which is also pertinent, on the interpretation of theoretical terms in a theory) and in a chapter called 'Theories'. In the course of his discussion Achinstein examines the various criteria of what constitutes a meaningful theory according to the axiomatic or hypothetico-deductive account, i.e., an account which requires theories to conform only to certain logical criteria. This is Achinstein's statement of such a viewpoint:

On the axiomatic account a theory is no more and no less than a set of sentences ('axioms') stated in a specified vocabulary (containing primitive and defined terms) together with all the consequences ('theorems') of this set plus the proofs of these consequences. In short a theory is a hypothetico-deductive system. Achinstein examines various criteria proposed to ensure that theories of this kind are meaningful or significant and concludes that none of these is satisfactory and that the inadequacy of these criteria is related to the positivist belief that terms interpreted as referring to something unobservable are either unintelligible or scientifically meaningless. Achinstein says, in relation to this:

Such claims are unjustified. The alleged problem of meaning is spurious because it is based on the unwarranted assumption....that terms interpreted as referring to unobservables are unintelligible. Nor have proponents of the Positivist account shown that their new concept allows them to distinguish legitimate from illegitimate metaphysics. Quite the contrary, since
given the explicated concept of meaning and the
criterion or criteria they propose, we have seen that
almost any term in any theory becomes meaningful.
Finally, the explicated concept of meaning does not
allow a more adequate reconstruction of theories, since
it renders the task of theory construction a trivial
one.\textsuperscript{127}

Achinstein notes Campbell's claim that theories must display
an analogy with known laws, but correctly rejects this as a
sufficient criterion since "we may construct any set of
postulates $T$ we like, making them similar in form to any known
laws we wish."\textsuperscript{128} Achinstein then goes on to show how theories
can be given significance by using terms "which are already
available and are used in connection with observables" or by
"attributing to the unobservable items properties that are
not identical with those attributed to observable ones but
similar in possibly many respects."\textsuperscript{129} Previously he has
said: "in this connection we might mention the importance of
analogies in understanding the interpretation of terms,\textsuperscript{130}
(as we have already claimed, Achinstein uses 'analogy' in the
sense of model)."

Elsewhere, in his own criteria for what constitutes a
theory, Achinstein mentions the following: (1) it is normally
believed that the theory:

provides, or will eventually provide, some (or a
better) understanding of something and that this is
or will be one of the main functions (of a theory)....

By providing an understanding I mean something quite
broad that can be done in a number of related ways,
for example, by explaining, interpreting, removing
a puzzle, showing why something is not surprising,
indicating a cause or causes, supplying reasons,
analyzing something into simpler, more familiar, or
and (2) a theory 'consists of propositions purporting to assert what is the case'.

We have already noted that providing explanations and describing physical reality are functions which have been claimed for models, and Achinstein himself in a later discussion of analogies (models) says:

Analogy is employed in science to promote understanding of concepts. They do so by indicating similarities between those concepts and others that may be more familiar or more readily grasped. They may also suggest how principles can be formulated and a theory extended.

We can see in Achinstein's account how claims about meaning, explanation and ontology are all related to the rejection of purely logical criteria for the formulation of theories, so that the actual logical structure is seen to be determined or selected out of the logical possibilities by some extralogical criteria. Achinstein makes no sweeping claims for models in this respect, although he does recognize their functioning in this way. However, other writers make the stronger claim that it is models which do determine the logical structure.

(i) (c). Some writers on the role of models in supplying logical structure cite examples to show how in fact this happens in particular cases, and generalize from these cases. Hutten, for example, uses such examples as wave, oscillator, particle and heat engine to show how models determine the logical or mathematical structure of a theory. For example, he says:

...the model gives the syntactic (including the mathematical) rules for using the expressions by which we describe the process. The model is the better the
more completely it provides the syntax of the new theory. 133

In another work Hutten says:

....it is exactly this procedure that is so typical of science: to start with vague concepts which are made more precise in subsequent theories. The model, visual or 'abstract', enables us to follow this procedure until, possibly, the concepts are clear enough to construct a more formal, and formalised theory. It is in this manner that we make use of experience, both to satisfy our psychological requirements and in order to proceed to a logical system. 134

Nash uses the examples of Mendelian theory and Toricelli's theory of the atmosphere to illustrate the following claim: Until quite recently the vast majority of all important scientific theories have been founded on the concept of some physical model, and make no explicit reference to any formalism. Often syntactic rules may well pass unmentioned because the theory requires, and simply assumes, no more than the simplest kinds of everyday reasoning. But even where more elaborate formal operations are involved they may give little or no overt notice. How then do we acquire the formalism needed to complete the theory?.....the scientific theory that apparently proposes nothing but a model actually conveys an implicit formalism. 135

Byerly uses examples such as the kinetic theory of gases to illustrate his thesis that 'model-objects' determine 'model-structure'. According to Byerly 'a model-structure.....is a definitely specifiable scheme of mathematical-logical relationships', and a 'model-object' is 'any sort of object from which a structure might be abstracted'. 136. Model-objects are used by scientists to 'isolate the significant structure
from a set of data. He goes on:

The scientist begins, I suggest, with an informal theory, with a set of assertions about some subject matter. These assertions are generally intended to reflect some noted or suspected regularities: the initial assertions are causally organized. The scientist then tries to construct a more precise theory for which the experimental data will, hopefully, provide a model-object.

Previously he has said: 'The model-object "carries" as it were the logical-mathematical structure of the model-structure.' Later he says, in relation to explanation:

"...explanation takes place in deductive systems which are commonly suggested by model-objects..." We use...causal relationships among characteristics of model-objects to suggest the law-like relationships among characteristics imputed to theoretical entities.

Once again we notice the ontological commitment (he describes his position as 'a realist view of theoretical entities') and the connection with explanation.

(ii) The other main way in which models are seen to be important logically is in connecting either calculi or theoretical propositions with empirical propositions. Hutten says:

'It is important to emphasize, I think, that the model connects theory with experiment. We may say that we explain, and possibly test, the theory in terms of a model; but we do not regard either the theory or the test made (i.e. the experiment) as the model. The model is, then, not an application of the theory: rather we apply the theory with its help.'

Nash stresses a different, though perhaps connected, function of models. They determine the course of deduction
in various ways. First, by providing 'thematic process', i.e. synoptic grasp of what the deductive system is about and hence how it should be organized. But the model has a more specific logical function:

Even where we need to make no approximations, our use of the syntactic rules of the formalism seems always affected by our awareness of the semantic rules we draw from the model. No scientific theory meaningfully considered as such has yet been completely formalized, and it is much doubted that any such theory ever will be. Quite generally we find in making our derivations, that nothing in the formalism supplies, or even implies, what we need to pass from one step to the next. We make that passage all the same, using assumptions borrowed, on the strength of the model, from other branches of science or from common sense.

Some modellists, however, deny that the model plays a logical role, although they assign a similar role to the model as Hutton does. This seems to be a difference of terminology only. Theobald, for example, says the connection provided by the model is 'not logical but stipulative':

Let $T$ be a theory, and $t_1$, $t_2$ descriptions of physical states in the theoretical language. Then

$$ t_1, T \Rightarrow t_2 $$

....Now we need some connexion between the theoretical descriptions $t_1$, $t_2$ and the experimental or phenomenological descriptions $o_1$, $o_2$. For this purpose we set up a stipulative connexion (interpretation rule) $S$ such that

$$ t_1 \xrightarrow{S} o_1 \quad \text{and} \quad t_2 \xrightarrow{S} o_2 $$

This enables us to compare the theoretical scheme
with its experimental analogy. What does not follow from this however, is
\[ o_1 \cdot T \Rightarrow o_2 \]

It seems rather odd to deny that empirical propositions can be deduced from the theory, and whether one accepts this way of talking depends upon what one counts as a theory, i.e. whether or not one includes the 'interpretation rules'; also what meaning is to be attached to the arrows in Theobald's scheme. On the face of it, it seems as if Theobald believes that there is a one-to-one correspondence between all the items in the theoretical language and the items in the experimental language, and that the theoretical items are only given meaning by means of the interpretation rules. As we have already seen, other modellists, and indeed Theobald himself, when discussing theoretical entities elsewhere, regard theoretical statements as having meaning in a more direct way.

The logical function of the model in connecting theory and experiment is not usually conceived at all in the manner of the 'axiomatic' or 'hypothetico-deductive' account. In this latter way of conceiving of theories the connection with experiment and observation can be made only by way of certain terms in the theory which are construed or translated or interpreted as being observational terms. A notable statement of this viewpoint is Braithwaite's:

What happens in an abstract science is that we use, as in all inference, a calculus which we interpret as a deductive system; but we do not interpret the calculus by attaching meanings to its formulae separately. We give direct meanings to those formulae of a calculus which we take to represent propositions about observable entities; we give indirect meanings to the other formulae as representing propositions
in a deductive system in which the observable propositions are conclusions. Thus we do not interpret the calculus all in a piece, as it were; we interpret the final part of it first, and work backwards towards the beginning. A zip-fastener is a better simile for the fitting of a deductive system to a calculus than is the measurement of a rod by the simultaneous superposition of its ends on points of a scale.\textsuperscript{146}

However, although writers such as Hutten and Nash also distinguish the formal part of a theory from the interpretative part, they see the interpretative part as functioning at all levels of the calculus or deductive system. This way of looking at it, which is also common to Hesse, Spector and Harre\textsuperscript{147a}, considers the relation between theory, and experiment and observation, as more like a row of buttons (to pursue Braithwaite's simile) which join the two sides at all stages of the deductive system; as much at the axiom stage, for example, as at the theorem stage. There is however an indirectness of interpretation at the purely theoretical level, which is of major importance for the metaphorical point of view. This indirectness is a function of the use of models. Nash calls it 'indirect notation' and relates it to what he calls the 'hierarchic model'. This is his account:

From the extreme abstraction of the formalism that offers neither overt model nor direct denotations for its primitive concepts, we work our way back through a sequence of progressively less abstract theories. Without even thinking about it, we loosely relate the concept of stationary state, in an advanced quantum mechanics, to the far more readily intelligible concept of electronic orbit in the older quantum theory. Thence presumably, we work still further back, to classical mechanics — and perhaps at last to the simplest
common-sense concepts. No one model or analogy here suffices. Only through a whole series of incomplete but overlapping models and analogies do we contrive ultimately to grasp the nature of the primitive concepts.¹⁴⁷b

Hutten says: "We anchor higher theories to experiment through chains of interpretation, i.e. by model and approximation."¹⁴⁸ In The Ideas of Physics he takes several case histories of such 'chains of interpretation', e.g. he considers the concepts of oscillator, rotator and heat engine, showing how there is a connection, through a hierarchy of theories, with ordinary experience, no matter how remote and abstract the concept becomes (even if it becomes simply an equation) in the 'higher' theories. It is this connection which, in the last analysis, enables us to interpret abstract theories into observation and experiment. Here, for example, is his summary of the concept of rotator:

The model for 'rotator' thus changes from a mechanical angular momentum to electron spin, quantised directions, isotopic spin and strangeness. As with the oscillator model, so it too is a series of ever-increasing abstractions in the properties for which the model is invented. We can use the 'abstract' model of strangeness because it is, in some slight way, connected with the original angular momentum. We have an 'abstract' picture of strangeness, by seeing it as a vector that may be added to other spins according to certain rules. The model is now a vector equation, but the rules for the manipulation of the symbols are given with it - that is by interpretation. Without the interpretation, there would be no model.¹⁴⁹

And conversely, we might add, without the model there would
be no interpretation.

On this view there is an epistemological hierarchy which reflects or repeats the historical order of development of theories. We have already noticed this connection in relation to the discussion of insight above.

Both Hutten and Spector stress that the use of models and the interpretation and consequent connection with experiment and observation which models give to theory necessitate the use of a metalanguage\textsuperscript{150}. Spector says:

If a semantic rule is to be successful in giving meaning to a theoretical term of the calculus, it must be stated in a 'metalanguage' which is 'already understood'.....the metalanguage must be a theory-uninfected observation language.....It would seem that familiarity should be sufficient; but let us grant the stronger requirement of observability for the present.\textsuperscript{151}

Hutten observes that the language of everyday life is the usual metalanguage, which would accord with Spector's requirement of a 'theory-uninfected' observation language. However Hutten goes on:

If there are other languages which we know equally well in terms of experience we can make use of them; indeed it is occasionally useful to employ as metalanguage a more abstract language when we wish to develop a scientific language system.\textsuperscript{152}

In practice, one would have thought, scientists do constantly use such 'higher-level' metalanguages in communicating with other scientists to whom such metalanguages are familiar and conceptually amenable. However, these other languages always depend, in the last analysis, on ordinary language for their explication, and will have had to be learned from a basis of ordinary language in the first
place.

There is one last point to be considered, which has already been mentioned above. How far all the interpretative or "translatory" rules which connect theory with experiment and observation are to be regarded as in fact part of the theory, in the sense of completing the deductive system of the theory, is partly a matter of terminology, and at least some of the controversy surrounding theories hinges on this point. Those who stress the calculus, the logical system and the deductive aspect of a theory, and indeed even regard the mathematico-logical structure of a theory as synonymous with the theory (e.g. Duhem and Hertz\textsuperscript{153}, the contextualists, such as Braithwaite, and others), are possibly simply excluding interpretative frameworks, 'correspondence rules' and so on from the theory. Others, however, include the interpretation and the complex structure of semantic connections which anchor mathematics and logical structures to experience. Campbell, for example, a modellist (by our definition), says that a theory consists of a hypothesis and a 'dictionary' (interpretative framework). In general modellists regard the interpretation as part of the theory. As Byerly says, à propos of Hertz's maxim ('Maxwell's theory is Maxwell's system of equations')\textsuperscript{154}:

If Maxwell's equations are considered formally as mathematical functions of numerical variables, we do not have Maxwell's theory at all, any more than the equation for a parabola is Galileo's law for falling bodies. Once interpretation and context are added to the equations, the maxim loses its punch.\textsuperscript{155}

It is this which Hutten has in mind when he denies the existence of mathematical models:

There are, however, no mathematical models in physics: the equation by itself is not the model. The wave
equation is a model only because we know it to represent the spreading of a wave through space. It is the interpretation which is attached to the equation due to previous application that we need for describing our experiments.\textsuperscript{155}

Again, it is this which leads Hash to say: '....a scientific theory is the aggregate of a formalism and a model'.

A good reason for considering the model or the interpretation or the 'correspondence rules' as part of the theory is that, viewed in this way, theories are more strongly predictive and capable of development and change. A stronger reason for regarding them as part of the logical system of the theory is that otherwise one cannot speak of theories as being confirmed or verified, or surviving falsification, i.e. theories cannot be tested or proved. As Fürth says:

It must be clearly understood that the laws for the functional behaviour of a certain model, as expressed in the form of a system of equations, are always 'true', provided there is no mathematical or logical inconsistency within this system. But such a law is not a physical law, unless it is possible to relate the parameters, appearing in the equations for the model, to well-defined and measurable parameters of a real physical system.\textsuperscript{157}

b. Prediction and Deployability. It has been claimed in the last section that unless we regard the interpretative framework or the 'correspondence rules' or the 'dictionary' as part of a theory, it will not be so strongly predictive, or capable of development and change. That models are an important part of theories because they do make the theory strongly predictive, or capable of extension and change is a recurrent theme among modellists. To use my terminology, it is because the model, is an imported analogue which brings with it an associated network of syntactic and semantic detail that it is possible to explore this associated surplus content or
'neutral analogy' as Hesse calls it. Black, Campbell, Harré, Hesse, McMullin, Nash, Spector, Swanson, Toulmin and Tricker all agree in regarding models as important for the extension or development of theories, McMullin, for example, says: The good model has a surplus content which enables the theory based on it to survive challenge and extend in all sorts of unexpected ways. This is why it is false to say that science is no more than a summary of previous experience. If it were no more than this, there would be no surplus content to guide discovery in the way that the Bohr model – and a myriad of other examples could have been chosen – actually did. Braithwaite, who, as we have seen does not regard models as essential to science, conceives of a model as an alternative interpretation of the calculus of a theory, but one which is 'epistemologically prior' (a deductive system is 'epistemologically prior' if its axioms have a direct meaning, from which the meanings of deduced propositions or theorems derive, whereas in a theory the meanings of the axioms are derived indirectly from the deduced theorems or conclusions – see above), and he examines the way in which models (on his definition) are capable of deployment and prediction in relation to theories. He concludes that models contribute towards the development of theories by suggesting 'new hypotheses relating together theoretical concepts of the theory' and 'extra hypotheses containing new theoretical concepts'. He admits that they do this in a way that a calculus in isolation would not do. However, he rejects the claim that the model has a greater predictive power on the rather odd grounds that such suggestions or pointers to extension do constitute a proof, or a safe inference. This rejection obviously depends on interpreting predictive power as entailing confirmation or verification, which is not a claim that any modellist would
make on behalf of models, or would assume in arguing that models have predictive power.

Duhem, in particular, and Hesse, in a qualified way, do argue that calculi alone are capable of being used for extension and prediction. However, according to Turner, Maxwell doubted that results presented solely in deductive mathematical terms are in fact obtained that way, in the manner suggested by Duhem, and Hesse's examples, as we have already indicated, do involve models of a kind, as Hesse admits.

Nash allows that formal models or calculi can lead to discoveries, but has reservations:

Even highly formal theories that dispense with the 'superfluity' of explanation, in terms of overt physical models or analogies can function as heuristic devices. I maintain only that they do not function as well as theories that accept some such superfluity.

A more extended discussion in Hesse's *Models and Analogies in Science* makes clear the way in which models with semantic detail function better than purely formal models or calculi. She argues that it is only by way of material analogies that a theory may be extended to new observables (not mentioned or considered when formulating the theory); and that such material 'pre-scientific' analogies can only be suggested by a non-formal model. It is this which makes a theory strongly falsifiable and strongly predictive. Perhaps one ought to substitute 'pre-theoretic' (meaning 'established prior to the theory under consideration') for 'pre-scientific', since undoubtedly some such analogies are based upon analogies established by previous theories rather than solely on the basis of ordinary language and its distinctions.

In general, it should be admitted, I think, that both material and formal models can serve to formulate and to extend scientific theories so as to lead to prediction, except that
with regard to extension and prediction formal models are restricted to theorems involving the same terms as were originally involved in a theory, and consequently they only permit variations on the same theme rather than the introduction of semantic novelty, unless there are already physical or other semantic associations with the formalism which are suggestive; but then, strictly, one is not dealing solely with a formal model. Girillί67 defends the use of 'formal models' and believes 'intellectual clarification and theoretical expansion,'ί68 can follow from their use, but he does not deny the explanatory and predictive value of less formal models ('physical analogies'). However he describes formal models as being able to 'provide derivational guidelines, didactic clarity, and expanded structural insights' and regards the functions of formal models and 'physical analogies' as being complementary rather than competitive.ί69.

7. Simplicity. The simplifying function of models is not much stressed, but it is perhaps rather fundamental. Furth, for example, says:

There is perhaps no a priori reason for assuming that the number of parameters necessary to describe the 'state' of a physical system is finite: in any case for all 'macroscopic' systems their number will certainly be very large......Hence the number of equations necessary for a complete description of the behaviour of the system in question must also be very large, and the laws of physics, as expressed by these equations, would appear to be extremely complicated......On the other hand most scientists are convinced that the fundamental laws of nature are essentially simple, and the search for these laws is characterised by the attempts to generalise, to simplify and to unify established relationships....
in actual fact the parameters, appearing in the equations meant to express the laws of physics, do not refer to actual physical systems, but to fictitious systems which are constructions of the theoretical physicist and only bear a certain resemblance in their behaviour to real physical systems. 170

It is the positivists who usually stress the simplifying, summarizing aspects of analogies and models. Mach, for example, having discussed how a comparison (based on analogy) involves great simplification in the description of phenomena, goes on to say that a theory or theoretical idea is such an indirect simplifying description (as previously quoted). He then examines the notion of theoretical idea:

....instead of a single feature of resemblance culled from memory, in this case a great system of resemblances confronts us, a well-known physiognomy, by means of which the new fact is immediately transformed into an old acquaintance. Besides it is in the power of the idea to offer us more than we actually see in the new fact, at the first moment; it can extend the fact, and enrich it with features which we are first induced to seek from such suggestions, and which are often actually found. It is this rapidity in extending knowledge that gives a theory a preference over simple observation. But that preference is wholly quantitative. 171

Tricker, who also seems to be a positivist, says:
The analogy thus reproduces the essential features of a scientific theory. In the first place it is capable of playing the role of a summary of experience which is probably the principal purpose which a scientific theory has to fulfil. 172

In summary, then, we have discussed a number of ways in which models are alleged to function in science. Although these
have been treated separately they are in fact interconnected. The modellist would claim that models are important in science because they are instrumental in giving meaning to theories in such a way that the theories are not mere correlating or predictive devices, but give us an insight into physical reality of such a kind that we understand nature in an ordered way, and are furnished with explanations of phenomena. They are also simplifying and summarizing devices, which play an essential role not only in organizing the logical structure of our theories but in completing the deductive link with empirical statements, which enable us to confirm, verify or otherwise test and prove our theories. They are able to do this the more effectively in that they enable theories to be strongly predictive and strongly falsifiable by leading to or at least suggesting laws connecting observables not considered when formulating the theory. As a matter of historical fact models have played a key role in the development of science.

Against these claims are those of the positivists, contextualists and operationalists who regard models as at best heuristic devices, either in the narrower sense of being aids to or instruments of discovery only, or in the wider sense of being devices which may be pragmatically necessary as a permanent feature of scientific theory but which by their very nature cannot be regarded as in any way descriptive of physical reality.
Chapter 3

Before proceeding to discuss metaphorical views of science, it will first be necessary in a preliminary way to consider the nature of metaphor and hence the relation of literal to metaphorical language.

Metaphor and simile may be classed together among the so-called 'figures of speech'. The genus 'figure of speech' is rather ill- and uninformatively named, however, since it is usually defined as 'a deviation from normal speech'. This, apart from being merely negative, leaves one in a puzzle about what a 'deviation' is in this context and how one recognizes 'normal' and 'deviations' from normal. A more positive idea arises if we observe that metaphors and similes are sometimes referred to as 'rhetorical devices' i.e. aids to speaking or writing effectively. We shall perhaps get a better idea of them if we can understand how and why they contribute to effective speaking and writing.

A simile, unlike a metaphor, is characterized usually by a particular form of words. It is a comparison and so uses the form of words appropriate to making comparisons, e.g. 'like.....', 'as.....', 'as.....as.....', However, similes need not have this form (e.g. 'at a snail's pace' is really a simile, rather than a metaphor, although it has a form more characteristic of a metaphor). Moreover one would hesitate, and rightly, to say that every comparison or even every expression using the form of words cited is a simile. Certainly, for example, comparisons of degree are not similes. 'Harry is as big as John' is not a simile but 'Harry is as bold as brass' is. A comparison which seeks to identify in a matter-of-fact way is not a simile. 'He looks like a poodle', said of a stray dog, is not a simile, although the same sentence, said of Uncle George, is. These examples show that the mere form of words is not sufficient to characterize a simile.
What is essential is that the things compared should not be those which are normally compared or should not be things which are quite matter-of-factly and literally compared as potentially the same kind of thing or because of their possession of exactly the same properties or of the same property or properties in some different degree. It is true that some similes compare things which differ only in degree, in relation, say, to a given attribute. But in this case the simile is often what might be called a 'dead' (cf. 'dead metaphors') or 'substitutive' simile. For example, to say of someone, 'he runs like a cheetah', is really only a fancy or alternative way of saying 'runs very fast', and except that it may also introduce other genuine novelties of comparison (e.g. 'ran like a rabbit' introduces ideas of fear or panic) almost any rapid runner could be substituted for 'cheetah'.

The purely conventional and substitutive nature of such similes is brought out by the fact that it is not even necessary for one to be familiar with the things compared for such comparisons to be made and for such pseudo or 'dead' similes to form an active part of one's vocabulary. (Not that one wishes to be so austere as to exclude such picturesque and sometimes, on first hearing, effective speech habits from normal discourse.) For example, 'he swears like a trooper' is confidently used by people who are not only unacquainted with the language habits of troopers but probably have not even a vague idea of what a trooper is either.

Since the mere form of words is not sufficient to define a simile and this indicates that similes are not merely comparisons or statements of resemblance, we need to investigate what further character true similes have. Part of the answer, at least, has been anticipated by saying: first, that similes are not normal or usual comparisons, secondly, that they are not matter-of-fact comparisons, and thirdly, that with pseudo
or 'dead' similes the user need not be acquainted with the objects of comparison. For we may say that, by contrast with expressions which formally resemble similes, but are nevertheless not similes, true similes are not usual, matter-of-fact comparisons and do depend on an acquaintance with or knowledge of the things compared.

Taking these in order, a simile must have a necessary element of novelty if it to to be effective as a figure of speech. It does this presumably by affording a new juxtaposition of ideas leading to vivifying and illuminating views of likeness or contrast, to seeing something in a new context or perspective with concomitant effects of making it more actual, more sensually, perceptually or conceptually present and thus attended to by the reader or listener. Moreover, the things compared are not being seriously approximated or classified together for general purposes. They are being brought together in what is perhaps only a glancing osculation although this may well coagulate into a more durable and extensive adhesion. However, even where a simile is so effective that it becomes a permanent cultural property, so long as it remains a simile and the class of things, events, properties or processes compared do not absorb one another or fuse together in general usage and reference, then the things which the simile unites will characteristicly be regarded as different, though similar. In short they will be recognized as not identical, but analogues and they maintain their conceptual distance. Lastly, the points of similarity (which need not be and perhaps usually are not fully and explicitly stated - but see below) are points of similarity which can only be perceived by the reader if the analogues brought together are both familiar enough for him to recognize the point of the simile.

Sometimes a simile can be analyzed into a specific like-
-ness (of attribute, property, quality or relation) without any appreciable loss of comprehension or effectiveness. Or similarly and relatedly, the point of comparison is sometimes relatively single and simple. However, this is never very clear-cut. Because of the unique connotations and sensual and sensible reverberations of words one can never be sure that a given simile can be paraphrased without remainder by merely specifying the likeness intended. For example, a small hat on a large head can be said to look 'like a tomtit on a round of beef'. It might be thought uncritically that such a comparison really amounts to no more than saying that the hat is disproportionately small but there are obviously additional elements (of comic incongruity) involved which melt and vanish under the too intense scrutiny of specific analysis.

Indeed the more vivid and creative and effective a simile the more complex and prolix an analytical paraphrase becomes and the more evanescent and evasive the finer points of comparison become. These fine points flutter on light wings of sense, imagery, insight and feeling and are irreparably crushed and destroyed if we try to pin them down. If we take any effective simile from say a poem we can see how difficult and frustratingly elusive an analysis of its 'meaning' can be.

Take, for example, the following three similes:

....flame streamed out behind, a crimson scarf of, as life blood out of a wound....

....vindictive the cold airs close down like a trap of steel and numb them from head to heel....

....dazed as the dead awoken from death....

All have the essential quality of true simile of not being completely translatable. In other cases it is even more difficult to 'unpack' the comparison involved satisfactorily. Again, take a more famous example:

Life like a dome of many-coloured glass stains the
the white radiance of eternity. We should I think if pressed to be explicit about such a simile be inclined to protest that literal explication is impossible. In the case of such defiance of analysis we should perhaps admit that the simile is essential to the thought. Only the simile can give the necessary perspective, the necessary vision, and can communicate the whole idea intended. It is worth noticing, incidentally, that in the last four examples the similes blend or dissolve into associated metaphors.

It is sometimes said that metaphors are elided similes and this assumption has been denied on the grounds that metaphors cannot be analyzed out into specific comparisons. However, if what I have said is true, both metaphors and similes share this uneliminable property of untranslatability. On the other hand, and perhaps not unexpectedly, metaphors and similes are to some extent interconvertible. Not only are they intermixed, so that there is a slide from a simile to a metaphor within a given phrase or sentence (cf. the examples above) but any given simile can be converted (without loss or alteration I think) into a metaphor. Whether this results in any difference in ontological claim or assertion I leave aside for the moment, merely noting that there does seem to be an important difference in saying (as in a simile) that A is like B and saying (as in some metaphors) that A is B.

There is quite a large literature on metaphor, mostly in a literary context. While I should like to take note in some detail of the sort of analysis to which the notion of metaphor has been subjected, what is omitted and what is included in this account is dictated by the criterion of what I take to be relevance to the scientific use of metaphor.

The first general thing to be noted about metaphors is disagreeably paradoxical. In the sense of being strictly delimitable and isolable elements of given propositions or sentences do not exist. There is no explicit form which they can be said to take. While sometimes metaphors
distort the ordinary rules of grammar and syntax, this is by no means a necessary or usual feature of metaphor. Nor is it always clear where a metaphor begins or ends or what words in a sentence constitute the metaphor it contains.

While I suppose that, in a phrase such as 'peal after ragged peal of thunder', 'ragged' is the metaphor, it is obviously only so in relation to its context and there is some sense in saying therefore that 'ragged peal' in toto is the metaphor. And even this distinction is much less clear if we take examples such as the following:

So, oft it chances in particular men
That, for some vicious mole of nature in them,
As in their birth, wherein they are not guilty,
Since nature cannot choose his origin;
By the o'ergrowth of some complexion,
Oft breaking down the pales and forts of reason;
Or by some habit that too much o'erleavens
The form of plausible manners— that these men,
Carrying, I say, the stamp of one defect,
Being nature's livery or fortune's star,
His virtues else, be they as pure as grace,
As infinite as man may undergo,
Shall in the general censure take corruption
From that particular fault.

Here we should be at some pains to isolate the metaphors, or, if there is one, the overriding metaphor. Rather we should say the passage both uses words metaphorically and is metaphoric as a whole.

Certainly dictionary definitions are usually misleading about metaphor. The following definition (typical of most dictionary definitions) seems to miss the essence of metaphor on a number of counts:

The figure of speech in which a name or descriptive
term is transferred to some object different from or analogous to that to which it is properly applicable.

In the definition 'figure of speech' is unclear and moreover suggests a too definite and isolable unit, 'name' and 'descriptive term' indicate nothing like the variety of metaphorical use, e.g. in verbs, adverbs, pronouns, and (very importantly) prepositions, 'analogous to' wrongly suggests a clear-cut, definite and familiar relation, and 'properly' begs a gigantic question.

When one comes to consider metaphor at more length than is possible merely by considering definitions, one is immediately led to consider what it is that makes us designate a given phrase or sentence as metaphorical. One clue is already provided by our analysis of simile, in which it was said that a simile involves novel juxtapositions or comparisons, and this is also what is certainly being sought after by the claim in the definition given that a name (or descriptive term) is transferred to something different from that to which it is properly applicable. If we now insist more closely on the relatedness of metaphor and simile and replace 'properly' (who is to determine 'propriety' in this context) by 'commonly', we will perhaps be more willing to be led on to say that in metaphorical speech words are being used in a different sense from usual, and to conclude that it is a question of meaning.

Of course, the meaning of meaning is a very vexed question and we cannot discuss it in all its ramifications. However, let us at least agree that the meaning of a word or phrase in all its fulness is not just a matter of verbal equivalence. To be able to replace a given word or phrase by other words which we regard as equivalent is not really to know the meaning. In any case, it is not even possible to
do such a thing unless we know that the alternatives have the same meaning, which immediately involves one in a regression. Further, what is a correct equivalent in one context is not correct in another. (For example, in 'He ground his teeth', and 'He ground the corn', 'ground' would be replaceable by different equivalents, and there are an infinity of other examples.)

From this two things follow. First, that verbal equivalence (which might be called the 'dictionary fallacy' of meaning) is not meaning, and secondly, that words only have meanings in their contexts. Indeed, it might be said that, in an important sense, only sentences have meaning and that words derive their meaning from the sentences in which they occur. Crawshay-Williams in *Methods and Criteria of Reasoning* effectively demonstrates that even sentences, in their turn, are only meaningful in certain contexts, and by this interpretation the meaning of words is doubly dependent on context. This is undoubtedly one feature of meaning of which account is taken in those theories of meaning which emphasize our knowledge of the use of a word (not just the use) of a word.

It seems generally agreed that words are arbitrary signs or symbols which refer to or stand for something beyond themselves (it is instructive to notice how in metalinguistic talk we are forced upon metaphors, witness the almost unavoidable use of terms such as stand for, refer, beyond, in the preceding). It is this reference which constitutes the meaning of words, but it is difficult to analyze what the reference consists of. Rather than plump for any one explanation (on the grounds that it is unlikely to be exhaustive), let us assume that this reference may be interpreted in a variety of ways appropriate to different contexts: as signifying
a state of affairs (whatever this means), as constituting a
cue for action or feeling, as indicating and hence conjuring
up an image (however complex), as disposing one to make
certain inferences, as recalling some determinate aspect of
our experience or a combination of determinate aspects of
our experience. However we interpret meaning, it will be generally
admitted that although words are in the last analysis
context-determined, there are relatively invariant (but not
context-free) and unambiguous features of the references
of words which enable us to understand the sentences in which
they normally occur and to decide (at least in principle)
whether, if they assert propositions, they are true or false.

Mention was made above of determinate aspects of experience,
and by this was meant that somehow all meaning is grounded in
or refers to (however indirectly) our actual sensual and
psychical experience of the world. This is but one aspect of
the vacuity of merely verbal definitions. To put it another
way, it might be said that in the last analysis all meaning
is ostensive, or to use a different terminology, dependent
on knowledge by acquaintance.

From our point of view, it is enough that there is a
relatively invariant and unambiguous aspect of the meanings
of a word which enables one to readily understand and act upon
or assent to or deny the truth of sentences in which the word
appears. Such meanings we shall call collectively the literal
meaning of the word (a terminology which I hope needs no
defence). Empson calls this the head-meaning.12

The first thing to notice about metaphors (or better -
the metaphorical uses of words) is that the sentences in which
they occur are not straightforwardly meaningful, unambiguous
and understandable, nor capable of securing clear-cut assent
or denial, as are sentences in which all the words have their
literal meanings only.

In metaphorical use there is a disharmony between words and context—or essentially between one word and another. We describe things, events or processes in unfamiliar ways or ascribe to them attributes, properties, behaviour and relations which cannot be located among the head—or literal meanings of the words used and which even sometimes seem at first sight incongruous or incompatible with these literal meanings.

Essentially there is in this situation a mixing of contexts. Well then, it might be said, we simply have a failure of communication. But we are well used, in fact, to handling such situations quite differently. We strive to make sense, to maintain coherence, in such situations. And we do so by trying to find some unity between the discordant contexts. We do this by exploring the less immediate associations of imagery, sensual and psychical experience and inference in order to find congruous aspects of the two contexts. In a way one may be said to recognize such situations as representing condensed or elided similes.

For example, if we encounter such an incongruity of contexts as occurs in 'whose eyelids curtain'd up their jewels dim' (Keats) we recognize that eyelids are being likened to curtains (because they are movable and opaque, and are adjustable so as to admit or exclude light) and that eyes are being likened to jewels (because they are sparkling and attractive to look at).

Richards, whose analysis of metaphor has been very influential in modern thought about metaphor, says that metaphor is 'fundamentally...a borrowing between and intercourse of thoughts, a transaction between contexts'. Of the example we have just given it might be said that the transaction in this case is fairly straight-forward (indeed 'jewels' for 'eyes' is a worn-out cliché). However, not all metaphors are
straightforward and obvious and of some metaphors it might be said, as Black does:

....it would (more)....illuminating in some of these cases to say the metaphor creates the similarity-ity than to say it formulates some similarity antecedently existing.  

Such a way of putting it is however slightly misleading. The only way in which such a similarity can antecedently exist is if it has already been perceived, and either literally or metaphorically. And in a sense every first perception of a similarity no matter how 'obvious' is equally creative. It is rather a question of the remoteness, almost the esoteric nature, of the similarity expressed by some metaphors which makes them especially novel and creative. Such metaphors are (in a non-pejorative sense of the word) recherché.

On the other hand Black's way of putting it is equally misleading, in quite a different way, if we lean too heavily on the word 'creates'. For it suggests that the metaphor 'creates' in the sense of 'formulates a similarity out of nothing' whereas one feels that the 'two contexts' must be at least potentially interpretable as similar.

Black could no doubt defend his statement on the ground that 'potentially interpretable as similar' covers almost an infinity of possibilities. Against this however it might be said that although a good metaphor is (in my description) recherché it often also has the faculty of striking a responsive chord, of securing a willing recognition, of calling forth a 'why didn't I think of that?', or of being, to rewrite Pope, 'what oft was half-apprehended but ne'er before expressed'.

Richards distinguishes in a metaphor between the 'tenor' and the 'vehicle' and by this he is interpreted by Barfield to be distinguishing the 'literal or surface meaning' (vehicle)
from 'any other meaning which it properly conveys' (tenor).\textsuperscript{18} Empson, in *The Structure of Complex Words*, gives another interpretation of Richards's usage when he says the vehicle is 'the thing said' and the tenor is 'the thing meant'. However if it is true that there is no sense to be attached to words alone but only to words in sentences, and if a word cannot receive one of its usual context-determined interpretations in a given sentence, then it can only have meaning, and hence 'say' anything, in a metaphorical way.

For this reason I think Black hits on a better terminology by speaking of 'principal subject' and 'subsidiary subject'.\textsuperscript{19} Here he refers not just to the words used metaphorically or the metaphor, but to the whole context in which the metaphorically used words occur, and to the meanings usually associated with the metaphorical words when they are used in other (literal) contexts. One might say that a given discourse is about something, it has some subject matter which may be regarded as the 'principal subject' and the words used metaphorically introduce another subject matter, or perhaps better 'ways of speaking appropriate to another subject matter' (the 'subsidiary subject').

Even Black's terminology does not seem entirely satisfactory. I am tempted to use my own terms, 'imported analogue' and 'topic analogue', for it is, so I would maintain, an analogical act, accompanied by a language shift, by which we describe the topic analogue in terms derived from the imported analogue, that is involved in the use of metaphor. Indeed, it is an analogical act which characterizes the formulation of both models and metaphors and accounts for the connection which is claimed to exist between them, and justifies our former stress upon and detailed consideration of imported analogy and analogical acts. However, I shall continue to use Black's terms and other terms such as 'contexts' since these are current in the literature.
Four aspects of this functional relation of metaphor to its context need to be emphasized, in view of their implications for metaphorical views about science.

1. The determinative or tensive function of the context is not peculiar to metaphor (as we have already noted). McCloskey expresses this view in the following passage:

> There is nothing peculiar about the context taking back part of what is implied by a word, because this happens whenever we use language, be it literal, metaphorical, or what have you. One does not string words together like beads on a thread for words modify each other in a way beads do not.\(^2^0\)

Richards puts the same case very forcibly, but adds the following important rider:

> ...we are aware of certain stabilities which hide from us this universal relativity or better, interdependence of meanings. Some words and sentences still more do seem to mean what they say absolutely and unconditionally. This is because the conditions governing their meanings are so constant that we disregard them.\(^2^1\)

Presumably, therefore, it is this sort of relative constancy of context and hence of meaning which, for Richards, constitutes literalness.

2. The contextual tension between the principal subject matter and the subsidiary subject matter of the metaphor cannot be resolved and the metaphor 'understood' or interpreted meaningfully without an awareness of the implications of the metaphor. Such a knowledge depends upon an acquaintance with and understanding of a whole network of what Black calls 'associated commonplaces'\(^2^2\). Black does not analyse what these commonplaces are or how they have come to be associated, but he does emphasize their connectedness by referring to the
the subsidiary and principal subjects as 'systems of things'.

Black says two further things about these systems of associated commonplaces. First, the metaphor 'selects, emphasizes, suppresses and organizes features of the principal subject by implying statements about it that normally apply to the subsidiary subject'. Secondly, the system of associated commonplaces need not be a natural system (i.e. such as we acquire through the ordinary contingent process of learning and experience) but may be specially attached, presumably by a process resembling a protracted stipulative definition. In Black's words:

Metaphors can be supported by specially constructed systems of implications, as well as by accepted commonplaces; they can be made to measure and need not be reach-me-downs.

3. Until the tension between the metaphor and context is resolved, the statements or sentences in which the metaphors (or metaphorically used words) occur will be meaningless, or if one prefers, absurd and nonsensical. It is precisely because the metaphorical word will not 'fit' in its ordinary accepted unambiguous usage that one is forced to resolve the apparent absurdity and 'make sense' out of the apparently meaningless. Henle says:

Ordinarily one takes words in their literal sense and that is impossible in a metaphor. Occasionally a metaphor occurs in which the literal sense is not absurd but merely conveys the wrong meaning. Whether taking all terms in their literal sense produces an absurdity or merely something incongruous the clash of literal meanings must be felt.

Van Steenburgh argues, similarly, that an unresolved metaphor consists of a false ('nonsensical') identification or attribution.
4. The last point about the relation of metaphor to context is that whether it is provided by a constructed or a naturally occurring system of associated ideas, a metaphor cannot be understood except by means of such a system and hence it assumes a cultural community between its user and his audience. As McCloskey says:

That people will understand a metaphor is guaranteed if the metaphor is utilizing cultural, as opposed to private, 'pictures'. The guarantee that such a metaphor will be understood is then, that people within the same culture group have been brought up in roughly similar ways, and become acquainted with the same sort of thing, and so tend to call to mind the same 'pictures'.

Enough has been said so far to show that metaphors and similes are very far from being dispensable graces and ornaments of language, and that there is at least a close similarity, in the way they involve an interanimation or interaction of contexts which are to some degree analogous, between analogical acts, the use of models and the use of metaphors.

Many of the writers on metaphor concur in claiming that metaphor is the very stuff of language - the instrument of thought. Language and thought grow together and the principle of growth is metaphor. Richards says of metaphor, in relation to language, that it is 'the omnipresent principle of all its free action'. Whether we are dealing with the novel perceptions of poetry or the discovery of some previously undesignated aspect of things in science, the communication or expression of a genuinely novel idea will require trespassing the boundaries of old literal meanings. There seem to be only two ways of doing this: (i) by ostensive definition, or (ii) by the use of metaphor or simile. Moreover, metaphor or simile
is more than a substitute for ostensive definition; it is a necessary auxiliary. For ostensive defining is not usually merely a matter of indication in the presence of what constitutes a sufficient warranty for making the necessary percept or framing the necessary concept. Very early on in the learning process we begin to supplement mere indication by verbal exposition and this necessarily involves metaphor or simile, and might be called 'analogical pointing'. 'Analogical pointing' by the use of metaphor, simile, analogy or model is not only ubiquitous but an essential auxiliary to the ostensive process.

It is perhaps considerations of this kind which lead Van Steenburgh to define literal and metaphorical in terms of ostensivity. For him a phrase (term) is literal when its meaning is learned ostensively (learned by observing instances) and metaphorical when its meaning is not so learned. This connects with the distinction I have made about the true nature of simile (and hence of metaphor). If one can substitute for a simile the specific attribute of likeness then the simile is merely an ornament. Similarly even though a word may be used quasi-metaphorically, if it amounts to a mere substitution for another word or words which have been learned ostensively then it is not really metaphorical but only pseudo-metaphoric. Van Steenburgh also distinguishes between what he calls perspective and prescinding metaphor. By this he distinguishes mutually modifying meanings in the normal process of metaphor (perspective) from the extension of meaning to a hitherto unknown 'x' (prescinding).

A somewhat similar distinction among metaphors is made by Cassirer in *Language and Myth*. He distinguishes 'translatory' (not his term, but his sense) metaphor from what he calls 'radical metaphor' in the following two passages:

.....the basic concept of metaphor requires scrutiny
and definition. One can take it in a narrow sense, in which it comprises only the conscious denotation of one thought content by the name of another which resembles the former in some respect, or is somehow analogous to it. In that case, metaphor is a genuine 'translation'; the two concepts between which it obtains are fixed and independent meanings, and betwixt them, as the given terminus a quo and terminus ad quem, the conceptual process takes place, which causes the transition from one to the other, whereby one is semantically made to stand proxy for the other.....

But such a use of metaphor clearly supposes that both the ideas and their verbal correlates are already given as definite quantities; only if these elements, as such, are verbally fixed and defined can they be exchanged for one another. Such transposition and substitution, which operate with a previously known vocabulary as their material, must be clearly distinguished from that genuine 'radical metaphor' which is a condition of the very formulation of mythic as well as verbal conceptions.....This involves not merely a transference.....but actually the creation of the category itself.32

Elsewhere Cassirer argues that even to see the world as a world of certain entities undergoing certain processes, in certain relations and with certain attributes, and so on, is post-linguistic. Language itself creates this world out of a previously heterogeneous experience whose only meaning is the concrete present and the emotions it engenders:

All theoretical cognition takes its departure from a world already preformed by language; the scientist, the historian, even the philosopher, lives with his
objects only as language presents them to him. It is easy to see that logical theory, which traces concepts back to an act of generalizing 'abstraction', is of little use here; for this 'abstraction' consists of selecting from the wealth of given properties certain ones which are common to several sensory or intuitive experiences, but our problem is not the choice of properties already given, but the positing of properties themselves. Cassirer's distinction between 'translatory' and 'radical' metaphor leads us to reconsider Black's characterization of metaphor as creative. While we are obviously not usually concerned solely with genuine pre-linguistic 'radical metaphor', what makes our metaphors creative obviously springs from the same source of creativity. Even though we are usually working within an established language and hence an established set of concepts, the reference of this language and the scope and limits of the concepts are not really so clear-cut or well-defined as Cassirer's account seems to suggest. While our ordinary use of metaphor is in a sense merely 'translatory' because it operates within an extensive well articulated and familiar vocabulary, there are, among the associations and inferences from a given concept or percept, unexplored areas of similarity which only light up or come crackling to life when two normally discrete ideas are, like two carbon rods, brought together. And for this reason there is a creative or 'radical metaphoric' quality to our use of metaphor.

Wheelwright expresses more or less the same opinion but he characterizes the 'translatory' element of metaphor as 'epiphonic' and the 'creative' element as 'diaphonic'. Henle uses the slightly different terminology of 'antecedent resemblance' and 'induced resemblance'. Barfield takes a similar position to Cassirer about the
general metaphorical origins of language, but Barfield deals more specifically with what he takes to be the myth that words were originally formulated in a context of practical matter-of-fact literal use (a world of material objects and their properties and relations) and have subsequently been 'corrupted' to imaginative, fanciful and, generally speaking, literary, philosophical and religious purposes since. This thesis which he is attacking, and which is, one would say, an extremely commonly held one at the common-sense level, might well admit metaphor-or as a principle of the growth of language and thought, but would nevertheless hold that language is literal in its origins and proceeds upon a general basis of literalness. Of course there are certain etymological and philological facts which look as though they provide confirmation for this. For example, it is supposedly a commonplace of the history of language that many words for abstract words were originally used for concrete ideas. Richards mentions (not to make this point, however) that:

.....historians of language have long taught that we can find no word or description for any of the intellectual operations which if its history is known, is not seen to have been taken, by metaphor, from a description of some physical happening.

More generally Whewell, for example, says that all abstract words were originally derived from sensible objects.

However, in spite of these claims, which really relate to languages which are already in a developed state, rather than to the aboriginal birth of languages, and in spite of our naive preconceptions, the idea of the original literal basis of language is impossible to maintain in the face of what we know about the etymology of words and from philological and anthropological researches on ancient languages and with primitive peoples. Rather all this evidence seems to point to the fact that the literal language is a later emergence
from non-literal language. Barfield draws these conclusions:

Literalness is a quality which some words have achieved in the course of their history; it is not a quality with which the first words were born. And let us be clear about the consequences. The born literalness which we have rejected is a literalness of the material, not the immaterial language. We mean by a 'literal' word or meaning, one which is not a vehicle with a tenor, or, let me say, one which is a vehicle without a tenor. But the vast majority of the words by which we today denote/objects of the outer world would have at some stage of their life been vehicles with a tenor, and, if that is so, it follows (except in places where a tenor was added by late and deliberate metaphorical construction) that they began life as vehicles with a tenor. They too can only have achieved a literalness with which they were not born.39

There are some assumptions in Barfields's account which are inconsistent with some of what has previously been said. For example, he speaks as if words (on their own and not in contexts) are literal and we have claimed that literalness is a matter of contextual consistency, i.e. unambiguity or constancy of context.

Is it possible then to interpret Barfield's account in a way which is understandable in terms of, and therefore can be seen to be relevant to, the account we have given? First, I think we should notice that neither Cassirer nor Barfield makes the mistake of saying that language was originally metaphorical. Cassirer, it it true, says that a process of radical metaphor engenders words but not that the primitive beginnings of a vocabulary of words so engendered was metaphorical. Indeed at this early stage it would be strange to
to characterize the words or their usage as metaphorical since there is presumably no such thing as the literal meaning or literal usage of a word. The distinction between literal and metaphorical is one which only emerges at a certain stage of complexity, when there is an extensive and regular 'material-object' vocabulary together with a common usage. (Unless it should be thought that I have smuggled in a new equivalence, between 'material-object' language and the literal, let me admit this but justify the equivalence on the grounds that such material-object language eminently satisfies the criteria about understanding and lack of ambiguity mentioned earlier and is perhaps the commonest domain of language.) What Barfield and Cassirer are claiming, then, is that words generally, in their aboriginal meanings and usages, had a broader more indefinite content than words have in their present literal usages. It is not that they were previously aggregates of the literal and the metaphoric but rather that they were ores or amalgams from which purely literal meanings have since been refined. The early vocabulary of mankind was both a-literal and a-metaphorical. This presumably accords with Barfield's opinion for he also says:

Just as our immaterial language has acquired its literal meanings by dropping the vehicular reference, so our material language has acquired its literalness by dropping the tenorial reference. 40

If literalness is an achieved or emergent feature of language we ought to consider this phenomenon of emergence in relation to science. And there seem to be good prima facie reasons why the emergence of literal language and science should be thought to be related. The very fact that we often uncritically equate the literal and the scientific indicates some connection. And of course conversely to dismiss the metaphorical language of poetry and much religion as unscientific. Also as we shall see, there was a strong antipathy to metaphor among the thinkers who ushered in the modern age of science in the seventeenth
century.

However, even if regard some scientific language as characteristically literal, and even if we believe that the emergence of the literal and the scientific have some connection, it does not follow that we should equate the literal and the scientific. For one thing, we also regard commonsense language and material-object language outside the special languages of science as being literal.

Richards, for example, while he connects the literal with the scientific:

Literal language is rare outside the central parts of the sciences. 41

also says (of metaphor):

Even in the rigid language of the settled sciences we do not eliminate or prevent it without great difficulty. 42

and elsewhere claims:

And that brings us to the third and worst assumption — that metaphor is something special and exceptional in the use of language, a deviation from its normal mode of working, instead of the omnipresent principle of its free action. 43

For the moment we need to note only that Richards refers to language in its working and talks of metaphor as a principle of its free action, rather than as a character of the more or less established and permanent corpus of language.

With views such as Richards's we have come a very long way from the view of metaphor as a deviation from normal speech, and we seem to be brought to the position of regarding the literal as the abnormal and the metaphorical as the norm. Such a position has a disagreeable air of paradox. And it will perhaps only go some way to clearing the air to insist on the small part played by pure exposition in all the diverse tasks of language, as Richards does:

......we have exaggerated enormously the extent of pure
exposition. It is a relatively rare occurrence outside
the routine of train services and the tamer more
settled parts of the sciences. More than a whiff of paradox remains and we ought to make some
further attempt to dispel it. First, perhaps we ought to beware
of confusing the subject matter and the style (for want of a
better word) of our discourse. While there are styles which
are a priori presumed to be appropriate to a given subject
matter we should beware of such assumptions. Obviously such
assumptions are behind the identification of poetry and
metaphor on the one hand and science and literalness on the
other. This goes with the equally specious but pernicious
assumption that literature is merely fancy talk and science
is plain talk. While there may be important differences between
the metaphors of science and those of literature, there is
really no a priori reason why metaphor should not be appropriate
to science, and it is equally perfectly possible for literature
to be literal.

An equally specious mistake it seems to me springs from
assuming that a practical, working everyday vocabulary is
wholly literal. And this in turn arises out of a more elusive
error still, which has its origin ( alas!) in a metaphor —
a metaphor about metaphors.

Metaphors are not without their critics and connoisseurs.
Some metaphors are good and some are bad. Some are illuminating
and pregnant with insight and meaning, and some are obscure,
incongruous, hackneyed or — worst of all — 'dead'. And
between the quick and the dead there is no degree of transience
or intermediacy. As far as metaphors go, they are either alive
or dead, and there is an end of it.

But surely this is wrong. While some metaphoric uses of
words no doubt do decline into the literal with repeated use,
others do not. It is perfectly true, as Wheelwright says of such a metaphor, that it does:

.....introduce an element of irreducible novelty when it is first composed, yet its novelty wears off as time flows on, and it may eventually, if used too much, become a piece of literal language or a cliche. and he gives the example of 'skyscraper'. However, it is also true that many metaphors suffer a fate better than death. They become a permanent part of the ordinary vocabulary, but they remain essentially metaphorical: 'in the mind', 'I saw through him', 'I took it all in', 'the content of a proposition', 'He felt superior', 'feelings', 'cut the conversation short', 'between hope and despair', 'on tenterhooks', 'on edge', 'follow a dream', 'hard times', 'soft music', 'low spirits', 'fall from power', 'fall asleep', 'press home', 'also ran', 'dry up', 'run out of' (and so on, ad infinitum) – all these are permanent metaphors.

Language is riddled with such metaphors. They are not only permanent, they are invisible. We do not notice them. They do not clamour for our attention as newly-coined metaphors do, but are quietly efficient and functional.

There is, of course, an objection to such claims. It is that such words are not metaphorical at all. Their use has now become, as Wheelwright says, a cliche. They are now literal. But surely there is a difference between words like 'skyscraper' and expressions of the type mentioned. 'Skyscraper' and similar metaphors are often (in their present usage) mere names without any of their metaphoric edge. The examples we have mentioned, however, still draw, at least partly, on their metaphoric resources. Why? Because, to use Van Steenburgh's distinction they are not learned or understood merely ostensively, as, for example, 'skyscraper' might be. No doubt the ostensive element is also present to a varying degree, but it also has, as an essential auxiliary, the support of transferred meanings from the other usages of
these words. We can understand what 'skyscrapers' are and use the word efficiently without ever having to reflect on the significance of 'scraping skies', but we cannot understand or use 'cut short' (of conversations) unless we recognize the analogy with 'cutting short' a tree, for example. The understanding of the words here derives partly from alternative current usages of the words concerned, and among these directly, or indirectly through these, there must be ostensive usages. The analogy, for example, between 'cutting short' in the one sense and 'cutting short' in other senses which can be demonstrated ostensively is an irreducible component of the non-ostensive sense.

For these reasons I should like to introduce into the classification of metaphor an additional category besides the 'live' and the 'dead' - the established metaphor. It is not dead, because it will not lie down. While it may no longer be lively, it is still alive.

With this item in the armoury of metaphor, I think we can more readily admit the truth of Richards's claim that metaphor is an all-pervasive, if not universal, feature of language. However, as we have already noticed, one of the areas where Richards admits the prevalence of the literal is within established science, so that we still need to make distinctions about the scope and application of metaphor within science.

It might be thought that an even more radical doctrine than that of Richards, namely, that all language is irreducibly metaphorical, might be more congenial to the view that science is metaphorical. Adler puts this doctrine with some persuasion in the following passage:

The distinction between literal and metaphorical statements cannot be defended when the symbolism of all language is revealed. 'Sugar is sweet' is no more literal than 'That man is an ass'; nor is the statement
that 'God is the perfect philosopher' any more metaphorical than 'Iron rusts'.....

Similarly metaphorical statements taken in isolation can neither be thoroughly understood or judged invalid or valid. It is only when they are interpreted by a contextual environment, although this itself be metaphorical, that their meaning becomes clear, and their appropriateness in the given environment determined. Science furnishes a much more adequate context for its metaphors than does poetry, and this may account for the suspicion that the former is literal in its statements, whereas the latter is metaphorical. Scientific statements may seem literal because they can be more uniformly and precisely interpreted and more readily judged true or false; but that is not because they are less metaphorical than other statements, but simply because science is a more orderly system of metaphors than occurs in poetry or in common speech.\(^{46}\)

I have quoted this passage at some length because it is illuminating in relation to some of our previous observations about the systematicity and contextual relations of metaphors. Nevertheless, considered merely as a statement about the generality of metaphor, it is, no doubt, open to serious objections. McCloskey objects to it on the grounds that Adler reveals that there is a difference between 'literal' and 'metaphorical' by using different words when he discusses the methods of checking 'literal' and 'metaphorical' statements. She says (her quotes are from Adler):

- The metaphor we 'judge to be valid or invalid',
- 'determine to be appropriate'; the literal statement we 'judge to be true or false'.\(^{47}\)
This criticism is itself based on the tacit supposition that there is a categorical difference between 'valid' or 'appropriate' (and even these seem queer bedmates) on the one hand and 'truth' on the other.

The question of truth in relation to metaphorical statements is an extremely important one, for if we are to make any claim that science is metaphorical or employs or uses metaphors, then we shall also be committed to evaluating the truth or falsity of scientific statements which are metaphorical in this way.

As McCloskey points out, Adler seem unwittingly to have distinguished metaphorical from literal statements in terms of whether we can evaluate them as true/false or merely valid/invalid (whatever this means in the context). McCloskey believes this to be a genuine criterion for distinguishing metaphorical from literal statements. Literal statements are either true or false. Metaphorical statements are merely appropriate or apt. Truth tests are not relevant to metaphors:

To use truth tests on a metaphorical statement is to take it literally and metaphorical statements are to be taken metaphorically.48

Once again, since scientific statements are usually assumed to be either true or false, this discussion is very pertinent to metaphorical views about science, and the problems it engenders need to be resolved.

It is interesting that Van Steenburgh regards literal and metaphorical as 'semantical predicates' on the same logical footing with 'true', for example. 'Just as a statement is either true or false, but not both,' he says, 'so the meaning of a term is literal or metaphorical, but not both.'

Van Steenburgh does not discuss, however, the relations of these different sorts of semantical predicates to one
another and particularly the important question whether the 'semantical' class of true/false statements is identical with, or a sub-class of, or has some other relation to the class of 'literal' statements. However, his classification implies a definite dichotomy between literal and metaphorical, and he supplies a definition or criterion of literalness, already mentioned, i.e. 'A term has a literal occurrence if and only if its usage is general and its meaning on that occurrence is ostensive.'\(^{50}\) He claims to avoid a given statement being ambiguously interpretable as 'literal' by some and 'metaphorical' by others. However, he weakens his case by admitting that 'ostensive' is a vague term and that there is no clear rule about whether a given term is ostensive. This is 'best decided on individual merits.'\(^{51}\).

We do not need to accept the whole of McCloskey's strictures to diagnose that there is something wrong with Adler's claims. For he commits the common philosophical sin of denying a distinction which he uses meaningfully in his own discourse. Having denied that literal and metaphorical can be distinguished, he goes on to talk about metaphorical statements as if they existed, and to say 'statements may seem literal' as if this were perfectly intelligible, which (on his interpretation) it ought not to be.

While it persuades us possibly that the literal/metaphorical/distinction is not what we thought it was, or that the distinction is more vague than we think or that the limits between the two ought to be set differently, it nevertheless seems to leave the proposition that there really is such a distinction undisturbed.

One of the purposes of our discussion of metaphor has been to divest it as far as possible of its purely literary associations and give it a more general epistemological significance. Such a task would not be complete and metaphor would no
doubt still trail literary strings if we did not now complete the severance of metaphor from any connotation of grace or ornament by discussing the significance of the meaning of metaphor.

In an earlier quotation from McCloskey, she describes the metaphor as utilizing 'pictures'. There is a danger in such a way of talking that metaphors may be associated with imagery. No doubt some imagery of a 'picture' or eidetic kind does occur when we are 'reading' a metaphor, but I believe that, like the emotive tone of a metaphor, this is not central to the meaning.

Because we think, correctly, of metaphor as something vivifying discourse and making it more sensual and actual, we might easily regard the meaning of metaphor as essentially concerned with eidetic imagery and affective tone. What a metaphor does, it might be argued, is produce feelings and images in us and that is its prime purpose. Indeed it is perhaps because of its ability to evoke emotion and persuasive images that metaphor is the stock-in-trade of the propagandist, the 'spieler', the rabble-rouser, the salesman and the advertising man.

However, in spite of their incidental ability to evoke feelings and images, metaphors are essentially connotative or cognitive, rather than affective or emotive, in their meaning.

The distinction between emotive and cognitive meaning was originally made by Ogden and Richards and was taken up with enthusiasm by some positivists, notably Ayer in Language, Truth and Logic, where certain metaphysical, poetic and religious statements are presumed to be cognitively meaningless although they have emotive or affective tone and express feelings, values or attitudes.

Both Empson and Wheelwright think a strict dichotomy between emotive and cognitive statements forces poetry into
a Procustean bed, because it falsely divorces affective or emotive tone from sense. Empson discusses this in relation to metaphor, and says:

Original pieces of thinking have, I suppose, nearly always been started from metaphor, and so far from being peculiarly 'emotive' and indulgent of folly a metaphor is often a loophole for common sense.

Further on he says:

.....there is no advantage for a theory of metaphor in confusing this kind of process (expressing thought and 'educing correlates') with one that drowns the intellect in a storm of passion.

Wheelwright discusses the question in the following passage in more general terms:

Two types or mode of discourse are sharply distinguished: typically called the referential and the emotive. Referential statements.....are postulated or defined to be true insofar as they correspond with, and truly describe, what is actually the case, false insofar as they do the contrary;.....Emotive discourse, on the other hand, is taken as expressing some emotive–conative state of the writer (or speaker) or as aiming to arouse such a state in the reader (or hearer), and therefore as not being intrinsically referential. The unguarded inference from 'intrinsically emotive' to 'not intrinsically referential' reveals the main logical presupposition of the Affective Theory: that language which is intrinsically the one cannot be intrinsically the other; that the terms, 'referential' and 'emotive' (or their synonyms), constitute a natural dichotomy. This is a presupposition which must now be challenged.
It is also intrinsic to Wheelwright's argument that poetry is concerned with truth, a point which we may compare with McCloskey's claim that metaphorical statements (which need not, of course, be poetical) are to be distinguished from literal statements precisely on the grounds that the notion of 'truth' is relevant to literal statements but not to metaphorical statements. Wheelwright says:

A poem affects a mature reader as it does partly because it seems to him, notwithstanding its fantasies and pseudo-statements, to be offering a kind of genuine insight and therefore to be revealing however obscurely and elusively, a kind of truth.\(^{57}\)

In a way, the passages which have just been quoted do not make a fresh point since a large part of our discussion has assumed that metaphor has cognitive meaning, but they do make explicit the point that even in poetry metaphors are not just emotive or image creating but have 'referential thrust'\(^{58}\) i.e. they have meaning in the way that a scientific term has meaning. A corollary of this is that to understand metaphor is to add to one's knowledge. Barfield regards the acquisition of knowledge as the most salient permanent feature of the understanding of a metaphor:

\[\text{......the absorption of this metaphor into my imagination has enabled me to bring more (to experience) than I could before. It has created something in me, a faculty or a part of a faculty, enabling me to observe what I could not hitherto observe. This ability to recognize significant resemblances and analogies, considered as in action, I shall call knowledge; considered as a state, and apart from the effort by which it is imparted and acquired, I shall call it wisdom. The elements in poetic diction which must conduce to it are, as we shall see, metaphor and} \]
and simile. 59

One last task in unpicking the web of literary and aesthetic threads which usually enmesh our minds when we think of metaphor remains. It concerns imagery. Traditionally literature and especially poetry is intimately concerned with imagery. It supposedly exercises our imagination and conjures up concrete at least specific sensual images. It is thought that the very essence of poetry is to be concerned with particular percepts rather than with the abstract or the conceptual.

However, once again this needs examination and is not as clear-cut as at first appears. Much of the unique specific sensual quality of any given piece of poetry results from its particular combination of sounds and rhythm. It is also likely to have, especially if it employs metaphor and simile in a creative way, a novel and therefore specific (or unique) association of ideas, or if you like, images.

But this by no means implies that it is concerned with eidetic imagery, that the mind of the reader of poetry is a sort of theatre in which a succession of detailed 'pictorial' experiences (in a wide sense - to include also sounds, smells and tactile sensations) are rehearsed. This is obviously not so - for a number of reasons. First, introspection tells us that it is not. Secondly, if it were so then there would be a conflict of some sort between the imagery and sense, especially whenever the developing sense was at odds with an already formulated image. Thirdly, it would seem to imply that a similar kind of image was a component of our thought in such different phrases as 'going to the dogs', 'you knavish dogs', 'unleash the dogs of war', and 'take the dog for a walk', which is obviously not the case.
Fourthly, if a person is asked to form an image of even a particular concrete thing such as 'the town hall at x' a demand for detail about the image such as 'what is the time by the town hall clock?' reveals that the image is in fact schematic and that the details are 'filled-in' in response to the question.

One of the prime sources of any inclination to think otherwise is Berkeley. For it is Berkeley's challenge, in his critique of Locke, coupled with the misleading empiricist epistemology which supposes ideas (or images) to be faint or less vivacious impressions or sensations, which typifies the attitude that images are primarily eidetic, and constitutes one of the greatest confidence tricks in the history of philosophy (although perpetuated in good faith, no doubt). Berkeley challenges us to produce the idea of a triangle which is neither scalene, nor isosceles nor equilateral. The irony is that, when we are not being too self-conscious about it, this is precisely what we do. Berkeley was misled by employing the false analogy of a reflection or (faint) less vivacious impression. The truth is that (never mind how) our 'images' are usually general and not particular. And this is equally true of the 'images' engendered by poetry, and hence by metaphor and simile, as it is of those connected with lawlike or general statements.

Richards is well aware of this, as is shown by the following quotation:

.....rhetoricians think that a figure of speech, an image, or imaginative comparison, must have something to do with the presence of images, in this other sense, in the mind's eye or the mind's ear. But, of course, it need not. No images of this sort need come in at any point. We had one instance of the vicious influence of this red-herring in my first lecture -
Lord Kames' antic with the mental picture he supposed we must form of Shakespeare's peacock-feather.....We cannot too firmly recognize that how a figure of speech works has nothing necessarily to do with how any images, as copies or duplicates of sense perceptions, may, for reader or writer, be backing up his words.

My main aim in this chapter has been to discuss the nature of metaphor and the relation of the literal to the metaphorical in such a way as to indicate the general plausibility of a metaphorical view of science and its language, and to raise some pertinent problems. I shall reconsider some of the points raised, in the process of expounding and evaluating the opinions of various writers on metaphor in relation to science in the next two chapters. It will be helpful, therefore, at this point to take a synoptic view of what has been said in this chapter, and to summarize its implications for a metaphorical view of science.

It has been claimed that metaphor and simile function not as ornaments or graces but as essential instruments of cognitive meaning. While they deviate from usual language habits, this is only so in the way of providing novel meanings, of expanding the scope of language and the wealth of its concepts. The ability of metaphor and simile to serve as instruments of growth and innovation depends on the fact that words do not have isolated and unequivocal unique meanings but have context-determined meanings which depend on indefinite networks or systems of integrated or associated ideas or images. The perspective which the subsidiary subject of the metaphor imposes on the principal subject arises from the novel juxtaposition which the metaphor provides. This novelty depends on the recherché nature of the juxtaposition, but the
metaphor or simile will remain absurd or meaningless unless a focusing or resolution of the connection between the normally exclusive contexts of the principal and subsidiary contexts is possible. Such a resolution depends on a cultural community between the purveyor (writer) and the receiver (reader). This community of understanding can exist at various levels.

At the lowest and most essential level it depends on the ordinary common vocabulary which one shares with all who speak the same language. This one might call the practical working vocabulary, by which I mean the vocabulary and complex network of concepts which most of us by adulthood uncritically pick up and learn by the various methods of ostensive learning, book learning, learning by rote, stochastic (adult-or-teacher corrected) learning, instinctive 'experimental' recombination and substitution, dictionary consultation, contextual interpretation, and reaction to expressed or tacit approval or comprehension, and so on. Such a practical vocabulary includes numerous abstractions, such as 'spirit', 'honesty', etc. With this vocabulary we are most of us capable of making sense of such metaphors as, 'He clotted his speech with multisyllables' or 'The garlic twang of the banjo cut through the bland syrup of the violins', whereas a different more esoteric community of understanding is required to make sense of 'clot' and 'garlic' in the following:

Garlic and sapphires in the mud clot the bedded axle-tree.

The understanding of this passage depends upon membership of a 'literary community' where the context of 'clot' and 'garlic' make metaphorical 'sense' of the quotation to the initiated or cognoscenti, because they are familiar with certain implications.
or connotations of the word used which the possessor of just the working or practical vocabulary is not familiar with. Where a complex significant metaphor is regularly used by either a particular author or a particular literary community as a recurrent or dominant metaphor it is often referred to as a symbol rather than a metaphor.

It is perfectly possible to learn some of the implications of such metaphors or symbols either by constantly encountering and making sense of them by a comparison of contexts or by some kind of exposition of their implications, although it is probable that such an exposition will be prolix, approximate, incomplete, and a serial piecemeal version of what is (in the metaphor) a simultaneous, integrated and not completely and exhaustively analysable or delimitable cloud of meaning. The important point is that a certain sort of education is necessary for the understanding of some metaphors, and a particular body of work or community of understanding may provide a sort of prescriptive context for them.

Since metaphors are capable of extending knowledge and understanding, they are also capable in some sense (to be more definitely determined) of being 'true' or 'false'. While the claim that all language is metaphorical is to be rejected, the salient role of metaphor in the growth of language and proliferation of concepts is accepted, and they seem to be almost indispensable supplements and auxiliaries to ostensive learning. This is particularly exemplified by the fact that our understanding of some of our ordinary language depends upon a permanent metaphoric transfer of some of the uses and meanings of words to other uses (permanent metaphors).

There seems good reason to suppose that the literal/metaphorical dichotomy emerges only at a certain level of sophistication and complexity. Although the dividing line between literal and metaphorical cannot be too confidently
or dogmatically drawn, there is a genuine distinction to be made. However, its elucidation is not immediately obvious, although possible criteria which suggest themselves from our discussion are those connected with 'ostensivity', 'meaningfulness' ('absurdity'), 'truth', 'appropriateness', 'ambiguity', 'normal usage'.

Since metaphor and simile do not exclude the cognitive and are not essentially or necessarily concerned with the particular, nor with the emotive or with eidetic imagery, there is no reason to proscribe them from scientific use or to be surprised if they turn up in scientific discourse. On the contrary, since scientific language is based on and works from ordinary language so that it must have once been explained by way of the practical working vocabulary, we should expect this explication to satisfy the normal conditions of language under expansion into wider and more complex conceptual fields, and hence to employ metaphor and simile. (It would hardly be tenable to claim that science, or even mathematics, can be taught ostensively and independently of the practical working vocabulary.) Our expectation of encountering metaphor and simile will be further increased by the reflection that they depend upon the formulating power of analogy to provide a perspective, and we have already assumed the role of analogy in scientific discovery and its communication. Moreover the fact that scientific terms and theories become a (relatively) permanent part of our vocabulary and our conceptual apparatus need not preclude them from being metaphoric, nor need the fact that scientific terms are integrated into complex logical and semantic systems.
Chapter 4

The previous chapters have been preliminary to the main topic of this thesis, which is a consideration of claims that have been made in recent years that science is in an essential way metaphorical or characteristically employs metaphors. Although most views of this kind have been put forward within the last fifteen or so years, at least two views about metaphor which have been influential in these recent writings had their origin some time before. One is the 'radical metaphor' view of Cassirer, mentioned in the last chapter. The radical metaphor view can be traced back well before Cassirer, however. Vico in the eighteenth century seems to have been the first person to have recognized metaphor as being a critical feature of the growth of language and thought. Perhaps the first concise modern version of this view is that of Hueller, in the nineteenth century:

Man was compelled to speak metaphorically, not because he could not control his poetic phantasy, but rather more because he was compelled to strive to the uttermost to find expression for the ever-growing needs of his soul. It was completely impossible to grasp and hold the outer world, to know and to understand, to conceive and to name, without this fundamental metaphor, this universal mythology, if you will. Metaphor in this sense was much less the carrying over of a word from one concept to another than the creation or nearer determination of a new concept by means of an old name.¹

On this view of metaphor, metaphor, far from being a mere ornament, is the key to understanding or at least the expansion of understanding beyond its current limits.

The other view of metaphor, while it also recognizes
the importance of metaphor as an instrument of the growth of language and thought, stresses the 'interanimation' of one field of thought by another, which takes place by means of metaphor. This view was put forward in its most influential version by Richards in such works as *The Philosophy of Rhetoric*. Richards's view seems to have been the main influence on Black, who stresses in turn a number of features of metaphor which have been very influential on views that science is metaphorical or employs metaphors. Black's main contributions to the theory of metaphor seem to have been:

1. that metaphors can be concerned with *systems* of ideas,
2. that such systems may be specially constructed,
3. that metaphors are projective in that they allow one field of thought ('subject') to organize, by selection, 'filtering', 'focusing', etc., another,
4. that models are extended and systematic metaphors.

In the last point he was possibly influenced by Hutten, whom Black quotes and who seems to have been the first person to have made the point at any length. This connection of models with metaphors occurs in a great deal of the literature about metaphor in science, as we shall see. In fact some hint of the connection between model and metaphor long predates either Hutten or Black, for Maxwell in the nineteenth century described his use of physical analogy as *scientific metaphor*.

As will become apparent there are a number of opinions about metaphor in relation to science which though perhaps related are sufficiently different to require distinguishing.

First there are those who base their ideas on rather general views about the nature of language and thought. Adler, whose opinions were quoted in the last chapter belongs to this group and so does Richards.
Perhaps the most original and interesting of such viewpoints is that of Buchanan, who seems to be out of the mainstream of ideas about metaphor in science, although he does dedicate one of his books, *Symbolic Distance*, to the 'authors of The Meaning of Meaning*, one of whom was Richards. However, his views do not seem to owe much to Richards at least in detail.

In his earlier work, *Poetry and Mathematics*, he puts forward his views in a more discursive and perhaps more obscure way than in his later work, *Symbolic Distance in Relation to Analogy and Fiction*, but the guiding ideas are the same.

The basis of Buchanan's ideas is his belief that analogy in the sense of resemblance of relations is fundamental to knowledge and understanding and to the growth of knowledge and understanding, whether this be in literature or science. It is by way of analogy that we have metaphor, simile, allegory, proportion, mathematics, poetry and symbolism, and much else besides:

Analogue thought is so common that we are surprised, like M. Jourdain, to learn at any given time that we are speaking analogy....I should say that any proposition or significant sentence contained an analogy in some form or other. Any history of thought might begin and end with the statement that man is an analogical animal. Of analogy, he gives the following definition:

It is the statement of the identity or similarity of at least two relations. It says in symbols, that the relation of A to B is the same as the relation of C to D. Of course the relations may be of any degree of complexity, provided the identity or similarity is not violated. The complexity may be increased or
diminished, apparently without limit. I shall call this property of analogies their expansiveness.\textsuperscript{10}

This notion of expansion is fairly central to Buchanan's theories, as is its converse, ellipsis or collapse. For example:

\text{...metaphors are suppressed or elliptical similes}

\text{...The metaphor, previously shown to be a suppressed simile, is now a suppressed analogy.}\textsuperscript{11}

The most obvious of these expansions (of analogy) is the allegory. The analogy has been expanded and stretched until finally the left-hand side has been torn away and the other remains, fantastic, enigmatical, fascinating, by itself.\textsuperscript{12}

Although Buchanan mentions epics, legends and sagas as examples of such allegories, i.e., he uses the term in ways that we might consider quite appropriate or normal, the starting originality of his point of view can be seen in the following application of his ideas to science and mathematics:

A 'system of electrons' is a very short formula for the account a biophysicist would give of a human body. His account can be briefly formulated as 'a collection of unit charges holding those relations to each other that are described in physical chemistry'. 'Man' will then mean 'a physical body of certain size and shape', and the whole metaphor ought to read thus: 'the parts of the physical body ordinarily known as man are to the whole body as the unit charges of electricity are to the electric field which contains them'. The search for greater explicitness in the formula would lead to more elaborate expansions which would stop only when the present stage of physical science has been exhausted.\textsuperscript{13}

Measurement is an analogy that asserts the similarity
of two relations, one between things and the other between numbers. Example: Distance A is to distance B as 1 is to 10.14

...(2) Quantity is the condensation of an analogy of measurement into a numerical metaphor. Example: Distance B is 10.....

(5) A scientific law is an analogy, or system of analogies (allegory), which asserts that the relations between things is similar to the relations between numbers. Example: A lever is in equilibrium when \( W_1 : W_2 :: D_2 : D_1 \).

(6) Science is an allegory that asserts that the relations between the parts of reality are similar to the relations between the terms of discourse.

(7) The natural universe is the things and their relations that enter into the allegories of science.15

This sort of analysis leads Buchanan to propose the possibility of a new logistic for scientific method:
The first proposition of such a logistic system would state that any proposition in the old logio would be a metaphor subject to expansion into similes, analogies and allegories. Such expansion would provide terms for the expression of any universe of discourse as an allegory whose atomic elements would be ratios.

If there is objection to generalising the term, ratio, in this fashion, we might speak of logoi as the constituents of allegories. It should be noted that such logical atoms are merely unit elements, not absolute simples which cannot be analysed. They are atomic only relatively, that is, with respect to any given allegory.16

As we shall see, Buchanan makes a more sustained attempt to formulate such a logistic in his other work, *Symbolic Distance*. 
However, before we go on to this, a further interesting application of the notion of expansion and ellipsis in *Poetry and Mathematics* deserves comment, since it relates to our discussion in the previous chapter of the relation between literal and metaphorical. Buchanan says:

What is a literal statement? Curiously enough, literalness is to be found in the more elaborate expansions of figurative statements....The literal interpretation of a proposition is to be found in the allegory.... Expanding analogies progressively reveal relations; argument by analogy is the fundamental technique in the process of abstraction....The farther the expansion is carried out the higher the degree of abstractness and explicitness achieved. If the allegory could be completely expanded and one side separated from the other, either side would be a literal statement. But curiously enough, the expansion can take place only when the two sides are allowed to interact symbolically; a *logoi* or *ratio* in one must lead to the discernment of its analogue in the other. There seems to be no end to this process and therefore no end to the expansion. A purely formal and therefore literal statement is never possible....The extreme case would be symbols expressing themselves....Discourse is allegorical or nonsensical....For the mathematician and the scientist facts are to be referred to principles and causes. The allegorical correspondence of his ideas with the facts is the truth. The world is a network of relations which his formulas reflect.  

There are some features of Buchanan's account both of his logistic and of his distinction between literal and metaphorical which are unclear, for example, how expansion is carried
out. Moreover, since the literal represents an asymptotic limit, the literal and the metaphorical are always comparative and relative to a given allegory or metaphor. However it does seem to be the case that knowledge is irreducibly metaphorical, on Buchanan’s view, science included.

In Symbolic Distance we get a much more explicit version of the logistic mentioned in Poetry and Mathematics. Here Buchanan develops a logistic based on a matrix, defined as follows:

A matrix consists of rows and columns of terms, such that the relational order in one row is repeated throughout the others, and the relational order in one column is repeated throughout the others.\(^{18}\)

This notion of matrix is combined with the concept of ‘symbolic distance’ which is a constituent of any set of symbols. Symbolic distance is ‘analogous to series of reflections in a set of mirrors.’\(^{19}\) The following is an example of a matrix:

<table>
<thead>
<tr>
<th>ambition</th>
<th>plan</th>
<th>success (Napoleon)</th>
</tr>
</thead>
<tbody>
<tr>
<td>hunger</td>
<td>hunting</td>
<td>satisfaction (wolf)</td>
</tr>
<tr>
<td>end</td>
<td>means</td>
<td>consequences (man)</td>
</tr>
</tbody>
</table>

which can be represented symbolically as

```
\begin{array}{ccc}
  a & b & c \\
  d & e & f \\
  g & h & i \\
\end{array}
```

A matrix is indeterminate and ‘as it stands has many meanings.’\(^{20}\)

In relation to such matrices, he says:

Metaphors are….elliptical expressions for a group or propositions that we derive as literal determinants from their matrices. The simplest literal determinant will be a proposition that states a dyadic relation holding between two terms of the matrix.\(^{22}\)

and elsewhere he says:

…..metaphors are condensed summaries of systems of
literal sentences.....

.....metaphorical statements appear to...to shift....
(literal determinants).....to the daigonal. A literary
purpose has been fulfilled by ellipses, which in these
cases has either crushed or staggered an analogy.
Metonymy and synecdoche play similar happy havoc with
matrices, and likewise allegories, myths and scientific
theories with more complex analogies....

Regarded in respect to its several diverse applications,
a metaphor becomes a general proposition containing, in the
language of modern logic, apparent variables..

The notion of symbolic distance is explicated by the fact that
matrices are not rectangular in the mode represented provisionally above. They converge in one direction and diverge in the
other so that in relation to one another the various columns
represent expansions or contractions. It is along the dimension
of expansion or contraction ('condensation') that one gets
symbolic distance measured through the various columns.

As opposed to the claims made in Poetry and Mathematics,
Buchanan now seems to recognize that metaphorical expansion
is not asymptotic in relation to the literal, although, like
many points in his exposition, this is by no means clear.

In general, it may be said that Buchanan's whole exposition is rather opaque and, although brilliantly suggestive and anticipatory of later schemes for handling analogy and metaphor, not at all clear in detail.

What does emerge is his conviction that science is
metaphorical, and, in so far as it is mathematical, typically so, and that the metaphorical is a complex function of the
literal, to which it stands in a relation like that of a
variable to any one of its determinants. The whole scheme of
matrix and symbolic distance is reminiscent of the indirect
meaning and system of hierarchical models mentioned above in
relation to Nash and Hutten. It is not quite clear from Buchanan how the matrix actually functions in relation to scientific discovery, except that it starts from analogy, but whether one starts from metaphor and then carries out the expansion or other resolution of the matrix or whether the metaphor is the end product of scientific activity is not clear. Possibly both sorts of move are possible, depending on the stage of development or the circumstances of the science in question, and its relation to other sciences.

A much clearer but simpler version of the metaphorical claim in relation to science made on the general grounds of the relation of thought to language is that of Robinson. It does not mention metaphor, but the process described is recognizably metaphorical:

What is here maintained, however, is the view that science receives the greatest benefits from ambiguity, not merely in virtue of the poetic elements inseparable from any scientific statement, but also in its strictly scientific aspect. This begins to appear when we ask how we are to symbolise new things for which there is as yet no symbol. At first the obvious answer seems to be that we should invent a new symbol; and in this attitude of mind Housman praises the man who invented the word 'gas', an entirely new root to convey an entirely new notion, conveniently brief, conveniently univocal, and submitting moderately well to inflection.....But we must ask how the inventor of this word got other men to understand what he meant by it. Inevitably he used a definition. And inevitably this definition was composed of old words, whose meanings were already familiar.....But still it is not first-class originality, because it is only a new combination of old elements, and contains no new
Here then is the difficulty; we hope to get a perfectly new idea from time to time; but we can only use the old words with which to get it, or a new one that has to be defined in terms of the old; and the old words only mean the old things. Our original axioms and postulates and definitions have been given to us once and for all (though Heaven knows how); and all we can do is to go on drawing their consequences...But if we hanker for something radically different, for something outside the system altogether, some entirely new axiom or definition, we cannot be satisfied.

Irreducible originality is possible through the fact of ambiguity. Great minds on the one hand, and the unconscious folk on the other, hint their new idea, and we divine it from them, by means of an old word in a new sense. The new sense is not given by formal definition, for that would only be a new combination of old ideas. It is given by 'relational univocity', that is, by the reaction of the context with the old sense. A strange context forces the word to take a new sense.

This account of Robinson's is in close accord with my own account of communicating the purport of an analogical act, and he relates the purport of a discovery to its communication and description as we saw that Mach did in relation to analogy and models.

The further important point that the incorporation of new ideas, arrived at and describable only in the way that Robinson suggests, into our system of language and concepts transmits a permanent metaphorical character to our concepts is made by Schön, in his book, The Invention and Evolution of Ideas.
The invention and evolution of ideas is, according to Schön, carried out by the 'displacement of concepts' and this is 'simply another word for the process of metaphor, in Cassirer's sense'\textsuperscript{28}. Here Schön is referring to Cassirer's notion of radical metaphor which we have mentioned above. Schön quotes a passage from Cassirer to make the point that radical metaphor is not a 'piece of language' but a 'process of thought'\textsuperscript{29}.

Having made the claim that metaphor is involved in the displacement of concepts, Schön goes on:

But language is the repository of our concepts. To speak of the growth of language is another way of speaking about the formation of concepts. The metaphorical character of language....is due to the fact that our language, at any given time, gives us a cross-section of our processes of concept formation or discovery. The metaphors in language are to be explained as signs of concepts at various stages of displacement, just as fossils are to be explained as signs of living things at various stages of evolution.\textsuperscript{30}

In a summary of the role of metaphor in the formulation of new hypotheses, Schön makes the following points:

.....Processes of metaphor are functional in the generation of new hypotheses.....It is worthwhile paying attention to the literal language in which theories are formulated. This is often suggestive of metaphors underlying the theory and therefore of further elaborations or of related metaphors, yielding new hypotheses.\textsuperscript{31}

These two quotations embody two of Schön's main points: that discovery and invention is metaphoric, and that metaphors underlie theories and are somehow permanently incorporated into them. The latter point he deals with at more length in a chapter
entitled 'the Conservative Function of the Displacement of Concepts'. In it he underlines the point made earlier that language is a repository of concepts which reflect their metaphorical origin in some way, and the point that old theories underlie new ones:

When old theories are displaced to new situations, all aspects of the old theories tend to locate themselves projectively in the new situation. Epaminondas-like, aspects of old theory may be taken as projective models for the new situation without being changed themselves in the process. They become fixed as assumptions about the new situation, often unrecognized, carried over uncritically from the old theory. The actual language of a theory contains metaphors which are vestiges and signs of the old theories whose displacement helped to form the new one. These metaphors are clues to the identity of the old theories.  

In summary, then, Schönh makes three related claims about metaphor in relation to science: (i) the process of discovery and invention is radically metaphorical, (ii) the incorporation of such radically metaphorical discoveries and inventions into our system of concepts give language and thought a metaphorical character, (iii) old theories underlie and are integrated into new theories, resulting in metaphors which serve as indicators of old theories.

Before we go on to consider the main group of writers on metaphor in science, whose ideas are related either to Black's theory of metaphor and/or to the role of model in science, there is one advocate of the metaphorical view whose ideas relate not to the processes of scientific discovery or the use of analogy and models in specific cases or in relation to the ordinary course of science, but to the general conceptual schemes which form a metaphysical basis for whole periods
of science and which are implicit rather than explicit in relation to particular theories.

In the chapter on models we noted that Deutsch uses the term 'formal model' in the unusual sense of a general conceptual scheme, and he illustrates this conception by discussing such general models as society, law, balance, wheel, organism, evolution and so on. Such general conceptual schemes were long ago recognized as constituting uncritically-held assumptions by Hume, in his celebrated attack on the Argument from Design, and Mechanism. The existence of such general conceptual frameworks is the basis of Turbayne's claims in The Myth of Metaphor that science is fundamentally metaphorical. His argument relies on a fairly broad definition of metaphor: 'the presentation of the facts of one category in the idioms appropriate to another' (the two sets of quotation marks are necessary because Turbayne is himself quoting from Ryle). He also calls the use of metaphor 'sort-crossing', and presents an analysis of the history of those metaphors which enter permanently into language and thought. Although he does not tie his account in closely with the use of models in science, he does regard the scientific model as a special case of metaphor; in the case of models there is some specification of the properties to be transferred by the metaphor from one sort or category to another, models are sustained or extended, and they can 'be supported by specially constructed systems of implication' (he is quoting Black in the last point).

The use of metaphor, Turbayne argues, involves an awareness 'first, that we are sort-crossing' and secondly that we are treating the two sorts 'as if they belong to new sorts' (we are given 'two ideas as one') . There are three main stages in the life of a metaphor, he says, and goes on:
At first a word's use is simply inappropriate. This is because it gives the thing a name that belongs to something else. It is a case of mis-using words, of 'going against ordinary language' and therefore of breaking the conventions. Our first response is to deny the metaphor and affirm the literal truth: 'Bodies attract each other' (only people attract or are attracted, though bodies may move together); 'Look around the world — you will find it to be nothing but one great machine' (only clocks, sewing machines and automobiles are machines, though the world moves and has an intricate arrangement of parts). The effective metaphor quickly enters the second stage in its life; the once inappropriate name becomes a metaphor. It has its moment of triumph. We accept the metaphor by acquiescing in the make-believe, the metaphor is used by us with awareness to illuminate obscure or previously hidden facts. The moments of inappropriateness and triumph are short compared with the infinitely long period when the metaphor is accepted as commonplace. The last two stages are sometimes described as the transition from a 'live' metaphor to one 'moribund' or 'dead'. But it is better to say that either the metaphor is now hidden or it ceases to be. In part of the third stage of a metaphor we no longer make believe. That sounds are vibrations, etc. sounds are nothing but vibrations. What had before been models are now taken for things modeled.

We are now, Turbayne says, in danger of becoming victims of the metaphor and being used by it rather than using it. This happens because of the continued association of the two
separate ideas involved, because of the fact that they are given the same name, and because the difference between the two ideas is no longer made explicit. It is perhaps the central point of Turbayne's thesis that our widely accepted scientific theories contain metaphorical assumptions of this kind, of which we are not aware:

The victim of metaphor accepts one way of sorting or bundling or allocating the facts as the only way to sort, bundle or allocate them. The victim not only has a special view of the world but regards it as the only view, or rather, he confuses a special view of the world with the world. He is thus, unknowingly, a metaphysician. He has mistaken the mask for the face. Such a victim who is a metaphysician malgré lui is to be distinguished from that other metaphysician who is aware that his allocation of facts is arbitrary and might have been otherwise.

Turbayne supports his argument by a careful consideration of the scientific methodology of Plato, Aristotle (briefly) and Descartes and Newton (at more length). In the case of Descartes and Newton he also examines their actual scientific practice in order to compare this with their methodology. He concludes that, in spite of their own claims to the contrary, they both postulated hypotheses of a metaphorical kind which imply a metaphysics:

Nevertheless, in all their explanations of nature, both men were constantly making use of hypotheses. The 'manifest qualities' that Newton found in bodies and that Descartes 'clearly and distinctly' found in them were not discoveries of fact but were, from one point of view, occult qualities, and from another, free inventions of their highly imaginative minds. While they believed that they were actually
giving true descriptions of the process of nature they were actually projecting the devices of one into the facts of the other. Thus from one point of view... they were metaphysicians malgré eux and their metaphysics was mechanism. 42b

Turbayne claims that Descartes and Newton were the victims of many sort-crossings, but the three which he isolates as examples are fairly radical to modern science. The first is the confusion of the 'deductive relation with the relation between events':

The former relation belongs to procedure. It is, therefore, the sort of thing that is invented; it was, in fact, invented by the Greeks as the best way of teaching. The latter belongs to the process going on in nature. It is, therefore, the sort of thing that is discovered.... What did they think they found, however, going on in the process? The answer is astonishing: The physicist's procedure is duplicated in the physical process. The principle of procedure that starts a demonstration is repeated in the 'active principle' that starts a causal process. Moreover, the relation between a principle of procedure and its deduced consequences is exactly the same as that between an 'active principle', such as gravity, and its effects. This relation is that of necessary connection.... Nature, it was concluded, obeys the logic of the deductive method. 43

The second sort-crossing, closely related to the first, is the 'inadvertent identification of explanation with physical explanation and this with causal explanation, that is the reduction of one to the other': 44:

..... both men thought..... explanation had to be causal, the one holding that physical forces, the other that
physical laws, cause events. 45

This led, for example, to the emphasis on forces as the principal kind of cause.

The third case of sort-crossing was 'the unwarranted identification of deduction with computation or calculation or any other form of metrical reckoning or counting'. 46 Turbayne goes on to say that the defining property of science is demonstration, not mathematics, and 'because mathematical computation is constantly used in science, we must not regard it as a defining property'. 47

There are various ways of exposing a metaphor, according to Turbayne: by uncrossing the sorts and demonstrating the limits of the analogy involved or by extending the metaphor, showing that it results in absurdities or paradoxes, or by drawing attention to failures of explanation and other difficulties. 48 However, Turbayne admits that there are limits to the process of uncrossing the sorts:

The attempt to re-allocate the facts by restoring them to where they 'actually belong' is vain. It is like trying to observe the rule 'Let us get rid of the metaphors and replace them by the literal truth'. But can this be done? We might just as well seek to provide what the poet 'actually says'.....we can never know exactly what the facts are.....We cannot help but allocate, sort, bundle the facts in some way or other. 49

This is an admission that science is irreducibly metaphorical. Accordingly, Turbayne concludes that the only way to remedy the situation is by substituting another more effective and satisfactory metaphor, but always with the awareness that we are using a metaphor. For the rest of the book Turbayne seeks to do this in a very detailed example. He tries to solve some outstanding problems about the theory of vision by substituting a linguistic model (or metaphor) for a
As a corollary to Turbayne's claim that the unwitting use of metaphor has profound effects on the methodological assumptions of science, we may cite an article by Laudan, 'The Clock Metaphor and Probabilism, (etc)', which contains an extremely detailed and well-documented argument to show how the metaphor of a clock and its mechanism affected English methodological thought. In general, Laudan claims that Descartes was instrumental in introducing an attitude towards science in England that regarded theories as hypothetical, conjectural and probable, rather than true and certain conclusions derived either by induction or by rational deduction from self-evident principles. The writers he examines are Boyle, Glanvill and Power. On the basis of this examination and of a comparison with the writings of Descartes, he concludes that these writers 'freely and enthusiastically accepted Descartes' suggestion that the scientist must be content with hypothetical principles and conjectures rather than true and valid inductions' and that the Cartesian view of the universe 'as a mechanical engine' or clock whose internal parts can only be conjectured about served as an important stimulus for the English writers on method:

Descartes's ideas were justified along the following lines:

We can propose mechanisms for how the internal parts of the watch might be arranged, though we can never, ex hypothesi, get inside to see if we are right. Because the watch might be constructed in any number of ways, it is sufficient if we outline some possible arrangement which would account for its external behaviour....

Of course we do not have complete freedom in proposing mechanisms, since there are first principles in physics which circumscribe the range of acceptable hypotheses by excluding certain entities, but although our first
principles, 'like the data, can inform us that certain hypotheses are wrong....they cannot tell us which hypotheses are right'. The central point of Laudans's analysis is not that Descartes used the clock metaphor to support the mechanistic philosophy, which presumably also played a part in prescribing the 'first principles' in the way that Turbayne suggests, but that Descartes used the metaphor to justify a hypothetico-deductive methodology and a probabilistic attitude to theories.

We now come to what I have called the main group of writers on metaphor in science, whose ideas are related to Black's and/or to the use of models.

Some writers seem to assume the connection of metaphor with model without any very explicit attempt to justify it. For example, Theobald in An Introduction to the Philosophy of Science gives a very brief account of metaphor and then says that 'science leans very heavily upon the use of metaphor and analogy, upon the use of models' but makes no further mention of metaphor in his actual discussion of models. Tricker, on the other hand, makes almost no mention of metaphor although he gives an account of the limitations of the use of analogy in scientific theories by demonstrating that the terminology derived from a model by means of analogy is apt to be misleading. This amounts to a demonstration that the words are being used metaphorically, and that caution should be used in the transfer of such terminology. He ends by adopting the position that science can never be more than analogical, but concludes from this that it constitutes a series of similes, rather than metaphors.

The writers I shall be mentioning relate metaphor more explicitly to the relation between model and metaphor. These writers are Berggren, Black, Harre, Hesse, Hutten, Ramsey, and Swanson. MacCormac does not mention models...
except in passing, but refers to Black, Hesse, Hutten and Berggren, so that he may be regarded as belonging to the same group for our purposes.

Hutten, who seems to have originated the comparisons and connections of model with metaphor, mentions metaphor in relation to model in two works, but the substance of his remarks is very much the same in both.

According to Hutten, a model (he uses the word in the sense of model) has a semantic function in relation to a theory. It provides an interpretation or at least a partial interpretation of the concepts used in an advanced theory; it thus can give meaning to the expressions used in an advanced theory and at the same time acts as an explanation. Moreover it enables us to introduce descriptive key concepts and to construct a theory which does not relate directly to experiment. In these respects it resembles a metaphor:

......the model has a logical function which remains indispensable, as an explanation, that is as an interpretation of a theory in simple terms. Models thus resemble metaphors in ordinary language. Metaphors are used to give a more precise meaning, or to add an important nuance, to our expressions; when words normally used in a given context seem to fail, we seek help through words which, usually, belong to another context. 69

......the semantic function of the model is to supply an interpretation. The interpretation, however, is only partial - otherwise it is not a model but the 'real thing'......the model does not, and cannot, give all the semantic rules; and an equation is always capable of many interpretations......the model is used to enable us to introduce technical concepts and to construct a more advanced theory which does not relate
directly to experiment. We are forced to employ models
when, for one reason or another, we cannot give a
direct and complete description in the language we
normally use. Ordinarily, when words fail us, we have
recourse to analogy and metaphor. The model functions
are (sic) a more general kind of metaphor. 70
It is generally held that the replacing of experience
by experiment is a characteristic of science. The
artificial experiment needs an artificial, or techn-
dical, language to describe it. Ordinarily when we wish
to speak about something new or unfamiliar, we employ
a metaphor. A large part of our normal language is,
after all, metaphorical. Starting from the infant's
restricted vocabulary we build a treasury of general
and so-called abstract words... We are forced to exten-
d our words and even to coin new words whenever we
describe some new experience. The new words and usages
follow, more or less, the established pattern of our
language but they do not exactly coincide with it. It
is this, sometimes hardly perceptible, difference that
enables us to advance.

The model is very similar to the metaphor. With
its help we build up higher theories from simpler ones,
and make use of concepts that are specially invented
and do not directly relate to experiment. But the model
is more than a metaphor. While the metaphor suggests
rules of usage, the model indicates more specifically
some of these rules: the model specifies the meaning
of an expression. 71

In the last passage quoted, apart from mentioning the
difference between model and metaphor, Hutten mentions the
description of experiment. Similarly, after the first passage
quoted he goes on to say:

In this way we extend the usage of our customary expressions; and this is necessary if we want to build up a technical language for describing an experiment artificially produced in the laboratory. In physics, we speak of a field of force, or of the flow of heat, and so on. Indeed technical discourse cannot do without metaphorical language. 72

Here Hutton seems to be making a different claim than the one about models, although the juxtaposition is misleading. Since he has said only that models resemble metaphors he cannot mean that this metaphorical technical vocabulary used in relation to experiment itself constitutes the model. However, elsewhere, he claims that 'model connects theory with experiment' 73 and, as we have seen above, he relates models to meaning and explanation. The clue to this difficulty is perhaps provided by the differences which he notes between models and metaphors: (i) the model specifies the meaning of an expression, which a metaphor does not. Such a specification is not done by definition, but by showing its usage, and the model indicates this in the following fashion:

The model prescribes a context, or gives a universe of discourse; it sets limits to what can be said; the content of the theory, and the logical range of sentences in it, is therefore determined; and the meaning of the sentences is specified. 74

So far Hutton's account of specification does not seem very unlike the way a metaphor gives meaning. It therefore seems necessary to fall back on Hutton's other differentia, (ii) 'A model is much more than a metaphor. It is an attempt, not only to extend the use of a single expression, but to build up a whole system of expressions, or a terminology.' 75
Presumably because it is more systematic, a model is able to provide *syntactic* rules as well as semantic ones.

The following seems to be a summary of Hutton's ideas:

(i) models are like, or function like, metaphors. They enable us to construct and meaningfully describe the concepts of theories in the same way as metaphors enable us to think about or describe things or concepts not normally describable in a literal vocabulary,

(ii) because they function in this way they give meaning to, and thus an interpretation of, theories. This in turn enables them to be explanatory and to connect theory with experiment,

(iii) however, models are more complex and systematic than metaphors, which enables them to specify the meaning they give to theories, and to give both semantic rules and syntactic structure to theories,

(iv) metaphors are also used in science to describe experiments, and they are an essential feature of technical language of this kind,

(v) (and here I think we must interpret Hutton rather than report him) part of the means by which models connect to experiment is through the metaphors which are used in relation to experiment and which form part of the system of expressions, the terminology, which the model provides for the theory.

Black's influence on the metaphorical view of science is not a straightforward one. It is not the case either that he puts forward a detailed argument for the metaphorical view or that he relates metaphors to models in any ways more definite or far-reaching than Hutton does. Black's role, I think, is to give separate accounts (which were in fact originally published separately) of models and metaphors which are very suggestive of their similarity even though
the analyses he gives are for the most part quite distinct. He also provides at least the grounds for a rapprochement between the two accounts by comparing them, although only briefly. For example, he compares Maxwell's and Kelvin's use of physical analogy and their advance by such means to ontological commitment as being akin to metaphor. He quotes Hutten's remarks approvingly, and adds some pertinent remarks of his own to Hutten's observations:

Certainly there is some similarity between the use of a model and the use of metaphor — perhaps we should say, of a sustained and systematic metaphor. And the crucial question about the autonomy of the method of models is paralleled by an ancient dispute about the translatability of metaphors. Those who see the model as a mere crutch are like those who consider a metaphor a mere decoration or ornament. But there are powerful and irreplaceable uses of metaphor not adequately described by the old formula of 'saying one thing and meaning another'.

He also considers under models the general conceptual schemes of the type that Turbayne claims are metaphorical, and which Black calls 'archetypes'. These are the sum of his comparisons of model and metaphor, except that he also recognizes metaphors as simple or embryonic theories, and he suggests that they are perhaps a necessary prelude to theory-making:

Perhaps every science must start with metaphor and end with algebra; and perhaps without the metaphor there would never have been any algebra.

On the other hand he introduces his remarks on models by saying that the use of models 'smacks of the metaphorical', which does not betoken a very close comparison, and he also quotes approvingly Toulmin's remarks that its 'suggestiveness and systematic deployability.....' make 'a good model something more than a simple metaphor'. 
However, as I have said, Black's main contribution to the metaphorical viewpoint in science is that he reveals similarities between models and metaphors in his separate analyses of them. One parallel is the one he draws attention to in the passage quoted, but there are a number of others. For example, he talks of the model and the field it is a model for as being domains (primary and secondary). This has its parallel when he talks of the principal and subsidiary subjects in relation to metaphor. Both metaphors and models give insight. The principal subject of the metaphor is 'projected upon' the subsidiary subject, while we use the language appropriate to the model in thinking about the domain for which it is a model. In general the model is in a relation of analogy to the primary domain, and the 'transforming function' of the literal into the metaphorical is analogy. Models and metaphors are both 'speculative instruments'. Black also says in relation to metaphors that they can be supported by specially constructed systems of implications, and this is interpreted by Turbayne, who quotes Black, as being characteristic of a model.

Another suggestive point in Black's analysis is his description of some types of model as being iconic in that they are signs for other things by virtue of their likeness to them. This is suggestive in relation to metaphor, for the reason that Henle puts forward an iconic theory of metaphor in 'Metaphor'. Lastly we may mention that Black claims that the use of theoretical models involves 'talking in a certain way', once again suggesting the metaphorical.

Ramsey's account of models and metaphor, although it makes frequent references to Black, is more an example of parallel development than direct influence. Ramsey acknowledges the similarity of his opinions to Black's and makes the interesting observation that they were both influenced by Richards, confirming my earlier statement about Richards's seminal role in metaphorical views about science. Ramsey's
book is not centrally about science at all but about religion, but it does underline some of the resemblances between model and metaphor and bring these two concepts even closer together. Ramsey claims to be basing his account of a model on the notion of 'analogue model', mentioned by Black, which aims to reproduce structure rather than provide pictures. In fact his remarks apply equally well for the most part to Black's 'theoretical model'. Ramsey stresses, as we have already noticed in our discussion of insight in relation to models, the insight or 'disclosure' aspect of models:

...the model must somehow or other chime in with and echo the phenomena. In this way the universe itself authenticates a model. The model arises in a moment of insight when the universe discloses itself in the points where the phenomena and the model meet. In this sense, there must be at the heart of every model a 'disclosure'. Such a disclosure arises around and embraces the phenomena and their associated model.

...In any scientific understanding a model is better the more prolific it is in generating deductions which are then open to experimental verification and falsification.

It is in both these ways that a scientific model fits the universe and enables us to be reliably articulate about it.

Later in the book he compares metaphors to models. They are alike in the following ways: (i)'.....metaphors, like models, enable us to be articulate and are born in insight', (ii) metaphors, like models, permit 'a whole cluster of inferences', (iii) metaphors, like models, are irreplaceable and necessary modes of research and discovery, (iv) they both represent
a tangential meeting of diverse contexts, (v) they both have 'ontological reference' and 'cognitive significance', (vi) they both point to 'the mystery and elusiveness' of what has been disclosed.

Enough has been said, I think, to show how closely Ramsey identifies models and metaphors. So close is this identification that it does not seem unreasonable to include Ramsey among those who take a fundamentally metaphorical view of the scientific enterprise.

Swanson is another writer who expressly refers to Black's theories. The main aim of Swanson's paper is to explicate the relation between a model and a theory for which it is a model. Swanson argues that any theory about models (such as Braithwaite's) which claims that a model is another interpretation of a theory's calculus or that places emphasis on identity of structure between model and theory is misleading in that it 'obliterates the essential asymmetry of model and theory'. Swanson then gives a logical analysis of the model/theory relationship to show that the model must have a 'richer substructure' than the theory if the use of the model is to have any scientific point. He recognizes that his logical analysis only applies to fully axiomatized models and theories, and that science in general is not axiomatized 'give or take a portion here and there by a Woodger or a Carnap', but claims that, were the model and theory to be axiomatized, the model would necessarily have more, or a deeper, structure. This asymmetry does not prevent theory and model being isomorphic in some respects, however.

Swanson then discusses Black's concepts of metaphor and model which he interprets as 'assimilating the notion of model to that of metaphor' and concentrates on the implication that Black's contention that a metaphor 'selects, emphasises, suppresses, and organises features of the
principal subject by implying statements about it that
normally apply to the subsidiary subject', in short that the
metaphor acts as a 'filter', can be carried over to models,
and used as a key to a technique of deploying the deeper
substructure of a model in relation to a theory:

......what does a filter do? It allows some things
to pass, disallows others. It organises the things
which pass through it in some sort of systematic
fashion. It selects some things over others. It
rearranges things. What a filter does is not so much
to exclude things, although it does that too, as to
let them pass in a certain order so that those pref-
erentially chosen will come first, those least
preferred will come last, and the rest ranked in
order in between.....It is clear that the character-
ization we have just given of a filter would serve
equally well as an intuitive account of a rational
preference ranking.

Swanson then goes on to describe the procedure of rational
preference ranking and its use in relation to a theory and
its model. This procedure is too complex to give in detail
but it amounts to asking users of the model and theory about
their 'preferences' (in terms, for example, of what they think
more probable, what they more firmly believe, or what they
are more willing to base their actions on) in relation to
alternative sentences which are proposed in order to carry
over meaning from the model to the theory (these sentences
being based on the richer substructure of the model). By
such means (I have necessarily rather distorted Sawson's
account in thus condensing it, but the details are not
important), Swanson maintains, a concensus rational preference
ranking might be obtained, even some sort of mathematical
measure of the 'heuristic kick' of a model made 102. The main
point of this in relation to metaphor is made by Swanson himself:

We assert - this is the second main point of the essay - that the metaphorical effect of viewing a theory through a model consists in carrying the RPR associated with the model over to the theory. This is to say that in addition to imputing the structure of the model to the theory, the model user either consciously or unconsciously transfers the RPR associated with the entire model, including associations based on 'commonplace knowledge', and on the unmapped substructure of the model, over to the theory.

In effect, what Swanson proposes is not only to take the metaphorical view of a model seriously but to use it as the basis of a definite procedure for facilitating the deployment of a model in relation to a theory.

In some of her earlier writings on models Hesse has given indications of some sort of metaphorical view of science. For example, in Forces and Fields, she makes the following observation:

The correct analogy for the relation of theoretical and phenomenal languages is not the relation between a number-code and English, or even between simple sentences in English and in French, but the translation of poetry into prose, or to look at it the other way round, which is the order of discovery, description of an experiment in ordinary language has a relation to the scientist's theoretical description which is similar to that between Holingshed and Shakespeare.

I think that this contains the germ of the ideas which she gives more explicitly in 'The Explanatory Function of Metaphor'. Hesse acknowledges that her view is due directly to
Black's articles on model and metaphor. To begin with Hesse discusses and adopts the essentials of Black's view of metaphor, which she calls the 'interaction view'. In relation to this she makes two important points. Having claimed that the kernel of the interaction view is that a metaphor changes the meanings associated with both the principal and the subsidiary subjects, so that novel meanings emerge, resulting from the assimilation of the two hitherto discrete areas of meaning, she goes on:

The consequences of the interaction view for theoretical models are... incompatible with assumptions generally made in the deductive account of explanation, namely that descriptions and descriptive laws in the domain of the explanandum remain empirically acceptable and invariant in meaning to all changes of explanatory theory.

The second point is that on the interaction view there are no usages which are 'irreducibly literal and others metaphorical'. On the interaction view language is dynamic, the metaphorical sometimes becoming literal, the literal metaphorical, and the distinction between literal and metaphorical relative to particular contexts.

Hesse then goes on to discuss the problem of what is the referent of a model or metaphor, and this leads her into a discussion of whether a metaphor/model can establish some sort of identity between the domain of the secondary system (i.e. the 'subsidiary subject' in Black's terminology, or the 'imported analogue' in my terminology) and the primary system (the 'principal subject in Black's terminology, or the 'topic analogue' in mine). She refers to McCloskey, Turbayne and Berggren (q.v. below) and cites them as examples of writers who reject the possibility of identification between the metaphor or model on the one hand and the domain
of the primary system or subject, or the context (in the case of metaphor), on the other. Hesse does not expound the views of these writers, and we have not yet considered Berggren, but as we have already mentioned the views of both McCloskey and Turbayne we can adequately explicate her argument by reference to these two.

In the chapter on metaphor some discussion occurred in relation to McCloskey of the important question of whether metaphors may be regarded as true or false, or merely appropriate or valid, and hence whether true/false statements are a sub-class of literal statements. McCloskey, it will be remembered, rejects the possibility of a metaphor being true (or false) and says:

To use truth tests on a metaphorical statement is to take it literally and metaphorical statements are to be taken metaphorically.111

Similarly, it seems to be implicit in Turbayne's whole thesis that to mistake the metaphorical for the fact is to be the victim of the metaphor, and this is perhaps only another way of saying that we must not accept the metaphor as true. However, it seems to me that this interpretation of Turbayne's position is not quite correct, although it is no doubt due to an inconsistency in Turbayne's own account. In a section entitled 'Presenting the literal truth' Turbayne concludes with a paragraph, part of which we have quoted above, to the effect that we cannot get rid of our metaphors or models ('we are always the victims of adding some interpretation'). Turbayne does not go on to discuss the problems involved in this view. However, I think it would be reasonable to interpret Turbayne as claiming not so much that metaphors and models are not true, but that they are not literally true. Since he regards them as an uneliminable feature of science, this would amount to a claim that
only be metaphorically true. However, this is my interpretation, and whether it makes sense to speak of metaphorical truth in this way remains to be demonstrated. As we shall see, however, Berggren does explicitly make the claim that 'scientific meaning and truth is irreducibly metaphorical'.

It should be noted in this connection that many modellists claim that models are not true (or false), but appropriate (inappropriate) or valid (invalid). Hutten, for example, claims that the 'model supplies primarily a terminology', and at the conclusion of his argument for this, says:

A model, then, may or may not be suitable; it always has its limitations; but it is not either true or false, or confirmable, or directly subject to what is called induction. The model functions in a different way.

A similar claim is one of the main themes of Toulmin's *The Philosophy of Science*. Wartofsky, who is here speaking of what he calls 'Constructivist' views of theories and models (i.e. the Positivist view which regards them as, perhaps necessary, fictions) says:

....a fictional construct is not meant to be true or false in the ordinary sense; it either serves its purpose or it does not; it is either adequate or inadequate. On these grounds, one may say the construct is neither true nor false, in any empirical sense, but serves only as an instrument of the understanding, more or less adequately.

The attribution of this position to Constructivists highlights a difficulty for the view that models are not true or false, but merely adequate, suitable, or appropriate. First, as we have seen, this tends to lump together modellists of a Positivist and non-Positivist persuasion. Moreover
such an account of models makes it difficult to reconcile
the other claims of modellists (which we have discussed in
Chapter 2) that models make existence or ontological claims,
that they provide explanations (in a stronger sense than
'provide premises for a deductive argument'), that they are
essential to connect theory with experiment, that they are
essential to the logical structure of theories, and that
they give insight. Similarly, that metaphors are not true or
false, but merely appropriate or inappropriate, seems to be
incompatible with any claims that metaphors are cognitive,
for surely 'cognitive' means something more than 'meaningful'
or something of that kind. As has already been argued,
in Chapter 4, there is a very good case for regarding
metaphors as cognitive, and hence capable of making truth
claims of some sort. This is very forcibly put by Herschber-
ger:

"...I believe metaphor does make statements about
the world. I might have called this pattern of
argument 'texture', merely amplifying the term, but
texture is so tactual and kinaesthetic a word that it
gives the enemy a means by which to refer the poet's
affirmations back into his viscera, where they have
been marooned so long. The use of 'argument' is my
contribution to the political drive to place poetry
on an equal level with science, complete with
usefulness, logical necessity, and true and reliable
statements. 117"

Hesse's view is that scientific models (and metaphors)
are intended to make truth claims (she makes similar claims
in her discussion of models in Forces and Fields 118):
Thus their truth criteria, although not rigorously
formalizable, are at least much clearer than in the
case of poetic metaphor. We can perhaps signalize
the difference by speaking in the case of scientific models of the (perhaps unattainable) aim to find a 'perfect metaphor', whose referent is the domain of the explanandum, whereas literary metaphors, however adequate and successful in their own terms, are from the point of view of potential logical consistency and extendability often (not always) intentionally imperfect. 119

The conclusion of Hesse's paper, and its main point, is to apply the 'interaction' conception of metaphor to the problem of scientific explanation. Before doing so, she briefly mentions that the adoption of a metaphorical view of science results in the abandonment of a 'meaning-invariance' thesis about the literal observation-descriptions of science, but as this is only mentioned briefly and is the main point of MacCormac's metaphorical view of science, we shall defer discussion of it.

Hesse then puts the case for regarding theoretical explanation (and not, for example, explanation by covering-law, which merely subsumes a fact under an established law for facts of that kind) as metaphorical on the following grounds:

(1) Difficulties exist about the deductive relation between a theory and what it explains, i.e. about the logical relationship between a scientific explanans and its explanandum. These are problems which the logical positivists, logical constructivists, operationalists and contextualists have been unable to solve, at least to the satisfaction of their critics. The metaphorical account solves some of these problems, e.g. problems about the status of correspondence rules, about the meaning of theoretical terms, about 'meaning-invariance', and so on. Moreover it does so while still retaining a deductive structure for theories. 'The situation',
Hesse says, 'is...this':

Given a descriptive statement D in the domain of the explanandum, it is usually the case that the E of an acceptable explanans does not entail D, but rather D' where D' is a statement in the domain of the explanandum only 'approximately equivalent' to D. For E to be acceptable it is necessary both that there be a deductive relation between E and D', and that D' should come to be recognized as a more acceptable description in the domain of the explanandum than D. The reasons why it might be more acceptable — repetitions of the experiments with greater accuracy, greater coherence with other acceptable laws, recognition of disturbing factors in arriving at D in the first place, metaphoric shifts in the meanings of terms in D consequent upon the introduction of the new terminology of E, and so on — need not concern us here.¹²⁰

According to Hesse, the replacement of an existing E with a new E and hence the replacement of D by D' or by D" (if there is an existing theory¹²¹) results in the metaphorical redescriptions of the phenomena:

Metaphors, if they are good ones, and ipso facto their deductive consequences, do have the primary system as their referents, for they may be seen as correcting and replacing the original literal descriptions of the same systems, so that the literal systems are discarded as inadequate or even false.¹²²

Presumably Hesse means, in accordance with her previous statements, that, in the case of the replacement of one theory by another, the correction and replacement is of what has come to accepted as literal (although once, on its introduction, metaphorical).
There are some difficulties about this account. For example, it is rather ambiguous to talk of the 'primary system' as being the referent, since it is in practice, and necessarily so if Hesse is right, difficult to separate the (presumably empirical) phenomena of the 'primary system' from the laws or theories by which they are interpreted. Moreover, what it is for a statement in the domain of the explanandum to be 'approximately equivalent' to a descriptive statement in this domain is not clear. This could mean, for example, 'having parameters with similar values', in terms of measurement. However, it seems more likely to mean 'semantically equivalent'. In any case this notion needs explicating.

The metaphorical account, Hesse says, evades problems about the status of correspondence rules, the meaning of theoretical (as opposed to observational) terms, and whether some terms (e.g. observation terms) are 'meaning-invariant' in various theories:

......because here there are no correspondence rules, and this view is primarily designed to give its own account of the meaning of the language of the explanans. There is one language, the observation language, which like all natural languages is being continually extended by metaphoric uses, and hence yields the terminology of the explanans.

2. Such an account is also in accordance with the requirements that satisfactory explanations be predictive. Metaphorical redescriptions of this kind would be strongly predictive, in the way that she has elsewhere claimed that models are, in the sense that new observation predicates will be involved.

In the foregoing mention was made of 'meaning-variance' It is on the grounds that a metaphorical view of science effectively solves problems about 'meaning-variance' that MacCormac advocates a metaphorical view.
The problem of 'meaning-variance' arises in relation to change in science, and how scientific terms acquire or change their meaning. The problem is part of a wider discussion about the relative status and interconnectedness of theoretical and observational terms in a theory.

It has been maintained by some writers, notably Feyerabend, that scientific terms can only be understood in relation to the theory in which they occur. While most people perhaps are inclined to accept that what are usually called theoretical terms are theory-dependent in this way, Feyerabend's thesis is that a scientific theory is all of a piece and that, consequently, all the terms, including so-called observational terms and observation statements, are theory dependent or theory-infected in this way. He even goes so far as to claim that common-sense straightforward descriptions of the world are really alternative theories, and therefore, in a sense theory-infected. Feyerabend's claims are put forward as constituting the only satisfactory alternative to positiv-ist claims that all scientific theories can be reduced to or constructed out of neutral elements which are common to all theories and indeed all sciences. At various times these elements have been construed as sensations, sense data, phenomena, and physical objects and their properties, as encountered in ordinary experience and described by the ordinary vocabulary of description. Such claims have turned out to be notoriously difficult to make good, because no such reduction to elements or construction of theories out of them has been successfully demonstrated, even for relatively simple theories.

However, theories such as Feyerabend's run into their own difficulties. We certainly use the same terms, such as 'mass', 'energy', 'force', 'atom', and so on, throughout a
whole range of theories which have succeeded each other, and there seems to be an overwhelming conviction that these theories are about the same subject-matter. Yet it seems equally clear that the meanings of terms are governed by or integrated with or at the very least modified by the theory in which they occur. The hypothesis and dictionary (or calculus and correspondence-rules) dichotomy does not solve the problem since it is presumably the case that what the correspondence rules or the dictionary translate into or relate to in observational terms or statements will vary with the varying implications of different theories, and the consequent structural variations in the theorems of the theory and significance of the terms of these theorems.

Achinstein has tackled this problem in 'On the Meaning of Scientific Terms' by arguing that (i) there are a variety of ways in which meaning is given to scientific terms, and (ii), as a result, at least some of the terms of a theory are unchanged from one theory to another, some are given meaning in ways which are relatively independent of the theory, and some are given meaning by connected or underlying theories which are common to successive theories\(^\text{132}\). In Concepts of Science Achinstein makes a more analytical approach to the same problem\(^\text{133}\). In essentials Achinstein's solution is that among the properties connoted by a term some are semantically relevant to constancy of meaning and some are not. He thus calls this feature of terms 'semantic relevance':

> By this term I mean that if an item is known to possess certain properties and lack others, the fact that the item possesses or lacks the property in question normally will count, at least to some extent, in favor of (or against) concluding that it is an X; and if it is known to possess or lack the property
in question may justifiably be held to settle whether it is an X.\textsuperscript{134}

Difficulties about Achinstein's account are that it is not always clear what is 'semantically relevant' as a defining property, and that in general Achinstein seems to require all the semantically relevant properties to be retained through theory change for meaning to be unchanged\textsuperscript{135}. However this seems to gloss over the problem of such terms as Wittgenstein discusses which connote meanings in such a way that, although there is some sort of continuous spectrum connecting the various denotations of the term, it is possible for a denotation not to share any connotations with a denotation in some other part of the spectrum\textsuperscript{136}. In a less radical way, it is presumably possible for only a sub-group of semantically relevant properties to be retained without one being prepared to abandon a term or fail to understand its meaning.

MacCormac criticizes Achinstein on somewhat similar grounds. Achinstein, he says, can only account for change of connotation from one theory to another with stability of meaning or intelligibility when the non-semantically-relevant properties change and 'seems to be tacitly admitting in the debate with Feyerabend that when the semantically relevant properties change, the meanings of the new term are determined by the new theory and we have the problem of how these terms can be intelligible'. MacCormac goes on to say that if we identify any properties or connotations which do change, without a resulting general change in intelligibility, as non-semantically-relevant then we are simply making Achinstein's claim true by definition, and the argument loses its point. Further it is sometimes difficult to isolate semantically relevant properties in actual cases where a term
A linguistic device which can account for a change in what Achinstein has called semantically relevant properties is the metaphor. Crudely stated, a metaphor uses the known to express the unknown. This might be done by juxtaposing familiar words not normally associated or by using a word to mean something different from its normal usage. The metaphor is partially intelligible because we recognize the parts of it or the old meaning of a term and while the new combination may stretch our imaginations, it is not beyond our comprehension. Some of the 'semantically relevant' characteristics may change while others remain the same.\textsuperscript{137}

MacCormac then gives a theory of metaphor which he believes enables one to make sense of radical change of meaning from one theory to another without loss of connection between the two uses of the same term and with the retention of intelligibility. He uses a fusion of two theories of metaphor—a tension theory and an epiphor/diaphor theory. The tension theory, which he attributes to Berggren (q.v. below), Black, Turbayne and Wheelwright, is one which attributes to metaphor an 'as if' quality, since if taken literally the metaphor would result in absurdity or contradiction or paradox. However, in order to make sense of it we treat the tension as a hypothesis about the principal subject which we resolve by assimilating the subsidiary to the principal subject and projecting some meaning from the subsidiary subject on to the principal subject, thus modifying the principal subject in some novel way. However, as I have indicated above, there is in fact some difference between Berggren's and Black's versions of the tension theory. Hesse
is right in interpreting Black's version as implying a permanent extension or mutual modification of meaning of the principal and subsidiary subjects which thus resolves the tension and moves the metaphor in the direction of the literally true; at least this is the case in the scientific use. Berggren as we shall see, and Turbayne, as we have already observed, regard the tension as a permanent feature of a metaphor, so that there is always error, or at least the possibility of error, in accepting the metaphor uncritically. In this sense, then, a genuine metaphor is never dead. Such a position on metaphor is, as I have already indicated, much closer to the positivist position of writers such as Vaihinger who regard scientific theories as necessary fictions.

MacCormac supplements his tension theory of metaphor with a distinction due to Wheelwright between the epiphoric and diaphoric elements in a metaphor. (This was mentioned in Chapter 3, above) The epiphor 'rests upon an analogy which we can experience (the physical picture of what happens)' and the diaphor 'suggests a new interpretation'. This distinction seems rather unclear stated in just these terms, but it becomes clearer in the accompanying exposition:

Diaphor produces meaning primarily by suggestion. The juxtaposition of words suggests possibilities for experience rather than expressing experiences that can be tested immediately.....Pure diaphor would be unintelligible since both referents would be unknown to us. Here the emphasis is upon the novelty of the diaphor, but in a later passage MacCormac stresses the non-semantic aspect of diaphor:

For some cases, especially among epiphors, the intelligibility of the new term does depend upon our recognition of some familiar properties
associated with the older scientific usage from which the metaphor arises. In many cases so many of the original properties are changed that intelligibility must come from some other source. Usually these are concepts or images that have little to do with its scientific meaning. Intelligibility, therefore, does not rest solely upon recognition of similarity of semantical properties. That which enables us to recognize a word as intelligible may in many cases rest upon our knowledge of semantical properties but in numerous other cases what allows us to recognize the term is not related to semantical properties at all but rather to the non-semantical characteristics commonly associated with it. This discovery that intelligibility of scientific terms rests not upon the solid rock of similar properties but in the case of hypothetical terms may depend upon imprecise and visual images or associations will not be comforting to philosophers of science who seek to discover a precise logic of explanation.

MacCormac then distinguishes four types of meaning change which can occur as a result of metaphor: (i) mainly epiphoric changes where some sort of category mistake or sort crossing occurs which is absurd but not logically contradictory, (ii) mainly epiphoric changes where there is a resulting logical contradiction, (iii) mainly diaphoric changes, where there is such complete absence of epiphoric connection that only diaphoric interpretations or resolutions are possible, (iv) mainly diaphoric changes, where there is also a logical contradiction. He thinks the latter case unlikely since 'so many of the properties of the term have changed, there seems little chance of generating contradictory statements from the two theories.' He cites the change in meaning
of the term 'entropy' in the move from classical thermodynamics to statistical mechanics as an example of (iii).

It can be seen that MacCormac's view is perhaps more radical than any of the others since it assimilates metaphor in science much more to poetic metaphor. This can be seen if one compares MacCormac's view to that of Hester, who subjects poetic metaphor to an extremely exhaustive analysis and concludes that it involves an introspective inner experience and distinction of meaning which he describes as follows:

Metaphorical seeing as is a seeing as between elements of an imagistic description.  
Metaphor is a fusion of sense and sensa because the seeing as in the metaphorical structure is half thought, half experience.

Hester is at pains to point out, however, that the 'imagistic' element is not concerned with eidetic images and it is fairly closely controlled by contextual considerations.

There are some features of our discussion of models which we may relate to MacCormac's account. Hesse, for example, we may remember, argues in Models and Analogies in Science that interpretation of terms and extension of a theory may involve what she calls pre-scientific analogies, and she gives the example of the analogy between pitch of sound and colour of light. It is also relevant to recall the claims of some modellists that the model is not to be identified with statements about it. For example McMullin says:

The theory is thus a linguistic and mathematical entity; the model is not.....The theory is derived from the model, therefore, not the reverse. Nor is
the model simply as an interpretation of the theory.

These and similar claims that models are concerned with (unperceivable) objects, processes or structures give some credence to MacCormac's claim that scientific thought can be conducted in terms of images, and other introspective procedures, of a non-logical, non-linguistic kind. If this is so then we can only talk about or communicate such insights by the 'analogical pointing' which metaphor permits.

Berggren's presentation of the metaphorical point of view is an interesting one because he expressly brings the literary and scientific use within the same general theory, while maintaining distinctions between them. Scientific metaphors, he argues, are essentially concerned with the comparison of structurally repeated ratios, as opposed to other epiphoric features such as 'pictorial likeness', or 'texture' ('common emotional charge, emotion-like feel'), as literary metaphors commonly are. 148

Berggren adopts a thorough-going tension theory of metaphor. He applies the theory both to scientific models and scientific theories, which he treats separately. By the latter he seems to be distinguishing mathematical theories, which involve theoretical entities, without reference to any models which may or may not be associated with them.

For Berggren the important kind of scientific model is a model, which is 'the intuitive creation of the imagination itself'. 149 He calls such a model a 'schematic model' and apparently has in mind that as a metaphor it relies on 'specially constructed systems of implications' (he mentions this expression of Black's in passing when referring Descartes's 'machine metaphor'). 150 Although such schemas or schemata are 'extraordinarily fruitful' and serve essential explanatory as opposed to merely
heuristic purposes by assimilating or integrating the scientifically anomalous with the scientifically accepted, it is as much a mistake to collapse them into the algebra to which they often give rise, as it is to reify them and confuse them with reality:

...if scientific schemata are to be profitably used, rather than mythically abused, stereoscopic vision is again essential. Though perhaps indispensable to the advance of science, the truth of such schemata, no less than their use, must be irreducibly tensional in character. They construe but they do not constitute scientific theories.....it is the implicit mathematical structure of scientific schemata which makes them capable of representing scientific theories.  

Berggren then considers scientific theories. Unless we are to make the 'mythic and absurd equation of mathematics with the world', or be satisfied with purely mathematical correlations, 'about whose truth it is meaningless to ask', our scientific explanations must be either empirical or theoretical. In the case of theoretical explanations (where the explicans is not simply another empirical law or combination of empirical laws), 'the explicans.....contains a set of theoretical laws.....which are not inductive generalisations but "free creations of the mind"'. In any case, the experimental explicandum 'must still retain its previously independent meaning and contingent truth, in conjunction with its newly acquired nomological necessity'. It should be noted that Berggren here seems to be maintaining not only 'meaning-invariance' but, in one aspect, 'meaning independence' for experimental laws in relation to theories. Berggren concludes that theoretical laws must be conjoined by co-ordinative definitions, on two different levels, to mathematical concepts on the one
hand, and to experimental laws on the other:

In neither case is an explicit definition involved. For,...no eliminations or replacement substitutions are possible. (He is referring to the failure of positivist and similar reductive programmes.) Indeed, any attempt to interpret such identifications univocally would again result in an inevitable absurdity—or myth.\textsuperscript{155}

Thus such a theoretical law 'must itself be plurisignative and tensional.\textsuperscript{156} (to be 'plurisignative' a 'sign focus' must have two or more referents; 'plurisignation involves a "both-and" rather than an "either-or" sort of meaning).\textsuperscript{157} The more laws about qualitatively disparate subject matters a theoretical law explains the more plurisignative and hence extensively metaphorical it becomes.\textsuperscript{158}

Berggren then turns his attention to self-explanatory paradigms of natural order (the ideals of natural order, or general conceptual schemes which were mentioned above and in previous chapters). Since such explanatory paradigms are not themselves 'discovered', for example by induction, but determine what sort of things are going to count as facts and thus 'partially create what they in fact reveal', Berggren regards them as also irreducibly metaphorical.\textsuperscript{159}

Lastly Berggren asks: 'what sort of reality is it that a theoretical entity may legitimately or intelligibly have?'. Taking the neutrino as an example, he concludes that:

.....we find that while no theoretical entity can be either empirically real in the sense of sticks and stones, or simply mathematically real, any such entity may or must claim a mode of being which is irreducibly tensional in character. The neutrino, that is to say, exists precisely at the point of intersection between pure mathematics and
experiments on beta-ray decay...

The last writer whom we shall consider who puts forward a metaphorical view is Harre. Once again we find that his opinion is associated with the idea of a model. However, Harre does not claim that models in general are metaphors, but does locate metaphors in science as a necessary part of the scientific structure. Moreover he regards the presence of metaphors as evidence for the alternative thesis to positivism which he puts forward, and which he calls 'ontological depth'. Fundamental to Harre's thesis is the claim that:

Scientists, in much of their theoretical activity are trying to form a picture of the mechanisms of nature which are responsible for the phenomena we observe.

Of the word 'mechanisms' he says:

I hope it will become clear...that I do not in the least intend anything specifically mechanical by the word 'mechanisms'. Clockwork is a mechanism, Faraday's strained space is a mechanism, electron quantum jumps is a mechanism, and so on. Some mechanisms are mechanical, others are not. I choose the word 'mechanism' for this use largely because it is the word usually used for this purpose. We talk of 'the mechanism of a chemical reaction', 'the mechanism of bodily temperature control', 'the mechanism of star creation', and so on.

In Theories and Things Harre puts forward a fairly general theory about metaphor in science. It is reached as follows. Scientists are seeking to explain phenomena in a stronger sense of explain than 'subsume events under general laws'. They are trying to form pictures of the mechanism or better still trying to find the real mechanism of nature. In
order to do so they use models, which are derived by analogy from parent situations (models,). By means of the model, the terms of the theory are connected to operationally-defined terms (observation terms) by bridge-statements which express relations of analogy. The parent situations (models,2) consist of mechanisms which are already understood at the common sense level. On the face of it some theories do not have this analogical connection with common sense mechanisms but when the terms they employ are clearly described there are some of them which turn out to be metaphors, and it is by means of these metaphors that one can establish the bridge statements which give the analogy relation to connect the theory with common sense mechanisms. How well the picture of the mechanism of nature, conveyed by this relation of analogy, fits is something which bears investigating. It may fit so well and the analogy be so close that one is justified in regarding the mechanism expressed by the model, as being the real mechanism.

A metaphor, Harre says, is 'an expression effectively definable with reference to one paradigm case, p.c.t., but not fully defined without reference to another paradigm case, p.c.t, where to is earlier than t'. Of a paradigm case procedure he says:

I shall say that a term has been defined with reference to a paradigm case (p.c.) if it could have been introduced by ostension, i.e., we could have indicated what it meant by pointing to something which it could have been used to describe. The paradigm case will be that to which we could have pointed in introducing the term, and the whole method of introduction I shall call a paradigm-case procedure (p.c.p.).

By means of these notions he distinguishes D-terms, definable
by reference to one paradigm case, from M-terms, 'effectively
definable by reference to one paradigm case but fully
definable only by reference to two or more paradigm cases;
provided that the occasions of the use of these paradigm cases
are historically distinct. A term has depth of meaning if
two or more p.c.p.'s are required to define it fully.'\(^{168}\)
D-terms and M-terms are used in theories. M-terms, as
can be seen from the definitions, are a sub-class of
metaphors, and when they occur in theories it is these
terms which give the theory a depth of meaning which provides
the bridge to common-sense mechanisms:

Why is the language of physics sprinkled with
M-terms like 'current', 'energy', 'force', 'repulsion',
'field', 'conductor', 'wave' and so on? To answer we
need to see what function M-terms perform. M-terms,
in contrast to D-terms, are picture-carrying
expressions....By carrying a picture into the
description, an M-term is used both to describe
and explain, though admittedly on a low level.\(^{169}\)
The essential outline of Harré's argument is contained in the
following quotation:

It follows from all this that when we use metaphors
the theory constructed with the help of such terms
has a hidden structure. And the general analysis
of M-terms shows that this hidden structure is
identical with the structure of a \( P_1 \) theory (a
theory which purports to describe a mechanism).
The simplest case would be a metaphorical term
effectively defined for the purposes of the theory
by p.c.p.\(_1\) but finally defined by another procedure
p.c.p.\(_2\). The use of the metaphorical terms would
then involve both p.c.p.\(_1\) and p.c.p.\(_2\). p.c.p.\(_1\)
corresponds to the factual element in a \( P_1 \) theory;
Thus it can be seen that while he does not claim that models are themselves metaphors, Harré does assign a role of central importance in the functioning of theories as explanations to terms which are metaphors. He also says, in the same work, that metaphors are the weakest types of theories (presumably by this he means single metaphors).

In a later book he gives two accounts of metaphor in science, again in relation to models, and since they concern different points from the previous exposition these deserve mention, as they constitute a qualification of the remarks above. Sometimes, Harré says, we use a model for a theory (he here seems to be using the word in the sense of model) which has an analogy to the things or processes described in the theory, but for which we do not wish to make any claims as to the model being like the real mechanism of nature, and where the model is not aiming to give a causal explanation. In this case we may use metaphors from the model without them having the function he assigned them in *Theories and Things*:

The model offers us nothing by way of explanation, and no existential hypotheses, but it does provide, in the system of metaphors, a picturesque terminology. Many metaphors are indeed just this, the terminological debris of a dead model.

Harré also mentions cases where a model itself (as opposed to terms connected with a model) functions as a metaphor. He distinguishes between models where 'model states' are 'linked to phenomena by hypothetical generative relations', and where the relations between the sentences used to
describe the model and the sentences used to describe phenomenal effects involve consideration of 'causal transforms'\(^{173}\), and models involving only 'model transforms'.

He says:

Where there is a causal relation the cause and effect are independent existents, and the mechanism by which they are related can fruitfully be asked for, opening up a new dimension of explanation. In the other kind of transform, which I shall call a modal transform, the relation between the states of the model, considered as a hypothetical mechanism, and the phenomena, is not such as would give them independent existence. The state of the model is existentially identical with the phenomena. For instance, from an existential point of view, reflecting light of a certain wave-length and the being coloured a certain hue of a surface are identical states of the world.\(^{174}\)

Later in the same chapter Harré discusses a model which involves only such modal transforms:

"...since all its connections with phenomena are mediated by modal transforms, it is a metaphor..... Its function is to illuminate the facts, to throw them into a new light, to make them more memorable. Its function, in short, is literary.....For any theory with only modal transforms we can look only for a function as metaphor and not anything else.....the sentences describing it become metaphoric-ical redescriptions of phenomena, and we commit ourselves to an effectiveness total unlikeness between mechanism and model.\(^{175}\)

This brings us to the end of our consideration of various metaphorical views about science. As is evident,
although many of these views are connected, there is usually at least a difference in bias, and often a distinct difference in kind, between the positions held. In the next chapter I shall attempt some sort of evaluation of these claims and their importance for science.
Chapter 5

The views discussed in the last chapter appear to characterize different levels or features of science as metaphorical and to rely on a variety of conceptions of metaphor.

The different views we have presented may be summarized as follows:

(1) For Buchanan it is the propositions of science which are metaphorical, ranging from the simplest statement of the measurement of a parameter to a whole general theory of a field of phenomena.

(2) For Robinson it is the activity of formulating, describing and communicating novel ideas which is metaphoric.

(3) This characterization of the process of thought in scientific innovation as being metaphorical is central to Schön's view as well. Schön also holds that the incorporation of the results of such processes of thought into the permanent corpus of scientific knowledge leads to the terms of scientific theories being metaphors which reveal the existence of underlying theories.

(4) While Turbayne seems to hold that scientific theories in general are metaphorical he in fact usually deals mainly with the general conceptual schemes which underlie or are assumed in a whole historical corpus of theories and with the general metascientific terms in which we conceive the structure and function of scientific theory.

(5) Black, Hutten and Ramsey couple assertions as to the essential role of models in science with an assimilation, in varying degrees, of models to metaphors. Thus they argue that models constitute a certain class of metaphors or quasi-metaphors which have an essential role.
in science.

(6) Berggren completes the assimilation of models to metaphors more explicitly and unequivocally, and goes on to argue more generally that all scientific theories, theoretical models and theoretical terms are metaphorical.

(7) Harré says that terms employed in theories are often metaphorical, thus revealing ontological claims about explanatory mechanisms. On the other hand some terms in scientific theories are metaphorical because they do not make ontological claims about explanatory mechanisms, but are based on the terminology of a model with only a formal analogy with the theory. Some models are metaphorical because they do not attribute causal mechanisms to the domain of the theory but merely illuminate it by redescription.

(8) Hesse regards the formulation of a new theory as a metaphorical redescription of the phenomena.

(9) MacCormac regards the relation to each other of the same terms in different (normally successive) theories of the same domain as metaphorical.

While these different accounts deal with metaphors of widely different scope and thus with scientific propositions at correspondingly varied levels of generality, i.e. from observation reports (e.g. measurements) to metascientific and metaphysical assumptions, and while some accounts are more specific and analytic about the place of metaphorical terms in scientific knowledge, e.g. some attribute metaphors to the explanatory parts of scientific theories, some to the terminology of a model, there is nevertheless sufficient connection among these views for us to establish some general themes for criticism. At the same time we may, in passing, refer to points which arise especially in relation to particular views.

In general, then, it might be said that the metaphorico-
-al view regards the process of concept formation and as a result the process of theory formation in science as essentially metaphorical, and involving the metaphorical use of terms. These terms remain in science permanently and constitute (usually unexamined) assumptions of an explanatory, descriptive or metaphysical kind. However, there is some disagreement as to how explicit such metaphors remain after their incorporation into science.

It will perhaps help to clarify these viewpoints if we first discuss them in the light of some current theories of metaphor, and then go on to consider the various difficulties for and objections against the metaphorical viewpoint, and finally summarize the main virtues and advantages of the metaphorical position.

As is usual where a number of theories still vie for acceptance, the subject of the theory of metaphor is still a vexed one, with many confusions unresolved and a certain amount of interfusion and overlap between the various theories. Beardsley provides a useful summary of the main theories in his article in The Encyclopedia of Philosophy and he differentiates four main theories of metaphor. These are: the emotive theory, the iconic signification theory, the comparison theory and the verbal-opposition theory\(^1\). However, since Beardsley says that the iconic signification theory 'grows out of' the comparison theory\(^2\) and allows that a modified version of the verbal-opposition theory 'comes close to' the iconic signification theory\(^3\), and since the other theories which we have mentioned such as the tension theory and the interaction theory also overlap some of Beardsley's four categories, Beardsley's account is obviously not exhaustive nor are his kinds completely exclusive.

The emotive theory has already been discussed in Chap-
-ter 3. If held in a thoroughgoing way, it would be completely fatal for any metaphorical view of science, but it is rarely, if ever, now held in such a way as to deny cognitive significance to metaphor, and it is, in any case, fairly easily refutable, so that we need only consider those versions of it which also assign a cognitive function to metaphor and are thus assimilable to the metaphor-ical view of science. In passing, it is worth noticing that emotive claims have been made for the use of metaphor-or in science in one area - Pederson-Krag claims that Freud's metaphors, used in psycho-analytic thought, were chosen both to clarify theory and to give subjective emotional understanding.4

Isenberg gives an aesthetic version of the emotive theory, but he combines it with a cognitive theory which vacillates between a comparison and an interaction theory (q.v. below)5. He regards it as essential that a metaphor be 'aesthetically moving'6. He concludes that a 'metaphor is such a transference of a word or phrase normally applied to one thing to a different but analogous object as to register upon the aesthetic sense and evoke some response, positive or negative'7. As it allows a cognitive role to metaphor such a definition might be applied to science if it could also be shown that the aesthetic criterion was fulfilled. It could, of course, be claimed that for those who first hear them scientific metaphors are aesthetically moving, and some sort of case in terms of a 'eureka' reaction to scientific discovery might be made to support this. A better answer to Isenberg's claims might be made, I think, by arguing that, since being aesthetically moving is not a differentia of metaphor, whereas perhaps cognitive transference of the kind mentioned is, aesthetically moving metaphors may be regarded as only a sub-class
of metaphors. It also seems to be valid to claim that a metaphor does not cease to be a metaphor after we have been aesthetically moved by it (unless we argue that every repeated reading of such a metaphor is aesthetically moving).

Herschberger gives an aesthetic account of metaphor, but regards this as resulting from the 'complex integrated .....experience'\(^8\) of a number of 'prose arguments' simultaneously referred to\(^9\). Herschberger also holds something like a substitution view of metaphor (q.v. below) but this will be dealt with later.

On the whole such analyses of metaphor have in common the belief that there is a transient emotive quality to metaphor, and they suggest that any attempt to pursue a metaphor for further cognitive purposes is not strictly metaphorical. They therefore raise the question, which we shall consider later, of whether metaphors can be in any sense permanent.

Henderson, who argues in relation to the metaphysical use of metaphor that such usages are both 'appraisive' and 'prescriptive'\(^10\), seems to be putting a point of view that runs counter to Turbayne's claims for metaphor at the metascientific or metaphysical level, but Henderson specifically repudiates those who would claim that metaphysical statements are not designative, and argues that metaphysical uses may imply 'important beliefs about the kinds of explanation which we can apply to events'\(^11\), and necessitate 'a fresh account of the contrast' between terms which we apply to events\(^12\). Henderson also gives a substitution account of metaphor.

On the whole, then, whether one can assimilate such accounts of metaphor to science will depend on how central one regards the cognitive part of metaphor, and whether
one regards metaphoricalness as essentially transient or not.

The iconic signification theory, put forward by Henle, has not, so far as I know, been used as the basis for any metaphorical claims for science, although, as I remarked in relation to Black’s assimilation of models to metaphors, Black does say that scale and analogue models are icons, and he quotes Peirce’s definition of an icon as “a sign which refers to the Object that it denotes merely by virtue of characters of its own, and which it possesses, just the same, whether any such Object actually exists or not. Anything whatever is an Icon of anything, in so far as it is like that thing and used as a sign of it.” By this definition theoretical models may also be regarded as icons, and it is perhaps surprising that no one has used Henle’s iconic theory to argue that models are metaphors. The assimilation of model to metaphor is the more plausible when we look at the details of Henle’s account. For example, Henle says that the icon, which is the referent of the metaphor, can signify something or some new undesignated aspect of something by virtue of its similarity to the novel or undesignated aspect, and that some ideas are inconceivable apart from such iconic signification. Henle regards such an icon as deployable, and therefore capable of elaboration. Enough has been said, I think to show that Henle’s theory is at least not inconsistent with some of the metaphorical claims that have been made for models, although it is not specifically associated with them.

The comparison theory of metaphor tends to be associated with the view that a metaphor can be translated without cognitive remainder into the literal. Black regards the
comparison theory as a special form of a substitution theory, which is a theory which holds that a metaphorical expression is used in place of some equivalent literal expression\(^\text{17}\). Pure substitutionists are hard to come by nowadays, but the view is represented by writers who claim that the referential or cognitive part of a metaphor is wholly translatable into literal language. Henderson, for example, (mentioned above) states that he makes the 'fundamental assumption' that 'metaphorical language can be translated, without significant loss, into other language'\(^\text{18}\). With regard to 'an interpretation in direct language' he says:

if it confines itself to the referential part of what the metaphor suggests, there is no reason, in principle, why a complete translation of this cannot be achieved.\(^\text{19}\)

Herschberger similarly claims that:

\(\ldots\) metaphor\(\ldots\) is fundamentally an expository, and - in its way - economic prose usage;\(\ldots\) in principle\(\ldots\) a metaphor is reducible to a multiplicity of prose arguments\(\ldots\).\(^\text{20}\)

Elsewhere, she says:

This means simply that all metaphor is susceptible to prose paraphrase.\(^\text{21}\)

These theories, or any view which constitutes a comparison theory, i.e. a view that:

A metaphor\(\ldots\) is an elliptical simile, that is, a collapsed comparison from which 'like' or 'as' has been omitted\(\ldots\).\(^\text{22}\)

are similar to the theory of metaphor embraced by Buchanan. We may remember that he says explicitly that 'metaphors are suppressed or elliptical similes'\(^\text{23}\). On the other
hand there is the complication that Buchanan, at least in one representation of his ideas, regards the literal expansion of the metaphorical as an asymptotic limit. One can, however, make sense of his remarks in the following way. Buchanan indicates that his logistic is similar to the process that Bentham used, to substitute the 'real' for the 'fictional': this is called 'paraphrasis' and consists of 'archetypation' and 'phraseopherosis'. Arche-typation is the process by which a term is made to represent a thing, and phraseopherosis is the 'filling-in' (or expansion into) secondary, tertiary, or n-ary symbols that mirror the primary archetypal symbols. An example of archetypation is 'Napoleon was a wolf' and the phraseopherosis of this is the substitute for the archetypation sentence of a system of literal sentences such as 'Napoleon was a soldier', etc., which indicate how the Napoleonic armies in their relation to peoples and princes behaved in a variety of ways that make sense of, and fill out, the cryptic implications of the metaphor. In this way the literal determinants of the matrix columns are words like 'soldier', 'plan', 'ambition', 'prey', etc. Now presumably these words themselves can serve as archetypations and thus may be expanded into further, or other, sets of determinants. Thus the metaphorical/literal distinction is relative to a given archetypation, and Buchanan may be said to hold a comparison theory of metaphor. There are, however, traces of other theories in Buchanan. For example, he holds that expansion is only possible 'when the two sides are allowed to interact symbolically', which seems to suggest a tension theory or an interaction theory. His ontological attitude, although, not clear, seems close to a positivist one, since he refers approvingly to both Bentham and Vaihinger, holds that fictions have a 'high pragmatic value', and, as we have seen, approximates his logistic to Bentham's theory of fictions. Although his view seems to be concern-
-ed with showing existing relations in different domains to be analogous, rather than creating new insights and formulations, he does refer to what he calls 'assimilation of experience':

The development of symbols goes on by assimilation of experience to the projective field and this is ordered by analogical connections. This assimilation can be pictured as the temporal process of accumulating new material from experience where the principles of selection and the principles of order come from past archetypes.\(^{28}\)

This sounds very like formulating a domain of phenomena by metaphorical means. The actual examples of metaphors which Buchanan gives in relation to science, e.g. in *Poetry and Mathematics*, where he lists seven metaphors about man ('Man is a system of electrons', 'Man is a machine', 'Man is an animal', 'Man is a bundle of habits', etc.\(^{29}\)), sound more like metaphysical statements than examples of genuine scientific theories, but they do bring out one important aspect of Buchanan's view. He seems to regard the various metaphors (archetypations) as equally valid ellipses of the same domain, dealt with in a variety of matrices. This relates to the notion of relative truths which we shall mention when dealing with difficulties of and objections to the metaphorical viewpoint (section 4).

Beardsley's last classification of theories of metaphor, the verbal-opposition theory, really refers to his own theory by which he argues that metaphor rests upon a clash between meanings. The metaphor cannot be made sense of in the ordinary way because there is a logical conflict of central meanings, so there is a shift to marginal meanings or connotations of the metaphorical words and/or
the literal context. There are common features to the
verbal-opposition theory and the tension and interaction
theories of metaphor. The main distinctions seem to be:
(i) The verbal-opposition theory emphasizes intension and
connotation as opposed to extension and denotation, e.g.
Beardsley says:

When a predicate is metaphorically adjoined to a
subject, the predicate loses its ordinary extension
because it acquires a new intension - perhaps one
that it has in no other context.  

Beardsley contrasts his view with those where a metaphor-
ical word is supposedly used denotatively to/two refer-
ents:

Thus there is no question of 'spiteful' in a
metaphorical context denoting spiteful people
and injecting them for the purposes of comparison;
the price it pays for admission is that it funct-
ions there only with its connoted characteristics.

Beardsley obviously here means 'connoted characteristics'
as restricted in the new context.
(ii) The tension theory in some of its version does speak
denotatively, e.g., Berggren, as we have seen, adopts
Wheelwright's term 'plurisignation' and speaks of a
metaphorical sign having two or more referents. The
tension theory is, naturally, at pains to emphasize the
existence of tension in metaphor in a permanent way, and
the existence of two referents provides some sort of
ground for this.
(iii) The interaction view emphasizes the creation of
novel meanings. However, it speaks of principal and subsid-
iary subjects, which seems to imply plurisignation,
although Black does speak of 'associated implications' and
'systems of things', rather than of 'things' simply.

However, all three versions of the theory stress the
creation of new meanings to some degree, and they are all therefore pertinent to that metaphorical view of science which sees the activity of scientific innovation and discovery as metaphorical. As we shall see, there is a difficulty for both the interaction view and the verbal-opposition view in that, if metaphor is a transient expansive activity by which extensions, intensions and consequently concepts and meanings change, there seems to be no particular reason, once this has happened, why the metaphor should not 'become literal', and this would preclude any claims that science has a permanently metaphorical character.

All the remaining accounts of metaphor in science, apart from Buchanan's, employ one of these three interconnected theories of metaphor. Black, naturally, as its originator, and Hesse, explicitly, hold the interaction view. Berggren and MacCormac state that they hold the tension view, although there are traces of the verbal opposition view in MacCormac. Ramsey, since he acknowledges his similarity of opinion to Black's may be presumed to hold an interaction view. The positions of Turbayne, Hutten, Schön and Harre are not so clear-cut, but, in view of their allowing some sort of permanent metaphorical character to science, I interpret them as being tension theorists. Robinson's position is, on the evidence, that of an interaction theorist.

We are now in a better position to consider some of the difficulties of and possible objections to the metaphorical view of science. These are as follows:

(1) The so-called 'metaphorical' use of terms in science is not metaphorical at all but simply the normal process of generalization or the extension of meaning to new
referents, with or without consequent changes in the connotations of a term.

(2) Metaphors can only refer to trivial, superficial, and not logically central aspects of meaning. So they cannot be important in the cognitive activity of science which is mainly concerned with the logically central.

(3) Metaphors cannot be used as the basis of any sort of argument. They are not therefore of any use to science since the principal use of scientific concepts is to serve as a basis for inference.

(4) Metaphorical statements are not true or false, but merely apt or inapt, appropriate or inappropriate. Scientific statements make truth claims and therefore cannot be metaphorical.

(5) Science has to deal with radical novelty. This cannot be explicated in terms of its resemblance to old concepts. Such explication is misleading and incomplete.

(6) Metaphors are at best heuristic. They become permanent only at the expense of becoming literal.

(7) Metaphors, like analogies, are dangerous, since they are double-edged. While they have a legitimate heuristic use, and are also suggestive, the suggestions they make are often the source of errors which would otherwise have been avoided.

(8) It is difficult to account for the success of scientific theories if they are in any way intrinsically metaphorical.

(9) Any theories based on metaphors are highly hypothetical.

(10) The theory that science is metaphorical does not distinguish between good and bad science. For example, science in the Middle Ages and Renaissance relied heavily on analogies and metaphors, and indeed this was one of the principal reasons why they were unsatisfactory
periods of science.
(11) There is a commonly recognized antipathy between scientific and literary language, characterized by the fact that the metaphorical is appropriate to literary and the literal to scientific language.
(12) The last point (11) can be illustrated by reference to the history of science, particularly in the seventeenth century, when a resolute attempt was made, which was permanently successful, to eschew metaphors from science.
(13) Metaphors are primarily aesthetic or affective, not cognitive.
(14) On a thorough-going metaphorical view of science, there is no such thing as the 'literal' with which the metaphorical might be contrasted. In particular, there are no neutral 'literal' facts, with which all theories are concerned.
(15) Metaphors are too ephemeral or insubstantial, too little concerned with the systematic to function in science.

We shall now consider these points in more detail.

(1) Both Stern and McCloskey give an account of metaphor which seems to preclude treating science as metaphorical. Stern distinguishes metaphor from 'nomination', which implies what Stern calls 'intentional transfer':

The intentional naming of a referent, new or old, with a name that has not previously been used for it, will be called 'nomination'.

Intentional transfers on a large scale occur in the naming of new referents in the course of scientific, technical or social progress.

He attributes emotive elements to metaphor, and says that a metaphor 'gives the emotion directly, instead of
talking of it\textsuperscript{36}, but he also approves of Stählin's claim that in a metaphor 'the transfer (of meaning) does not involve an essential identity of the two referents', and remarks that this serves to distinguish metaphor from regular transfer\textsuperscript{37}.

McCloskey does not refer to intentional transfer, but makes a similar point by giving an account of metaphor which emphasizes that metaphor is not an intentional transfer, and, unlike the literal use of words, is not concerned with central meanings, and hence not concerned with essential identity:

\ldots the difference between the resemblance between things called by the metaphorical word, and things called by the literal word, and the resemblance between one thing called by the literal word and another, is not merely a question of the number of points of resemblance: but a matter of whether the points of resemblance are striking but superficial, or unobvious but important. Literal language utilises resemblances which are clues to other resemblances, metaphors those which are not.\textsuperscript{38}

Again, later, she says:

When language is used literally, the logical and psychological aspects of language work together. In a metaphor, however, they so to speak work in opposite directions. The context makes it impossible to use the logical aspects and leaves us with the psychological.\textsuperscript{39}

Such an account of metaphor as Stern's and McCloskey's would invalidate my assimilation of Robinson's account of innovation to the metaphorical. In general it might be said that it also tends to invalidate claims such as Hesse's, which apparently restrict the description metaphorical to the moving frontier of scientific discovery and progress.
For Hesse speaks of the distinction between literal and metaphorical as being relative[^40], implying that the metaphorical becomes literal. She speaks, for example, of metaphorical redescriptions replacing 'the original literal descriptions of the same system'[^41], which implies that previously established descriptions (even though, in the case of theory-replacement, these were originally metaphorical redescriptions) are literal. The emergence of the literal from the metaphorical is also implicit in her claim that the metaphorical descriptions become part of the observation language:

> There is one language, the observation language, which like all natural languages is continually being extended by metaphoric uses and hence yields the terminology of the explanans.[^42]

Presumably both Stern and McCloskey would deny that the metaphorical can become literal in this way. Stern would no doubt claim that what is involved here is simply 'nomination' or 'intentional transfer'. And since it can hardly be asserted that theoretical redescriptions are superficial, unimportant or non-logical, McCloskey would regard them as literal.

Such denials of the description 'metaphorical' to the processes of scientific innovation and theory-forming would probably be countered by Hesse or other holders of the interaction theory of metaphor by the claim that it is essentially the interactive, interanimating, effect of the extension of existing language to new domains which characterizes metaphor, and this is present in both the literary, emotive case (where there is no question of identity) and the scientific case, where presumably some ontological claims, and therefore at least partial identity, are involved. The ascription of 'metaphorical'
by Harre to cases where there are no identity or causal claims involved in the use of a certain sort of model, and MacCormac's account of primarily diaphoric changes of meaning from one theory to another, attribute a sense to 'metaphorical' which is quite consistent with Stern's (disregarding the emotive criterion) or McCloskey's account of metaphor.

Berggren, Buchanan, Hutten, Schön, Turbayne, and Harre (in Theories and Things and when speaking of models involving only modal transforms) all seem to be clearly at variance with Stern and McCloskey, however, in regarding metaphors as being cognitive and permanent, and involving essential identity and potential inference, although there is a weakness in the former group's arguments, in this connection, to which Turbayne in particular is susceptible. For Turbayne stresses that scientific metaphors are often, or usually, taken to be literal. Stern and McCloskey might well argue that what Turbayne claims to be metaphorical is commonly taken for literal for the simple reason that literal. This difficulty, though less central, is also present for Berggren, Harre and Schön, since they also allow that metaphors sometimes appear to be literal. Schön, for example, defines metaphors as 'the traces left by the displacement of concepts', and thus seems to ensure that metaphors are permanent, but he explicates this with the following simile:

They bear witness to complex processes of displacement of concepts over time just as present living species bear witness to biological evolution.

However, it is far from the case that living species bear witness to their evolution in any self-evident way, and, similarly, it may be that only the antiquarian
interests of the etymologist will recognize the traces of displaced concepts for what they are, i.e. concealed metaphors. For the rest of mankind the words concerned will be taken, and perhaps ought to be taken, as literal.

This problem is very much aggravated in Turbayne's case. Unless there is some clear criterion by which metaphors remain metaphorical, even after long incorporation into scientific knowledge and belief, there is some difficulty in claiming that what passes generally for literal really is metaphorical. On the other hand if there is a clear criterion, then it is difficult to see how we become the victims of metaphor. We can perhaps soften the impact of this sort of difficulty if we argue that although there is a clear criterion for distinguishing the metaphorical it is not one we usually attend to. Edie, for example, says:

Once established, the metaphorical use of the original word is no longer noticed; its essential ambiguity tends to fall far below the level of awareness from the moment that it is taken as designating another, now distinguishable, experience. With metaphors, as with all words, what fixes our attention is not primarily the verbal expression itself but that of which we are speaking through words.46

In Turbayne's case the force of this difficulty about recognizing that a usage is really metaphorical is greater because he denies that any revelation of the literal is even possible. ('The attempt to re-allocate facts by restoring them to where they"actually belong" is vain.') The method for making metaphoricalness explicit is not very clearly given in Turbayne, and is presented more by example than by rule. One way is to show that the use in
question is different from or is not consistent with the implications of some other use of the term concerned. Thus, for example, he seeks to show that using the word 'force' in reference to gravity is 'hylopsychism' and that necessity is transferred from logic to events. Presumably therefore Turbayne is not claiming that there are not literal uses of the words which are used to describe the scientific 'facts', but that the 'facts' are not describable by literal uses of words (however, this distinction is not clearly made by him). In practice Turbayne seems to put most faith in the method of substitution for exposing metaphor. A metaphor is exposed by showing that an alternative metaphor will do the job just as well, if not better:

Perhaps the best way to avoid being victimized by a metaphor worn out by over-use is to show that it is expendable. The best way to do this is to choose a new one.

Horsburgh suggests a number of ways in which hidden metaphors might be exposed, and, although I have adapted Horsburgh's analysis to different purposes from his, and used different examples, the terminology relating to criteria and the leading ideas in the following are taken from him. Possible ways of exposing hidden metaphors are:

(i) The Criterion of Standard Questions. For example, in relation to 'force', standard questions which apply to 'force' as used in relation to ourselves (i.e. the supposedly original use of 'force') might be applied to the scientific use. We might ask, for example, in relation to gravitational force, 'Who's doing the pushing (pulling)?', 'What's it being used for?'. The lack of sensible answers to such questions, it might be claimed, makes it evident that we have a metaphor in the scientific use. However, there seem to be several ways in which such
standard questions might fail to 'expose' a metaphor. First, some sort of answer in terms of the suspect use can often be given, e.g., 'Gravity's pushing, that's who,', in answer to the first, and 'To keep the planets in orbit' in answer to the second, of the questions asked above. Or it might be denied that such questions are legitimate because not central to the meaning of force. Or a more sophisticated answer might be, 'The word "force" means different things in different contexts. Obviously your questions don't apply in this context'. All of these re-actions to standard questions would make the charge of 'metaphor' hard to lay at the door of the user, and would certainly not indicate him to be any sort of 'victim'.

(ii) Criterion of Standard Echoes. By this criterion words are metaphors when they always do as a matter of fact suggest or bring irresistibly to mind implication drawn from some literal use, but not present in the metaphorical one, so that we are conscious of the metaphor being a distortion of some more aboriginal meaning and therefore misleading. However, I think Edie's remarks quoted above are obviously correct, and that, for example, when we think of an electric current, we are not simultaneously attending to our concept of a river current. Moreover, such a criterion would vary subjectively, and even if it led to an awareness of 'sort-crossing', it would hardly result in one becoming a 'victim', because of the very fact of this awareness.

(iii) Criterion of Logical Dependence. According to this criterion, no content or meaning or implication can be unpacked from or elucidated by reference to the metaphorical word alone, and without some reference to some other use in some other context. Of course, it is normally the hallmark of the original new use of a metaphor that there is this dependence, but this criterion would argue that such
dependence is permanent.\textsuperscript{54}

I am convinced that this criterion is in the region of the truth of the matter. It relates to my own claims that there are such things as established metaphors, and to Van Steenburgh's account (mentioned above in Chapter 3) of the metaphorical. Van Steenburgh claims that a word is literal in use only if its meaning on that occurrence is ostensive. He allows that metaphorical expressions do 'wear thin by usage', and goes on:

But it does not follow that metaphorical meanings become literal. They become trite, perhaps, but not literal. For no meaning can become ostensive, i.e. literal.\textsuperscript{55}

A similar linking of the literal with the ostensive occurs in Harré, although he does not use the term 'literal'. He says:

One can describe something with or without the use of metaphors. When a description in non-metaphorical terms is given I shall, for the sake of brevity, call the terms used D-terms.\textsuperscript{56}

Later he says:

I shall say that a term has been defined with reference to a paradigm case \ldots if it could have been introduced by ostension, i.e., we could have indicated what is meant by pointing to something which it could have been used to describe\ldots.\textsuperscript{57}

A term is a D-term if it is fully definable by reference to one paradigm case\ldots.

A term is an M-term ("metaphorical term") if it is effectively definable by reference to one paradigm case but fully definable only by reference to two or more paradigm cases; provided that the occasions of the use of these paradigm cases are historically distinct.\textsuperscript{58}

There seems to be a difference here from Van Steenburgh
in that Harré seems to refer the metaphorical to multiple ostensions rather than to the non-ostensive. Van Steenburgh holds that metaphorical terms acquire meaning in a given context only by transference from the literal (i.e. ostensive) meaning. Harré says that metaphorical meanings are obtained from several ostensions. However, it seems from the example that Harré discusses that he is using 'ostension' in rather a different sense from the usual in relation to some levels of paradigm-case-procedure, but it seems to be implicit in his account that explanation can only be derived from the level where a paradigm case is 'picture-carrying'. I do not think therefore that it is falsifying Harré, although it must be admitted that he does not make this clear, to claim that he resembles Van Steenburgh in that he makes the metaphorical uses of words 'parasitic upon literal uses' (to employ McColloskey's illuminating expression) i.e. dependent upon an 'ostensive' use, in the normal sense of 'ostensive'.

Harré's rather unusual use of the word 'ostensive', e.g. he talks as if a theoretical definition is an 'ostension', bears out Van Steenburgh's remarks:

'Ostensive' is a vague term. Many abstract terms and certain psychological terms are ostensive. Thus, if 'triangle' is ostensive, 'triangularity', naming not a Platonic substance but an observable property, is ostensive. And terms naming certain feelings, e.g., 'dizzy', 'thrill', are ostensive.

What is clear, I think, is that what is ostensive is not co-extensive with sense-date, or sensations. I incline to think that it is co-extensive with what is perceptible, or what is 'directly' perceptible. By this I mean that what is 'perceptible' in the derived sense of 'inferred from what is actually perceived' is not a candidate for
the ostensive but what is actually perceived is capable of being a candidate for the ostensive. However, there are a number of problems which would need clearing up. For example, one might be inclined to admit 'honesty', used to describe a witnessable honest act, as perceptible, and therefore capable of 'ostensive' demonstration, and therefore of being used literally, but one hesitates over 'honesty' as applied to character, or even more in the context of 'honesty is the best policy'. It is because of such difficulties that I inclined in Chapter 3 to talk about the practical working vocabulary and its complex network of concepts which we all, more or less, acquire by way of our ordinary education and upbringing as the base from which we are able to 'understand' metaphors of the usual non-esoteric kind, and I had in mind that it is this language which comprises the literal and coincides to a large extent with the 'ostensive'. However, there is the difficulty that even ordinary language, and the notion is admittedly vague, seems to contain what I have called established metaphors, so that we must confine the literal or the ostensive (if they are equivalent) to a narrower compass.

It seems to me that there is some recognition of this literal or ostensive basis to all our theoretical descriptions which motivates the positivists in their abortive attempts to reduce all science to phenomenal elements of some kind.

Some writers equate the literal with the observable, and this is perhaps not unlike regarding the literal as the ostensive. However, 'observable' is subject to the same vagueness of connotation as 'ostensive', and is in addition too narrow. Ballard relates the literal to the observable in the following passage:
I wish to suggest that the thing to which the literal symbol immediately refers — which convention chooses to be the 'thing' — is merely what is felt to be most clearly cognized. This procedure of convention is illustrated by the empirical faith which looks to the experience of the senses in order to confirm our beliefs. This faith holds that truth is to be found most clearly, perhaps only, in sense experience. Consequently it is held that the 'thing' to which all signs and propositions refer or ought eventually to refer is observed or observable fact. Thus literal symbols refer immediately to observed fact. Signs used metaphorically will refer to these literal signs. 62

It cannot be pretended that this attempt to draw a distinction between metaphorical and literal according to a criterion which would give metaphor a permanent status, and thus justify claims that science is permanently metaphorical, has been entirely satisfactory. Nevertheless I believe that Van Steenburgh's account is correct in general, although the notion of 'ostensivity' needs a more careful analysis than it has hitherto received.

(2) It has already been noticed above that Stern and McCloskey regard metaphor as being not concerned with central meaning or with an attempt to identify. This is perhaps the same thing as saying that metaphors are concerned with the trivial or superficial resemblances between things. This seems to be the point of Bunge's critique of the metaphorical point of view in the following:

When a factual theory attains maturity it involves only literal interpretations: it does
not involve as if's. Surely scientific explanation, if deep, is more than just a deduction from laws and data: it will be a subsumption under general-ites all right but, among these, some mechanism hypotheses will figure - that is, some assumptions will go beyond external or input-output relations. Yet such explanations in depth, or interpretative explanations....are alien to metaphorical explanations, which are superficial for being limited to similarities and for failing to concern the real thing. Correspondingly the metaphor view of scientific explanation.....which has recently been recommended in place of the deductive account, is totally inadequate.63

In a footnote to the phrase 'the metaphor view of scientific explanation' Bunge cites Hesse, Models and Analogies in Science, p. 157 - 'the deductive model of scientific explanation should be modified and supplemented by a view of theoretical explanation as metaphoric redescription of the domain of the explanandum'.64

There is an additional point of criticism of the metaphorical point of view here, because Bunge claims not only that the metaphorical is superficial but that it is proposed in place of the deductive account. However, the quotation from Hesse itself effectively disposes of this claim. For Hesse proposes that the metaphorical account should supplement and modify the deductive account of scientific explanation. While some writers who put forward the metaphorical view, especially those like Schön, who is concerned to give an account of the role of metaphor in scientific discovery, do not specifically deal with the deductive aspects of scientific explanation, it is more that they take these for granted as complementary to the use of metaphor than that they regard the use of metaphor as replacing the deductive
account. Moreover other writers who put forward the metaphorical view do specifically refer to a deductive system as part of a theory. Hutten, e.g., says:

...a theory, at least of physics, consists of two parts: a formal calculus and an interpretation linking the theory to experiment.65

Again, Berggren says:

....while scientific explanations, at least of scientific laws, are always deductive in character, such explanations may also be either empirical or theoretical.66

Hesse specifically discusses how metaphor relates to (but not displaces) deducibility.67

In general, there is no writer who puts forward the metaphorical view as an alternative to a deductive account. On the other hand, the way in which metaphors relate to the deductive structure of a theory is not made very clear in many accounts of the metaphorical view. There are two ways in which metaphors might relate to the deductive structure: (i) by making theoretical terms, and consequently the propositions employing them, intelligible or meaningful, and in some sense explanatory, at every level of the deductive structure. In this way the metaphors accompany or fill out the deductive structure with meaning throughout the theory, or (ii) by giving meanings to terms in those propositions or theorems of the theory which are capable of being directly interpreted experimentally or observationally in such a way as to make such an interpretation possible. In general, if we relate metaphors to models or to the language of models, and recall the various ways in which modellists think models function, it is reasonable to conclude that most writers on metaphor would want to attribute both roles to metaphor.
The main point of this is that if metaphor is a complement to rather than a replacement of deductive structure (of course, it may also help to formulate the pattern of deductive structure) it may be possible for the metaphor itself not to be concerned with the logically central, and while one perhaps would not want to characterize the meaning contributed by the metaphor as trivial or superficial it may not be concerned with essential or defining properties. This seems to be the position of MacCormac. He says, it will be recalled:

"...intelligibility of scientific terms rests not upon the solid rock of similar properties but in the case of hypothetical terms may depend upon imprecise and visual images an associations." 68

However, most holders of the metaphorical view do not give this sort of analysis of the use of metaphor in science, and would want to claim that metaphors in science are not concerned with imprecise and visual associations, but with important and perhaps central properties. The difficulty here is that metaphors which concern such properties seem to be in danger of collapsing into the literal and the alleged metaphorical activity in science becomes nomination, or transfer or generalization.

There are, I think, two ways out of this difficulty. One is to stress that where metaphors are used in science one is postulating a hypothetical mechanism or structure, which is in principle not verifiable as real, and therefore not capable of becoming literal. We may say therefore that in science we are mainly concerned with a 'prescinding' use of metaphor. This is described by Van Steenburgh in relation to its use in philosophy:

Philosophers more often than poets develop a conception (\textit{sic}) of \( x \), the nature of which is
initially unknown. Therefore they rely on except-
-ive clauses or devices other than false identity,
in order to change the literal meaning of a pred-
icate. 'God' and 'substratum' are originally
unknown \( x \)'s. Gradually they take on meaning by
predication of terms whose literal meanings have
been altered otherwise than by false identity.....
metaphors that 'build' meaning for terms originally
referring to unknown \( x \)'s are not of the perspective
variety.....Perspective metaphor involves present-
ation of two things situations and one is used
as a perspective on the other, reorganizing it in
a way that precludes, on principle, a literal
account of it. But the kind of metaphor now under
consideration - I shall call it 'prescinding
metaphor' - presents us with only one kind of
thing or situation. The other is an unknown \( x \)
and therefore, initially at least, the one can
hardly be used as a perspective on the other.

Such a prescinding use of metaphor might well 'build up'
certain properties for unknown \( x \)'s, i.e. theoretical
entities, processes or structures, without prescinding
logically central properties from the literal meanings.

This brings us to the second way out of the difficulty.
It is that we may well regard a use of a word as metaphor-
ical rather than literal even though we may, by using the
metaphor, be referring to logically central properties -
properties which make the use of the metaphor necessary,
even inevitable, but which are not sufficient to warrant
regarding the use of the word as literal. For example,
we might regard scientific entities as being in essential
ways (ways which, incidentally, can be used as a basis
for inference) 'particles' or 'waves' or 'currents', or
whatever, without regarding them as literally being particles, forces, waves or currents, on the grounds that some properties, while not logically central are semantically relevant to the literal use. A recognition that it is not always clear-cut 'defining' properties which determine our literal use of a term is contained in the discussion, which rather canvasses the question than proposes any solution, in Beardsley's 'The Metaphorical Twist'.

There Beardsley distinguishes 'criteria' for the literal use of a word from defining properties.

It may thus be questioned whether McCloskey and Bunge are correct in attributing the use of metaphor only to non-logically-central properties.

(3) Black quotes the following from Sir Walter Scott's *The Fortunes of Nigel*: 'Metaphors are no arguments, my pretty maiden'. In a more sustained version of the same claim, McCloskey contrasts metaphorical with literal language as a basis for inference. She says:

The most important feature of literal language is that one can make inferences from it. And she goes on to claim that we cannot similarly make safe inferences from a metaphorical statement. Of the metaphorical example she uses, 'The clouds are made of cotton wool', she says:

The hearer can infer from what the speaker says... that they are white and fluffy; and nothing further.

The speaker can infer nothing. She concludes from this that one cannot make new inferences from such a metaphor. In explaining why this is so, she says it is because the ordinary inferences which one might make are checked by the tension provided by the context in which the metaphorical words occur:

What is peculiar to metaphorical language is the
extent to which what we might call this continual frustration of the potential inferences is carried.

A number of points need to be made about these claims of McCloskey's. First, it is evident that some inferences can be made, even in the example quoted. And presumably some metaphors are more pregnant than others. Again, presumably the amount of frustration provided by the context can vary. To take her own example, one might easily infer that the clouds look soft, amorphous, teased-out, and so on, without encountering any frustration of inference. It is true that, without knowing the context, one cannot safely make such inferences, but presumably one of the virtues of scientific method is that it does provide some test of any inferences which might be made, and the virtue of models or metaphors is not intended to be that they license inferences but that they suggest them. Therefore although inferences from a metaphor or model are not prima facie valid (is any normal scientific, or even common sense, literal inference?) a metaphor or model does constitute a sort of argument. The very fact that it makes some sort of sense or point in a given context is an argument, no matter how weak, in favour of a metaphor or model. If we are capable of understanding a metaphor or model, and especially if we find that we can develop or deploy it, then it has already made a case for itself, even if some of the inferences from it are thrown out of court. There are two further points to be made. One is that the context serves not only to frustrate, but also to guide and promote, inference. It is central to any interaction view of metaphor that the effect of the context is not wholly negative, but that it has an important positive role. Another and connected point is that it depends a great deal on the 'speaker' and the
metaphor whether a metaphor encourages or allows a speaker to make further inferences, and hence whether it can lead to new inferences for him as well as his hearer. The account that was given in Chapter 1 of the analogical act and the way it can provide an intuitive perspective on a wide domain of phenomena, the details of which need to be made explicit to and followed up by the discoverer himself, suggests that metaphors can reveal hidden riches of potential inference to the 'speaker'. It can surely not be maintained that the metaphor of 'machine' as applied to nature or man was either limited in inference or greatly frustrated by new contexts. It is the virtue of good models or metaphors that they tend to exploit their novel contexts rather than be frustrated by them. The positive function of the context can be reinforced and the frustration lessened if the model or metaphor is supported by 'specially constructed systems of implications'.

(4) The claim that metaphors are not true/false but merely appropriate/inappropriate has already received some discussion in the chapter on metaphor. A thorough discussion of the problem of truth in relation to science would obviously take us far beyond the limits of this thesis, but it is sufficient to say that very few philosophers of science would regard the truth status of scientific theories as being a straightforward question about the answer to which there is general agreement. If there is any consensus of opinion it is likely to be on the side of agreeing that scientific theories are not generally regarded as final, or absolutely true or false; they are in an important sense 'corrigible' or even replaceable, which suggests that true or false are too ambitious as terms with which to evaluate theories. As
Black says:

We are beginning to understand how very far from being a literal generalization about observable facts the theories of any advanced science must be taken to be.\(^7\)

It might indeed be claimed then that the terms in which we evaluate theories can not inappropriately be linked with the different terms with which we might evaluate a metaphorical statement. A link of this kind is suggested by the sense of 'probable' mentioned by Williams in *The Ground of Induction*:

These suggest a new dimension of probability, the dimension connoted by \(\gamma \kappa \xi \zeta\), 'verisimilitude', 'likelihood', and 'Wahrscheinlichkeit'. Whereas our classical doctrine of cases may be paraphrased as describing the probable as what is usually true, this concept would have us describe it as like the truth 'in itself, in its evidence, in some more or fewer of its circumstances'.....The new dimension, which we may call 'analogy', we may grant deserves more attention than we have here given it.\(^7\)

The quotation in this passage is from Butler's *The Analogy of Religion*, and in Butler's discussion of this sort of probability there is a slight but extremely significant difference from Williams's account, for Butler talks not of 'the truth' but of 'some truth' - 'that which chiefly constitutes Probability is expressed in the word Likely, i.e. like some truth, some event'.\(^8\)

The significance of the difference here is that Williams's use suggests that a theory might be approximately true, or in the region of the truth, and this seems to imply that there is a truth involved, to which all such probable truths asymptotically tend. On the
other hand Butler's way of stating it suggests a way in which we might interpret Berggren's notion of 'metaphorical' truth. This interpretation follows our intuition that metaphors are not precisely or unambiguously true in the way that we ordinarily take some literal propositions to be. Instead of regarding theories (and metaphors) as like the truth we would perhaps be more correct in regarding them as professing a truth. Just as 'April is the cruellest month, breeding Lilacs out of the dead ground' is for certain purposes and within a certain intention capable of being meaningfully assented to or denied, and just as such assents and denials require explication, so it might be said that a given theory, employing a model of structure or process for some domain of phenomena and describing the phenomena in certain ways suggested by the model, is for certain purposes and within a certain intention capable of being meaningfully assented to or denied, and also requires explication, but in a more explicit and systematic way than the metaphor does. Just as the metaphorical statement does not state the only truth about April so a theoretical statement does not state the only possible truth about its domain of phenomena. Obviously there are great differences between the literary metaphorical use and the scientific use, and the scientific use has a much more rigorously circumscribed context of purpose and intention, particularly in that it employs a stock diction and is under the limiting control of a number of phenomenal considerations which are regarded as relevant; moreover the scientific use is required at least to harmonize with fairly complex logical structures and systems, including mathematical ones. However, in both cases, the poetic and the scientific, we are dealing with a
truth, not the truth. Looked at in this way, scientific truths, at least as far as they occur in theories, are relative.

(5) In a critique of the use of analogy, model and metaphor in quantum theory Bunge claims that there is a point in science where these devices are bound to fail:

if the world is variegated then analogy is bound to exhibit its limitation at some point, for what is radically new is precisely that which cannot be fully accounted for in familiar terms. 

Elsewhere he says that if we try to explicate an unknown field of science in terms of a familiar analogy it is possible for the analogy to fail:

And if the analogy fails utterly, we shall realise that some radically new ideas are called for because B is in some respect radically different from A.

Bunge goes on to argue that one should eschew analogies, metaphors and models from science, and substitute a 'literal' interpretation. (He does admit that models have some legitimate place in science, but only in the formal sense in which 'every physical theory is a model (of the underlying mathematical formalism)' or the sense in which 'every specific physical theory (but not every theory) contains a model or sketch of its particular referent'). He says:

we know that metaphors and analogies are at best didactic props, at worst didactic traps, always *Ersatz* for the real thing. We therefore try to avoid them in research. We wish science to be concerned with what things are not with what things look like: science is neither poetry nor theodicy nor occult science. While we
are willing to let analogy guide our preliminary explorations (notice the metaphor), we feel it is wrong to allow it to play any role in mature theory: we want to depict the thing itself rather than something that superficially looks like it. In other words we want literal interpretations - even if they are assigned no familiar visualisations - because we want objectivity. Grown-up science is literal as much as it is objective. Its epistemology is consequently realist.

When we come to Bunge's explication of literal interpretation we are given the following account:

A physical theory is assigned a literal and objective interpretation by assigning every one of its referential primitive symbols a physical object - entity, property, relation, or event - rather than a mental picture or a human operation.

In a further explication, he says:

A literal and objective interpretation of a basic (primitive) symbol s occurring in a physical theory T assigns s a physical object p, be it an entity (e.g. an atom), a property thereof (e.g. the atom angular momentum) or a change of it (e.g. a jump in the angular momentum value). In short, $p = \text{Int.} (s)$. A literal and objective interpretation of a whole physical theory T will accordingly consist in mapping $\text{Int}: S \rightarrow P$ of the set S of the basic symbols of T into the set P of their physical partners.

In reply to Bunge's claims, one can point to a number of difficulties in his own account. To begin with it is not quite clear how we investigate or describe the radically new, except in familiar terms. For the only alternative seems to be to account for it in unfamiliar
terms - which is absurd. As Jevons says:

If we could ever meet a thing wholly suigenus, presenting no analogy to anything else, we should be incapable of investigating its nature, except by purely haphazard trial.\[89\]

The matter can be put even more strongly than this since it is difficult to attach meaning to the 'nature' of a thing or imagine what sort of activity 'haphazard trial' might be in the circumstances.

The metaphorical account, however, especially on the interaction view of metaphor, does indicate a way out of this paradox, for it suggests that radical novelty can be dealt with by a creative modification of reference and meaning which adapts familiar terms to unfamiliar situations.

There is some evidence that Bunge equates the use of analogies and metaphors with eidetic imagery. He talks of the necessity of being concerned with the thing itself rather than what it 'superficially looks like'. He refers to 'familiar visualisations', and to a 'mental picture'. Elsewhere he talks of keeping models apart from 'visual analogues'.\[90\] It is true that he recognizes that 'few of the models that pass for visual representations are picturable anyhow',\[91\] but this does not seem to suggest to him that perhaps he is attacking a 'straw man'. For, as we have seen in the chapter on metaphor, it is a mistake to equate metaphor with imagery. To do so is to become a victim of metaphor, or to confuse model\(_2\) with model\(_1\). To use a metaphor may be to interpret the unfamiliar in terms of the familiar, or to make the theoretical meaningful, but this does not mean at all to make the unfamiliar or the theoretical visualizable. To use metaphors and analogies is to make the theoretical
or the unknown or the unfamiliar imaginable, perhaps, but not in any narrow sense of conjuring up eidetic images. Perhaps a better description of the use of analogy and metaphor would be that they serve to make things, processes or structures intuitable, and related to ordinary experience and its categories, while not constituted out of such experience.

There is also some difficulty in Bunge's account of the literal. He indicates that literal interpretation is effected by way of axiomatization:

.....as soon as the new theory is on hand it should be subjected to a critical examination with a view to dismounting its heuristic scaffolding and reconstructing the system in a literal way - this being one of the uses of axiomatisation.

Disregarding the fact that most of science is not 'axiomatised' and that axiomatization of even relatively simple theories can be extremely prolix, it is not clear how axiomatization is to be accomplished except within the categories and symbolism in which the theory is originally formulated. Bunge's own account of literal interpretation betrays this fact for he talks of 'its referential primitive symbols', as if these were somehow neutral self-evident elements of a theory and then goes on to equate the literal with the 'physical', where it is evident from the examples he gives that 'physical objects' are in fact theoretical objects (e.g. 'atom', 'jump in angular momentum value'), and this begs the question, for many people would not regard these as literal at all - in fact they might regard Bunge as simply falling back on some established metaphors.

There is an additional difficulty that if such a literal interpretation as Bunge envisages were possible then presumably quantum theory would be capable of being unequivocally judged true or false in some way that
would make it unsusceptible of modification or replace-
ment. Such an interpretation of theories would threaten
to make science die of clinical antisepsis. For science
needs the inoculation of ambiguity and the semantic haze
that surrounds the neutral analogy of a model or the
unexplored resources of a metaphor if it is to marshal
its resources for survival and growth. Too doctrinaire
an axiomatization, or literal reconstruction, even
supposing this were possible in any neutral untheoretica-
-ly-infected way, might free science from the infection
of error due to uncritical reliance on a model or
metaphor, but it would kill it stone dead in the process.
In such circumstances every new empirical discovery
which was inconsistent with the established 'literal'
axiomatization would be radically novel in a completely
unassimilable way.

(6) The question of metaphors (and also models) being
merely heuristic and either not permanent in science or
only permanent at the expense of becoming literal has
already been discussed. It is worth noticing, however,
that there is some ambiguity in what is meant by 'heuris-
tic'. 'Heuristic' can be used to mean merely 'employed
in the process of discovery' (but not necessary to that
process), or it can be used to mean 'necessary for the
process of discovery' (but thereafter dispensable). On
the other hand it is sometimes used (as, for example,
by Vaihinger) to mean 'aiding the understanding' or
even 'necessary to the understanding'. In the latter
sense, to claim that models or metaphors are merely
heuristic would not be to regard them as inessential
to science, but perhaps only to enter some ontological
disclaimer.

(7) There are not wanting those who 'wants to make
our flesh creep' by dwelling on the difficulties and
dangers of analogical and metaphorical thinking. Such dangers are real but can be exaggerated. Familiarity, in the shape of analogy and metaphor, may breed contempt, and it is probably true of the scientific use of model and metaphor what Braithwaite says of models: 'The price of the employment of models is eternal vigilance'. We are reminded that this is also said to be the price of democracy, and it is on the whole one we are prepared to pay for the freedom and progress it permits. Moreover, a scientific education, like a democracy, provides its own system of checks and balances. The alleged pictorial and other delusions of analogical thinking have perhaps been overestimated. After all the kinetic theorist does not have before his mind constantly a vast concourse of material particles in motion. There is no image of this kind involved. Our metaphors and analogies in science are all set about with negatives. We do not for a moment imagine that sound or light waves will splash us, nor do we 'picture' electro-magnetic undulations. We work with some of the connotations of wave, especially those such as periodicity, amplitude, length and velocity which are mathematically and operationally amenable. But this is as far as it goes.

However, it does seem as if the history of science can illustrate the claim that models and metaphors have been misleading, and sometimes, as in the case of 'absolute space' and 'absolute time', metaphors have been the source of deepseated confusions and difficulties. However, the moral is not that we should eschew metaphors and models, for if our analysis of scientific discovery in terms of analogical acts is correct they
are indispensable adjuncts to scientific progress, but Turbayne's moral that we should use metaphors and models with more critical awareness.

(8) One possible criticism of the metaphorical view is that if scientific theories are no more than metaphors, it is difficult to account for their success. This is the substance of the following point from Mays's review of Turbayne's *The Myth of Metaphor*:

> If our physical theories are just metaphors, how is it that they have been so strikingly successful in the past in enabling us to exert some control over nature? It is partly because of this that we assume that our physical theories have some causal structure with the world.\(^96\)

The full explication of the pejorative sense of 'just metaphors' in this passage would no doubt involve making some of the points already considered, e.g. that metaphors are only appropriate/inappropriate, not true/false, that metaphors cannot be used to make reliable inferences, that metaphors are concerned with the superficial or the visual. In general, perhaps, this sort of claim involves regarding metaphors as essentially decorative literary devices, or, at best, as cognitive only in some non-scientific and transient sense. These objections have already been discussed, but since literary metaphors certainly do differ from scientific metaphors in terms of their systematicity, their association with formal calculi, and their permanence, Mays's kind of criticism could be interpreted as an unwillingness to broaden the class of metaphors to include the scientific kind, or to regard the hallmarks of literary metaphor as being the defining characteristics of all metaphor. Such reservations amount to a desire to stipulate the definition
of metaphor in some way. In these circumstances it is perhaps incumbent on the proponent of the metaphorical view to justify the retention of the description 'metaphor' in the scientific case. The arguments in favour of regarding scientific theories or some of the language of scientific theories as metaphorical will be summarized below.

(9) Once again Mays may be cited, on the subject of the unsuitability of the 'hypothetical' attitude involved in metaphor for a scientific metaphysic or for scientific belief (he is criticizing Turbayne):

...if Galileo and his successors had taken Bellarmine's advice and spoken hypothetically, it is questionable whether astronomy and physics would have advanced very far.\(^97\)

If my attempted explication of the notion of 'metaphorical' truth above has been correct, the point of this criticism has already been turned. For, if we accept an established theory as asserting a truth (an important and systematic truth) then we are not strictly regarding it as hypothetical, although we may regard it as corrigible or replaceable in certain circumstances.\(^98\) Mays's criticism is, however, perfectly just in relation to Turbayne's position that metaphors are hypothetical:

Now all hypotheses, by definition, involve make-believe. Many of them, like those under consideration, involve sort-crossing, and are therefore metaphors. The conclusion of this section is the decision to adopt the actual technique of Plato and, in addition, to follow the advice of Cardinal Bellarmine.\(^99\)

Difficulties about the way we use words in metascientific talk are involved here, for normally 'hypothetical'
is used now in senses that are ambiguous between 'unproven', 'corrigible' and 'not claiming to be true' ('make-believe'), and it all depends on context whether we mean one or other of these when we use the term. Obviously the hypothetico-deductive account of scientific theory implies that all theories are essentially 'hypothetical' but this use does not exclude some sort of truth claims for such 'hypotheses', and if it implies that all theories are 'unproven', this is not in the sense of ordinary scientific proof, but in an apodeictic or rationalist sense of 'proof'. However, if a scientist says: 'of course, at this stage it is extremely hypothetical', he does mean 'unproven' in the usual scientific sense of 'not yet incorporated into the structures of scientific belief, by means of experimental and observational tests'.

The fact that the ontological attitudes implicit in the metaphorical view are much closer to those actually maintained by scientists in relation to accepted theories than the attitudes sometimes implied by the description 'hypothetical' is a point in favour of regarding theories as metaphorical, and of replacing the description 'hypothetico-deductive' by the phrase 'metaphorico-deductive'.

(10) One reason for treating the metaphorical view with caution might be that it is commonly thought to be a feature of Greek, Medieval and Renaissance thought that they employed analogies and metaphors in place of proper scientific theories. Therefore, by characterizing modern science as metaphorical we are not distinguishing between good and bad science, or, indeed, between what is accepted as science and what is hardly science at all. No doubt this distrust of metaphor and analogy is connected with the recognition that argument by analogy is not, by the usual logical canons, valid. Bunge is correct
when he says:

.....arguments from analogy may be fertile but they are all invalid: their success, if any, does not depend on their form but on the nature of the case. 101

However, as Bunge's remarks indicate, the logical validity of the use of metaphors and models is no measure of their fertility or success. As Hogben says, of analogy:

The truth is that analogy is a very powerful instrument of scientific reasoning and most of the really fruitful facts about nature have been discovered by reasoning from analogy, often analogy of a very crude kind. 102

And Lindenbaum Hosiasson, a logician, says, of one kind of reasoning by analogy:

.....this reasoning, universally employed, seems to have such a intuitive force that one would wish to say 'if it cannot be properly supported - so much the worse for logic' 103

Some attempts have been made to evaluate argument by analogy on purely logical grounds, either by attempting to show that any argument by analogy is per se more probable than an alternative hypothesis not based on analogy, or by seeking to evaluate one analogy (or analogically-based hypothesis) in relation to another analogy (or analogically-based hypothesis) 104 .

I do not wish to enter upon the large subject of the logic of induction and analogy, except to make one essential point, which is in fact recognized by Bunge. It is that metaphors, models and analogies are in themselves neither good nor bad unless they have been evaluated, and the whole critical apparatus of scientific method and the logic of science with which much of the
philosophy of science is preoccupied is directed not to discrediting analogies and metaphors but to evaluating them, once suggested. No doubt there was something wrong with the analogies and metaphors of the Greeks, the Middle Ages, and the Renaissance, if we regard them as making properly evaluated and tested claims of a suitably systematic and unambiguous kind, but they failed, not because they were analogies and metaphors, but because, from our point of view, they were not accurately predictive, or associated with deductive systems or calculi, or sufficiently precise or qualified to prevent erroneous inferences being made from them. There were three features of the scientific revolution of the seventeenth century which resulted in a marked change in related to analogy and metaphor. One was that, from Bacon onwards, implicit in the new philosophy and in the new scientific practices were the outlines of a new methodology, which amounted to a more refined and discriminating critical apparatus for evaluating models and metaphors. The second was the increasing use of that particularly fertile and abstract group of 'thin' analogies and metaphors which is mathematics. The third was the fact that mankind at last hit upon some extremely apt and deployable models and metaphors. The difference between Renaissance and later scientific thought is well characterized by Mazzeo as follows:

While modern thought is aware of the tentative nature of scientific reasoning, earlier thought tended to consider an analogy as an end in itself and to rest content in an aesthetic and essentially poetic awareness of the feeling of understanding the analogy brought....
Even when such thinkers gave indication of being aware of the metaphorical nature of their thought, they adhered to inadequate analogies; for the probabilities of various analogies change with the state of science, and the degree to which a particular analogy is convincing is a function of the intellectual state or scientific culture of the concluding mind.

It is important to note here that the tendency of Renaissance quasi-science and philosophy to be satisfied in inquiry with aesthetic contemplation of a metaphor or analogy is parallel to the use of metaphor in poetry where it serves no further end and is offered as a final statement. 106

(11) and (12) It has already been argued that there is no reason to regard metaphors of a certain kind as alien to the language of science. It is not intended to claim that scientific metaphors are the same as literary metaphors. Literary metaphors, for example, no doubt do have, in addition to the greater diffuseness of their cognitive aspects, emotive and affective tone, which is irrelevant to the scientific use. Perhaps the sole trace of this in the scientific use is the 'satisfaction' that terms such as 'force' and 'wave', etc., give as explanatory terms. Two other important differences between the literary and the scientific use are that the context of metaphorical description in science is always general, while in literature it is frequently or even usually particular. Connected with this is the fact that in literature the metaphor is employed in a context which is not usually concerned with any objective reality. Thus the 'meaning' of a literary metaphor is circumscribed by all
the particular details of subject matter, event, aim, circumstance and style which belong to the poem or other work in which they occur. They are therefore designed to integrate with the author's total descriptive and aesthetic-intention and not with independent neutral cognitive criteria.

In general, then, the differences between literary and scientific metaphors depend on their uses. In both literature and science metaphor is a means to an unfolding, a revelation, a new perspective or focus, but while in literature metaphor seeks to shake off the routine and familiar and reflect and refract in new lights so as to vivify and colour a world grown grey and featureless from too much familiarity, in science metaphor seeks to identify, to make the strange and unknown familiar, the complex and anarchic simple and ordered, and, by contrast with literature and art, to disencumber our experience from the sensual and the concrete and the particular circumstances of time and place.

Possibly one of the most serious difficulties for the metaphorical view of science, relating to the question of whether metaphor is not par excellence the language of literature rather than of science, is the historical one that the scientific revolution in the seventeenth century was marked by a general antipathy towards metaphor and by an attempt to reform the English language for scientific purposes so as to avoid the misleading effects of metaphor. This difficulty is reinforced by the fact that it might be held that the tendency then established to distrust metaphors and to try to eschew them from science has formed a permanent feature, perhaps not explicit, but nonetheless definite, of scientific attitudes.

This question is important enough to deserve a
separate and more extensive treatment, which I give it in the next chapter.

(13) Enough has already been said, I trust, about the cognitive aspects of metaphor, to refute the purely emotive view of metaphor. Such a view is not to be confused with those views, such as Herschberger's, which unite an emotive with a cognitive view of metaphor. However, as Beardsley says, in his article on metaphor in the *Encyclopedia of Philosophy*, the (purely) emotive theory of metaphor has been broached, but 'never very thoroughly worked out'\(^{107}\). It is significant that Richards, who with Ogden, was the originator of the emotive/cognitive distinction, never seems to have held a purely emotive theory of metaphor and later came to hold an extremely influential cognitive view.\(^{108}\)

(14) Another major difficulty for any metaphorical view of science, which, like Turbayne's, holds that all scientific statements are irreducibly metaphorical, is brought up by Olscamp in his long review article of Turbayne's book and of other review articles about the book. The difficulty is as follows:

.....if, as Turbayne says, 'The attempt to re-allocate the facts by restoring them to where they 'actually belong'' is vain,' if 'We cannot say what reality is, only what it seems like to us,' then how could we discover a sort-crossing in the first place? For surely the very discovery presupposes that we do have some ideas about the 'correct allocation of the facts,' at least in so far as the knowledge of the proper use of language to describe the facts is concerned.\(^{109}\)
The point of this problem has really already been considered in the discussion above of Adler's view that all language is metaphorical, and in the discussion of what constitutes literal language. For Turbayne seems to be denying that any literal statement of the 'facts' is possible, which is presumably one with Adler's claim that all language is metaphorical, and, as we have already said, this makes any literal/metaphorical distinction impossible to maintain, since it is a distinction without a difference.

One possible line of defence against Olscamp's criticism which would be open to Turbayne if he were a phenomenalist, as Olscamp and another reviewer incline to think, is the positivist one. This maintains that although not in fact reduced to phenomenal statements, all scientific propositions are, in principle, reducible to phenomenal language. This phenomenal language, at least potentially, is then presumably the only language in which literal statements can be made, by means of so-called 'protocol statements', which are incorrigible observation statements couched in phenomenalist terms. All theoretical statements or theory-infected observation statements are translatable or reducible to phenomenalistic statements. As has already been mentioned, the main objection to this solution of Olscamp's problem is that, in spite of persistent attempts, no one has been able to carry out such phenomenalistic reduction in practice, and it is not clear what, other than an a priori conviction, persuades positivists that it can be. In any case, as far as Turbayne is concerned, it is difficult to maintain Olscamp's and Mays's claims that Turbayne is a phenomenalist, in view of his statement that 'we cannot say what reality is'.

For this reason, the following argument of Feyerabend's
looks more promising as a solution to the problem. Feyerabend is dealing with the problem of how observation statements, if their meaning is always theory-determined (as he claims), can possibly be in conflict between one theory and another. This can easily be adapted to the problem about metaphorical/literal, since it amounts to asking how, if all statements are metaphorical, they can possibly be compared to the literal statements of the same domain, and hence how various metaphorical statements about the same domain can come into conflict or be compared with one another. Feyerabend says:

It is bound to happen, then, at some stage that the alternatives do not share a single statement with the theory they criticize. The idea of observation that we are defending here implies that they will not share a single observation statement either. To express it more radically, each theory will possess its own experience, and there will be no overlap between these experiences. Clearly a crucial experiment is now impossible....because there is no universally accepted statement capable of expressing whatever emerges from observation. But there is still human experience as an actually existing process, and it still causes the observer to carry out certain actions, for example, to utter sentences of a certain kind. Not every interpretation of the sentences uttered will be such that the theory furnishing the interpretation predicts it in the form in which it has emerged from the observational situation. Such a combined use of theory and action leads
to a selection even in those cases where a common observation language does not exist. The theory - an acceptable theory, that is - has an inbuilt syntactic machinery that imitates (but does not describe) certain features of our experience. However, it is not judged by the truth or falsehood of the prediction-statements, but by the way in which the prediction sentences are ordered by it and by the agreement or disagreement of this physical order with the natural order of observation sentences as uttered by human observers, and therefore, in the last resort, with the natural order of sensations.

This avoids the problem of reduction to a common observation language, and hence, in terms of Olscamp's critique of Turbayne, of literal reallocation of the 'facts', but only at the cost of creating another problem. For Feyerabend's solution implies that observation statements can come into conflict with phenomena, rather than statements about phenomena. Either the 'phenomena' or, to put a more generous interpretation on Feyerabend's claims, thoughts about phenomena are presumed therefore to be independent of language. However, it seems doubtful if much meaning can be attached to uninterpreted experience. In a sense language creates the world of phenomena, and all cognition no matter how purely 'phenomenal' is 'Worthhaftigkeit', word-laden. For example, even the tendency to 'see' the world in terms of thing, property and process springs from the noun, adjective and verb structure of our language.
However, as I have suggested above, there really does seem to be a language which is neutral to theory and which may be regarded as the literal language, and I have suggested that it is the language of direct perception or of 'ostensivity'. While I do not want to suggest that all the propositions of a theory are expressible in this language (indeed if the metaphorical view is correct, large parts of theoretical language are only expressible metaphorically), it is nevertheless true that the experimental or observational phenomena are expressible in this neutral (to theory) or literal language, and the existence of such a language is an essential auxiliary to science, if science is to be corrigible, and capable of qualification, replacement and other forms of progress. It is, I believe, this literal language to which Gerr refers in the following passage:

Science must evolve. Accordingly it requires not only an absolutely precise, purely symbolic language like mathematics to deal with the more assured fields of knowledge, but also a more flexible and more suggestive, as well as more 'universal' language to keep pace with its constant evolution. This is provided by what might be called the everyday language of science and technology, which is, in essence, no more than the common language with its rational structure and factual vocabulary enormously developed.

In this account we shall limit ourselves to consideration of this non-mathematical, functional language which performs a triple
function for science: it acts as a guide in the execution of the 'imaginary' experiments so vital to the progress of science; it serves as a framework for rational formulation of scientific knowledge; and it records and communicates the tentative results of scientific procedure. It is the language in which the bulk of scientific and technical literature is at present formulated; nor is it too much to say that it must always be an important factor of expression in an evolving science.113

(14) While it is true that metaphors in literary use are frequently transient, there are even in literature recurrent metaphors which achieve some permanence and are deployed in ways which approach the systematic complexity and fertility of scientific metaphors. For example, Embley in Metaphor and Meaning discusses at length what he calls 'five metaphors from the modern repertory', viz., the prison, the wasteland, the monster, the machine and the hospital, to show how they occur as leit-motifs in modern literature114. Such a use of metaphor in literature demonstrates that the difference between literary and scientific metaphors is, in cognitive terms (and disregarding any aesthetic or affective tone) essentially degree rather than of kind. If it is true that metaphors can be permanent, as I have claimed, then there seems to be no place where one can draw the line between the more transient and local metaphors of literature and the established and systematic metaphors of science.

It remains to consider some of the reasons, apart
from those that have already been discussed, why we might be justified in regarding science as metaphorical in important ways. As our discussion of the literal/metaphorical distinction showed, there is some possibility of disagreement about what we regard as the essential characteristics of metaphorical language and of metaphor, and to some extent what one attaches the terms 'metaphorical' or 'metaphor' to depends upon one's theory and definition of metaphor. Since one has some degree of freedom in this regard, and one can, in such circumstances, be stipulative (within limits) if one wishes, it behoves the defender of the metaphorical view of science to give reasons why we should stipulate a definition and use of the term 'metaphor' which allows it to be applied in science in the ways and contexts which have been indicated. In addition to those already given, the following seem to be valid reasons for wishing to comprehend scientific practices and uses under the rubric 'metaphor' or 'metaphorical':

(1) It serves to explain the occurrence of and retention within science of a large number of words which have, in addition to their use in a specific scientific context, either an everyday use and consequent surplus connotation, or else an alternative use within other theories or fields of science. Quite apart from the numerous cases of a Greek or Latin prefix or suffix, whose meaning in English is well understood within the field concerned, and which has as a consequence known meanings which are concealed metaphors, there are a huge number of terms in science which have or have had in past theories alternative or surplus meanings in this way: corpuscle, particle, field, atom, vortex, discharge, emit, travel, wave, spin, charge, potential, equilibrium, strain, stress, energy, force, tension, attraction, repulsion, affinity, inertia, centrifugal,
centripetal, function, bond, valve, oscillator, conduction, impedance, resistance, latent, susceptibility, permeability, positive, negative, nucleus, orbit, chain, natural selection, aberration, polarity, pressure, refract, diffract, inhibition, repression, cell, etc., etc., etc. — one has only to turn the pages of any textbook or encyclopedia of science to find them tumbling out at one. One has only to reflect on the loss of intuitive understanding and descriptive facility which the replacement of these terms by neologisms would entail to recognize the essential role they play, and this role is, I think, only comprehensible in terms of the model and metaphor account which has been given.

(2) Metaphor seems to be the key to understanding both the methodological and creative aspect of scientific discovery and progress, as I have attempted to describe them in terms of imported analogy and the analogical act, and also the various essential functions and role that have been assigned by philosophers of science to models. In particular metaphors are instrumental in providing meaning for theoretical terms, explanation (in the wide sense, not restricted to deducibility or prediction), and insight, in permitting prediction in a strong sense (involving new observation predicates), in linking calculi to experiment and observation, in satisfying ontological aspirations, and in assisting in the simplifying functions of models.

(3) The metaphorical view provides an overall theory of science which is deployable in relation to a large number of problems in the philosophy of science, e.g. induction, theoretical/observational distinctions, meaning-invariance, hypothetico-deductive account of
theory, explanation, logic of discovery, verification, confirmation, positivism, prediction, counterfactual conditionals, meaning of theoretical terms, structure of theory. In all these cases it is capable, I believe, of providing supplementary or alternative solutions to some of the problems which arise, and it is perhaps worth stressing that it provides such solutions by offering a single unified, synoptic and integrative account of what are often treated as discrete problems.

(4) The history of science provides ample evidence for the metaphorical view. This cannot be illustrated in detail but several studies exist of the historical use of analogies and metaphors in science\textsuperscript{115}. The metaphorical view therefore appears to conform admirably to the following criterion of scientific method, proposed by Black:

\begin{quote}
I propose we treat 'scientific method' as a historical expression meaning, among other things, those procedures which, as a matter of historical fact, have proved most useful in the acquisition of systematic and comprehensive knowledge.\textsuperscript{116}
\end{quote}

(5) Analogical or metaphorical ability, rather than mathematical or logical ability, seems to be the \textit{sine qua non} of scientific ability. It hardly needs arguing that logicians are not eminent in the sciences or that scientists are rarely, if ever, accomplished logicians. What is perhaps not so obvious is that the examples of Darwin, Faraday, Hutton and Lyell, among the front rank of seminal scientific minds, indicate that a lack of mathematical interests or ability is no bar to scientific greatness, while all of these scientists used models, analogies and metaphors with facility and far-reaching consequences. Of course facility with
with analogies, models and metaphors can be, and often is, associated with mathematical ability. Indeed if Polya is right, analogical thinking is an important requisite for mathematical creativity\textsuperscript{117}. So it should come as no surprise to find mathematical ability associated with an aptness for metaphor in a scientifically creative mind. Kelvin is a good example of this. His mind, one of his pupils tells us, was 'full of fancies and brimming over with metaphors\textsuperscript{118}.

(6) On the practical side it seems at least worth seriously considering the metaphorical view on two counts:

(a) \textit{Teaching}. If the model and metaphor views of science are correct then the explicit recognition and explication of the key models and metaphors of a given domain of science is surely a necessary part of scientific education and communication. A rationalized and ordered presentation of these with an explanatory analysis of their role and function would then play a recognized and indispensable part in the structure of courses in science. While this is no doubt already done in an occasional, opportunist, discursive way, subject to the whims of the teacher and the vagaries of course structure, and the idiosyncracies of textbooks, there seems to be no reason why it should not receive the same explicit and ordered treatment as, say, the mathematical part of science does.

Both in recognizing and recruiting people with creative potential in science some recognition might be given to testing and encouraging analogical and metaphorical ability. This might well bring back to the scientific fold people who would otherwise fail to identify themselves with the aims, aspirations and fulfilments which belong to genuine scientific accomplishment, and encourage a more enterprising and less
dogmatic approach to established theories than is sometimes evident among scientists doing normal science.

(b) Discovery. Perhaps more importantly, a recognition of the role of model and metaphor in science might well encourage the use of techniques of discovery and progress which embody this recognition and exploit it. In discussing Swanson, it was noted that he used the metaphorical view as the basis for a questionnaire procedure devised to facilitate the extension and deployment of models. This was the technique of Rational Preference Ranking. It was also noted that Buchanan suggested basing a logistic on the literal expansion of metaphors. This logistic was formulated as a matrix system. Somewhat similar systems have been discussed in relation to thesauruses by Hesse, and there seems to be no reason why such lattices as Hesse discusses should not be adapted to scientific terminology and to scientific purposes. As explicated by her, they give some measure of the analogy relation in quantitative terms. Matrix systems are also suggested, with examples of their use for discovery, in Moles's *La Creation Scientifique*. Such systems are at present more an embryonic idea than a developed technique but they seem at least promising. In relation to discovery Gregory says:

If analogy is the route of discovery, as Lotze suggests, this logical form conforms more closely than other such forms to the actual process of mind....If the analogy is not a ground of proof, as Lotze also says, the logician naturally enough gives it the cold shoulder, which
he seems to be doing. Probably no logical form, which is necessarily abstract and analytical, can adequately render the full synthetic conc-
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-

reteness of the mental process but the analogy seems to come closest to it of all logical forms.

What I am suggesting is that we follow the lead that Swanson, Buchanan, Hesse, Moles and others have given, and develop a genuine 'logic' of discovery based on the recognition of its essentially analogical and metaphorical nature.

(7) A last reason for adopting the metaphorical view is that an important part of scientific activity is the informal talking and thinking that goes on in relation to experiment, observation and speculation. It is here rather than in the finished scientific memoir, paper, or monograph that the language of science is most actively and meaningfully employed. It is here, I think, that the terminology of models and metaphors is in fact used, whether we regard it as philosophically or methodologically justifiable or not. As Darwin noted, in relation to science:

Every one knows what is meant and implied by .....metaphorical expressions; and they are almost necessary for brevity. 122

Brevity and a synoptic understanding are essential for efficient speculative scientific thinking and metaphors and models are the natural vehicles for such thought. Empson quotes Herbert Read as saying:

'Metaphor is the synthesis of several units of observation into one commanding image; it is the expression of a complex idea, not by anal-
-
-ysis, nor by direct statement, but by a
sudden perception of an objective relation.\textsuperscript{123}

In a comment on this Empson says that metaphor is a device for 'seeing whole' and 'for letting you handle the proportions of the matter intuitively, instead of fiddling about with first one part and then another.'\textsuperscript{124}

In the last chapter I shall give more detailed consideration to the problem of why modern science has not generally been regarded as metaphorical, an attitude which has its origins in the seventeenth century scientific revolution.
Chapter 6

Of the many methods of attack which the seventeenth century writers employed against Aristotelian and Scholastic thought not the least effective was that which we should now classify as linguistic analysis. On every hand we see attempts to show that scholastic and other current terms have no constant or clear meaning and hence are incapable of functioning effectively in scientific discourse and, more seriously, are definitely misleading. Examples which spring to mind are Bacon's remarks in Book I of the Novum Organum, Galileo's attacks on the Aristotelian notions of generation, corruption, and perfection, Harvey's critical discussion of 'spirits', and Boyle's long polemic on 'elements'. Not surprisingly this awareness of the deceits and traps of scholastic terms widened to comprise a more general critique of language and meaning in general. A central turn in the seventeenth century revolution in science was the scientists' disencumbering themselves from the toils of Elizabethan poetry and prose. Quite apart from the shackles which scholastic terminology imposed upon scientific thought, science was also enmeshed in an inappropriate prose style.

The language current in England at the beginning of the seventeenth century was a rich, elaborate rhetorical style, much enlivened by metaphors, similes, and other figures of speech. Exuberant and luxuriant, but verbose, uneconomical and imprecise, it was ill-suited to the communication of concise and unambiguous description and argument, and consequently a poor vehicle for the new rapidly growing science then coming into being. Many of the leading figures in the scientific revolution such as Bacon, Hobbes and Boyle urged
the necessity for a reform of language for scientific purposes, and the Royal Society actually formed a committee for improving the English tongue, 'particularly for philosophical (i.e. scientific) purposes'. And probably the most quoted passage from *History of the Royal Society* is that where he records:

There is one thing more, about which the Society has been most solicitous, the manner of their Discourse....They have extracted from all their members, a close, naked natural way of speaking, positive expression; clear senses; a native easiness, bringing all things as near the Mathematical plainness as they can.\(^5\)

R.F. Jones in his account of the permanent effect which the advent of science had on English prose and (ultimately) poetic style says:

More than to any other linguistic defect, scientists objected to a word's possessing many meanings or the same meaning as another, and especially to the use of metaphors. The desire to make the word match the thing or action, explains their exaggerated antipathy to metaphors and such figures of speech.\(^6\)

No doubt there were a number of other reasons than the linguistic aspirations of scientists for the changing attitudes towards language. For example Tuve remarks of the influence of Ramus's logic on sixteenth and seventeenth writings:

Since the Romantic period, imagery has by many been accepted as an author's chief means of making his work sensuously convincing, of conveying his experience with a semblance of reality. But we may find, in sixteenth-
and seventeenth-century writing at least, most images are not primarily sensuous experience. They did not arise that way, and we do not thus apprehend them; when an author makes a comparison we follow him in a logical step, we perceive not a thing but a relationship, we are called on for a response far less perceptual than conceptual.

There is no better example of this conceptual use of 'images' than Bacon, the prophet of the new science, whose cognitive use of metaphor and simile is discussed at length by Knights. This regularizing of metaphor and simile may be regard as marking a natural transition to a stage of rejecting metaphor and simile altogether, but I prefer to regard it as illustrating that the stated rejection of metaphor and simile of the period was in fact too doctrinaire and at variance with actual practice. This ambivalent approach to metaphor and simile is nicely encapsulated in the following:

> Figures of Speech, which Poets think so fine,  
> Art's needless Varnish to make Nature shine,  
> Are all but Paint upon a beauteous Face,  
> And in descriptions only claim a place.

This is from a seventeenth century Essay upon Poetry and condemns figures of speech (including metaphor presumably) by means of a metaphor.

However, it is obviously necessary for any metaphorical view of science to come to terms with the apparent anomaly that the scientific revolution which so successfully ushered in the era of modern science also distrusted metaphors and sought to eschew them from scientific discourse.
Probably the shortest answer to this problem is to claim that the seventeenth-century scientists and philosophers were simply mistaken about their success in or even the feasibility of doing away with metaphors. But such a short answer would greatly oversimplify the nature of the seventeenth-century response to the problems involved in the relation between language and science. Besides, if the seventeenth-century thinkers did make such a mistake it is interesting to investigate the details and dimensions of their error since there seems little doubt that it has persisted as a salient feature of scientific thinking even to the present day.

To a great extent the key-note to seventeenth century ideas about language is struck in Bacon's writing:

There are also Idols formed by the intercourse and association of men with each other, which I call the Idols of the Market Place, on account of the commerce and consort of men there. For it is by discourse that men associate, and words are imposed according to the apprehension of the vulgar. And therefore the ill and unfit choice of words wonderfully obstructs the understanding. Nor do the definitions or explanations wherewith in some things learned men are wont to guard and defend themselves, by any means set the matter right. But words plainly force and overrule the understanding, and throw all into confusion, and lead men away into numberless empty controversies and idle fancies.
This aphorism of Bacon's is part of the doctrine of the 'Idols' (or phantasms), in which Bacon considers the main hindrances and obstacles to the proper progress of science. There are four classes of Idols, of which the Idols of the Market Place mentioned above is one. However, in a more extended later consideration of the Idols, it is significant that he says the Idols of the Market Place are 'the most troublesome of all'. He goes on to say:

For men believe that their reason governs words; but it is also true that words react on the understanding; and this it is that has rendered philosophy and the sciences sophistical and inactive. Now words, being commonly framed and applied according to the capacity of the vulgar, follow those lines of division which are most obvious to the vulgar understanding. And whenever an understanding of greater acuteness or a more diligent observation would alter those lines to suit the true divisions of nature, words stand in the way and resist the change.  

It is axiomatic with Bacon that there are true divisions of nature to apply words accurately to. This finds a common expression in Bacon in his antithesis between words and things. He wishes, he says, to restore the commerce of the mind with things, not words. In a later passage in the Novum Organum he reproves Aristotle for 'imposing countless.....arbitrary restrictions on the nature of things; being always more solicitous to provide an answer to the question and affirm something positive in words, than about the inner truth of things.'  

It is evident/here and elsewhere  that it is the
inappropriateness of language to things which is under criticism. We get a similar attitude in numerous other writers. For example, in Descartes:

On this account the majority attend to words rather than things; and thus they very frequently assent to terms without attaching to them any meaning, either because they think they once understood them, or imagine they received them from others by whom they were correctly understood.¹⁴

Or again, in Galileo:

For names and attributes must be accommodated to the essence of things, and not the essence to the names, since things come first and names afterwards.¹⁵

The noticeable thing about this attitude is that it comprises a kind of semantic atomism, which, under a variety of guises (e.g. Lockean and Berkelean Idealism), is characteristic of the period.

It is assumed that, in experience, we always encounter well-defined or discrete 'things', and the business of language seems to be conceived of as the adroit matching of words to these separately given 'things' of which they are the mark or sign. It does not much matter whether these things are entities such as material objects, or 'ideas'. There is a tacit assumption that they have, as it were, an independent material, perceptual or conceptual existence and that language, far from in any sense creating or focussing meaning and thus playing an active ontological role, is merely a sign system in which there is or ought to be a single one–one relation between the signs and the things they stand for.
Locke's half-way nominalism merely substitutes a single discrete idea whether simple, complex or abst-rect for the thing of the naive realist. Locke's own statement of this is quite unequivocal:

The use, then, of words is to be sensible marks of ideas, and the ideas they stand for are their proper and immediate signification. 16

and again:

It is true, common use by tacit consent appropriates certain sounds to certain ideas in all languages, which so far limits the signification of that sound, that unless a man applies it to the same idea, he does not speak properly.... 17

Perhaps the strongest evidence for the existence of this philosophy of language is the persistence of attempts in this period to found a universal characteristic, which was to be both a philosophical language and a concise and precise symbolism. On the symbolic side the intention was to extend the unambiguous, explicit and international symbolism of mathematics into all realms of meaning. On the 'philosophical' side the intention was to compile a basic system of element-ary ideas (signified each by its own symbol) out of which by conjunction of symbols (and hence of ideas) complex symbols (words) could be built to express any other idea. Numerous people proposed or pursued such an enterprise in this period, including some of the greatest names, such as Bacon, Descartes, Newton and Leibniz, not to mention lesser lights such as Wilkins, Ward, Petty and Dalgarno 18. Not surprisingly from the names of the people mentioned and from the natural alliance that we have mentioned to exist between the new philosophy and linguistic reform
an important part of the benefits of such a universal characteristic were those which would accrue to science. Descartes, for example, says that such a language:

.....is in my opinion the greatest secret one can have for acquiring good scientific knowledge.19

and Leibniz thought that his 'universal characteristic' would serve as an instrument of discovery and demonstration 20.

In most of these reformative schemes an express antipathy to metaphors can be found. A clear statement both of this antipathy and the reason for it can be seen in the following passage:

To conclude, The light of human minds is Perspicuous Words, but by exact definitions first snuffed, and purged from ambiguity; Reason is the pace; Increase of Science, the way; and the Benefit of man-kind, the end. And, on the contrary, Metaphors, and senseless and ambiguous words, are like ignes fatui; and reasoning upon them is wandering among innumerable absurdities; and their end, contention, and sedition, or contempt. 21

This is a common theme with Hobbes, the author of the passage. Elsewhere he says metaphors 'profess their inconstancy' 22, and among the abuses of speech he lists:

Secondly, (men abuse speech) when they use words metaphorically; that is, in other sense than that they are ordained for; and thereby deceive others. 23

In spite of the general antipathy to metaphor of the period, we see some signs of a more discriminating
approach to metaphor in those circles which perhaps stood to lose most by the wholesale condemnation of metaphor - in religion. For example one religious writer objected that in Divinity things 'are not capable of being declared in Logical and Metaphysical terms; Metaphors may not only be allowed but are most accommodated to the assisting us in our conceptions..."

Much more significant from our point of view is the following passage from Bacon, one of the main instigators of linguistic reform:

Those whose conceits are seated in popular opinions need only but to prove or dispute; but those whose conceits are beyond popular opinions have a double labour; the one to make themselves conceived and the other to prove and demonstrate. So that it is of necessity to have recourse to similitudes and translations to express themselves.

This underlines the ambivalence we have already noted in the seventeenth century attitude. Undoubtedly, on the whole, and in relation to the language of science, the desire to give a precise and unambiguous 'cash-value' (to use the Pragmatist phrase), in terms of empirical content, to scientific terms and avoid the emotional and aesthetic irrelevancies and casual 'similitudes and resemblances', together with the inconsistencies and irregularities of reference, of literary types of metaphor in science, was well-founded and the reforms effected were of the utmost benefit to science. However the seventeenth century writers were too sweeping
and doctrinaire in their condemnation of metaphor and simile, and allowed no room for or recognition of the fact that they themselves employed metaphors and similes, even in the 'new philosophy', although these were often much more sustained and cognitively detailed than the more transitory figures of rhetoric and poetry. An ironic example of the powerful use of a sustained metaphor, in view of his strongly stated distrust of metaphor, is Hobbes's *Leviathan*, which treats of the state or commonwealth as 'but an artificial man, though of greater stature and strength than the natural'.

The failure of the seventeenth century writers to recognize their own ambivalence in this has reasons which are not far to seek. One, which we have already mentioned, is their static 'labelling' concept of language and its relation to things, which blinded them to their more fluid and dynamic use. This philosophy of language was considerably reinforced by the quest for rationalistic apodeictic certainty which characterized the Cartesian approach to science. A rational or rationalized language, which serves as a permanent basis for scientific knowledge and scientific discovery, and a rationalist epistemology go hand in hand.

Contrary to what is sometimes supposed, from the projection of post-Humean attitudes on to the seventeenth century, no general radical change was effected by the scientific revolution in the belief in the permanence and incorrigibility of properly-established scientific knowledge.

Newtonianism merely substituted indubitable empirical premises for the indubitable apodeictic principles and premises of Descartes. For Newton
claimed that his axioms were 'deduced from phenomena and made general by induction' and he objects to 'hypotheses' on the grounds that they are not so deduced. The whole tenor of his writings indicates that he did not think of his own findings (except where he explicitly states that they are hypothetical, or implies it, as in the Queries) as provisional or merely highly probable. He certainly had no 'hypothetico-deductive' view of his dynamics and optical theories. He speaks of the 'true system of the world' and 'absolute, true and mathematical time'. In spite of the claims of Blake in 'Isaac Newton and the Hypothetico-Deductive Method' I cannot agree that Newton thought his laws provisional. Both Keynes in 'Newton, the Man' and Sabra, in a chapter on 'Newton's Dogmatism', give contrary evidence.

On the more general picture, Cassirer argues that the view of scientific laws as intrinsically provisional and corrigible emerges in the thought of this period, but is not general. Van Leeuwen devotes a book to arguing that the view that science is hypothetical (in the modern sense) and provisional emerges in this period under the influence of similar attitudes which arose in relation to religious and theological knowledge. Laudan attributes a similar influence to Descartes and the Clock Metaphor, mentioned above. However, according to Laudan the influence of 'probabilism' or 'hypotheticalism' was only temporary, and he concludes his paper as follows:

After Power, writers like Hooke, Newton and Cowley enlarged on his theme, suggesting quite confidently that improved instrumentation promised hope of discovering the true mechanisms of nature and conclusively establishing
(or refuting) the corpuscular philosophy. As the faith in the unlimited magnifying powers of the microscope grew, English hypotheticalism waned......The demand for a science free of all hypotheses, which was widely circulated after Newton, could never have gained such enthusiastic adherents if the hypotheticalism of Descartes, Boyle and Glanvill had not died such a quick and needless death at the hands of those who thought nature's clock had no secrets which man's instruments could not seek out and know with certainty.37

I think we may at least conclude that, even if some ideas of science as probable or hypothetical rather than certain did emerge in this period, they did not represent the prevailing attitude.

A belief in the certainty of science was no doubt supported by the belief in a God-ordered universe. We see this in Descartes's belief that God would be no deceiver, in relation to our empirical knowledge, and the belief of Newton, for example, and indeed the whole Deistic bias of Enlightenment thought, in a God-designed orderly universe capable of being understood by man's reason. It was to knowledge of a God-given and therefore real existent order of real things that man's reason was to win through. The order of things could be known with certainty, and reason leads to certainty, and therefore to the literally true. This conviction is only slightly eroded by the advent of hypotheticalism and, in some quarters, an awareness of the analogical or metaphorical nature of the new philosophy.

Newton himself was not unaware of the metaphorical
nature of some scientific expressions. It is ironical that Turbayne should claim, in a work stressing the metaphorical nature of scientific theory, that: 'If Newton was using "attraction" as a metaphor he did not say so,' because Newton did, in effect, even though he did not use the word 'metaphor':

I likewise call attractions and impulses, in the same sense, accelerative and motive; and use the words attraction, impulse or propensity of any sort, towards a centre promiscuously and indifferently one for another.....wherefore the reader is not to imagine that by these words I take upon me to define the kind, or the manner of any action, the causes or the physical reason thereof, or that I attribute forces, in a true or physical sense, to certain centres (which are only mathematical points).....

And again:

I here use the word attraction in general for any endeavour whatever, made by bodies to approach each other, whether that endeavour arise from the action of the bodies themselves, as tending to each other or agitating each other by spirits emitted; or from the ether or of the air, or any medium whatever, whether corporeal or incorporeal, in any manner impelling bodies placed therein towards each other. In the same general sense I use the word impulse... Surely Newton is here claiming to use the words 'attraction', 'impulse' and, by implication, 'force' in a different sense from the usual.

A similar nicety about usage, and awareness of using a term out of its usual meaning, is revealed in the following passage:
And if at any time I speak of light and rays as coloured or endued with colours, I would be understood to speak not philosophically and properly, but grossly and according to such conceptions as vulgar people in seeing these experiments would be apt to frame. For the rays, to speak properly, are not coloured.42

However, Turbayne's claim is true in spirit since Newton certainly did not seem to be actively aware that his use of terms such as 'force' (and varieties of this such as vis insita, which he replaces with the different, possibly better metaphor, vis inertiae, designed to comprehend both 'resistance' and 'impulse')43 and 'motion' (e.g. in 'quantity of motion') is essentially metaphoric.

The extreme ease and unforced naturalness of the metaphors of force, motion, power, energy, etc., both in their use by Newton and others and in their general acceptance and comprehensibility obscure the fact that they are metaphors at all. Like 'travel' (applied to light), 'pulsion', 'wave', 'ray', 'vibration', 'resonance', 'matter' (e.g. in 'quantity of matter'), 'fluid' (applied to air), 'element', 'ether', even 'light' (applied to invisible process) their specialized use in science seems so natural that they become, in a sense, 'invisible' metaphors, and pass for straightforward descriptive terms.

It is the very ease and naturalness of these metaphors which makes the new philosophy acceptable and persuasive. This is particularly true of the central notion of 'force' which has, in relation to motion, such a marked connotation of efficacy or cause that, in combination with other words with linked connotations such as 'matter' (mass) and 'motion' (momentum), it
is readily accepted as a *vera causa* or 'true and physical cause' (to use one of Newton's phrases) or as an 'ultimate cause'\(^44\), which itself requires no further explanation.

The reason why force, matter and motion are such persuasive metaphors to use as the basis of the mechanical philosophy is undoubtedly due to the metaphoric transfer or perspective from the ordinary use of these terms to refer to our own sensations of causal experience. We know directly what it feels like to exert force, overcome resistance, move, and experience the impenetrability and solidity of matter. When we describe the external world, in so far as it is *not capable* of being felt, seen, touched, kinaesthetically encountered, or in any way sensed, by means of terms derived originally from our direct sensual experience of the world, the properties, processes and structure we attribute to the external world seem causally meaningful without further explanation.

This aspect of the concept 'force' has struck a number of writers. Sir John Herschel, for example, says there is only one case where we have an 'immediate consciousness' of an 'act of direct causation' - 'the production of motion by the exertion of force'\(^45\).

Whewell, in a chapter which attempts to analyse the original meaning in terms of sensual experience of a number of physical terms such as 'force', 'matter', 'solidity', 'body' and 'inertia', says:

.....we adopt the term *Force* and use it to denote that property which is the cause of motion produced, changed or prevented. This conception is, it would seem, mainly and primarily suggested by our consciousness of the exertions
by which we put bodies into motion. An even more explicit statement of the metaphorical nature of force in Newtonian dynamics is that of Maupertuis:

The word force in its proper sense expresses a certain feeling which we experience when we wish to move a body which was at rest or to change or stop a body which was in motion. The perception which we then experience is so constantly accompanied by a change in the rest or movement of the body that we are unable to prevent ourselves from believing that it is the cause of this change. When therefore we see some change taking place in the rest or movement of a body we do not fail to say it is the effect of some force. And if we have no feeling of any effort made by us to contribute to this change and if we can only see some other bodies to which we can attribute this phenomenon, we place the force in them, as though it belonged to them.

He also says that force is originally only 'a feeling of our soul'. Applied to bodies it is 'only a word invented to supply our lack of knowledge'.

However undoubtedly the most sustained, effective and significant argument for the metaphorical nature of the concept of 'force' (and similar concepts) is that of Berkeley in *De Motu*:

Solicitation, and striving or effort really refer only to beings that have life. When they are applied to other things they must be taken metaphorically. But philosophers should avoid metaphors. That those words have no clear and distinct meaning when not referring to either
animal sensibility or the motion of the body will be plain to anyone who considers the subject seriously. 49

And a little later he says:

Force also is attributed to bodies. The term is used however as if it meant a quality that is known, yet is not motion, shape or any other sensible object, nor a feature of animal sensibility; which a little inspection will show to be nothing but an occult quality. Animal effort and bodily motion are commonly regarded as concomitants and measures of this occult quality. 50

Here we have the tables turned with a vengeance. Berkeley levels against the new philosophy the very charges which it brought against the Aristotelian philosophy and scholastic and other language, for he goes on to say:

....force, gravity and such-like terms are more often and more suitably used with concrete meaning, to stand for the body moved, difficulty in our resisting, etc. When they are used by philosophers to mean something altogether different from that — something that neither falls under the senses, nor can be understood by any power of the mind — sooner or later they beget error and confusion... 51

Berkeley is no doubt right in his claim that the new philosophy gave houesroom to its own bêtes-noires, metaphors, but wrong in supposing this is itself improper. For, under the control of fixed definitions (as in Newton) and the semantic and logical precision of mathematical calculi the analogues involved in the
permanent metaphors of science function differently and with much more cognitive exactness than the transient, subjectively interpretable, casual metaphors and tropes of Elizabethan prose. However we should acknowledge that Berkeley is right on the main count. He is even correct in his diagnosis that error and confusion will result. For problems did arise due to the unselfconscious use of such metaphors, even within the temples of the new philosophy, although some were to be a long time developing.

Whewell in The History of the Inductive Sciences mentions 'impetus', 'power', 'momentum', 'virtue' and 'energy' as examples of terms which are used in the ensuing period with no distinct and steady meaning. He also cites Newton's use of the word 'motion' to signify both 'moment of inertia' and 'momentum' as an example of inconstancy of usage. Elsewhere he discusses the conflation and confusion of statical and dynamical problems:

The close analogy and connexion which exists between the principles of equilibrium and motion often led men to confound the evidence of the two; and this confusion introduced an ambiguity in the use of words as we have seen in the case of Momentum, Force and others. The same may be said of Action and Reaction which have both a statical and a dynamical significance. And, by this means, the most general statements of the laws of motion are made to coincide with the most general statical propositions. For instance, Newton deduced from his principles the conclusion, that by the mutual action of bodies, the motion of the centre of gravity cannot be effected...
This passage from Whewell brings out the dangers of using words metaphorically, but it is not part of the metaphorical view that we should use metaphors, come what may, without caution. By being aware that we are using metaphors the benefits which accrue from using them are not so liable to be off-set by unwitting confusion. Even in the case in question, mutual benefits to statics and dynamics resulted. What Whewell does bring out is the metaphorical nature of the use of such terms and the analogical nature of the reasoning they support, even where they occur in closely adjacent fields as in this case.

Probably one of the most striking examples of metaphoric confusion of this kind was the controversy between Leibniz and Cartesian thought over quantity of motion and its conservation. It was not until 1743 that D'Alembert correctly diagnosed this controversy as based merely on semantic inconsistency between the protagonists.

Berkeley, although correct in his diagnosis that the new philosophy employed metaphors, was nevertheless wrong to condemn them uncritically. But his reason for doing so is clear, for Berkeley shares the general theory of his time about language and meaning. As Davie says of Berkeley in *Science and Literature 1700–1740*:

\[
\text{...for him a metaphor is merely a figure of speech, not a figure of thought. Here is certainly a contraction, not in the field of metaphor, but in man's notion of its validity.} \]

Davie goes on to say:

There is some evidence that scientists, and still more, scientific philosophers, are
at least partly to blame for this contraction, but to explore this would be another story. It is a tentative sketch of this story which we have in fact been making, and we can now highlight one of its main points of dramatic tension. This is well focussed in the thought of Locke, the representative philosophical spokesman for the new science.

On the one hand Locke subscribes to the prevailing philosophy of language which regards language as static (or nearly so) and as the labelling of the 'given':

The same liberty also that Adam had of affixing any new name to any idea, the same has anyone still.....but only with this difference, that in places where men and society have already established a language amongst them, the signification of words are very warily and sparingly to be altered: because men being furnished already with names for their ideas, and common use having appropriated known names to certain ideas, an affected misapplication of them cannot but be very ridiculous.....But in communication with others, it is necessary that we conform the ideas we make the vulgar words of any language stand for to their known proper signification.....or else make known what new signification we apply them to.57

In the following passage from a chapter on 'The Abuse of Words' Locke seems clearly to be thinking (among other things) of metaphorical transfer:

Another abuse of language is an affected obscurity, by either applying old words to new and unusual significations, or introducing new and ambiguous terms, without defining either,
or else putting them so together as may confound their ordinary meaning.\textsuperscript{58}

While recognizing figurative language as having a role in rhetoric and poetry his verdict is quite unequivocal as to its cognitive role:

But yet if we would speak of things as they are, we must allow that all the art of rhetoric, besides order and clearness, all the artificial and figurative application of words eloquence hath invented, are for nothing else but to insinuate wrong ideas, move the passions and thereby mislead judgement and so indeed are perfect cheats, and therefore they are certainly in all discourses that pretend to inform or instruct, wholly to be avoided....\textsuperscript{59}

On the other hand Locke thinks that knowledge is solely concerned with our ideas:

Since the mind, in all its thoughts and reasonings, hath no other immediate object but its own ideas, which it alone does or can contemplate, it is evident that our knowledge is only conversant about them.\textsuperscript{60}

We cannot have true knowledge of the mechanisms of the world because (in the doctrine of primary and secondary qualities) we are not directly acquainted either with the interconnection of the primary qualities (although we are acquainted, in effect, with the primary qualities themselves since our ideas resemble them) or with the secondary qualities (since our ideas do not resemble them). But we do have some guide to the hidden causes of things:

Concerning the manner of operation in most
parts of the works of nature: wherein though we see the sensible effects, yet their causes are unknown, and we perceive not the ways and manner how they are produced. We see animals are generated, nourished and move; the load—stone draws iron; and the parts of a candle, successively melting, turn into flame, and give us both light and heat. These and like effects, we see and know: but the causes that operate, and the manner they are produced in, we can only guess, and probably conjecture.... Analogy in these matters is the only help we have, and it is from that alone we draw all our grounds of probability.\\n\\nLater in the same passage he says:

This sort of probability, which is the best conduct of rational experiments, and the rise of hypothesis, has also its use and influence; and a wary reasoning from analogy leads us often into the discovery of truths and useful productions which would otherwise lie concealed.

In this Locke is echoing earlier thoughts, notably those of Bacon. Although, as I have claimed, Bacon's whole method is based on manifest analogy, he also recognized a limited role for imported analogy in a way similar to Locke. Speaking of 'Instances Conformable or of Analogy', he says:

.....although they are of little use for the discovery of forms, they nevertheless are very serviceable in revealing the fabric of the parts of the universe, and anatomizing its members; from which they often lead us along to sublime and noble axioms, especially those which relate
to the configuration of the world rather than
to simple forms and matters. 64

And of supplementary or substitutive instances which
serve 'by gradual approximation or by analogy' he says:
It (analogy) is employed when things not
directly perceptible are brought within the
reach of sense, not by the perceptible oper-
ations of the imperceptible body itself,
but by the observation of some cognate body
which is perceptible. 65

The problem for Locke is how one is to talk about
things known only analogically. Locke has said:
The use then of words is to be sensible marks
of ideas; and the ideas they stand for are
their proper and immediate signification. 66

and:

......common use by tacit consent appropriates
certain ideas in all languages, which so far
limits the signification of that sound, that
unless a man applies it to the same idea he
does not speak properly...... 67

How then do we talk about (or find words for)
things or processes which are not ideas or composed
of ideas (in Locke's technical sense of idea) at all?

The problem is of course aggravated by Locke's
Idealism but exists although in a different form for
any sort of linguistic conservatism such as Locke's.
The problem is how to accommodate a supposedly fixed
linguistic system to genuine conceptual novelty.

L.J. Cohen describes well the linguistic conser-
vatism of the period, of which Locke's is only a
typical example:

Until about two centuries ago almost no one
would have ascribed them (words) meaning in any very different sense. The uses of words changed then as they do now. But whatever their interest in language people almost always talked about meanings in much the same timeless kind of way as they still talk about the ratio of two to four. Metaphor was recognized, but only as a figure of speech, not as a process by which eventually the non-metaphorical use of a word may be extended. If a usage was metaphorical on one occasion, it must also be so on any other occasion. The meaning of a word was thought of as far too integral a property of that word, far too intimately linked with the word itself, whether by nature or by convention, for the word ever to lose one meaning or acquire another. Occasionally one author might stipulate a special sense for his own purposes or argue what the true meaning of a word was. But the basic meanings of every word and construction in the language, whether properly acknowledged or not, were assumed to be invariable.

This might be contrasted with the markedly different attitude expressed in the following, written about a century after the period of which Cohen is speaking:

As it is with ideas, so it is with their symbols, words. New ones are perpetually coined to meet the demand of an advanced understanding, of new feelings that have sprung out of the decay of old ones, of ideas that have shot forth from the summit of the tree of knowledge;
old words meanwhile fall into disuse and become obsolete; others have their meaning narrowed and defined; synonyms diverge from each other and their property is parted from them; nay, whole classes of words will now and then be thrown overboard, as new feelings or perceptions of analogy gain ground.

In spite of their conservative philosophy of language, the seventeenth century scientists, as we have said, did make unwitting use of metaphors. Certainly they were more stable, consistent, developed and sharply defined and analysed, and, in effect, quite different in use from literary metaphors, because explicitly cognitive and meant to be so taken. But they were metaphors all the same, acquiring part of their meaningfulness from their old associations and yet only to be properly understood by someone familiar with the sometimes complex context of their new usages. Without their old associations they would not have seemed so commonsensically meaningful and matter-of-fact, so little esoteric that the description they offered of the world came to be accepted as the only possible and true description. Yet their full scientific meaning was only to be understood by someone able to make sense of them by learning the appropriate new linguistic and mathematical context in which they now appeared.

That there had been, rather than an annihilation of, a regularizing of metaphor and simile is underlined by what happened at this time in literature. It is not the case that, for example, eighteenth century poetry eschewed metaphor. It was that for the most part the metaphors were stable - part of a 'stock diction'. Furthermore they had in many cases lost their
original emotive and aesthetic overtones and become cool and self-conscious - established ways of speaking with cognitive, even theoretical, overtones. In an interesting and detailed study of the nature language of eighteenth century poetry⁷⁰ Arthos makes these points at some length, with a wealth of illustrations. Arthos connects the stock diction of eighteenth century poetry explicitly with science:

.....certain poets were interested in establishing a stock diction for poetry because natural philosophy had convinced them of the value of a stock language for all thought. And in describing nature they could do no better than adopt the language of natural description worked out by scientists.⁷¹

Yet, that this new language of description is still metaphorical is well brought out by the fact that it was capable of being taken into the stock diction of poetry without any loss of comprehension, the terms still retaining enough of their old and familiar connotation to be understood. This, it might be argued, simply means that they had never been employed, even in science, in any except their old familiar ways. However, in many cases an interesting transition had occurred. They were now used in literature in such contexts that they had meaning only as they had become engorged with new scientific connotation. Ordinary language gained from the transaction. After the capital of ordinary language had been invested by metaphor in science the words were returned to ordinary language with the accrued interest of their scientific associations. Davie notes that it is a mistake to think that the eighteenth century literature makes no use
of metaphors. On the contrary it employs a large number of metaphors taken over from the fashionable new philosophy.

On the whole the verdict of West on the seventeenth century in the following passage is correct. This passage occurs in a chapter entitled 'The Death of Metaphor' but, allowing for the author's restriction of the word 'metaphor' to a 'literary analogy', and bearing in mind that scientific models or analogues, far from merely 'summarizing' experiments, also provide the structure and terminology of the explanatory theory of such experiments, we may interpret him as claiming that the seventeenth century saw a dissociation of two kinds of 'metaphor' rather than the elimination of metaphor:

An analogy today may be of two kinds, scientific or literary. The first kind serves as a scientist's working model. The old picture of atoms as being perfectly round, perfectly hard elastic balls, moving at various speeds, was a model of this kind. So was Rutherford's later picture of a heavy atomic nucleus, positively charged, with a number of planetary light electrons circling round it. This type of analogy is used in summarizing the knowledge gained from experiments, and suggesting further properties for which it may be tested in later experiments. If the predictions are fulfilled, the model is either modified or abandoned. A literary analogy, usually a metaphor, has quite different purposes. It is designed to impress the reader with the truth of a statement by a happy comparison directed at the
feelings. It is a very temporary affair compared with the scientific model, which is in permanent use for making calculations and predictions until it is superseded. Moreover, over the metaphor may illustrate or adorn an argument, but it is not one in itself. In other words, it is not to be taken really seriously. Yet before the Great Intellectual Revolution these two kinds of analogy were nearly, if not completely, one.

Some writers have remarked on the profound general transformation the developments in this period wrought in literature, religion and philosophy. However, the central and fundamental transformation was in science. I would like to claim that a fundamental part of this transformation was the setting up of a whole new language firmly rooted in a relatively few, pervasive and well developed metaphors. These metaphors were so natural and unforced that they blended imperceptibly with the prevailing metaphysic and method and in the process rendered that metaphysic so bland that it leaves no aftertaste in the descriptions of the world which that metaphysic and method distils. The very presence of metaphor and analogy (at least in any salient way) is only incidentally detectable. The long thrall of a naive materialistic realism over the minds of most scientists, and mankind generally, begins.

The change from the philosophical science of Athens to the analytic sciences of the Hellenistic period has been compared with the change from scholasticism to the science of Galileo and Newton, and some
parallels have been drawn between the two periods.
We may now remark a further parallel. In both cases
the change is accompanied by a critique of scientific
language, particularly of metaphor: in the one case
by Aristotle predominantly⁷⁶; in the other by a number
of people. In both cases the attitude is similar. The
desirable corrective to the previous uncritical use
of metaphor overreaches itself into a too sweeping
antipathy to metaphor in science. This antipathy
reaches far enough down into scientific thinking
to give a beneficial trim to the former wild over-
growths of language, but not so far down fortunately
that it inhibits the intuitive operation of well-
disciplined metaphorical habits altogether.
FOOTNOTES

Works are referred to simply by author's name and page reference (where appropriate). In the usual case the title and publishing details of the work will be found in the Bibliography. However, where there is more than one author of the same name in the Bibliography, identifying initials are given. Where more than one work of the author concerned is listed in the Bibliography, the title is cited in the relevant text or footnote.

Introduction

1. Among writers who have made such claims are the following: Berggren, Black, Harré, Hesse, Hutten, Schön, Swanson and Turbayne. The Bibliography, under the section METAPHOR gives details of references for these and other writers under the following headings: General Discussions of Metaphor, including those relating to Science; Literary Use, Philosophical Discussion; Philosophical Use (including Analogy); Metaphors and History of Science. The views of these writers will be discussed below.

2. See the section on METAPHOR in the Bibliography under the headings: General Discussions of Metaphor, including those relating to Science; Literary Use, Philosophical Discussion.

3. On the whole the metaphorical view has not excited much comment, but see, for example, the long review article by Olscamp of Turbayne for an exception.

Chapter 1

1. Poetica, 1457 b.

2. Sewell, p. 42.

3. See, e.g. Perelman, 'Analogie et Métaphore,(etc.)', and Schön, Chap. 3, 'Analogy and Metaphor'.

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4. Pepper, pp. 47 ff.
6. For similar linkings in title and context see Canguilhelm, Dambska, Gonseth, Harris, Kargon, Naslin, etc. in the Bibliography.
7. The Ideas of Physics, p. 60.
8. Duhem uses the term 'model' mainly to refer to the so-called 'mechanical' models of Kelvin and others. 'Analogy' is used by him sometimes to mean a relation, sometimes a 'physical analogy' and sometimes a formal mathematical analogue.
9. Mellor, p. 263, notes this confusion and suggests the same solution I have adopted.
10. Webster's Seventh New Collegiate Dictionary.
12. Mill is referring to what I call manifest analogy (q.v. below).
14. Dorolle, p. 42. On the irreducibly analogical nature of colour resemblance, see Church, Chap. 8. Hesse, 'Aristotle's Logic of Analogy', discusses some of the difficulties associated with unanalysable resemblance.
15. Guyenot, p. 16 (my translation).
17. See Keynes, A Treatise on Probability, Part 4, and especially Chap. 23 for a historical summary of the British empiricist approach to analogy.
18. See Novum Organum II, 4 and 10 -16, especially 4 and 16, for the basis of Herschel's and Mill's methods of experimental enquiry. Blake, Ducasse and Madden has
good chapters on the relation between Bacon, Mill and Herschel, and Keynes, *A Treatise on Probability*, has a discussion of Mill's relation to Bacon.


20. References given below are to Anderson, F.H. (ed.) *The New Organon and Related Writings*, which gives a reprint of the standard translation of the *Novum Organum* from Spedding, Ellis, and Heath. The page references in brackets are to the Anderson edition.

21. I, 47 (p.51).
22. I, 45 (p.50).
23. II, 27 (p.173).
24. II, 27 (p.175 f.).
25. See II, 1 - 5, for a discussion of 'forms' and 'natures'.
26. I, 105 (p.98 f.). Plato's method was to use *analogy*
27. II, 11 (p.130).
28. II, 13 (p. 142).
29. II, 12 (p. 132).
30. e.g. II, 2 (p. 122) where he says:
   For though in nature nothing really exists besides individual bodies, performing individual acts according to a fixed law, yet in philosophy this very law, and the investigation, discovery and explanation of it, is the foundation as well of knowledge as of operation. And it is this law with its clauses that I mean when I speak of *forms*, a name which I rather adopt because it has grown into use and become familiar.
31. Blake, Ducasse and Madden, p. 63. See also on the 'forms' Larson, pp. 44 ff.
32. Larson (p. 443) says: '...Bacon's "forms" or "laws" really are descriptive generalizations, based on his natural histories, rather than scientific laws in our sense of the term'.

33. I, 70 (p. 88).
34. I, 83 (p. 88).
35. II, 10 - 20 (pp. 130 ff.).
37. ibid. (p. 156).
38. II, 27 (p. 173)
39. II, 42 (pp. 216 ff.).
40. ibid. (p. 218).

41. See below for a discussion of these terms.
42. Keynes, A Treatise on Probability, p. 268 f.
43. ibid., p. 271.
45. The Philosophy of Discovery, p. 263 f. This passage is cited in Book III, Chap. 9, Section 6, p. 282, in Mill.
47. Quoted in Guyenot, p. 35 (my translation).
48. See Coleman for a detailed analysis of Cuvier's method and philosophy.
49. See Coleman, Chap. 5.
50. See Coleman, p. 120. See also p. 125.
51. See Coleman, passages quoted from Cuvier (translated by Coleman) on pp. 120 and 189.
52. Hesse, Models and Analogies in Science, p. 78.
54. See Galileo (S. Drake ed.), both works, passim.
55. pp. 60 ff.
56. This is not to deny that they play some part in the procedures of what has come to be called (following Kuhn, The Structure of Scientific Revolutions, see pp. 10 ff.) 'normal science', for example in the design and evaluation of controlled experiments and in the elucidation, elaboration and qualification of the insights afforded by other methods. (See, e.g. Copi, Chap. 12, Section 4, on controlled experiments.)
57. For a celebrated critique of Bacon see Macaulay, Vol. II, pp. 290 – 393, especially p. 385. Also see Keynes, passages cited above, and on Mill, see Keynes, A Treatise on Probability, Jevons, Pure Logic and Other Minor Essays. For Whewell see Blake, Ducasse and Madden, Chapter on Whewell. On Herschel see Agassi, 'Sir John Herschel's Philosophy of Success'.
60. Davy, Vol. 8, p. 308.
61. Gilbert, 'The Origin of Hypotheses (etc.)', p. 2. See also his 'The Inculcation of Scientific Method by Example, (etc.)'.
62. Oppenheimer, p. 130.
67. See Popular Scientific Lectures ('On the Principle of Comparison in Physics'), pp. 236 ff., and
Erkenntnis und Irrtum (etc.), pp. 220 ff.
68. See Turner, J. 'Maxwell on the Method of Physical Analogy', p. 325.
70. Galileo's first published reference to inertial motion refers to the ship analogy. See History of Sunspots in Discoveries and Opinions of Galileo, p. 113 f. See also below on the ship analogy.
73. Wells, 'An Essay on Dew'.
74. Cf. Holes, who says: '....there is hardly any difference in the original intellectual processes of a 'great' and a 'small' discovery.' Similarly he is making something like the same point when he says: '....we do not in general make any distinction between pure and applied science, between theoretical work, discovery, invention, technological improvement of any kind whatever, believing that, in principle, the mental process is rigorously the same....' (p. 8) (my translation). Similarly, in The Flash of Genius, designed to be a sort of case study of scientific creativity, the editor, Garrett, presents both technological and pure scientific discoveries on an equal footing, and he says in the introduction: 'No priority has been placed on the assumed value of the discovery but rather on the art of discovery'. Again, in Schön, the author uses routine technological innovations as suitable examples for the analysis of invention and discovery in general.
75. A good account of Kepler's discoveries is given in Dreyer and Koestler.
76. Koestler, p. 336 f.
77. Dreyer, pp. 405 ff.
78. See Lavoisier.
79. For a detailed consideration of this see Dorolle, pp. 73 ff.
80. See Guettard.
82. See Gillispie, pp. 375 ff.
83. See the account in Turner, H.H., p. 105.
84. *An Essay on the Psychology of Invention in the Mathematical Field.*
86. *ibid.*, pp. 13 ff.
87. *Ibid.* See also Polya on analogy in mathematics.
89. Quoted in Garrett, p. 85.
90. 'Induction et Intuition'.
91. p. 34.
92. p. 24 and pp. 36 ff.
93. p. 29 (my translation).
94. p. 29 (my translation).
95. p. 23.
98. p. 375.
99. p. 34.
100. p. 43 n. 1.
104. p. 788 f.
105. p. 214.
106. pp. 154 ff.
108. See excerpt in Clagett, pp. 601 ff.
110. Clagett, *ibid.*
parenthetical remarks briefly indicate the modern equivalents of the concepts which Galileo establishes. See also *Letters on Sunspots* in *Discoveries and Opinions of Galileo*.

112. See, e.g. Kuhn, 'A Function for Thought Experiments', and Ariotti.

113. On the role such analogies have played in the history of science see Metzger.


115. p. 201.


118. See on this Burtt, especially Chaps. 3b, 4a, 7a.

119 and 120. p. 149.

121. See, e.g. Bunge, 'Analogy in Quantum Theory (etc.)', where he equates 'literal' with 'mathematical', at least by implication (see especially Sections 7 and 8).


123. For an outstanding example of this aspiration see Thompson, especially the introductory remarks.

124. See Aristotle in Bibliography.


126. Lloyd, p. 413 f.

127. See Lloyd, pp. 412 and 363 ff.


130. A good survey of this is given in Hurlbutt.

131. Dorolle, Chap. 1.


133. See *Poetica*, 1457 b and 1459 a.

Chapter 2

1. See sections on ANALOGY and MODELS in Bibliography.
2. See, e.g. Black, Models and Metaphors, Brodbeck, and Hutten, 'The Role of Models in Physics'.
3. See Kelvin, pp. 8 - 12, and, for a discussion of Kelvin's use, Duhem.
4. See Turner, J, both items, Kargon and Duhem.
7. See Duhem, p. 71.
9. ibid., pp. 93 ff.
10. ibid., p. 97.
11. ibid., p. 55.
14. See Mellor, passim.
15. See, e.g. Beament, Brodbeck, and Harre, The Principles of Scientific Thinking, Chap. 2.
17. Keynes, loc cit.
20. See McMullin, p. 386 f. for a discussion of this.
22. See, e.g. Achinstein, 'Models, Analogies and Theories'.
26. 'Models and Method', p. 262.
27. ibid., p. 263.
29. Achinstein, 'Models, Analogies and Theories', p. 331
32. Deutsch, pp. 232 ff.
33. p. 22. Quotes in original.
35. 'Models, Analogies and Theories', p. 333.
36. For a discussion of the relation of modellist claims to those of logical constructionists, operation-alists, and positivists, see Hesse, Forces and Fields, Chap. 1, and for a discussion in relation to Positivism see Harre, Theories and Things. See also Braithwaite, 'Models in the Empirical Sciences'.
39. ibid., p. 231.
40. Braithwaite uses the term 'modellist', loc. cit.
42. Campbell, pp. 129 and 132.
43. 'Theoretical Models' pp. 103 and 105.
44. Models and Analogies in Science, Chap. 1, passim, and Forces and Fields, p. 27.
45. 'Models and Theories', p. 140 f.
46. 'Two Views about the Function of Models in Empirical Theories', p. 60.
47. p. 231.
49. 'The Role of Models in Physics', p. 288 f.
50. For a discussion of a wide variety of metaphysical attitudes towards the ontology of hypothetical and theoretical entities and processes, see Feigl, 'Existential Hypotheses', and for a symposium on that paper, see Churchman, Feigl, 'Logical Reconstruction (etc.)', Frank, 'Comments on Realistic versus Phenomenalistic Interpretations', Høgel, 'Science and Semantic Realism', Rasmperger.
52. on cit., p. 140.
53. loc cit.
54. Fürth, p. 327.
57. See, e.g. Hesse, 'Models in Physics'.
60. Chap. 2.
61. Chaps. 3 and 4.
62. See ibid., Chapters 3, 4 and 5, passim.
63. p. 250.
64. p. 250 f.
65. p. 251.
66. p. 251 f.
For an example see Theobald, 'Models and Method', passim.

82. Metaphors and Models, pp. 227 ff.
83. 'Models and Theories', p. 125.
98. p. 240 f. See also preceding remarks on p. 238 f.
100. p. 178
101. p. 179
103. p. 23.
104. i.e. 'The meaning of a proposition is the method of its verification'.
105. The so-called 'genetic fallacy'.
106. See above, p. 40 f.
107. p. 269.
109. p. 318 f.
110. 'Maxwell on the Logic of Scientific Explanation', p. 46.
111. See Turner, *op. cit.*, footnote 33.
112. p. 38.
113. p. 39.
114. pp. 41 ff.
115. 'Analogy in Quantum Theory (etc.)'.
117. p. 284.
118. p. 278.
119. p. 281.
120. pp. 289 ff.
121. See, e.g. Fourier's assumptions about the nature of heat (p. 204 f.) and Green's and MacCullagh's assumptions about the nature of the aether (p. 206 f.).
122. p. 129 f.
124. See pp. 67 ff. and 121 ff.
125. p. 129.
126. p. 90.
127. p. 91.
128. p. 82 f.
129. p. 118.
130. p. 117.
131. p. 123 f.
132. p. 208.
133. 'The Role of Models in Physics', p. 288.
134. The Language of Modern Physics, p. 89.
135. p. 233.
136. p. 135 f.
137. p. 136.
139. loc. cit.
140. p. 143.
141. p. 144.
142. p. 144.
143. 'The Role of Models in Physics', p. 288.
144. p. 229.
145. An Introduction to the Philosophy of Science, p. 59.
146. Scientific Explanation, p. 51.
147 b. p. 238.
148. 'The Role of Models in Physics', p. 300.
149. p. 77 f.
150. Hutten, 'On Semantics and Physics', Spector, 'Models and Theories'.
151. p. 140 f.
152. p. 116.
153. For Duhem, see the discussion above, pp. 40 ff. For Hertz, see below.
154. Quoted in Byerly, p. 142.
155. loc. cit.
156. 'The Role of Models in Physics', p. 290.
159. p. 395.
161. ibid., p. 230.
162. pp. 93 ff.
163. 'Models in Physics'.
165. p. 248.
166. pp. 41 ff.
167. in Formal Models and Achinstein's "Analogies" .
168. ibid., p. 100.
169. ibid., p. 98.
170. p. 328.
172. p. 187. Götling also takes a simplification view of model, see p. 60. Maxwell has a similar view, see Turner, J. 'Maxwell on the Method of Physical Analogy', p. 227.

**Chapter 3**

1. But see below on 'dead metaphors'.
2. See discussion of such examples in Wheatley, p. 106.
3. I do not take up the question of whether scientific models or theories may be regarded as similes, as there is little discussion of this in the literature.
4. A delightful simile which was a remnant of my father's Suffolk boyhood.
5. All from Cecil Day Lewis's fine narrative poem, 'A Flight to Australia'. See his *Selected Poems*, (London, 1951), pp. 57 ff.
7. See Bibliography, section on METAPHOR.
11. See, e.g. Christensen for a detailed analysis of meaning.
12. See *The Structure of Complex Words*.
13. p. 94. For a detailed study of Richards's ideas see Hotopf.
15. This nevertheless seems to be limited by a criterion of aptness or appropriateness. One sometimes speaks, for example, of a metaphor being powerful or illuminating, or, on the other hand, weak or strained. But see the next point.


17. p. 96.

18. In 'The Meaning of the Word "Literal" '.

19. In a footnote to Models and Metaphors, p. 47.

20. p. 221.


23. ibid., p. 43.

24. p. 44. Cf. my analysis of the formulating function of analogy.

25. p. 43. I presume I am correctly interpreting Black's rather cryptic statement on this.

26. p. 43.

27. Henle, p. 182 f.


29. p. 232. See below on the question of 'pictures'.

30. p. 90.

31. p. 678. As Van Steenburgh points out, by his definition literalness and metaphoricalness are thus subjective, and metonymy and synecdoche are not metaphorical (p. 679).

32. Language and Lyth, pp. 86 ff.

33. ibid., p. 28 f.

34. In Wheelwright, 'Semantics and Ontology', p. 5.
35. Henle, p. 191.
36. 'The Meaning of the Word "Literal"'.
37. p. 91.
40. ibid., p. 55.
41. p. 120.
42. p. 92.
43. p. 90.
44. p. 41.
45. 'Semantics and Ontology', p. 6.
47. p. 217.
49. p. 680.
50. p. 678.
51. p. 681.
52. In The Meaning of Meaning (etc.).
53. The emotive theory of metaphor is considered again in Chap. 5.
56. op. cit., p. 47 f.
57. The Burning Fountain, p. 45 f.
58. Wheelwright's expressive phrase.
59. p. 55.
60. See Berkeley (ed. Fraser) Of the Principles of Human Knowledge, Introduction, Section 13, in Selections from Berkeley, p. 22.
61. p. 98.
62. My examples.

Chapter 4.
6. See p. 115 f. above.
7. In view of his references to Bentham’s theory of fictions in Symbolic Distance and Ogden’s interest in Bentham, reflected in his critical edition of Bentham’s Theory of Fictions, there would seem to be as much influence of Ogden as of Richards.
9. ibid., p. 141.
10. ibid., p. 82.
11. ibid., p. 82 f.
12. ibid., p. 84.
13. ibid., p. 82 f.
14. ibid., p. 96.
15. ibid., p. 96.
16. ibid., p. 97.
17. ibid., p. 99f.
Symbolic Distance,
18. p. 36
19. ibid., p. 16.
20. ibid. See pp. 29 ff.
21. ibid., p. 33.
22. ibid., p. 45.
23. ibid., p. 28.
24. ibid., p. 44.
25. ibid., p. 83.
26. ibid. See pp. 62 and 89 ff.
27. Robinson, 'Ambiguity', p. 148 f. On the point that ideas of this kind are not communicated by formal definition, see Kaplan, who claims that many scientific terms are not given a specific definition, nor is it desirable they should be; rather they are 'defined' by 'indicators' which 'specify a more or less indefinite meaning by giving the conditions under which the new term is likely to apply' (p. 284).
29. p. 37.
30. p. 51.
31. p. 94 f.
32. pp. 111 f. and 112 f. See Harré, pp. 174 ff. below, for somewhat similar claims.
33. pp 232 ff.
34. See Dialogues concerning Natural Religion in Wollheim, especially pp. 124 ff. where Hume shows a clear insight into the role of analogy and resemblance in scientific thinking and progress, and gives a cool and critical appraisal of the analogical and metaphorical nature of basic metaphysical beliefs such as mechanism, teleology and atomism.
35. p. 12. The quotation from Ryle is not a definition of metaphor for Ryle, however, as Turbayne twice points
out (pp. 12 and 18), but a definition of a 'category-mistake' or categorial confusion.

37. p. 19.
38. p. 17.
41. p. 27.
42 a. Chap. 2.
42 b. p. 52.
43. p. 64 f.
44. p. 47.
45. p. 47.
46. p. 49.
47. p. 49.
49. p. 64.
50. p. 81.
51. p. 97.
52. p. 100.
53. p. 75.
54. p. 77.
55. p. 80.
56. p. 81.
57. p. 9.
58. pp. 53 ff.
60. See Conclusion, pp. 192 ff., especially p. 194.
61. 'The Use and Abuse of Metaphor' (I and II).
62. 'Metaphor' and Models and Metaphors.
64. Forces and Fields and 'The Explanatory Function of Metaphor'.
65. *The Language of Physics* and 'The Role of Models in Physics'.
66. *Models and Mystery*.
67. 'On Models'.
68. 'Meaning Variance and Metaphor'.
69. *The Language of Modern Physics*, p. 84.
70. 'The Role of Models in Physics', p. 289. The last sentence is quoted by Black, *Models and Metaphors*, p. 236, and Ramsey, p. 50, as: 'The model functions as a more general kind of metaphor'.
71. *ibid.*, p. 293.
72. *The Language of Modern Physics*, p. 84.
73. 'The Role of Models in Physics', p. 289.
76. *Models and Metaphors*, p. 236.
77. *ibid.*, p. 236.
82. *ibid.*, p. 236 f.
83. *ibid.*, pp. 41 and 238.
84. *ibid.*, pp. 35 and 229.
89. p. 13 f.
90. p. 48.
91. p. 48.
92. p. 49 f.
93. p. 52 f.
94. pp. 55 and 58.
95. p. 52 f.
96. p. 300.
97. p. 303.
98. p. 303.
100. p. 305.
103. Rational Preference Ranking.
104. p. 308.
105. p. 19.
106. pp. 249 ff.
107. p. 252.
109. See above, p. 116 f.
110. See above, pp. 140 ff.
111. p. 218.
112. See, e.g. Chap. 1 of The Myth of Metaphor and Turbayne's talk of mistaking the mask for the face.
113. p. 64.
114. 'The Role of Models in Physics', p. 296.
115. passim.
117. p. 443.
118. See, e.g. pp. 22 and 27.
120. p. 257.
121. I am interpreting Hesse here. She does not in fact distinguish between theories about phenomena which have been previously unexplained and replacement of one theory by another.
122. p. 257.
123. Cf., e.g. Furth, p. 329 on the replacement of
models.

124. Such an explication is the subject of part of Achinstein's 'Concepts of Science (etc.), pp. 10 ff. and 98 ff., and MacCormac's article.

125. p. 258.

126. See Models and Analogies in Science, pp. 43 ff. and see above, p. 87 ff.


128. See four items in Bibliography.

129. See 'Explanation, Reduction and Empiricism', p. 29, for a succinct statement of Feyerabend's position.

130. See 'Explanation, Reduction and Empiricism', p. 76, and 'Reply to Criticism', p. 239 ff.

131. For a good brief account of this, see Frank, 'Einstein, Mach and Logical Positivism'.

132. pp. 500 ff.

133. Chaps. 1 and 2 and pp. 98 ff.

134. ibid., p. 51.

135. ibid., p. 101.

137. p. 149.
138. See, e.g. *The Philosophy of 'As If*', passim, and reference above on p. 62 f.
139. p. 154.
140. p. 155.
141. p. 156 f. Vaihinger distinguishes the first two types, see Vaihinger, p. 16.
143. p. 176.
144. p. 187.
145. See pp. 133 - 175.
146. pp. 34 ff.
147. p. 387.
149. p. 452.
150. p. 241.
151. p. 456.
152. p. 458.
154. p. 458.
155. p. 458.
156. p. 459.
157. p. 239.
158. p. 460.
159. p. 462.
160. p. 463.
162. *ibid.*, p. 36.
163. pp. 18 - 22.
164. p. 34.
165. pp. 28 ff.
166. p. 38.
167. p. 35.
168. p. 36.
169. p. 37 f.
170. p. 40.
173. p. 53.
174. p. 53.
175. p. 59.

**Chapter 5.**

1. 'Metaphor', p. 285 f.
4. 'The Use of Metaphor in Analytic Thinking'.
5. 'On Defining Metaphor'.
7. p. 621.
8. p. 435.
13. op. cit.
15. p. 186 f.
18. p. 9.
23. See above, p. 131.
24. *Symbolic Distance (etc.)*, pp. 16 ff.
25. p. 28.


27. See *Symbolic Distance*, *et al.*, pp. 16 ff.; quote is on p. 76.

28. ibid., p. 68 f.

29. p. 82.


31. ibid., p. 299.

32. Berggren, p. 239.

33. *Models and Metaphors*, p. 44.

34. p. 282.

35. p. 294.

36. p. 308.

37. p. 298 f.

38. p. 224.

39. p. 231.

40. 'The Explanatory Function of Metaphor', p. 256.

41. ibid., p. 257.

42. ibid., p. 258.

43. See, e.g., Schön, p. 111 f., Berggren, p. 456 and Harre, p. 33 f. Harre is on the whole much less susceptible to this sort of difficulty than the others, since he mostly seems to assume that words and expressions are self-evidently metaphorical by the paradigm-case criterion, q.v. above, pp. 175 ff.

44. p. 41.

45. p. 41.

46. p. 547.

47. p. 64.

48. p. 48 f.

49. p. 46 f.

50. p. 65.

51. It was in fact some such analysis that led to Berkeley's critique of 'force', see pp. 253 ff. below, and similar considerations which led to the claim
that Newton was postulating 'occult' causes.
52. See Horsburgh, pp. 233 ff.
54. Horsburgh, pp. 243 ff. I am most conscious of having adapted, and perhaps distorted, Horsburgh's remarks in relation to this criterion.
55. p. 680.
56. Theories and Things, p. 34.
57. ibid., p. 35
58. ibid., p. 35 ff.
59. pp. 682 ff.
60. ibid. See p. 38, for example, where he seems to equate 'change in temperature differential with time' with a paradigm-case-procedure, and hence apparently with an ostensive definition.
61. p. 681.
62. p. 211.
63. p. 285, in 'Analogy and Quantum Theory, (etc.)'.
65. 'The Role of Models in Physics', p. 296.
66. p. 458.
68. p. 155.
69. p. 687 f.
70. p. 306 f.
71. Quoted in Models and Metaphors, p. 25.
72. p. 215.
73. p. 220.
74. p. 221.
75. We have already discussed this in relation to models when considering Braithwaite's claim that models do not provide greater predictive power than theories without models. See above, pp. 86 ff., and 'Models in the Empirical Sciences', p. 230.
76. Black, Models and Metaphors, p. 43.
77. p. 116 f.
78. 'The Definition of Scientific Method', p. 90.
79. p. 52 f.
82. On this idea of relative truth see Cassirer, 'The Influence of Language upon the Development of Scientific Thought', p. 321.
83. 'Analogy in Quantum Theory, (etc.)', p. 269.
84. ibid., p. 269.
85. ibid., p. 281.
86. ibid., p. 281.
87. ibid., p. 280.
88. ibid., p. 281.
89. p. 629.
90. 'Analogy in Quantum Theory, (etc.)', p. 285.
91. ibid, p. 284. Cf Götzlind, who says: 'It is the models of models that are visual' (p. 62).
92. ibid., p. 282.
93. See Woodger for some biological examples.
94. Scientific Explanation, p. 93.
95. See below, p. 255 f. See also Mindel for a discussion of a good example of the uncritical use of scientific metaphors.
96. p. 80.

97. p. 79.

98. We may, for example, need to assimilate new data, or eliminate anomalies, or we may simply hit upon new and better metaphors and models.

99. p. 53. Turbayne has previously quoted Cardinal Bellarmine's injunction to Galileo to 'speak hypothetically', Turbayne, p. 40.

100. On the Greek use of metaphors, see Lloyd, passim, and Stanford, passim. On Renaissance use of metaphors and analogies, see Mazzoc, 'Universal Analogy and the Culture of the Renaissance'. Generally on the history of analogy in early science, see Metzger. On the microcosm/macrocosm in particular, see Conger, passim.


103. p. 361 (my translation).

104. See, e.g. Hesse, Models and Analogies in Science, Chap. 3. For an attempt to cast reasoning by analogy in a quasi-deductive form see Polya, Vol. II, Chaps. 12 and 13, pp. 3 - 54.

105. To use Buchanan's expression. See Symbolic Distance (etc.), p. 35.

106. 'Universal Analogy and the Culture of the Renaissance', p. 300.


108. Hester, pp. 14 - 16, 70 f., 89 - 91, and 161, gives some discussion of the emotive theory and the footnotes refer to some of the main literature on the subject.

109. p. 128.

110. Olschamp, ibid., p. 120 f. and Hayes, p. 79.

111. 'Problems of Empiricism', p. 214 f.
112. For a discussion of this see Urban, p. 347 f.
113. p. 151.
114. op. cit., passim.
115. See, e.g. Arber, Bonner, Beament, Buchdahl, Canguilhem, Conger, Farber, Hall, Kargon, Laudan, Lloyd, Metzger, McGuire, Nash, Oppenheimer, Pederson-Krag, Ritterbush, Sabra, Temkin, Turner.
116. 'The Definition of Scientific Method', p. 81.
117. Polya, op. cit., passim.
118. Wilson, D., p. 19.
119. 'On Defining Analogy', pp. 84 ff.
120. See, e.g. pp. 82 ff.
121. Gregory, J.C., p. 250.
123. p. 339.
124. ibid. A good example of the way metaphors and models are used in creative scientific thinking and speculation is provided by Wilkie's account of Galton's speculations on heredity and evolution. See Wilkie.

Chapter 6.

1. In the Novum Organum, I,43,59,60and 63 (pp. 49, 56 ff., 60).
2. See, e.g. Dialogues concerning Two Chief World Systems (etc.), First Day, pp. 38 ff. See also discussion in Geymonat, p. 52.
3. pp. 140 ff. in A Second Disquisition to John Riolan, Jun., (etc.).
4. Boyle, passim.
5. p. 111 and p. 113.
8. See Chapter on 'Bacon and the Seventeenth-century Dissociation of Sensibility' in Explorations (etc.).
Century (3 vols., 1908) quoted in Burton, p. 41.

10. Novum Organum, I, 43 (p. 49).

11. ibid., I, 59 (p. 56).

12. ibid., I, 63 (p. 60).

13. e.g. ibid., I, 60 (p. 56).


15. Letters on Sunspots in Discoveries and Opinions of Galileo, p. 92.


22. ibid., p. 110.

23. ibid., p. 102.


25. The Advancement of Learning (page reference not given), quoted in Barfield, 'Poetic Diction and Legal Fiction'.

26. See the discussion of this in Conger, p. 68 ff.

27. From letter to Cotes in Thayer, p. 6.

28. See letter to Cotes just cited.

29. Thayer, pp. 135 ff.

30. See the Scholia on space and time and at the beginning of Book III of Mathematical Principles.
31. In Blake, Ducasse and Madden.
32. In The Royal Society Newton Tercentenary Celebrations.
33. See especially p. 285.
34. See Cassirer, The Philosophy of the Enlightenment, Chap. 1. Cassirer, significantly, connects the growing recognition of the provisional nature of scientific thought with the recognition of the legitimacy of analogical reasoning. See also on this Sampson.
36. Chap. 4, p. 145 f.
37. Laudan, p. 102 f.
38. See, for example, Meditation III from the Meditations on the First Philosophy in Descartes, especially p. 110.
40. Newton, Mathematical Principles of Natural Philosophy, p. 5.
41. ibid., p. 192.
42. From the Opticks, in Thayer, p. 100.
43. See Definition II in Mathematical Principles of Natural Philosophy, in Thayer, pp 12 f. Whewell claims that Kepler is thought to have introduced the word 'inertia', see Whewell, The Philosophy of the Inductive Sciences, Vol. II, p. 191.
44. See the discussion in Ducasse, C.J. 'John F.W. Herschel's Methods of Experimental Enquiry' in Blake, Ducasse and Madden, especially pp. 165 ff.
45. Ducasse, op. cit., p. 165.
48. ibid.
50. ibid., p. 204.
51. *ibid.*, p. 204.
52. Consider for example the eventual labyrinths into which terms such 'ether' and 'absolute space' and 'absolute time' were to lead.
53. Vol II, p. 35.
56. Davie, p. 85.
59. *ibid.*, Book III, Chap. 10, Section 34.
60. *ibid.*, Book IV, Chap. 1, Section 1, Vol. II, p. 308.
61. *ibid.*, Book IV, Chap. 16, Section 12, Vol. III, p. 110. Note here again the equivalence of probability with analogy. Also on this see McGuire and Yost.
63. See above Chap. 1, pp. 6 ff.
64. *Novum Organum*, II, 27 (p. 173).
65. *ibid.*, II, 42 (p. 217).
69. Archdeacon Hare, quoted in Trench, p. 1 f.
70. *The Language of Natural Description in Eighteenth Century Poetry*.
71. p. 28.
73. pp. 107 ff.
74. See, e.g. Willey.
75. See Arthos, p. 81.
76. See Lloyd, pp. 404 ff.
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Rather than give a straightforward list of books and articles consulted, I have grouped the works together under various topics as indicated. The groupings of topics are not exclusive, however, and in many cases the book or article concerned deals with topics in other sections. The grouping by topics is intended rather as a guide to the main emphasis of the works in question.

All references to 'Proceedings of the Aristotelian Society' and 'Mind' are to New Series. The following abbreviations are used for journals:

Amer. Phil. Q. = American Philosophical Quarterly.
B.J.P.S. = British Journal for the Philosophy of Science.
Inter. Phil. Q. = International Philosophical Quarterly.
Jour. of Phil. = Journal of Philosophy.
Phil. and Phen. Research. = Philosophy and Phenomenological Research.
Phil. of Sc. = Philosophy of Science.
Phil. Q. = Philosophical Quarterly.
Phil. Rev. = Philosophical Review.
Introductory

Any serious study of analogy and metaphor ought to start with the main historical and philosophical source of ideas on these subjects - Aristotle. The following are the main sections of Aristotle's works which deal with one or other aspect of analogy and metaphor. The best edition to consult for this purpose in English is Ross, W.D. (ed.) The Works of Aristotle Translated into English (by various authors), (Oxford, 1908 - 1928). The references below are to volume nos. in this edition. Good discussions of analogy and/or metaphor in science in relation to Aristotle are to be found in Dorolle, op. cit, Chap. 1, Hesse, M.B., 'Aristotle's Logic of Analogy', loc. cit., Lloyd, op. cit., especially Chap. 6, and Hurlbutt, op. cit., Chap. 6.


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