THE UTILITY OF ANALOGY IN SYSTEMS SCIENCES

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DECLARATION

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ABSTRACT

THE UTILITY OF ANALOGY IN SYSTEMS SCIENCES

The structure of the thesis reflects the three main areas of investigation. The legitimacy of analogy as a systems concept, the derivation of a model of analogy for systems thinkers and the description of a framework for practice. In the first section we are concerned with establishing an appreciation and understanding of the potential utility in the concept of analogy for systems thinkers. Having briefly surveyed the history of analogy in systems thinking and acknowledging the current methodological interest in metaphor we note that our interest in analogy has been a target for our critics and led to a loss of credibility.

The thesis calls for a re-evaluation of this situation and we hence describe a system thinker's view of science as the grounds on which the utility of analogy is normally dismissed. The first three chapters show that the basis on which science attacks analogy as invalid and inappropriate is itself contentious and that identified 'weaknesses' in the scientific framework can become strengths in the re-conceptualisation of a model of analogy. We consider and distinguish the dynamic relationships between analogy, model and metaphor.

In the second section having established the potential value of analogy as a concept, the thesis develops an explanation of how a model of analogy for systems thinkers can be conceptualised. In the development of the model we will consider particular implications of three types of analogy, 'positive', 'negative' and 'neutral' analogy and discuss the suggestion that they reveal possibilities for exploring different and contrasting rationalities; these issues will be discussed looking at the relationship between analogy and rationality and in this context the validity of the argument from analogy. In the final section the thesis asserts that that systems thinking should not shy away from explicit use of analogy and shows how can use the framework of analogy to re-conceptualise systems concepts.

PREFACE

1.1 INTRODUCTION

This thesis is concerned with developing an understanding of the concepts of analogy and metaphor and their usefulness in systems thinking.

The twin concepts have enjoyed a growing interest and relevance in recent years because the issues their examination raises have dogged philosophers for centuries, although they also remain distinctly and intriguingly contemporary. They include, for example, how we can investigate 'reality', how we can learn about it and derive knowledge of it that is transferable.

For this reason the over-arching concept of analogy and the not altogether subsumable sister concept of metaphor, are particularly stimulating for the development of systems thinking. In this discussion the main concern is with establishing the value and legitimacy of the perplexing sorority as interpretive conceptual tools. This kind of quest is not unfamiliar to the systems community.

The thesis is theoretically based and develops three main themes; the validity of analogy as a philosophical concept, the nature of *'reasoning by analogy'* and the potential of the latter for use in practice, as well as theory.

1.2 BACKGROUND

> *'The world is round and the place which may seem like the end may also be only the beginning.'*
> Ivy Baker Priest, Parade, Feb 16 1958.

Analogy, as a form of modelling, is very important to the systems way of thinking since it is clearly a non-reductionist modelling approach. Alternative conceptualisations positively 'emerge' in a rigourous and developed analogical model. This appears to suggest that the structure of a given analogy can act to release concepts, interpretations or information [in a broad sense] from usual interpretations, which are inherently involved with paradigmatic and rational frameworks of derived understanding and utility.

In the format in which this quality of analogy is examined, it is suggested that the heuristic legitimacy and value derivable by the use of a structured analogy can be formalised in a framework. This matter itself leads to deriving methodological principles with which to work fruitfully with analogy. In this way metaphor too, can be given a place on a so-called 'methodological continuum'.

In the attempt to examine and develop the notion of analogy as a model, a number of puzzling matters were encountered. Modelling, even in the social sciences, has traditionally rested on the underlying assumptions and methods of the natural science paradigm. This thesis takes the view that the wholesale transference of modelling assumptions from natural science will not do for systems thinkers. It is timely to attempt to develop alternative approaches to model building and ones which are better suited to the systems framework. But for any such process to be useful we must try to gain an understanding of the concept of modelling analogy, within a robust philosophical arena, or credibility will be foresaken, even before we start. It is hence in this vein that the thesis joins the debate in systems sciences on the utilities of the diverse rationalities operating non-reductionistically in systems thinking.

A *'framework of analogy'* is introduced and explored in the thesis. Within the framework, the structure of the model works to juxtapose modelling rationalities and hence begins to promote a capacity to make them accessible, or 'systemically reducible'. In this way there is also an umbilical link through to the developing discourse of critical systems thinkers.

Overall, the main concern is with developing an understanding of mechanisms in analogy which can lead to the expansion of theories across new conceptual domains. This activity is very important, particularly when exploring systems concepts and their underlying theoretical potentialities, prior to their incorporation in pragmatic methodologies. The benefits of specific methodological uses of systemic concepts will not be understood until this task is undertaken.

1.3 STRUCTURE AND OBJECTIVES OF THE THESIS

To paint a clear a picture of the thesis for the reader I shall now outline the structure while highlighting the objectives of the investigation into 'The Utility of Analogy in Systems Sciences'.

The single most important aim is the introduction of a developed model of analogy through which a discussion and demonstration of its utility for systems sciences can emerge.

The line of argument follows the three themes linked by the idea that analogy is of interest and potentially of value to systems thinkers, in terms of analogy as a concept, as a form of reasoning and in method terms.

The thesis hence develops arguments to meet the over-arching objective, via discussion of sub-objectives in each chapter. For example, in order to reach a position whereby we can begin to consider a systems thinker's concern with analogy, we first need to establish an understanding of analogy as a concept.

We begin with a survey of the use of analogy in systems thinking to date. In the first chapter we find that systems thinking has an intimate and sometimes uncomfortable relationship with overt or covert analogical principles. 'Intimate' because it will be argued that binding notions of the 'open system' and other systems concepts are inherently analogical. 'Uncomfortable' because both friends and enemies of the systems movement have been scandalised by the apparent reliance on what appears to be an extraordinarily feeble basis for sound inference.

Equally, metaphor is arousing more and more interest in the systems community, since it is taken to offer help in creative problem solving. Several studies in systems science have already been directed towards using metaphor to augment or initiate systems methodologies. For this reason, we must beware.

The credibility of the systems approach has suffered for its lack of theoretical self examination in the past. One might argue that it is characteristic of the systems movement to plough ahead pragmatically, if prematurely, with the practical applications of intuitively appealing systems concepts and then be either disappointed with the results, or embarassed that we are not taken seriously by other disciplines. This accounts, perhaps, for the systems practitioner's complaint that,

> *'Systems assertions are often understandable, undeniable and matters of fact. The substantive difficulties for the practitioner arise in establishing the relevance of any particular systemic description of a problem as*

being relevant to the issue under consideration.' Eden and Graham [1983].

Of course, the other side of the coin is that there has already been 'too much talk' and that it is time for action in the real world application of systems concepts. My own opinion is that if we go full steam ahead in the introduction [or arguably, the *re*-introduction] of analogical and metaphorical concepts in systems methodologies, we risk at least two things.

Firstly and obviously, that we will risk missing the full potential insights that these two concepts can offer the systemic problem solving perspective. The point has been made by Eden and Graham above. Metaphor and analogies particularly, are easy to understand and use at the superficial level. So are many other systems concepts, but it must be time to go beyond the superficial.

This brings me to the second danger we risk overlooking. If we have not undertaken a structured and philosophically based investigation of analogy, what can we learn from their use beyond an immediate thrill of creative insight or conceptual novelty? How, without a model of analogy, can we take what we learn elsewhere? Systems thinkers have to take this aim to heart, for we should aim to be nothing if not peripatetic.

Therefore the thesis begins with a brief discussion of the uses of analogy and metaphor in the systems movement so far. The first chapter will set out the ways systems thinkers have developed their notions of analogy and describe criticisms these conceptualisations have prompted. It will be argued that a clearer understanding of analogy and metaphor is required and that criticisms directed towards their use in reasoning are misplaced and of dubious origin.

In the second chapter we pursue the aim of establishing a clearer understanding of the concept of analogy and its potential utility in systems sciences. The task of establishing a credible notion of analogy is initiated by developing a view of the characteristics of science, in order to show the basis from which analogy is normally criticised. Within that chapter, I hope to show that the notion of a robust, 'factually based' scientific rationality will dissolve away, allowing two features of analogy to emerge as relevant to systems thinking.

These two characteristics are pursued in the next chapter, which consists of a philosophical discussion of analogy. In the discusiion, we will draw out these main points. Firstly, that analogy as a form of comparison has traditionally been conceived as a 'loose' identification of resemblances, but it will it also be shown to be rigourous and highly important in theory development. Secondly, that the validity of the argument from analogy, which has much concerned philosophers, can be assessed in terms of an argument relating to any form of model, since models are vital ways of developing appreciations of theories and concepts.

The potential utility of analogy to systems thinkers established, the succeeding chapters will hence consider two main areas; What is a useful model of analogy for systems thinkers? When is an argument from analogy a valid argument, that is, under what circumstances can the practical explorations of analogy be useful for theoretical development in systems theory?

In discussing the characteristics of the framework of analogy, the notions of positive, negative and neutral analogies are introduced. These conceptualisations are vital to the investigation of the utility of analogy for systems thinkers. Particularly important is the suggestion that positive, negative and neutral analogy reveal possibilities for exploring different and contrasting rationalities. This will be illustrated with examples of re-conceptualisation of a number of systems concepts.

By the end of that section, we will have debated the importance of the concept of analogy to systems thinkers and how we can get to grips with its use through a model/method which describes its processes. For example, we will establish that systems thinking should not shy away from explicit use of analogy, and illustrate how analogical reasoning can be drawn upon in systems thinking, as well as highlighting some ways in which it should not, by considering two main issues. Firstly that analogy is generally useful when used explicitly and rigourously, specifically such as in re-conceptualisations of systems ideas. Secondly that analogy has flawed usage when it is used in partial observation, typically to point out aspects of similarity, the most superficial appreciation of the framework of analogy.

Lastly, in the concluding chapter and having established the utility of analogy in the systems sciences, I will make some reference to issues of praxis. The tenor of this thesis is principally theoretical although during the course of the research two case studies were carried out attempting to use analogical problem solving principles in real world problem solving contexts. The case study reports submitted to the organisations concerned are recorded in full in the appendices. Hence, in my concluding remarks I will make some references to the practicalities of developing the utility of analogy further.

I have represented the structure of the thesis in a table for the convenience of the reader who may find it necessary to refer to this summary of the arguments presented in the main body of the thesis. The table is illustrated at the end of this Preface.

1.4 NOTE

The text is presented as per the format required by Senate regulations.

The thesis is divided into chapters. At the beginning of each I will give an introduction to the argument presented and describe the aim of the chapter. At the end of each, the reader will find a set of 'conclusions' drawing out the most significant themes for discussion in the succeeding chapter.

To add clarity, the chapters have been sub-divided although the reader will note that some chapters are more so divided than others. The reason for this is that in some chapters discussing the concept of analogy, broader brush strokes are necessary. The initial argument is highly discursive and concerned principally with extracting the theoretical, contextual themes pursued in greater detail as the thesis develops. With the greater detail comes a more refined sectionalisation as the characteristics of the model of analogy are presented.

With regard to referencing, the Harvard system has been used. When a text has been widely used or is considered of special importance to the argument, I have referenced the author and the date of publication in the body of the text. Further details of all references are given at the end of the thesis. If the work is quoted directly in the text, I have followed the quotation with the page number of the original text. Some additional material, however, is referenced in footnotes at the end of each respective page. Footnotes have been used when the text concerned has been considered of secondary importance to the argument, or has been consulted only once. Articles which have appeared in newspapers are also referenced in the Footnotes, which are numbered throughout each individual chapter.

1.5 STRUCTURAL SUMMARY.

'THE UTILITY OF ANALOGY IN SYSTEMS SCIENCES'

CHAPTER	THEME	SUMMARY
One The Utility of Analogy In Systems Thinking; A Survey.	Establishing legitimacy for the CONCEPT of Analogy.	*Critics have unfairly lambasted the systemic use of analogy. A reconsideration is overdue.*
Two A Systems Thinker's View of Science.	... CONCEPT of Analogy...	*Arguments that analogy is unscientific raise questions on the nature of science itself. We argue that features of science do not support its criticisms of analogy.*
Three Analogy; A Philosophical Point of View	... CONCEPT of Analogy.	*Reviews of analogy and metaphor stress their potential use in modelling.*
Four: A Model of Analogy	Introducing a MODEL of analogy for systems thinkers.	*Discussion of the characteristics of a model of analogy and in what ways it is of interest to systems thinkers.*
Five Analogy and Rationality	....a MODEL of analogy for systems thinkers.	*Positive, negative and neutral analogies introduce different rationalities within the framework of analogy.*
Six The Utility of Analogy	... a MODEL for analogical **PRAXIS.**	*Using the framework of analogy we discuss the potential utilities of diverse rationalities and describe the re-conceptualisation of systemic concepts.*
Seven Lessons for Systems Sciences	A CONCEPT, A MODEL AND A METHOD OF ANALOGY	*Summary of the arguments for 'The 'Utility of Analogy in the Systems Sciences'.*

CHAPTER ONE

USE OF ANALOGY IN SYSTEMS THINKING: A SURVEY

1.1 INTRODUCTION

In this chapter I will present a brief historical survey of the systems thinker's use of analogy and latterly of metaphor. In the descriptions, criticisms of the use of analogy both actual and potential, will become evident. The objective of the chapter is to establish the necessity for a theoretical re-evaluation of the concept of analogy.

1.2 ANALOGY IN SYSTEMS THINKING

An analogy compares one thing with another in order to indicate resemblances between them and thereby to increase understanding of the lesser known of the two. The systems approach relies quite heavily on the explicit use of analogy, the 'Brain of the Firm'[1] considers a neurocybernetic analogy, and the notion of the 'Open System', for example is inherently metaphorical. More recently many systems thinkers are increasingly interested in the associated use of metaphor, a more subtle, creative, 'calling forth' of resemblance, also inherent in the concept of the 'Open System' but also in '*Diagnosing* the System for Organisations'[2] and in notions of '*Hard*' and '*Soft*' systems thinking.

These are, however, concepts which conjure up many problems for all those who become interested in them. Philosophers, psychologists and linguists, for example, are interested in explaining by what criteria certain sorts of 'structured comparison' are deemed more insightful than others and their concern is in establishing a credible role for analogical and metaphorical phenomena in theory. But equally, methodological practitioners of the pragmatic persuasion are also keen to exploit the pervasive, 'common sense' utility of these approaches for real world problem solving ends.

The impetus of this research has involved looking at the way analogy is useful to the systems concept and then in more detail, at how utility may be made more

[1] S Beer, 1981, Chichester, John Wiley & Sons.
[2] S Beer, 1985, Chichester, John Wiley & Sons.

explicit for a clearer view of the ways in which it can be incorporated in methodology and systems theory.

But first it is worth looking at the ways analogies and to a lesser degree, metaphors, are prevalent in systems thinking.

1.2.1 *An Analogical Legacy*

In Lilienfeld's [1978] analysis of the emergence of the systems approach, he comments on many of the first documented efforts to develop systemic concepts. These relied quite openly on analogies and metaphors drawn across many established intellectual boundaries and their free usage in the early days of the development of systems thought is taken to illustrate, by Lilienfeld, the triteness and baselessness of systemic ideas.

Of these original thinkers we might briefly consider in a more sympathetic tone, the legacy of analogy bequeathed to systems thinkers.

1.2.1.1 Stephen C.Pepper's *'Contextualism and Organicism'*.

Stephen.C.Pepper [Lilienfeld, 1978, p8] developed the concepts of six 'Root Metaphors' in his work[3] to describe the principal metaphysical systems which he proposed as able to comprehend and account for the world of experience. He dismissed two of the six, dogmatism and mysticism, virtually straight away. Mysticism on the grounds that it was simply too private and dogmatism because it relies on 'infallible authorities' which could appear contradictory.

Pepper went on to develop a metaphysics of *'world images'*, each one determined by a root metaphor able to supply a conceptual framework capable of analysis and extrapolation. The four world images Pepper did consider worthy of study were Formism, ['Platonic Realism'], Mechanism [the Newtonian view], Contextualism and Organicism, the latter two images clearly the more powerful contributions to systems thinking.

Contextualism was the name given to the philosophical implications of early pragmatism in which the world is seen as an infinite complex of novelty and

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3 Pepper, S. C., 1970, *World Hypotheses-A Study in Evidence*, University of California Press, Berkeley and Los Angeles, [originally published 1942].

change, order and disorder. From this flux metaphor, investigators select certain contexts to serve as organising gestalts or structures without which the vast array of real world details would either drive the investigator into a state of catatonic shock, or perhaps worse still, remain invisible and meaningless. The organising context, in creating a theme, acts to fuse into a unity the items which in other contexts remain discrete entities. The largely metaphoric concepts of quality and fusion, undoubtedly holistic are the most significant,

> *'Fusion is an agency of qualitative simplification and organisation... some fusion must remain in the quality of an event, otherwise the event would break apart and we would have, not a single event, but two quite unconnected events.'* [Pepper, 1942, p271].

The interest of contextualism is clearly linked to the utility enjoyed when a particular analogy is developed or a metaphor is called forth and explored for a given purpose. The contextualist assertion that all conceptual schemes occur within a universe of applicability and can never grasp the total structure of events possibly accounts for the profusion of analogies used in our everyday thinking, language and learning.

The root metaphor of organicism, also derived by Pepper is more popularly employed in systems thinking. This is more overtly based on conditions of actual analogy, whereas contextualism reflects at a more abstract level, the process and putative 'structure' [model] of analogical reasoning, to which we shall come in due course.

Hence as Lilienfeld [op.cit. p11] stresses, the contextualist uses the category of integrating structures, [contexts], to explain experience but denies to these integrating structures, as Lilienfeld puts it, any 'reality of significance' which we might otherwise take as 'any meaning in its own right'. By contrast, the organicist maintains that the integrating structures surrounding and extending through given events are more numerous, coherent and more real than a contextualist could permit, thus attributing to the fundamentally analogical organising structures, a meaning in their own right.

The organicist, then, views knowledge via the organismic metaphor of *'the organic relatedness of material fact'* [Pepper, op.cit. p310]. This view proposes the overthrow of a given scientific theory, now usually seen in the Kuhnian [Kuhn, 1962] framework of scientific 'revolution'. Incidentally, Kuhn's view does not allow for the accumulation of 'truth' between paradigm shifts, and this does not lead to chaos but rather the replacement of a relatively limited integrating structure by a more comprehensive and accurate form. In Pepper's terms, the materials of experience are not lost over time, but transferred from systems in which they did not belong, to a system in which they do belong. He makes the following remarks on this,

> *'Each level of integration resolves the contradictions of the levels below and so removes the errors that were most serious there; each level brings about improvement in judgement. Each level exhibits more truth through higher integration of the facts'.* [Ibid. p307].

We shall have more to say on concepts of fact, truth and judgement later in the next chapter of the thesis.

1.2.1.2 L.J. Henderson's *'Equilibrium'*.

Lilienfeld also briefly sketches the work of L.J.Henderson [1879-1942], whose influential use of the term system, passion for quantification and as the former disparagingly describes, his *'enthusiastic and somewhat simplistic belief that systems models can adequately encompass the totality of society'*, [Lilienfeld, op.cit. p14] mark him out as a forerunner of systems thought. Henderson based his sociological writings on the work of Pareto, the concept of a system derived by the physicist Josiah Gibbs, and additionally on biochemical and physiological analogies.

In Pepper's terms, Henderson's pragmatic emphasis rendered him a contextualist, in that he believed that concepts used in science had only a provisional value and that man put far too much faith in 'the reasonableness of their ideas and actions', underestimating the pervasiveness of the irrational action and motive. The systemic concept to which Henderson's[4] work principally contributes is that of equilibrium, which he considered as essential in the understanding and study of social processes.

Thus, by implication, the importance of maintaining an 'equilibrium' was directly related to a *'health analogy'*, although in much of his writing Henderson was concerned to play down the significance of its analogical basis, betraying the 'naive scientism' for which he is strongly criticised by Lilienfeld. Hence,

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[4] L.J.Henderson, 1970, *The Social System: Selected Writings*, ed. B.Barber, University of Chicago Press, Chicago, pp136-139.

'In order to fix our ideas let us consider a relatively simple mechanical system. It may seem that we are reasoning from analogy but this is not so. On the contrary, we shall be reasoning logically from premises stated above, because the mathematical formulation necessary to describe this mechanical system would be formally identical with that necessary to describe the analogous social system'. [Ibid, pp136-139].

Flood [1988a, p314] notes that Koehler[5] argued forcefully against an 'equilibrium' theory for organisms. While he recognised that the two principal ideas of machine theories, [the second law of thermodynamics and the law of dynamic direction] are compatible and relevant to a development of a concept of equilibrium based on mechanistic analogy, unless a broader view of these principles is taken the analogy is misleading. We can consider Koehler's three main objections to the analogy, as summarised by Flood,

'(a) no organism is detached from the rest of the world, thus the principles are not directly applicable to living systems-they are not closed;

(b) organisms are not in equilibrium with their immediate environment-at rest many organisms are in an unstable position; and

(c) from the point of view of physics an (presumably) young adult (human, say) contains substantial stores of energy compared to a child-development of life is associated with an increase in such energy.' [Ibid.].

In many ways Koehler's objections to Henderson's equilibrium theory for organisms can be seen to be directed towards the analogical features which informed the basis for inference and hence transference of the the principles concerned. As we have seen, Henderson himself would have probably denied the analogical basis. The criticisms, however, are typical of those applied to analogies; clearly in any analogy, it is as easy, if not easier, to point out ways in which phenomena are not alike, than ways in which they are or might be so. It is the issues underlying the basis for establishing whether matters are or are not comparable that are the interesting ones.

[5] Koehler, W., 1938, *The Place of Values in the World of Facts*, Liverright, pp314-328. [Reprinted in Emery, F.E., 1969]

1.2.1.3 Walter B.Cannon's *'Homeostasis'*.

Henderson's Harvard colleague, Walter B.Cannon, has been described as another early systems thinker but one who, this time, worked more openly with analogies, developing his highly regarded medical knowledge influentially in his book 'The Wisdom of the Body'[6].

Cannon's main systemic theme was *'homeostasis'* and although much of his book was concerned with the physiological description of this concept, Cannon in his conclusion used analogy to argue from the biological to the social,

> *'Are there not general principles of stabilization? May not the devices developed in the organism for preserving steady states illustrate methods which are used, or could be used, elsewhere? ... Might it not be useful to examine other forms of organisation-industrial, domestic or social-in the light of the organisation of the body?'* [op.cit. p305].

Cannon draws a potent analogy and this drives his ideas that insights derived from physiology might prove fruitful for the study of society. Lilienfeld argues that Cannon's analogy develops into an argument against individualism. Lilienfeld, perhaps unwittingly, is ascribing a considerable degree of credibility to the analogy in making his accusation. But Lilienfeld's position is paradoxical and is an indication of a rather superficial understanding of analogy. He takes seriously the potential argument against individualism which he find in Cannon's comparison and in this attributes power intrinsically to the vehicle with which Cannon has developed his ideas. Yet throughout his commentary Lilienfeld negates the value of analogy.

Lilienfeld's objection that the analogy represents an argument against individualism is based on the idea that although individual cells engage in a certain amount of self regulation, in the more complex organisms these cells become fixed in place in specific organs and, in effect, resign many individual problems of survival -getting food, disposing of waste- to the central nervous system. Hence Cannon's physiological analogy is drawn, in sociological terms, with primitive food gatherers, who move about and rely on their immediate environment for sustenance. More complex social environments give rise to divisions of labour and mutual dependencies, giving relative freedom from other functions by developing centralised controlling mechanisms.

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[6] *The Wisdom of the Body'*, 1963, Norton, New York, [originally published 1932].

Cannon is criticised by Lilienfeld for drawing a 'direct' analogy. I have assumed that Lilienfeld is using this term to refer to an analogy which places importance in aspects of similarity only, Lilienfeld himself does not define the characteristics which he takes as identifying a 'direct' analogy. The analogy is between the *'fluid matrix of animal organisms'* and the transportation systems of states, with common carriers, [trucks, trains, boats and so forth] directly analogising with the blood and lymph. Money and credit, in that they facilitate exchange, are taken as integral parts of the *'fluid matrix of society'*.

The conditions of such an analogy are broad and far reaching. The central administrative organs of the 'body politic' are deemed as dealing with 'accumulations of toxins', 'problems of dispersal' and 'medico-social' intelligence. Hence in the notion of 'medicosocial' intelligence, just as it eliminated plagues and epidemics, its ability to solve other problems is instantaneously appealing to the naivete of 'common sense'. But such broad analogies are too loose and therefore as we have already noted, it is possible to name as many, or more dissimilarities, as similarities. Conversely the analogy is also, in some ways, infinitely expandable, the result being that it is very easy to criticise and ridicule.

But I would suggest that criticism of an analogy which is aimed at these points itself dissects an analogy prematurely. In this case I would argue that the criticisms are based on a superficial scan of the features of similarity between the body and society. As will be argued in a later chapter the 'negative' aspects of an analogy [the areas of dissimilarity] or what will be introduced as the 'neutral' aspects [the *'don't know'* areas] can be taken as of as much, or more, value than areas of similarity in deriving the new and creative views of particular features or situations, for which analogies are most used and useful.

1.2.1.4 Ludwig von Bertalanffy's *'Open System'*.

Lastly, in his brief survey of systems thinkers Lilienfeld turns to Ludwig von Bertalanffy whose work is perhaps best known and to whom many systems concepts are attributed via his development of the concept of the *'open system'* which first emerged in his essay, 'The Theory of Open Systems in Physics and Biology'[7]. In the ideas expressed in this influential piece, von Bertalanffy

[7] Printed in Science, 1950.

aimed to establish the foundations of systems thinking on a biological rather than, as Lilienfeld puts it, a philosophical or merely formalistic basis. I would argue that the biological basis was also intrinsically analogical.

The concepts of complexity, hierarchical levels of organisation and emergence led von Bertalanffy to conclude that higher levels of organisation involve new laws not deducible from laws appropriate to lower levels. Not surprisingly, this rather more advanced conceptualisation of organicism caused von Bertalanffy to believe in a fundamental unity of the sciences. Even Lilienfeld felt bound to admit that the *'parallels he finds in these various spheres are more that mere analogies'*, [op.cit.p17] but he also calls for three levels to be clarified;

> *'1. Analogies. These are scientifically worthless-superficial similarities in phenomena that do not correspond to underlying factors or laws operating in them.*
> *2. Logical homologies. Here phenomena differ in causal factors involved but are governed by structurally identical laws- for example the flow of fluids and of heat conduction are expressed by the same law.*
> *3. Explanation in the proper sense, dealing with the appropriate conditions and laws.'* [op.cit. p18].

The role of General Systems theory, according to von Bertalanffy, was to distinguish between analogies and logical homologies [although in the example given above, analogies played an important role in developing the theory] and act as a screen to what Lilienfeld calls 'incorrect' analogies, but how this was to be done was not specified. No wonder notions of General Systems Theory have been treated with some disdain by systems thinkers in recent years. But we will come to more on this in a later chapter.

1.2.2 *Due Cause for Concern.*

In his attack on the systems viewpoint, Lilienfeld takes the use of analogy by these thinkers as the main pivot of his argument. In his chapter on the 'Convergence of Systems Thinking', he cites the following,

> *'Systems thinkers... behave as camp followers of 'science', not of sciences, picking up whatever details serve to illustrate their views... they collect analogies between the phenomena of one field and those of another, (preferring to call them isomorphisms, though the difference is not discernible), the description of which seems to offer them an esthetic delight that is its own justification'.* [op.cit.p191].

It is pertinent at this stage to pursue a brief aside as regards the differences between analogies and isomorphisms of which systems thinkers are, I would suggest, increasingly aware.

1.2.2.1 Analogy and Isomorphism

An analogy, as has been mentioned at the opening of this chapter is mainly used in an act of comparison between two 'things'. The things might be objects, isolated features of phenomena, or situations, with the purpose of indicating their 'resemblances' but this is not an end in itself, although critics, as above, seem to think it is.

Analogy must also have an inherent relationship with theory development for it to be useful. Further, the construction of a model of an analogy in systems between two domains is merely the starting point of a structured method of investigation into a directed area of inquiry, incorporating a variety of procedures and concepts. If critics are to offer useful criticism, surely it should be toward how a structured investigation is to develop to which they should turn their attention and not to bland dismissals of how what they take to be tenuous similarities, first come to be observed.

This is clearly different from the concept of isomorphism which the systemist sees as concerned with the similarities or identities of form. In this way, a map and the countryside it represents, or a number of apparently dissimilar differential equations may be 'isomorphic'. Isomorphic concepts are valuable in science because, for example, it may be possible to work through one feature of a system simply because for one reason or another, it is more convenient to do so and then be sure that a similar process will have a similar effect or yield similar knowledge in the isomorph of that system. No such assumption is possible when we are dealing with an analogy and making this assumption perhaps accounts for poor applications and trivial investigations or appreciations of analogies to date.

There is clearly a fundamental difference between the concepts of analogy and isomorphism in this respect. We are interested in analogy because an analogy may use resemblances to aid knowledge development but the conditions of resemblance must each be carefully scrutinised, and explored according to the purposes of constructing the analogy.

1.2.2.2 Is Systems Thinking an Analogy?

Later in his chapter on 'Systems Theory as Ideology' Lilienfeld goes as far as to suggest that the principal characteristic of systems theory, is *'that it is an analogy, despite the denials of many systems theorists'* [p247].

I will not dispute this last point, but an objection will be raised to Lilienfeld's implication that analogies are themselves specious, although again there are clearly many examples [and not isolated to systems circles] of analogies poorly or superficially applied. This is not sufficient reason, however, to disown the potentials of the concept concerned. Further, as Iberall [1972: Preface, xii] points out, the general theses of the systems approach are often plagued by the criticism that *'only analogies are being proposed, rather than real principles of science'* which as we shall argue in the next chapter are largely ephemeral in any case.

It is apparent that the use and role of analogies have been taken as of fundamental importance to the systems 'movement' by our critics, even if this has not been fully admitted by systems writers. This again is not surprising given the hostile reception the main idea of using analogies seriously to develop knowledge and theory usually stirs up.

But in some ways the difficulties of showing rigour and logic in analogies are symptomatic of other associated problems of theory and practice in systems sciences, again a point for which it is often lambasted. It is in making the transition from the utility of the 'praxis', to then deriving legitimate theoretical concepts which may be subsequently developed for future application which is the most problematic area for systems thinking. This is arguably a very sensitive issue, especially since many of the concepts involved are so intuitively appealing that often practitioners have very high expectations of their practical potentialities.

In this context we must now consider the concept of metaphor and in notions of systemic metaphor, how systems thinkers are hoping to apply it in real problem solving exercises.

1.3 METAPHOR IN SYSTEMS THINKING.

Metaphor is turning up increasingly in systems thinking, both in theoretical terms in the notion of systemic metaphor and particularly in that of 'creative metaphor' for methodological application.

In this chapter I have presented under two different headings, analogy and metaphor in systems thinking respectively. But I would suggest that systems thinkers typically do not make sufficiently clear the theoretical distinction between the two. We have already argued that analogy and metaphor are intuitively appealing and suggested that this makes them readily incorporated in methodological analysis.

In their paper, 'Creativity and Metaphor in Soft Systems Methodology'[8] Davies and Ledington consider the incorporation of metaphor in Checkland's Soft Systems Methodology [1981] and take as one of its strengths that,

> *'... it allows for the separation of the real world of the problem situation for the conceptual or abstract world of the development of systems representations.'* [op.cit. p31].

Davies and Ledington point out that many users of SSM find this stage of the methodology problematic, experiencing difficulties in deriving useful models for effective comparison and debate, although the users are offered the guideline to develop relevant models without a clear indication of how this criterion can be met. Davies and Ledington suggest that in depth exploration of a 'problem situation' may lead to three types of problem.

Firstly the exploration can lead to a proliferation of material making the user feel overwhelmed and this can lead to a *'rather generalised and somewhat neutral model being developed'*, [Ibid]. Also, for problems in which ideological issues are of central importance, there are problems associated with convergent thinking and conservatism in choosing relevant systems.

Secondly, there are situations which the analyst feels that the situation is *'fairly self explanatory'* implying that intuitive problem solving is appropriate. This leads to a lack of variety in the choice of relevant systems and to ideas about

[8] Journal of Applied Systems Analysis, Vol.15, 1988, pp31-35.

certain models being the 'only ways' to describe the problem situation, rendering SSM *'unnecessarily impotent at the comparison stage,'* [op.cit. p32].

A third problem can arise as a result of difficulties in dealing with the abstract nature of a conceptualisation of a problem situation. Davies and Ledington put it this way,

> *'... the problem situation may be expressed as one related to information, or communication, or even human relations, costing, organisation, development, survival, innovation, etc. All these are terms are often uncovered as issues in organisational analysis but they have no concrete substance and so are difficult to express in image form.'* [Ibid].

The crux of the Davies and Ledington paper is that these three problems can be alleviated with the use of metaphor to stimulate creative thinking and link studies concerned with innovation and problem solving to set the scene for the introduction of metaphor in SSM. Their objective is clearly to refresh processes of conceptualisation in developing relevant systems with metaphor. Metaphor is linked by the authors to the area of creative thinking,

> *'... writers looking at problem solving have recommended creativity techniques to break out of convergent thinking and this has been called lateral thinking or the breaking out of 'mind sets'... Such techniques may be useful in encouraging divergent views of relevant systems and the leaving behind of the real world in using SSM.'* [Ibid].

We will see in Chapter Three that a number of commentators on metaphor [Black, 1966, 1979; Boyd, 1979; Lackoff and Johnson, 1980] would take issue with the notion that metaphor, unlike SSM, can be explicitly linked to *'the leaving behind of the real world'*. Many of their arguments are based on the premise that any conceptualisations of a real world and certainly any description of it, are themselves inherently metaphorical and that disaggregation in this respect is nonsensical.

The motivation behind Davies and Ledington's proposition is chiefly pragmatic; *'We believe that the conscious use of metaphor in SSM modelling is worthwhile...'* but I would suggest it demonstrates that the implications of theories and analyses of concepts of metaphor have not been seriously absorbed. This must be seen to jeopardise the credibility of their use of metaphor, making it appear ad hoc and wholly driven by the individual circumstances of the SSM case study. It is thus possible to argue that their position is uncertain and potentially indefensible on theoretical and methodological grounds. For example, we can consider their following assertions,

> *'Another important point with... analogy is that the chosen analogous representation will indicate something of the emotions and dominant ideology being expressed...'* [Ibid].
>
> *'Metaphors are useful because they often allow a more concrete representation of something with an esoteric nature. It is far easier for an object such as a dog, or a car, or a building to be modelled in action than such esoteric notions as organisations management or information. It is easier to expand the analogy into a wily dog slinking around, a sports car speeding around or an ancient building crumbling into ruin. This is one way which metaphor aids modelling.'*
> [op.cit.p33].

With regard to the first passage, Davies and Ledington have already failed to make clear the distinction between analogy and metaphor in their paper and so use the terms interchangeably. They also fail to explore what special characteristics of analogy facilitate a demonstration of *'emotions and dominant ideologies'* beyond describing analogy principally in terms of an exercise in abstraction. Unless we can go further than this, we are forced to ask ourselves why we are bothering to use analogies at all.

Equally, unless we have a model of the characteristics of analogy it makes acknowledging that it is possible to recognise a *'dominant ideology'*, or form appreciations of *'emotions'* contained therein highly dubious. There are no terms of reference unless other features of the analogy or metaphor are exposed. In this analysis I will indirectly consider aspects of 'emotion' and 'dominant ideology' within the more workable framework of the relationship between analogy and rationality.

Of the second passage I would suggest that many theorists would be outraged at the suggestion that metaphors, *'... allow a more concrete representation of something with an esoteric nature'*. The analysis of metaphor has found the distinguishing quality of metaphor to be frustratingly 'non-concrete'; for example, Black [1979] talks of the *'open-endedness* [9]*'*, and Leatherdale [1974], of the *'non-literalness* [10]*'* of metaphor. Atkinson [1984], a systems thinker,

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[9] See Section 3.4.1.
[10] See Section 3.4.1.

thinker, also describes the concept loosely as *'romantic metaphor* [11]'. These concepts clearly do not imply concreteness and the theorists concerned also argue that metaphor is itself nothing if not esoteric. Further, principally because Davies and Ledington have not sufficiently distinguished between analogy and metaphor, I would suggest that the examples given are analogies and not metaphors, on the grounds that images are to be dissected, and not fused, to add insight to the selection and development of relevant systems.

But systems' interest in metaphor does not end at the methodological level. It has been raised also to near epistemological heights and this is evident in that it appears to be increasingly modish to talk now of *'systemic metaphor'* and the *'open systems metaphor'*. Atkinson's [1984] thesis on 'Metaphor and Systemic Praxis' is concerned, with conceptualising the adaptive whole system and, among other things, with *'giving credence to the argument for the existence of such a root metaphor'* and with exploring *'the possibility of alternative systemic metaphors.'* [op cit.p119]. Metaphor is indeed a current issue in systems thinking. But what do we mean by this? What are we saying about the systems viewpoint by adopting this terminology? I do not believe that this issue has been adequately addressed. Equally as I have suggested, there is confusion between concepts of analogy and metaphor.

Flood [1988a] claims, for example, that the 'open system metaphor' is limited in dealing with conflict. I would suggest that it is the rationality within which the open system metaphor is explored in a given situation and not the metaphor itself which we should question. In our discussion of metaphor we will be arguing for a notion of fusion between domains inherent in metaphor and in this fusion, rationalities become invisible with emphasis directed to the immediate insight called forth by the metaphor.

The rationalities involved in the use of metaphor or analogy, I shall argue, might be invisible although this does not mean that they have disappeared altogether; but perhaps this accounts for the misappropriation of the concept metaphor in methodology and arguably its implied incorporation in systems epistemology. In sum, however, I consider the use of the term 'systemic metaphor' to be apt in systems thinking since metaphor is how we see the world and metaphorical and analogical influences are intrinsic to our languages

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11 ATKINSON, C.J., 1984, *Metaphor and Systemic Praxis.* Ph.D.Thesis. Department of Systems, University of Lancaster.

describing it -they are structured analogically and through our eye for resemblances. We must know more about what the potential contributions of analogy and metaphor are to the systems viewpoint.

Later in the thesis I will argue for points of distinction between analogy and metaphor. I will also present the case that in pragmatic concatenation of analogy and metaphor in methodology, the utility for systems modelling is reduced and not enhanced as it might be when the concepts are conceptualised in terms of an analogical/metaphorical continuum. On such a theoretical continuum formal, ideal type models of analogy and metaphor, [the 'Formal Analogical Model' and Atkinson's[12] concept of 'Romantic Metaphor'] conceptualised at each end. In the framework of analogy we will work to establish a clearer understanding of these concepts and explore notions of positive negative and neutral analogy as aspects of investigative rationality.

Overall, there is much speculation in 'systems thinking' about the role of analogy and metaphor in the theories and practices of the systems approach. The controversies that have arisen have prompted a vigorous discourse. This thesis will contribute to that debate.

1.4 CONCLUSION

In this chapter we have noted that systems thinking has undergone a cynical attack for its use of analogy. We have also suggested that the use of analogy has been translated into an interest in metaphor but argued that a clearer distinction must be made between the two concepts and that we must increase our understanding to justify its importance to systems thinking.

We will now move on to consider the context of the systems approach and look at how systems thinkers view science, since it is on the grounds that analogy is unscientific that systems thinkers fight shy of a more explicit analysis of its utility in systems sciences.

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12 Ibid.

CHAPTER TWO

A SYSTEMS THINKER'S VIEW OF SCIENCE

2.1 INTRODUCTION

In the Preface it was stated that analogy is of great interest to systems thinking and in Chapter One we have briefly surveyed the use of analogy and metaphor in systems thinking. In order to reach a position whereby we can begin to reconsider a systems thinker's concern with analogy, we need first to establish an understanding of analogy as a concept. This we initiate by developing a view of the characteristics of science in order to show context in which analogy is normally criticised. We will be concerned to show that feature of factual science are based on concepts which are not absolutes but negotiable within a rational framework.

The objective of the chapter is to show that the characteristics of science are themselves contentious and using that critique we will go on to build a more philosophical view of analogy in Chapter Three.

2.2 THE UBIQUITY OF SCIENCE

In this chapter we will explore a systems thinker's view of science, because science affects all our lives and in the academic community particularly notions of science are ubiquitous if not altogether consistent.

For these reasons a discussion of the characteristics of science is pertinent and necessary. The scientific framework must be described in order to set the tone for the rest of the thesis which will attempt to establish some ideas about analogy and 'reasoning by analogy' as valid and legitimate conceptual tools. It will be seen however that these ideas will not be cast in the mould of traditional scientific reasoning. Further, systems thinking is still referred to in many quarters as 'Systems *Science*' although we differ from traditional scientists of both natural and social persuasions on many fundamental issues.

Thus before we can begin an analysis of analogy as a concept we will begin with a broad discussion of what informs and what is entailed by the scientific way. This exercise will show that for the philosophical and methodological misdemeanours analogy is called to answer, science itself has no alibi.

A familiarity with matters of scientific advance is an intrinsic part of existence in our contemporary society, demonstrable in the tendency to 'take it for granted' that high degrees of innovation will not only improve our future prospects but will also ameliorate the resource excesses of the past. But despite the recognition and acceptance of the notions of 'scientific advance' and 'technological progress' there has been relatively little interest in understanding why and how such 'advances' come about.

Rose and Rose have made a most interesting observation on society's view of science which they propose is normally seen through the embodiment of the actor in the process, namely the scientist,

> *'Before the Second World War, scientists generally appeared in novels, children's stories and so forth as rather endearing, absent-minded figures, possibly mad... but on the whole fairly innocuous, inventors of machines which did easy things in a complex way but which often failed to work. This is far from true today. The scientist, in novel, play, film and comic, is a figure of power, sometimes sinister, sometimes naive and virtuous but if so a helpless tool in the hands of those who wish to misuse him... above all, he is no longer a figure of innocent fun. No one remains innocent about the potential of seemingly highly theoretical research'.* [[1970.xiii].

The cliche of the absent minded or 'mad' professor is no longer a humorous characterisation but now potentially a threat. Arguably, science is now about real people and not an altruistic quest for pure knowledge. In this context we can note Porter's recent remarks,

> *'Serenely soaring in the realms of pure thought, dispassionately devoted to discovery; this is the beau ideal of science often presented to us. But in the real world science doesn't work like that. There is the science of the military, of the oil companies, and the food and drugs multinationals; and lining up against these today, the science of Friends of the Earth and Greenpeace, science for the planet and the people.*
>
> *That's not to prejudge matters, implying one is true and the other all bad faith. But it does mean that science can no longer tower above the fray. Like everyone else, scientists have their passions, polemics and politics.'* [1990[1]].

One wonders on which side science of the universities or science of the state would stand? But we are not really confronting anything new here. Science and technology have been intimately linked to political development ever since

1 Porter, R, 1990, *'From monads to monkeys and Man'*, Book Review, 15/4/90, The Sunday Times.

Galileo who in order to have funds for other research, made a prompt and lucrative sale of the telescope to the Venetian State- as an instrument of war. Galileo, in this context, made his position clear in a letter seeking employment to the Grand Duke of Tuscany,

> *'Great and remarkable things are mine but I can only serve (or rather be put to work by) princes, for it is they who carry on wars, build and defend fortresses, and in their royal diversions make those great expenditures which neither I nor any other private person may.'* [1957[2]].

Perhaps because of a growing awareness of these kinds of links, the tenor, the methods, the broad 'objectives' of science are coming under an increasingly particular scrutiny. In an age already populated with inquisitive 'watch dogs', how long can the 'beau ideal' of science be convincingly sustained, or how far should we go to protect it?

We also have to ask ourselves if the answer to both questions will be influenced by the fact that we live immersed in an artificial environment which is largely the result of scientific research. Our present environment and arguably its problems is the 'successful' result of struggle to overcome *'forces of Nature'*. This was what Bacon saw as the goal of science, necessary in order to bring about *'the relief of man's estate'*, [Easlea, 1973, p248]. Further, as a direct result of our situation, how far is it now possible for us to stand back from the 'real world' and equally, the conceptual environments we have made for ourselves and take stock of science and its products? Some might argue that from our very concern with the links between science, politics and big business, it is getting easier all the time.

Additionally, there are associated problems. Science has given us the confident expectation that -in the enforcement of our dominance over nature- it will provide us with explanations and predictions. In a world riddled with the complexities of visible and invisible interrelationships, the method of science will yield knowledge, truth and successful and objective application of its outputs. Even to a born again systems thinker [who treasured these hopes for the systems approach] it seems a dangerous and arrogant posture.

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2 Drake, S., 1957, *'The Discoveries and Opinions of Galileo'*, Doubleday Anchor, p63.

Rude facts of the matter are emerging all the time. The scientific rationality which sustains the momentum of science is not the only one, and arguably it is one rather inadequately familiar with the real and potential consequences of its explanations, predictions and interventions in systems which have different appreciations, expectations and criteria for acceptability. There is evidence all around us, for example, the far reaching sociological, economic and political consequences of the birth control pill. Or more recently, the Chief Scientist of British Nuclear Fuels who advised that employees worried about the genetic consequences of the levels of radioactivity in their working environments should refrain from having children. It was definitively the scientifically rational answer to their concerns. Most would agree it was also inappropriate and facetious.

Further, the means by which scientists seek knowledge can appear 'out of step' with the needs of the rest of society, an example being the vivisectionist methods of natural science in an increasingly 'green' environment and the activities of the 'scientific' research centres currently generating worrisome waste levels in the delicate ecological system of Antarctica.

But not being able to foresee the immediate or future consequences of one's actions is evidently not merely the prerogative of the scientists. For the scientist however, 'not knowing about knowledge' arguably does have some more immediately apparent benefits, unavailable to others. This is such that the isolated perspective of the immediate moment can make many endeavours of past scientists look 'silly', and by contrast many achievements of the present can look 'miraculous', if the concepts have not been placed in the framework of their full historical perspective. A timely warning on this sensitive issue comes from Bunge,

> *'Those who do not approach science with a philosophical and historical attitude tend to regard every scientific formula as trivial as soon as they learn to handle it and the latest theory as the final, or at least the penultimate one, the last being, of course, one's own forthcoming contribution.'* [1959, p24].

Different accounts questioning 'what science is' and what, therefore we can 'realistically' expect it to do, strongly highlight the increasing importance of the relationship between science and philosophy. Very diverse 'models of science' are arising more and more frequently, partly we shall argue as a result of the increasing popularity of the 'metaphoric' view of science. Additionally, partly because the long standing assumption that there is a single, timeless, correct, 'scientific method' has lost its potency.

It is the significance of other factors which are now of most consuming interest. An historian of science[3] goes as far as to suggest that,

> *'The conventional accounts of great scientific discovery may be myths designed to conceal the true nature of science... Scientists have created the myths surrounding the heroes of discovery because they would like us to believe that knowledge is generated by simply observing what exists in the world. To admit that new theories involve new ways of conceptualising nature is dangerous because it implies that scientists have to think creatively and then persuade others to take their ideas seriously. Cultural, religious and even social images may have shaped the way new theories were constructed and the way in which they were received.'*

New models for thinking about science are developing. Hesse, [1976, p1], gives the example of the hermeneutic account which takes its models from the problems of interpretation of texts, those problems associated with understanding an alien culture and the ideological challenges of Marxist and other, [e.g. Foucault, 1970] potentially radical interpretations of our own society and culture. Bunge [op.cit.] too stresses the need for an historicist emphasis to counter what he proclaims to be a 'conservative' attitude in philosophy, which is unhistorical and evidently a hindrance to the development of theories and modes of theorising from without and within.

The task of the philosophy of science for a long time stood as discovering the criteria of a 'correct method' in science. The way it was to be established was through the analysis of scientific theories as 'scientific', i.e. by criteria internal to the object of study and without regard to external factors, such as the social conditions at the time the theories emerged or the psychology of the individual scientists involved. In this context Bunge suggests that,

> *'... he who forgets the historicity of ideas tends to regard the last scientific theory as the final one and is prone to build scholastic shells around theories with a view to protecting them from heterodoxies.'* [1959, p15].

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3 Bowler, P., 1990, *Darwin: Origin of the Specious*, Article, The Correspondent, 15/4/1990.

One of the most interesting accounts of science in recent years has been Kuhn's [1962] seminal work expounding his view of science as 'paradigmatic'. This viewpoint sees the history of science in terms of 'successive paradigms and revolutions' between which there are few or no rational links or more influentially, accumulations of truth. Masterman [1965] argues that the value of Kuhn's contribution and additionally the surprisingly two-faced reception it has received from philosophers of science and 'research workers in the sciences', lie in the same source. This is the fact that Kuhn 'really looked at actual science' in several fields instead of confining his field of reading to that of the history and philosophy of science, in other words, to one field.

Kuhn's work has, of course, been strikingly influential although perhaps more implicitly than explicitly so. Nevertheless, a recent book on 'The Politics of Evolution' by Desmond [University of Chicago Press, 1990, pp503] is a discussion of evolutionary theories reflecting Kuhn's conceptualisation of pre-science in pre-Darwinian evolutionary science,

> *'Those were heady times for science. Explorers were returning from all corners of the earth with the most bizarre finds, a warm blooded creature with a bill, that lays eggs but suckles its young? No wonder naturalists rowed over the platypus. Geology was baring strata upon strata of fossil fauna whose eons long history boggled the mind. Science was on the move. Theory cancelled theory, scientists fell into factions and accusations of jobbery, rabble rousing and plagiarism filled the medico -scientific press.'* Porter, [Ibid.]

Three competing schools are discussed by Desmond. In revolutionary France, Lamarck had attributed the diversity of species to evolution-from monads to man- a descent which the critics railed against as '*disgusting to religion morals and human dignity alike.*' Paley, an Anglican Churchman and Cuvier, the French naturalist, proposed a universe of design; fish had fins and horses hooves because God had custom built every last bone and tissue and each species had been perfected for its place in its own niche. Lamarck's English disciples scoffed at such ideas and proposed instead that similarity of form across species showed that creatures of higher orders had developed from lower.

Morphological unity though without the concept of evolutionary change was crucial to a third school. From the comparison of fins, wings and arms, many scientists hoped to demonstrate the concept of underlying forms; living creatures arose as variants upon basic archetypes within a single plan.

Desmond examines these three models of nature and attempts to show that these hypotheses were not simply plucked out of thin air but rather each embodied the extra-scientific experiences, aspirations and interests of its advocates. Design Theory was tailor-made for scientific gentry who, as Porter describes them were, *'snugly ensconced in the Royal Society or Oxbridge colleges'*. This scientific sect were convinced that the British constitution was itself providentially designed down to the last pocket borough. Their social vision was inherently hierarchical and they projected their *'all is for the best'* dictum on to their model of nature. Desmond stresses, to those that already believed that all was perfect, what possible need was there for change?

But what of the other players in the scrum? Porter's description cannot be matched,

> *'...the foes, the Lamarckians and fellow travelling "philosophic anatomists?" Petty bourgeois medico-scientific meritocrats, fuming on the scientific margins, battling to oust the oligarchs. Such radicals believed power must spring from the people. They put their faith in change. No wonder they promoted a view of nature itself as "self made" propelled by creative forces from below.'* [Ibid].

Desmond's analysis concludes that both the radicals and reactionaries were bound to lose, *'high politics in the age of Peel saw moderation triumph'*. In science the Royal Society reformed and the medical corporations modernised. In this atmosphere the third model of science, that of *'orderly progression under transcendental law'* flourished. In the dominant view of the 1840's the victorious model of nature seemed eminently British, displaying unity in diversity and continuity in change. This theory had won, suggests Desmond, because its hour had come. Its chief exponent Robert Owen was rewarded with a pension at thirty eight, a house in Richmond and regular dining privileges at Downing Street. It had won, although not forever, since the Darwinian model was already lurking on the horizon.

Thus the programme for this chapter is an analysis of 'science' explored via a 'model of natural science'. Exploration of the general concept of science through a specific model will unfortunately introduce a level of abstraction arguably undesirable at this early stage. Common sense would suggest that for a discussion to be useful, we must be ostensibly concerned to strip the concept ['science'] to its bare bones and not to disguise it with an abstract and ponderous contemplation of merely one facet of an essentially multi-sided figure. But that criticism inherently requires that we retain a concept of the 'beau ideal' and that we cannot do. This intention is, in essence, ultimately irrational and even impossible, because it takes literally what we shall be suggesting has no literal interpretation. Science is a metaphor among metaphors and this is the crux of the matter; in this case, natural science is as good as any of a number of images of science and additionally, because it is more visible, better than most.

Initially speculation on this matter brings us necessarily to a brief exploration of the role of models in and of science and although this bogey will arise again at several stages throughout the thesis, it will be dealt with swiftly in this initial chapter. Masterman [1965] has already noted the potential ambiguity of 'modelling of science by analysis of a model in science', although claims that the ambiguity itself is deliberate and useful. The ambiguities arising from the study of a model of science [natural science] are not of course, particular to the investigation of science. They stir up similarly perplexing issues for other disciplines besides. But arguably these matters are a more familiar concern to any self-conscious examination of human inquiring activity which does not make it any less important outside the social science to which, until Kuhn, such issues were usually confined.

Carr, [1964] in his acclaimed reflections on the theory of history and the role of the historian discusses the point in these terms,

> *'In the social sciences subject and object belong to the same category and interact reciprocally on each other. Human beings are not only the most complex and variable of natural entities but they have to be studied by other human beings not by independent observers of another species... This sets up a relation which is peculiar to history and the social sciences between the observer and what is observed... history is shot through and through with relativity.'* Carr [1964, 1987 edition p70].

But the point is that the situation Carr describes is not peculiar to the human sciences, science too is permeated with greater or lesser degrees of relativism. For this reason we can support Masterman's claim. Hers is an argument which might also be accommodated in systems theories pertaining to levels of recursion and in this context worthy of full quotation,

> *'There are models in science (models of crystal structure, cosmological models, models of conflict, etc.) and there are also models of science (the positivist, the hypothetico-deductivist, etc.). I have sometimes thought of distinguishing the second kind from the first by talking about images [metaphors] of science rather than models. First a model of*

science in history, philosophy or sociology of science is a model in the sciences of history, philosophy or sociology of science, since it is a model of a human and social enterprise, namely science, natural or social. Therefore, the use of models in and models of may help to illuminate each other. Second, the paradigm-critique itself compels us to consider self- reflexive situations in which the critique is turned in on itself. That Kuhn gives an interpretation of science in terms of paradigms itself both creates and is a partial consequence of a paradigm change in the understanding of the history of science. However vertigo inducing this insight may be, it implies that we cannot prematurely seek to escape the potential logical circle by trying to make distinctions which may only be appropriate to a pre-paradigm empiricism.' [op.cit.p3].

Hence we will tentatively and arguably ambiguously, proceed as follows. Following a perusal of the generalised ideas we have on science through the vehicle of 'a model of factual science', a number of points will emerge as being of interest and consequence to the next stage of our discussion. Principally these will concern the negotiabilty of fundamental characteristics of the scientific enterprise and the legitimacy and appropriateness of various methods of science in different domains. In consideration of the 'legitimacy and appropriateness' of different methods across diverse domains I will make first mention of the potentials for analogy to aid theory development and then lastly in this chapter, sketch why I see the systems framework as the most appropriate destination for evolving models and concepts of analogy.

2.3 'SCIENCE' SEEN VIA FACTUAL SCIENCE.

A model of factual science which may also be projected onto a philosophical level will serve as a breaking point into the 'logical circle' to which Masterman in the above quotation refers.

I have already given an indication as to why I am am looking at science through the microcosm of factual science but additionally factual science is held to be characteristic of 'science' because it is the dominant embodiment of the empiricist account in which Hesse [1976, p1] sees science as objective, cumulative, success-oriented and value free. Hesse goes on to argue that from the perspective of the emergent models of science as introduced above, that this conceptualisation is no longer adequate as either an ideal or methodological model of science [particularly in the context of the social sciences].

As we have and will continue to argue, the challenge of the various models of science has overrun the empiricist account quite comprehensively. Of them the Kuhnian [Kuhn 1962] hermeneutic interpretation of science is the most powerful. Its interpretation is in terms of successive paradigms and revolutions between which there are few or no rational links or accumulations of truth. Additionally the notion of successive paradigms which are irreducibly dependent on their own social culture or on the subculture of their own scientific elites has spawned development in spheres beyond empiricist accounts.

For the moment we will look at each of the ascribed attributes of factual science. Science as variously and coterminously objective, cumulative, success-oriented and value free will be examined to establish a critique against which the meta-concept of science itself can be considered: first the model *in* and then the model *of* science will be considered.

2.3.1 *Factual Science is 'Objective'.*

> *'The grand aim of all science is to cover the greatest number of empirical facts by logical deduction from the smallest number of hypotheses or axioms.'* Einstein, Life, January 9, 1950.
>
> *'Science is built of facts the way a house is built of bricks, but an accumulation of facts is no more science than a pile of bricks is a house'.* Henri Poincare, La Science et Hypothese.
>
> *'All fact collectors ... are one story men.'* Oliver Wendell Holmes, The Poet at the Breakfast Table, 1892.

This subsection, in discussing whether factual science is 'objective', will be referring to the idea that science is concerned with establishing the 'facts'. In the fourth subsection I will be concerned with a related issue, that is, the usual dilemma caused by concepts of 'objectivity' and 'subjectivity' in terms of whether science can be described as 'value free'.

The issue of what constitutes a 'fact' is one which in every day life we do not consider very often. We take them as given, they appear to have some kind of embodiment, we assume a reality in which there are facts with some kind of real existence. Emmet [1964, p109] gives an interesting description and it will be used in this brief analysis to illustrate how misleading this assumption might be if we allow it to penetrate too deeply into our ideas about science, or perhaps even in making other 'everyday' decisions and evaluations.

Emmet asks us to imagine we are comparing the lengths of two sticks, 'A' and 'B'. If one is longer than the other, he suggests that it will be generally agreed that the decision regarding which is longer would generally constitute a matter of 'fact'. The decision could even be put to public test by putting 'A' and 'B' next to each other, if we were in any doubt.

But what if the lengths of the two sticks were indistinguishable to the naked eye? Then we would require instruments of some kind to 'measure' accurately between them. Emmet stresses that even though two observers, using their naked eyes, might have come to different decisions about which is the longer of the two sticks, it is likely that they would still agree that the 'actual length' of either of the sticks, is a matter of fact and not opinion, one is longer and one shorter. The measuring instrument can give a reading on this question to the degree of accuracy to which it is capable, even if it is unable to separate them, it is still likely that the matter would be resolved if a more accurate instrument is applied. Their lengths remain a matter of fact, even though those facts are difficult to discover.

Further, what if 'A' and 'B' are of different materials, which enjoy a degree of expansion or contraction depending on the surrounding ambient temperature? Which length would be the 'real length' of either one, or more importantly, when could the act of comparison between them most usefully be recorded; what is the role of 'fact' and 'opinion' now?

Overall, the example illustrates that 'facts' are not independent entities but for all practical purposes reliant on a plethora of other concepts and frameworks. Even in this simple case we introduced ideas of measurement, levels of accuracy in instruments, variations of test conditions and so on.

If we could establish what we count as a 'fact' in factual science, we could then go further in deciding whether science is concerned with establishing the 'facts', what characteristics they have and whether this activity is worthwhile.

The dictionary [4] defines a fact as *'a thing certainly known to have occurred or be true, a datum of experience'*. Confirmed statements about facts are usually called 'empirical data', although this does not give us an indication of the means by which candidate 'facts' are initially selected for potential confirmation. One

4 Concise Oxford Dictionary, 7th Ed., Oxford, 1982.

clue might lie in the methods of the confirming procedure, since Bunge suggests that scientists;

> *'... reject the bulk of perceived facts as being a mass of accidents, they select what they regard as relevant facts, they control and when possible they reproduce facts in experiment.'* Bunge [op.cit.p38].

The rejection, selection and identifiable reproduction of 'facts' suggests that something must precede even the initial stages of possible experimentation. 'Facts' can only take their meanings and relevancies from something beyond their mere 'existence', in a relationship which selects the putative fact from the masses of sense data or conceptual stimuli available to us. Science can, to an extent, describe their apparent presence, but to be useful science must have a relationship with philosophy to explain them; although there is even more to it than that. Peter Bowler, an historian of science, makes some vivid remarks on 'scientific myths' in this context and goes further in suggesting that,

> *'... the familiar images of the 'heroes' are popular manifestations of a deliberately slanted view of history presented by scientists themselves. The stories focus our attention on the discovery of facts as the key to any scientific breakthrough and played down the role of conceptual innovation in the construction of new theories... the true story always involves far more than the straight discovery of facts...'* [op.cit .1990].

In the chapter to follow we will look closely at and argue strongly for the significance of the role of analogy in conceptual innovation and we should not be surprised that the above remarks spring from an historian of science. It arguably results from the output of the continual self-questioning, self-consciousness in inquiry that Carr had summarised for us earlier. In some ways the tradition of, if it can be allowed, pseudo-empiricism in historical analysis must come under an even more stringent and peevish scrutiny than the accumulation of empirical data in science, which can usually acquire legitimacy through repeatability. But we will make another of Carr's points, which I am arguing, present an appropriate and valid critique of the status of fact and inherently the status of objectivity in science.

Carr, as an historian argues persuasively and wittily for a dualist perspective on the nature of fact. In his profession he finds that,

> *'Facts are not really at all like fish on a fishmonger's slab. They are like fish swimming about in a vast and sometimes inaccessible ocean and what the historian catches will depend partly what part of the ocean he chooses to fish in and tackle he chooses to use, the two factors being*

determined by the kind of fish he wants to catch. By and large the historian will get the kind of facts he wants. History means interpretation.' [op.cit . p23].

Carr's view of history is such that he stresses the historian can view the past and achieve understanding only through the eyes of the present. The scientific preoccupation with fact is an attempt to deny this interpretive feature of what must go on 'behind the facts'. Bunge's argument was that theories of the present can look miraculous and those of the past appear silly when we lack an historical perspective. The matter also has a connection with notions on facts because the basis -the rationality- on which facts are selected is not timeless but changes over time. Looking at Carr and Collingwood[5], another historian,

'The historian is of his own age and is bound to it by the conditions of human existence. The very words he uses, words like democracy, empire, war and revolution have current connotations from which he cannot divorce them...' [Ibid p25].

'St Augustine looked at history from the point of view of the Early Christians; Tillamont from that of a seventeenth century Frenchman; Gibbon from that of an eighteenth century Englishman; Mommsen from that of a nineteenth century German. There is no point in asking which is the right point of view [although this is effectively what science asks]. *Each was the only one possible for the man who adopted it..'* [Brackets added.] R.Collingwood, [1946] p xii.

The point of critique is clear, in its task of offering explanations and predictions, science is concerned to establish what is the 'right point of view'. Scientists must *conjecture* what is behind apparently observed 'facts' by inventing concepts for use in hypotheses or systems of hypotheses [theories] although these concepts might have no empirical counterpart, since they do not correspond to percepts.

We can draw finally in this context from Carr's views since he argues most cogently that,

'It does not follow that because a mountain appears to take on different shapes from different angles of vision, it has objectively either no shape at all or an infinity of shape. It does not follow that, because interpretation plays a necessary part in establishing the facts of history and because no existing interpretation is wholly objective, one interpretation is as good as another...' [op.cit. p27].

5 Collingood, R., 1946, *The Idea of History*, p xii.

Clearly these issues do not reduce the utility of 'fact' in a framework ostensibly based on empiricism. From factual science we can consider as examples such empirical concepts of 'mass', 'energy', 'adaptation', 'selection' and from social science those of 'social class' or 'historical trend', which have no perceptual counterpart although this does not seriously threaten the 'factual' connotations of these concepts within their scientific research areas.

But the mere 'gathering of facts' in science cannot be taken as the sole purpose of 'scientific research'. Such data must be capable of being incorporated or interpreted by means of a theory, if it is to be useful for understanding and application. Overall, the nature of what constitutes a scientific 'fact' is to be found in a complex relationship between observation and experiment, perception and theory. Necessarily, data must always be party to an indivisible relationship with an implied theoretical framework, in that empirical data cannot stand alone, nor can it be obtained without reference to some kind of theory and the data itself forms the raw material for the elaboration of theory.

A standard text book on scientific method by two American philosophers describes the method of science as essentially 'circular';

> *'We obtain evidence by appealing to empirical material to what is alleged to be 'fact'; we select, analyse and interpret empirical material on the basis of principles.'* M R Cohen and E Nagel, [1934, p536][6].

Additionally, the methods of Rutherford, the esteemed scientist were described by one of his most distinguished research colleagues, Sir Charles Ellis,

> *'He had a driving urge to know how phenomena worked in the sense in which one could speak of knowing what went on in the kitchen. I do not believe that he searched for an explanation in the classical manner... as long as he knew what was happening he was content.'* [1960[7]].

This perhaps accounts as to why scientists are hesitant to accept 'new facts' unless their 'authenticity' can somehow be evaluated and established with reference to 'what is already known'. The criteria for how we come to establish what we are prepared to classify as 'already known' will be discussed in the following subsection.

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6 M.R.Cohen and E.Nagel 1934, *Introduction to Logic and Scientific Method*, 1934, p536.
7 In Trinity Review, Cambridge, Lent Term, 1960, p14.

Meanwhile, Bunge [op.cit.p80], gives an example of this tendency in suggesting that this is the reason why most scientists distrust reports on 'extra-sensory perception'. So-called 'psi' phenomena contradict the main body of 'what is known' in psychology and physiology and thus in some way, 'do not fit the facts' which are, broadly speaking, taken as collective experience and theory. But a much more relevant and contemporary example is at hand than magical notions of 'ESP'. We can consider a recent article reporting on an academic conference,[8]

> *'Last Easter, in the confused aftermath of the astonishing announcement by Martin Fleischmann and Stanley Pons that they had achieved cold nuclear fusion, one thing seemed clear: their test tube fusion experiment at the University of Utah was so straightforward that the hundreds of other laboratories around the world trying to replicate it were bound to establish whether the claim was valid within a few months... A year later, the "truth" about cold fusion remains elusive.'*

In the article the issue of cold fusion is presented as involving a debate between two camps, that of '*twenty five reputable research groups so far reporting positive evidence of at least one of the three signs of nuclear fusion*' and those '*scientists who have already dismissed cold fusion as complete delusion*'. The debate can be seen as focussing on whether notions of cold fusion 'fit the facts'. The facts are not at all clear, and establishing them is evidently a highly emotive matter. In some ways the article too invokes a religious metaphor in its presentation of the issues,

> *'Early suggestions that the process could be developed quickly into a cheap, clean and unlimited energy source for the next century have proved very over-optimistic. But there is a widespread impression particularly in the UK, that the whole affair was a ghastly scientific mistake and that only Fleischmann, Pons and a small band of true believers are still pursuing cold fusion...'* [Ibid].

Other factors are also at work. For example, Fleischmann and Pons are angry about what they report as the consistently hostile attitude of the science journal Nature whose editorial, preceding the conference had been entitled, "Farewell [not Fond] to cold fusion". Their central problem appears to be why the process is so capricious. Some scientists who claim to have detected signs of cold fusion say that their experiments run for days or weeks without giving off any results and then suddenly give off bursts of neutrons and/or tritium and/or

8 Cookson, C., 1990, *'Sparks still flying over cold fusion'*, Article, Financial Times, 6/4/90.

heat. Not only is the timing completely unpredictable, but there is no obvious correlation between the three signs of fusion. There do not appear to be any clear facts from which the investigators can immodestly wrestle the truth.

In other words researchers are certainly experiencing non-repeatability of evidence in their experiments. This, of course, allows sceptics to dismiss occasional positive results as background effects or random variations. Not only are the true believers faced with practical operational difficulties, but according to the conventional theories of physics, nuclear fusion is impossible in a Fleischmann-Pons cell. Nevertheless all these factors appear to be acting as spurs and not restraints to research; some distinguished physicists are working out new theories to show how cold fusion could occur in the Fleischmann-Pons' "palladium lattice". Scientists in Japan are funded by central government and are working in over forty groups to research the possibilities of cold fusion.

Ayer's [1973] comments on the staus of fact are appropriate here and the following point illustrates its vicious circularity,

> *The factual content of the theory will be identified with the account which is derivable from it of what is actually observable...it is to some extent an arbitrary question what is to count as purely factual.*[op.cit. p33].

Of course, it is a matter of contention, the scale of which is dependent on the source of the objections, as to whether every branch of science as an enterprise can be said to empirically seek what is known as 'factual knowledge'. Science can never examine an 'object', a phenomena, a conceptual relationship merely as it is, but only through the interface that object has with the scientific activity. For example a biologist can sometimes change or destroy the object of his research and the anthropologist, no matter how subtley he arranges to study an alien community, will find [and must assume that] his presence is accommodated in their behaviour.

Logic and mathematics are examples of formal or ideal science and the statements they establish consist of relations among signs, whereas the factual or material sciences refer to events and processes. Hence formal sciences are concerned with describing formal entities and the relations between them and cannot therefore lay claim to establishing 'factual knowledge'.

Nevertheless, the individual fields of physics, chemistry, physiology, economics and so forth, as spheres of scientific activity, come to employ mathematics as a tool for the precise reconstruction and manipulation of the complex relations that it purports to describe among 'facts'. Formal sciences rely on logical and demonstrative methods of exploring their theories, and in this way, mathematics and logic are deductive sciences, their truths are consistent only in terms of a previously admitted system of ideas. In other words they are not 'absolute' but require a logical consistency in order to maintain that a given statement is 'probably true'. Factual science, on the other hand, finds this insufficient in exploring its hypotheses and also uses observation and experiment in techniques of verification and/or falsification.

But for scientific knowledge to be described as 'objective' means, after Bunge [op.cit.], that it;

> *'a) agrees approximately with its objective i.e. that it seeks the attainment of factual truth, and*
> *b) it verifies the adaptation of ideas to facts by resorting to a peculiar commerce with facts (observation and experiment) an intercourse that is both controllable and reproducible.'* [p36].

M'Pherson [1974] has also stressed the apparent objectivity of what he describes as 'the scientific way',

> *'It is important to have a clear view of the nature of science... The accent in science is on the nature of the test by which the validity of the hypothesis is demonstrated. From this validatory basis we have theories that save the appearances very well, until, of course, a new hypothesis arrives which suggests that we see observed phenomena in a new light. But science itself is not only experimental method, it is a particular way of reasoning, the correctness of which can be tested by observation'.* [op.cit. p219.]

M'Pherson's description makes no mention of how potentially controversial hypotheses could arise and in addition, the implication in his conceptualisation, is that correctness is verifiable by empirical observation. It could be suggested that this view is rather myopic since we have noted that not all concepts are also percepts. It would have been helpful too, to receive some guidance as to how the particular scientific 'way of reasoning' supersedes the 'experimental method' science itself has developed, if we are then to test 'correctness' whatever that might be, by observation.

Empirical facts are usually derived by means of experiments involving the deliberate isolation of some factor-the object of the experiment-and its subjection to controlled stimuli of one sort or another. But the 'experimental method' can be taken outside the laboratory and sometimes not include actual experiments at all. Astronomers do not conduct experiments with celestial bodies but astronomy still counts as a scientific activity and indeed is firmly placed in the factual science framework.

Bunge [op.cit, p72] and Chalmers [1978] have both cited the useful example in this context of the discovery of the planet Neptune by Adams and Le Verrier and claim their procedure in this accomplishment was typical of modern science. Adams and Le Verrier did not, it is suggested, perform a single experiment, nor did they even start with 'the facts'. Their problem was to explain 'certain irregularities' found in the motion of the outer planets, although these irregularities were not observable; they were discrepancies between observed and calculated orbits. The 'fact' that they set out to account for was not sense data, nor was it in the first instance observable as sense data, but rather merely a clash between empirical data and consequences deduced from the principles of celestial mechanics.

Scientific knowledge as we have noted also 'rationalizes' experience instead of merely describing it. A scientific account of 'experiences' is not just a list, but a propositional explanation, referential to a whole set of ideas that cannot, in any final sense, be judged for correctness by experience. Explanations are offered in terms of, in some senses, 'negotiable' hypotheses and systems of hypotheses [theories], and not 'facts'.

Science, therefore, is unable to refer to facts as 'objectively existent' things, qualities or relations, since these exist only by means of an inference from experiential and conceptual sources and are thus meaningful, not in terms of observables but only in certain theoretical contexts. For science to be useful it has to go beyond immediate experience to make the necessary jump from observation to theory by winnowing the criteria of 'meaningful' propositions, from the chaff.

2.3.2 *Factual Science is 'Cumulative'.*

> *'Everything has to be taken on trust; truth is only that which is taken to be true. It is the currency of living. There may be nothing behind it,*

but that doesn't matter, so long as it is honoured.' Tom Stoppard, Rosenkrantz and Guildenstern are Dead, 1967, Act 2.

'There can be no final truth in ethics any more than physics until the last man has had his experience and said his say.' William James, The Moral Philosopher and the Moral Life, The Will to Believe, 1896.

The sense in which 'cumulative' is meant here is such that the above would otherwise read, 'Factual Science is formed by successive additions'. This section will briefly but necessarily consider the ascribed role of 'truth' in science and thus also the relationship between science and philosophy.

'Truth' is that usually taken as the property implicitly ascribed to a proposition by belief or assertion of it. There have been many theories on the nature of truth, the most common being the 'correspondence theory of truth' which proposes truth as a correspondence between a proposition and the fact, the situation, the state of affairs which is taken to 'verify' it. The correspondence may, but need not, be regarded as some sort of natural similarity or resemblance between proposition and fact.

Nevertheless, some philosophers holding that all 'awareness of facts' is itself propositional, i.e. that it necessarily involves the assertion of some proposition, maintain that truth is a relation of 'coherence' between propositions. Pragmatists by stringent contrast, define truth in terms of 'vital truths', and the 'satisfactoriness of belief'. Thus assertions are believed or not believed just for the sake of convenience, quite independently of their rational and/or empirical foundation, hence the empirically verifying fulfilment of expectations is only one possible form of this. Another and more common view among the pragmatists is operationalism which takes truth 'contextually' and is happy to define it as, 'the successful working of an idea'.

Popper attacked the instrumentalist position on the grounds of their negation of the importance of 'truth'. Instrumentalists hold that scientific theories are merely useful tools for ordering certain domains of experience. Hence according to the instrumentalists, Mach [1943], Duhem [1954] and Poincare [1958], it is meaningless to ask whether a given theory is true or false since they can only be more or less useful. Theoretical terms are free creations of the human mind and not related to physical entities or reducible to observable terms.

Popper argues that in contrast to the highly critical attitude requisite in a pure scientist, the attitude of the instrumentalist is one of complacency at the success of applications.

This implies that truth has been taken as a real quality rather than a relation, a view which has some plausible connection with analytic propositions whose truth depends not on something external to them, but on the meaning that is intrinsic to them.

Another widespread criterion of truth has been 'self evidence'; that which is acceptable at first sight, that which, in sum, is intuited. The issue of 'intuitive knowledge' has occupied significant philosophers from the ancient, Plato and Aristotle, for example to the modern times of Husserl and Bergson. It refers to that knowledge which is taken as immediate, 'direct', attained without intermediary steps or procedures. It may be knowledge of propositions, of sensory objects or of spiritual objects. Kant goes as far as to describe our acquaintance with sensory objects as 'sensory intuitions'.

Bergson, Husserl and other intuitionists have shared the opinion that essences can be grasped without further ado and additionally, naive rationalism maintains that there are self evident truths and principles which, far from having to pass any test, are the source of every other proposition, 'formal or factual'.

The success of Newtonian physics convinced many physicists that part of 'the truth' had been found. Popper describes their feeling of triumph,

> *'A unique event had happened in the history of thought, one which could never be repeated: the first discovery of the absolute truth about the universe. An age old dream had come true. Mankind had obtained knowledge, real certain indubitable and demonstrable knowledge, not merely... human opinion'*. [Quoted by Easlea, op.cit. p4 [9]].

But for us to accept a piece of knowledge as 'scientific' it is not necessary that it is true, rather the scientist is more concerned with how it has come, or how it is coming, about. Ayer, has also puzzled over the notorious question, 'what is truth?' He suggests in his discussion that,

9 Popper, K., 1963, *Conjectures and Refutations*, Routledge, p93.

'...the purpose of a 'theory of truth' is simply to describe the criteria by which the validity of the various kinds of propositions are determined....all propositions are either empirical or a priori...' [1973, p116].

Ayer points out, by implication, that 'truth' in science is thus reduced to a concern with the qualities of the propositional forms of scientific activity, i.e. all of it. Although usually our concern with aspects of truth apply to a wider field than this, that is, we are most preoccupied with whether our 'beliefs', 'opinions', 'judgements' and 'values' are 'true', those are not the business of factual scientific inquiry.

The disinterested pursuit of truth has had a stormy history and we should examine why we seemed to have expected otherwise. In our discussion on facts it was implied that facts are meaningless unless structured by some other framework than inductivist observation. There is a parallel in the area of scientific 'truth' since clearly scientists are not searching for 'any kind of truth'. For example as Easlea points out,

'... they would scarcely be grateful for the gift of a large collection of empirically true statements about events observed at random.' [op.cit.p4].

Rather scientists are searching for facts and truths which are 'interesting'; those which can lead to predictions and explanations of what they find interesting. Concern with the issue of truth in science is concern with the theory of knowledge, in other words, its epistemology. This would involve coming to a definition of what constitutes knowledge, to determine what sorts of propositions can be known to be true and to explain how they come to be so.

Ayer [1973, p175] has pointed out that the first of these concerns is unimportant stressing that where we draw the line between knowledge and true belief is relatively arbitrary. What is important though, is the way in which we come to justify our belief. Following Ayer's [op.cit.] argument, a number of significant matters arise pertaining to whether we are to take factual science as 'cumulative'.

For example, Ayer suggests how we are to justify a belief in a proposition which is related to an event, that is, if it is implicitly empirical ['factual'] and one we cannot claim to either perceive or remember. If we are asked why we might accept such a proposition, one way would be to adduce another proposition or set of same, with which we believe it to be connected, usually by generalisation. If those who want to see justification also accept the generalisation, then we can end the exercise here. If they do not accept the other 'related' proposition or the generalisations, then these too, must be justified in some way. This will either require other propositions or generalisations or an accordance with evidence, whatever form that might take.

Being acceptable or not, in this way is clearly nothing to do with whether a proposition is 'true' in an abstract metaphysical sense. But significantly Ayer [op.cit.] notes that those propositions which are 'acceptable' are bound to be identical to those which are seriously put forward as being 'true'. Hence,

> *'To admit that they may still not be true is in practical terms to admit that we may still have occasion to reject them. Consequently, the conclusion that a belief has been justified is always subject to revision.'*[op.cit. p176].

Ayer then proceeds with the point as to whether it is sufficient that the propositions which are used for justifying a belief should actually be true, or whether it is also necessary that we have 'good reason' to believe that they are true. The advantage of the first, he suggests is that we then have a definite criterion by which to evaluate our beliefs. The disadvantage is that we could not then decide when we had complied with the criterion.

The danger of the second is evidently that having 'good reason' for the reason by which we must decide, means that we could embark upon infinite regress. To deal with that we would require to formulate 'special rulings' on the validity of judgments based on perceptual material, generalisation, memory, and so forth, prima facie and lose the potential contribution to such a procedure to be made by consistency. In other words we still would not really be tackling issues pertaining to what would constitute evidence for a given statement to be established as *true*.

There is a standard view of science in which it is described as progressing on two levels, the theoretical and the observational. Whereas the second tier makes reference to observed things and processes, theories make use of terms and concepts that are not, as we have noted, directly observable and thus cannot be directly tested. Their function is to explain observational laws that are themselves generalisations of directly observable phenomena.

Nagel argues that it is a special feature of this two tier picture of science that,

> *'... the experimental law even when explained by a theory retains a meaning that can be formulated independently of the theory and it is based on the observable evidence that may enable the law to survive the eventual demise of the theory.'* [1961, p86].

In the falsificationist account of science, to which we shall come in the next section, even when a theory is in disagreement with experimental results and is replaced by one that is in agreement with the data in question, the 'genuine facts' accounted for by the old theory are not discarded. Hence science while it is not cumulative at the theoretical level it is such at the observational. From Easlea,

> *'... through the ever changing flux of scientific theories, there is therefore a solid growth of knowledge which represents progress in empirical understanding.'*[op.cit., p6]

We will, therefore conclude this subsection by noting that an encompassing notion of scientific 'truth' is largely chimerical. Bunge has pointed out that it is not claimed that scientific knowledge, unlike philosophical or technological knowledge be true, rather,

> *'...truthfulness which is a goal, does not characterise scientific knowledge as uniquely as the means, way or method by which scientific research approaches problems and checks proposals of solutions'.* [op.cit.p61].

If we are to take scientific 'truth' as a blind alley in discussing the way in which factual science is said to be cumulative, what can this assertion be referring to? That is, factual science is formed of what kind of additions? By what means are they selected and by what criteria or mechanisms are they incorporated with other features of factual science?

2.3.3 *Factual Science is 'Success Oriented'.*

> *'By their fruits ye shall know them'*. The Bible, Matthew, 20:7.

We have implied so far that science is concerned with establishing 'interesting truths and facts' for purposes of prediction and explanation of what the scientist considers an interesting phenomenon. This broad assertion requires further analysis and begs the questions, why do a particular group of men/women find certain phenomena interesting and what form does their interest take?

We will look at the second question first and suggest that the form scientific interest takes is that of a scientific theory. But what does this entail? Firstly we are aware that theories imply universal statements and therefore they cannot be induced from observed facts because no-one has ever observed the universal statement to ensure that the theoretical assertion is the case. Nor, of course, could it ever be so observed and the universal set may hence be thought of only as an imaginative hypothesis.

In discussing factual science as success-oriented, we will consider two criteria by which factual science is adjudged to be successful, firstly the steady process of empirical confirmation of hypotheses as described by the verification principle and secondly in the latter's inverse, the disconfirming process of falsificationism. There is a point of controversy in these two areas concerning the fundamental issue of what constitutes a scientific or unscientific theory.

The debate led Popper to suggest that it was a mistake to suppose that the essential feature distinguishing a scientific from a non-scientific theory was that the former was 'verifiable'. Verification concerns the establishment of a belief or proposition as true. It is a criterion most popularly used by logical positivists who require a proposition, if it is to be taken as significant, to be verifiable by sense experience [sense datum] or by attention to the meaning of words expressing it or by inference from propositions that are directly verifiable by induction or demonstration.

Ayer describes the verification principle in the following way,

> '*...a sentence is factually significant to any given person if and only if he knows how to verify the proposition which it purports to express, that is, if he knows what observations would lead him under certain conditions to accept it as being true or reject it as being false.*' [1936, p35] .

Ayer then goes on to distinguish between two senses of the term 'verifiable', talking of strong and weak verification. The former refers to a condition in which 'if and only if' the truth of a proposition could be conclusively established in experience. The latter case of weak verification describes when it is possible for experience to render it merely probable. These definitions are not, of course, two genuine alternatives and equally there is an argument which suggests that all empirical propositions are hypotheses which are thus continually subject to the test of further experience.

Therefore it is not logically possible for the truth of any such proposition to be conclusively established in which case there could be no universe of applicability for notions of 'strong verifiability', thus rendering the strong/weak distinction otiose. Ayer proposes that there is a class of statements which may be conclusively verified, the 'basic statements' which refer to the content of a singular experience.

But the vast majority of the propositions and hypotheses occupying factual science are not singular experiences-since the scientific objective is explanation and prediction. So how is the principle of verification as a criterion of 'success' and meaning to be understood?

Again, Ayer comments that it is only by the occurrence of some 'sense content' and consequently by the truth of some observation statement that any statement about a material thing is actually verified-but for any test that we actually carry out to establish sense datum there is an infinitely large number of other tests which might have given the same result. This, he suggests,

> '*...means that there is never any set of observation statements of which it can truly be said that precisely they are entailed by any given statement about a material thing.*'[1936, p16].

Hence even from the logical-positivist viewpoint, to which the principle of verification is most commonly attributed, a number of problems arise in clearly establishing what verification requires, for procedural application in factual science. The problems concern philosophical difficulties with experiential propositions [singular statements] being universally taken as observation-statements recording an 'actual or possible observation'. This is too liberal a criterion for verifiability since Ayer stresses, 'it holds good for any piece of nonsense one cared to put'. Further and perhaps most significantly, the assumed relationship between principles of verification and sense datum overlooks the fact that most empirical statements are to some degree vague.

Popper suggests that on the contrary we can identify scientific theories because they are capable of empirical falsification in which every genuine test of a theory is an attempt to falsify it. We are, in the sense of the establishing the 'success orientation' of science involved in creeping up on truth from behind. The scientific method of research then is, as Popper suggests,

> *'... not to defend [our present conjectures] in order to prove how right we were. On the contrary we try to overthrow them. Using all the weapons of our logical mathematical and technical armoury we try to prove that our expectations were false... in order to put forward in their stead new unjustified and unjustifiable anticipations, new rash and premature prejudices.*[10]*'* [Easlea, op.cit.p4] .

We can also perversely explore the 'success-oriented' description of science in terms of falsificationism. The falsificationist freely admits that observation, [accumulation of fact] is guided and pre-supposed by theory and can therefore abandon any claims implying that theories can be established as true or probably true in the light of observational data. While it is not suggested that any putative theory is true, the falsificationist prefers the working assumption that a given theory is the 'best available' or the 'best yet'.

But what is to happen when there is an inconsistency between two sets of hypotheses, one theoretical, one observational? Which of the sets is it 'rational' to reject? The matter cannot be arbitrary and therefore we must discuss the putative rules which Popper submits to this issue.

The matter of selection between the two sets of hypotheses throws Popper's demarcation of scientific and non-scientific into some difficulty. If, for example, observational statements are to any degree theory-dependent, then theories are not compared against 'neutral experience' but against interpreted data. Therefore the criteria by which theories are falsified are not as straightforward as Popper might hope. When fact and theory disagree, it is the theory of experiment which needs revision.

In this context, Popper argues that precisely because experiments are open to interpretation, the danger is that it is always possible and arguably 'easier' to verify a theory rather than to strive to falsify it. Therefore Popper suggests that we,

> *'... adopt a highly critical attitude towards our theories if we do not wish to argue in circles'*.[Easlea, op.cit. p8[11]].

10 Popper, K., 1934, *The Logic of Scientific Discovery*, Harper, 1965 pp279-280.

11 Popper, K., 1934, *The Logic Of Scientific Discovery*, Harper, 1965, p107.

The argument develops such that when there is a clash between low level observational data and high level theory, it is the theory which must be rejected. Easlea notes that in this conceptualisation,

> *'There is no other way out. A scientist must be a man who seeks and welcomes the opportunity to confess the error of his ways. Dogma and commitment must play no part in the scientific method.'* [op.cit. p9].

The arbitrariness of Popper's methodological rule is evident and the claim that science is a rational enterprise must become suspect. Popper stresses that it is our way of choosing between theories on a certain problem situation that marks science as rational. He expands, after Easlea, ... we are told... that of the two theories T1 and T2, the theory T2, is to be preferred to the theory T1, which has failed the severe tests that theory T2 had passed. The criteria for this choice is outlined by Popper on the basis that,

> *'As long as there are no revolutionary changes in our background knowledge the relative appraisal of our two theories, T1 and T2 will remain stable.'* [1963, p235]

The assumption that background knowledge will not change is arbitrary and for rationality to be maintained he makes an arguably most unscientific suggestion,

> *'... almost all the vast amount of background knowledge will for practical reasons, necessarily remain unquestioned; and the misguided attempt to question it all -that is to say- to start from scratch, can easily lead to the breakdown of critical debate.'* [Ibid.]

The requirement is clearly set for a reductionist viewpoint, and his hence rather unpalatable to the systems thinker,

> *'Thus all criticism is piecemeal, we should stick to our problem and try to solve no more than one problem at a time'.* [Ibid. p 238].

In the first subsection I tried to briefly survey the notion of a 'fact' under the heading 'Factual Science is Objective'. I remarked at that stage that a pure interest in the facts as a manifestation of objectivity was merely one facet of the issue. I will now return to that theme, this time to consider a related area, principally, a discussion of the assertion that factual science is 'value free'.

2.3.4 *Factual Science is 'Value-Free'*

This assertion is initially rather ambiguous. Does it imply, for example, that factual science can itself exist, in a 'disembodied, abstracted, conceptualised' way, that parallels Benjamin Constant's (1767-1834) view of art, derived from Kant's work on aesthetics:

> *'Art for arts sake and with no purpose: any purpose perverts art. But art achieves a purpose which is not its own'.* Journal Intime, 11 February, 1804.

This conceptualisation might easily confirm the fears on the isolationist nature of science expressed at the beginning of the chapter, but will not be pursued as the most interesting issue here. Rather, the assertion that factual science is 'value free' is more likely to refer to the abstract and indeed, 'disembodied' character of factual science that is ascribed to it as a process. Both possible interpretations carry a strongly metaphysical implication.

But the fundamental suggestion is that any individual who adopts such a process will in no way whatsoever influence its outcome. The position describes what has been otherwise named [Mattesich, 1978, p19] as the 'value neutrality of science'. According to this prescription, any discipline aspiring to the status of science and in this context particularly factual science must *'guard its boundaries against the intrusion of value judgements'.*

Value judgements are of interest, because they arise as a result of a relationship between a given object, situation or 'fact' and a person or the senses of that person. Emmet [op.cit. p143] points out that it is contrary to common sense and indeed to experience to treat the effects of this relationship as necessarily the same among individuals or in such a way as to suggest that there is some sense in which they 'ought' always to be the same [i.e. ethical theories]. Value judgements reveal a private side to factual science which is normally viewed through the lens of public fact.

We implicitly return here to the first of two questions asked in the preceding section on why a particular group of men/women find certain phenomena interesting. In this context is it really sensible, then, to neglect the influences of and on the individuals who conduct the scientific process? Surely the influences of these individuals, that is, certain inherent philosophical principles, [for example, their views on the nature of reality, their criteria for establishing knowledge], must take part in a tacit way in scientific research. Perhaps this neglect has been a serious contributor to the problem mentioned at the outset, namely that we think of and treat science as either somehow already separated, or conceptually separable from, the ways of the world in a broader sense.

In other words, no less important to the spectacular achievements of applied 'factual science' are the consequences: overarmament, overpopulation, potential energy and resource shortage, unemployment, pollution of mind and environment. Perhaps we should switch our attention to uncovering the value judgements and points of emphasis inherent in the development of the 'whole hierarchy', as Mattesich puts it when suggesting,

> '*...we cannot escape the fact that value judgements, so obvious in politics, business, law and other practical areas are embedded in a hierarchy of norms penetrating right down to the applied sciences.*' [1978, p6].

Value judgements then must be seen to have some consequence to factual science, if only, as Bunge puts it,

> '*...because investigation is done by human beings who are unwilling* [or unable] *to take off their world views when undertaking scientific research.*' [op.cit., p13]. Brackets added.

Bunge, at a later stage, goes on to discuss the influence of the individual scientist on the process of science and implies that the most interesting point about the latter is its positivistic concern to develop methods and techniques which will enable opinions to be formed which are justifiable [forming objective knowledge], or vice versa. An example is readily available in the assumed neutrality of the manner by which scientists select 'relevant facts' above others, as we discussed in the first subsection.

Bunge describes the way in which the notion of a value judgement in its broadest sense of a 'subjective' decision is viewed within factual science:

> '*Ask a scientist whether he thinks he is entitled to endorse a statement in the field of science just because he likes it, or because he regards it as unassailable dogma, or because it seems to him to be self evident, or because he finds it convenient. He will probably answer in this guise: None of these would-be truth criteria warrants objectivity and objective knowledge is the target of science. That which is accepted only because*

of taste, authority, alleged self evidence (habit), or convenience is just belief or opinion but not scientific knowledge.' [op.cit., p60]

We have already seen that notions of objective knowledge or truth criteria are not as straightforward as they might first appear and it is likely that inspection of the ideas surrounding the notion of 'value judgement' will show it too, to be equally spurious.

Ayer, [1936, p28] pointed out that a usual assumption made on this subject in factual science is that questions of value are 'really' questions of 'fact'. It is not altogether surprising that he should put forward this view, since it is consistent with his positivistic philosophical stance and therefore continues to allow the process of factual science to stand aloof from what are in some senses ethical matters. It clearly assumes that notions of 'good and bad, right and wrong' are identifiable or at least vaguely recognisable in a given situation implying that they are in some sense 'absolute' standards. Ayer is also obliged to note in this context that,

> *'..."statements of value" are genuine synthetic propositions, but they cannot with any show of justice be represented as hypotheses, which are used to predict the course of our sensation: and, accordingly, that the existence of ethics and aesthetics as branches of speculative knowledge presents an insuperable objection to our radical empiricist thesis.'*[op.cit.p137].

The view that questions of value are reducible to questions of fact results in the assumption by factual science that value judgements, should it be necessary, are also amenable after a fashion to the empirical method. A.J.Ayer [op.cit.] describes his view that ethical judgements are very often a *factual* classification of an action belonging to some class of actions by which a certain moral attitude is habitually aroused. He goes on to give an example to demonstrate the assertion that an ethical value judgement is actually a matter of fact,

> *'Thus, a man who is a convinced utilitarian may simply mean by calling an action right that it tends to promote, or more probably that it is the sort of action that tends to promote, the general happiness: and in that case the validity of his statement becomes an empirical matter of fact. Similarly a man who bases his ethical upon his religious views may actually mean by calling an action right or wrong, that it is the sort of action which is enjoined or forbidden by some ecclesiastical authority and this also may be empirically verified.'* [Ibid. p27].

The apparent illegitimacy of value judgements in the process of factual science implies, as I have mentioned, that it is concerned with establishing methods by which to render value judgments justifiable, or in some senses unnecessary. It implies that the scientific procedure will be able to derive methods to decide questions [or negate them] on the subject of inquiry. For example, we can consider the task of answering the everyday question, 'which of these two chairs is the more comfortable?' To answer this, science, by establishing 'facts' pertaining to the majority [by sample testing], and then statistical generalisation, would hypothesise a prediction as to which chair, under given circumstances, a majority of a sample group would find most comfortable [or stretching a point, *could tolerate for longer*], for a given purpose.

An important distinction in this discussion of factual science should be made on two main interpretations of what constitutes an 'opinion'. Firstly we must distinguish between being asked for an opinion relating to matters of taste and personal preference and secondly opinions which call for us to exercise judgement, which may or may not involve some kind of testing or learning procedure.

In the first sense of 'opinion' we are implying an act of choice and choice itself involves a comparison of, sometimes, a highly generalised character. Hence the answer to *'which chair is more comfortable?'* might be in the first case, *'it all depends'* but in the second case, in forming a judgement on the issue, it is normally taken to concern a *'matter of fact'* open to confirmation or contradiction.

In sum, the problem of the role of value judgements in factual science is fundamental to all epistemological investigation. It is an unresolved and highly controversial issue. Many commentators have insisted and it is probable that they will continue to do so, that every branch of science must be 'neutral' and therefore free of values.

Further, the assumption is then, of course, that this issue is more pertinent in the social rather than what has here been described as factual science [although it might be otherwise known as 'natural' or 'applied science']. The noted sociologist Max Weber strongly defended this view, introducing the idea of *Wertfreiheit,* [value freedom] as the exclusion of value words and value judgements from the discussion of human and social affairs. The exclusion was intended to remove the moral and political influences of the sociologists from their view of their study, with the aim of minimizing possibilities of disagreement and eliminating from scientific work controversial and disputable material.

Clearly, for social scientist the idea represents the influence of the factual science framework in this area, paradoxically itself reflecting a methodological value judgement in that to count as scientific, social science can accept only those assertions which could be established as true or at least 'reasonable', and curiously without disagreement even though we are no closer to establishing what this might mean.

2.4 THE NATURE OF SCIENCE: A Discussion

We began this chapter by stressing that it is important to note the historicity of ideas lest we be accused, among other things, of claiming too much for present findings. Before we are tempted to draw a curtain on the discussion of the 'model of science' discussed so far, some contextual consideration is clearly appropriate.

From the above discussion it is apparent that many of the matters which preoccupied ancient philosophers and scientists of other paradigms have been 'excommunicated' from our present ideas on how to conduct a 'rational' investigation of the world, or worlds around us. It has been noted that by the end of the fifteenth century man no longer viewed himself as a pious spectator of 'God's works' but an active participant in nature's processes. Man had come to a conclusion that he was,

> *'... a creature who could, by gaining knowledge that would give him power over nature... make what he willed... Nature was to be mastered by new science...'* [Easlea, op.cit. p 253.]

The Aristotelian image of science and nature had clearly not resulted in 'gaining power' over it and this became a central aim in the development of science. Easlea suggests that in Bacon's eyes science was the opportunity for man to restore his dominion over nature that although originally granted by 'Divine Bequest' had been lost at the Fall. Scientific knowledge was hence redefined in this style. There was evidently a transition from the belief that,

> *'..Nature which not only inspires but also oppresses men is not so much to be dominated and exploited... as understood and interacted*

with, so that her oppressive ways can be increasingly made to yield to those of her ways that nourish life.' [Ibid. p 252]

From the point of view of the 'new science', causal knowledge was deemed to be all that was required to enforce a power over Nature and speculation on the purposes and goals of phenomena, as in the Aristotelian, teleological school was now considered irrelevant.

The quest for causal knowledge was readily embraced by the new thinkers. Descartes asserted that,

> *'It is possible to obtain knowledge highly useful in life... we can have a practical philosophy by means of which knowing the force and actions of fire, water, air and the stars and heavens... we may in the same fashion employ them in all the uses for which they are suited, thus rendering ourselves masters and possessors of nature.'* [Easlea, op.cit. p 253[12]]

It was clearly a view which could be used to justify any intervention in nature since no longer was nature taken to have organic characteristics of sensitivity, emotion, intelligence that up to the Renaissance, many thinkers had developed. In fact the conceptualisation of the world had become the very opposite of the spiritual, magical metaphor above. The overriding conceptualisation of nature was that of 'matter in motion' and nothing more. Obviously as Easlea points out, [op.cit.p256] by implication no moral self-examination is required by those who might wish to turn such a conceptualisation to their advantage.

Further, even notions of tastes, colours, odours did not belong to the new conception of science. Galileo insisted that they were no more than names residing only in the consciousness and hence if the living creature were removed all these qualities would be wiped away and annihilated and there would be nothing left but that which was required in the mechanistic view... shapes, numbers, slow or rapid movement. The reductionist conclusion inevitably followed and Descartes was able to put in a nutshell a faith which has dominated many fields of science since. Thus,

> *'If anyone could know perfectly what are the small parts composing all bodies he would know perfectly the whole of nature.'* [Vartanian A., 1953, p47.]

12 From Descartes' *Discourse on Method*, Part 6.

Further, the strikingly influential 'Clockwork Metaphor' [punning aside] clearly began at this moment. Kepler argued that,

> *'At one time I believed that the motor causes of the planets was a soul... The aim I have set myself here is to affirm that the machine of the universe is not similar to a kind of divine animated being but similar to a clock.'* [Rossi, P., 1970, p141.]

The machine analogy was so successful in permeating the conceptions of the world that Descartes argued not only that animals had no souls but that they were fundamentally no different from complex machines. Live animals were promptly nailed to boards and opened up to view their blood circulations. Vivisection began at that moment since as Easlea puts it, *'mere matter in motion has no rights'*, [op.cit.].

There was, of course, some resistance to the mechanistic view, but evidence and history suggest that the Aristotelian paradigm had been struck a blow from which it never recovered and not only did it appear intellectually *inadequate* but also *socially unacceptable*. Bacon's verdict on Aristotelian thought was damning,

> *'Such teachings, if they be justly appraised, will be found to tend to nothing less than a wicked effort to curtail human power over nature and to produce a deliberate and artificial desperation.'* [Quoted by C. Hill[13]].

Kepler too attacked the views of his mystical contemporary Dr Robert Fludd[14]. Easlea notes that, to the outdated plea that,

> *'... English Universities should turn to the teachings of the Renaissance and that profoundly learned man Dr Fludd, ...the Oxford Astronomer Seth Ward angrily replied that it would be utter foolishness to dwindle after the windy impostures of Magick* [sic] *and Astrology'.* [op.cit. p256].

Easlea also points out an interesting parallel in another aspect of society reflecting the influence of the change from an organismic/animistic to a mechanistic world view.

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13 Hill, C., 1965, *The Intellectual Origins of the English Revolution*, Oxford, p90.

14 Pauli, W., 1955, The Influence of Archetypal Ideas on the Scientific Theories of Kepler, in C.G.Jung and W.Pauli, eds., *The Interpretation of Nature and the Psyche*, Routledge.

> *'We find that seven years before capital punishment was abolished in England for the crime of witchcraft (a social activity credible only with a magical world view) capital punishment had been introduced for the breaking of machines'.* [Ibid.].

In the discussion of science as objective, cumulative, success oriented and value free, all that we have so far been able to establish is that the characteristics of factual science have largely appeared to be what we might call 'negotiable', within an apparently 'rational' framework. We took four broad characteristics of what we have called 'factual', otherwise, 'hard', 'material' and 'natural' science and surveyed each in turn.

The claim that factual science was 'objective' was examined with an interest in its concern with 'facts'. It soon became clear that the 'bare facts' of empiricism are not there for the taking and the relationship between science and the 'facts' is implicitly theoretical, with that theory deriving from a framework which 'goes behind the facts'.

We are obliged, therefore to make some comments on the development of theory in science. Bunge notes this requirement and suggests that the characteristics of theories are not what we might call 'unilateral'. We have noted that Bunge stressed,

> *'We have a division of the sciences, viz., into formal or (ideal) or factual (material). This preliminary branding takes care of the object, or theme of the respective disciplines; it also accounts for the difference in kind between the statements which the formal and the factual sciences try to establish... whereas formal statements consist in relations among signs, the statements of the factual sciences refer in most cases to extra scientific entities, to events and processes...*
>
> *... In other words, the factual sciences have to look at things and whenever possible they have to change them deliberately in attempting to ascertain to what degree their hypotheses fit the facts.'* [op.cit.p31].

In the formal sciences of relation, theories are wholly deductive and experience must be seen to play merely a suggestive part in developing the points of departure, that is the axioms. Thus it is important to note in this respect that experience is far from being the 'judge' of theories even in the factual sciences where hypotheses are confirmed or refuted.

Theories are checked with facts and with other theories. Theories themselves are enacted within a framework of scientific rationality since they are not, as

Bunge [op.cit.p78] has stressed built 'ex nihilo', but on certain bases which support them before and after any test.

Notably Bunge goes on in this vein to point out,

> *'A hypothesis with a factual content is not only sustained by empirical confirmation of a certain number of its particular consequences, e.g. prediction. Scientific hypotheses are, or tend to become embodied in theories; and theories are all related among each other; the sum total of them constitute the intellectual culture.'* [op.cit.p78].

The nature of the scientific culture/rationality must therefore effect and be affected by the development of theories and as we shall argue, corresponding models. This latter relationship will be discussed further in the following chapter. Nevertheless, in this context we have not yet established what scientists ascribe as a 'theory'. Hence Bhaskar asks,

> *'... can theory do what experience and deducibility fail to do, i.e. provide a rational ground for our ascriptions of natural necessity.'*'[1975, p155].

Bhaskar goes on to suggest that the answer clearly depends on the extent to which the former contains components irreducible to the latter. He also notes that in solving this question, the onus is on the philosopher to locate the 'surplus element', or the emergent element as systems thinkers might put it, in the systematic organisation of our knowledge or the capacity of theory to explain many different laws or predict new facts.

In Achinstein's 'Concepts of Science', [1968] he examines the various criteria of what constitutes a meaningful theory according to the axiomatic or hypothetico-deductive account, that is an account which requires theories to conform to certain logical criteria, i.e., firstly assuming the case antithetical to Bhaskar's suggestion.

In this tone, Achinstein suggests,

> *'On the axiomatic account a theory is no more and no less than a set of sentences ['axioms'] stated in a specified vocabulary, together with all the consequences... plus the proofs of these consequences. In short, a theory is a hypothetico-deductive system.'* [op.cit., p129].

Significantly, Achinstein's examination of the various criteria given to ensure that theories of this kind are 'meaningful' leads him to propose that the hypothetico-deductive definition of theory is not sufficient to explain the role of theories in science. He suggests, in fact that it is related to the positivist rationality, holding that terms interpreted as referring to something unobservable are either unintelligible or scientifically meaningless. Achinstein has the following to say on this matter,

> *'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 finally seen that almost any term in any theory becomes meaningful. Finally, the explicated concept of meaning does not allow a more adequate re-construction of theories, since it renders the task of theory construction a trivial one.'* [op.cit., p118].

Interestingly, Achinstein goes on to claim that theories can be given significance by using terms which are already available and are used in connection with observation, or by attributing to the unobserved items properties which are not identical with those attributed to observable ones, but which are similar in many possible respects. In other words Achinstein is strongly implying an analogical feature to this conceptualisation of theory and this is a matter to which we will be referring throughout the rest of the thesis. Further by his own criterion for what constitutes a theory [which clearly opposes that which he described as the hypothetico-deductive account], Achinstein suggests firstly that it is normally believed that the theory,

> *'... provides or will eventually provide some, (or a better) understanding of something and that this will be one of the main functions [of 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, analysing something into simpler, more familiar or more integrated components.'*[op.cit.p 123].

Secondly that a theory consists of propositions which purport to assert 'what is the case'.

Overall, Achinstein's account of theories rejects a purely logical criterion for the formulation of theories and therefore one might assume that the logical structure is seen to be determined or selected out of the logical possibilities by extra-logical criteria. Some writers, for example Leatherdale [1974, p70] suggest a stronger position in that it is specifically the role of models which functions in this way. This is an indication of a point to which we shall come in due course. For the moment, for illustration, we note that Leatherdale quotes Hutten in this respect,

> '... *the model gives the syntactic rules for using the expressions by which we described the process. The model is the better, the more completely it provides the syntax of the new theory...*'[1954, p300].

It was clear in our examination that blunt concern with the observation and cataloguing of facts is an insufficient explanation of 'the scientific way', for as we saw from the discussion of M'Pherson's claim, it does not account for how hypotheses might arise. We shall also be arguing that 'discovery' has much more to do with the roles of models and analogy than identification of facts in science, since, as Bunge describes,

> '... *that the discoveries of science are not the automatic result if a conscientious routine application of certain infallible rules... that discovery is a tortuous process in which the "flash" occurs only after a period of preparation and worry and moreover it is not the final word, but a hypothesis that has to be checked.*' [op.cit., p17].

At the following stage we considered the proposal that factual science was cumulative, that is it is formed by successive additions of 'truthful' assertions of scientific knowledge. We discussed truth as a proposition concerning facts and soon found a variety of accounts of truth, none of which were specifically attributable to factual science. Further, it was suggested that since the distinction between truth and belief was to some degree arbitrary, the epistemological consequences for the nature of scientific knowledge again appeared to reflect a fundamental negotiability. More important than truthfulness of the accumulation of scientific knowledge became the way it is collected, and thus the 'good reason' for believing it or receiving it as 'truth', came to reside wholly in the self fuelling rationality ofthe scientific methodology itself.

Thirdly we looked at the assertion that 'science is success oriented' accepting the implied ambiguity of acknowledging verificationism and falsificationism as twin faces of establishing scientific credulity. Lastly the points on factual science as 'value free' brought together the groundlessness of the other characteristics in raising the issues that facts are only meaningful when viewed through a 'framework'; truths and beliefs may and do merge indistinguishably highlighting the necessity to evaluate how 'facts' come to be derived; and the success orientation depends on the goals of investigation established by the perspective of the framework. In this context Bhaskar has suggested that the reliance on theory and by implication their inherent explication by models carries with its own problems in this context. This is such that the,

> *'... criterion is clearly capable of selecting a theory within a given metaphysical schema such as that provided by the classical mechanical world view. But it is not capable of judging between different schemas, when it is precisely the nature or limits... that is in question. To take an obvious example: Aristotelian and Galilean dynamics are in conflict over whether when a stone falls to the earth, the earth should be conceived as fixed (Aristotle, Ptolemy and Tycho Brahe) or as moving (Copernicus, Giordano Bruno, Kepler and Galileo). Now try as you may, there is no neutral way of conceiving the falling of the stone'.* [1975, p155].

Thus any references to the theoretical framework must be clarified with a consideration of the 'scientific rationality'. The interpretation of data with reference to a rationality consists of behaviour that satisfies at least two conditions, consistency and the satisfaction of certain aims. To be 'consistent' can be interpreted in a number of ways, either that in certain circumstances a specific action must always be taken: in other forms it implies a logical transitivity. The second condition requires that decisions are made purely on the basis of achieving certain aims, the success orientation.

Thus the fundamental tenets of science appear to rely on conceptions of facts, objectivity and rationality. Easlea notes a description of science as emphasising,

> *'The fundamental feature of science is its idea of objectivity, an ideal that subjects all scientific statements to the test of impartial criteria, recognising no authority of persons in the realm of cognition.'* [Easlea, op.cit. p1[15]] .

The scientist then, depends on impartial and independent criteria to establish beliefs. Further, it can be argued, in the manner of Easlea, that it is a defining characteristic of a 'rational' man that he finds objective ways to distinguish valid

15 Scheffler, I., 1967, *Science and Subjectivity*, Bobbs-Merill, p1.

from invalid beliefs, and it follows that science represents the distinctive features of the rational quest.

But what is it to be rational? We will be looking at this in more detail in a later chapter. Popper explains it as an attitude that seeks to solve as many problems as possible by an 'appeal to reason', that is, to clear thought and experience rather than appeal to emotion and passion. The latter case is represented by the rather unfairly named 'irrationalists' who insist that emotions and passions rather than reason are the mainspring of human action. Popper defends the objective view with the rather the curious point that while the irrationalist's point might be so,

> *'... we should do what we can to remedy it and should try to make reason play as large a part as it possibly can... irrational emphasis upon emotion and passion leads ultimately to what I can only describe as crime.'* [Easlea, op.cit.p3[16]].

Popper sees science as of great significance in the promotion and strengthening of rationality. We will consider a reversal of Popper's emphasis and discuss the juxtaposition of rationalities in the model of analogy. The most welcome feature in the promotion of a rationality Popper sees as being the reliance of the test on impartial and independent criteria and this means that concepts of subjectivity, ethical and aesthetical content are routed from the area of inquiry.

We have discussed the view that rationality is an essential feature of that kind of knowledge which is held to be scientific. The scientific rationality itself is accepted on the basis that it is made of concepts, judgements and reasoning and not of sensations, images and patterns of behaviour. Evidently, in terms of the 'value free' and 'factual' attributes characterising factual science, the real bone of contention is in the issue of an ascribed 'objectivity'. Of course, the scientist 'perceives', 'visualises models' and makes operations and both the starting and end points are ideas. The influence of philosophical frameworks in this respect is tacit, in that scientific research is conducted by human beings who must have their various weltanschauungen.

'Rationality' also implies that ideas might be combined in accordance with some set of logical rules to produce new ideas by deductive inference. This is the

16 Popper, K., 1966, *The Open Society and Its Enemies*, Routledge, 5th Edition, Volume 2, Hegel and Marx, pp225, 233, 234, & 236.

epistemological importance of the scientific process. But significantly, any ideas developed in this way are not new from a logical point of view since, they are entailed by the premises of the deduction, they are dependent on the conditions of their rational framework. The situation begs the question as to whether the rationality bound process of science can be the most effective route to creative thought for problem solvers. Yet ideas derived in this way can still be thought of as epistemologically new, in so far as they express knowledge which was not in evidence prior to the deduction.

Hence for a given concept, at a given moment, coherence with a previously accepted systems of ideas is necessary but not sufficient with regard to formulating factual statements. In other words, factual knowledge although 'rational' must be seen as essentially only probable; it is the product of both deductive and hence demonstrable, and probable, hence inconclusive, inferences.

We noted earlier a repudiation of the naive hypothetico-deductivist account of science. In that process of imaginatively constructed theories being systematically rejected, should any of their predictions conflict with the solid rock of direct observational experience, a number of other issues is raised. These concern how observational data is conceived and how anomalies are dealt with. Easlea argues that the criteria of the falsificationists leads to a position whereby once a course has been embarked upon, the scientist is committed to it come what may. It is evidently rather dogmatic. Rationality, despite Popper's protestations to the contrary, has been reduced to a commitment to an arbitrary methodological principle of *one problem [or conceptualisation] at a time but never all together.*

It was in this context that we also noted Kuhn's [1962] radical and influential attack on this implied concept of a dominant rationality, otherwise a conventional wisdom and its related belief that science advanced cumulatively towards an ever greater understanding of physical reality. Kuhn noted that, in the context of science based on falsification, it is,

> '*... the incompleteness and imperfection of the existing data-theory fit that at any time, define many of the puzzles that characterise normal science. If any and every failure to fit were ground for theory rejection, all theory ought to be rejected at all times.*' [op.cit., p145].

The concept of 'scientific revolution' most clearly illustrates the argument for the non-cumulative episodes in which an older paradigm is replaced in whole or in part by a new and incompatible one. Although Kuhn acknowledges that while normal science is a highly cumulative enterprise, the fundamental incompatibility between successive paradigms is a most significant aspect of Kuhn's conception of the process of science.

This issue emphasises the non-cumulative nature of scientific theory since the transition between the incomensurable cannot be made 'a step at a time', driven by logic and 'neutral' experience but must occur all at once. Kuhn then talks of how a scientist, once s/he has made the 'gestalt switch' between paradigms, works to convert the entire profession or relevant group to accept an entirely new view of science and the world.

Kuhn's description of a 'conversion' taking place is important since it implies a sort of mystical experience which is not compatible with previously expressed ideas on rationality. Easlea notes,

> *'In science, as distinct from religion or politics, faith is unnecessary: one can know when one is wrong. Kuhn thinks differently. According to him there are no objective criteria by which the revolutionary scientist can show his colleagues, committed to another paradigm, the error of their ways... since each paradigm will be defended by the criteria it dictates for itself.'* [op.cit. p15].

The view clearly threatens that of Popper whom we have noted as stressing that as a scientist in the disinterested pursuit of the truth, one cannot believe what one wants to believe, since the criteria are 'independent'. We might reiterate his view that the notion of rational science has given us an increased appreciation of,

> '... *responsibility of belief, embodied not only in a firm commitment to impartial principles by which one's own assertions are to be measured, but in a further commitment to make those principles ever more comprehensive and rigourous'*. [I Scheffler, 1967, p4].

But as we have discussed, besides an implied 'rationality', what is usually regarded as important in a 'factual statement' in science, is that it must also make reference to 'experience', commonly, experience made manifest [rather narrowly] by the tenets of empiricism. The significance of the logical structure means that ideas, as they develop, can take their place in a 'systems of ideas'.

Paradoxically, we have considered a view of science through a model of 'factual science' which in the spectrum of scientific activity, one might have taken as the least problematic. Indeed, some of the problems associated with the metaphysical view of science, that is to say, as abstracting the scientific enterprise beyond ordinary happenings are also relevant to the empiricists' approach. For example, some argue that empiricism itself is conducive to a kind of over-preoccupation with immediacies which may distract attention from critical, larger questions.

Bunge [op.cit.] goes on to stress that further progress in the 'legalisation of non-physical phenomena' requires, above all, a new attitude towards the concept of 'scientific law'. In the first place it must be realised that there are many types of laws, none of them being necessarily better than the remaining types. In other words as we have discussed, there is nothing fixed, permanent, factual about 'rationality', any more than any other concept. Rationalities, it has been suggested, are negotiable and just as there are many types of scientific law [morphological, kinetic, of association, of composition] there are many types of explanation. For example, as we have noted for a long time, in the philosophical tradition it was believed that to explain a phenomena was to point out its 'cause'. But it is increasingly recognised, indeed it is intrinsic to the systemic ethos, that causal explanation is merely one type among many.

The development of science in hypotheses involves concepts in a progressive transformation of 'ordinary language uses' by incorporating them in theoretical schemas, although some concepts are regarded as primitives, others take their meanings from their contexts. The way science dealt with the transition from observation to theory shows a heavy reliance on a rationality with a strict logical consistency and a highly specific practical aim, prediction. Of course the bounds of the rationality and the nature of explanations fully dictate the range of possible predictions, since scientific predictions depend on 'laws' and on items of specific observation which are promptly interpreted as information. Failed predictions in science therefore can be wholly accounted for in terms of the sets of assumptions of the concepts and rationality in which they exist.

2.5 CONCLUSION

In this chapter we have drawn out three main points; firstly that 'facts' cannot be considered meaningful unless you go behind them, that is, the enterprise of science cannot be principally concerned with facts. Secondly that criteria distinguishing truth and belief cannot be unequivocally established. Arguably, science is based on belief as much as on 'fact' or 'truth'. Thirdly, that science carries within it the values of any scientific rationality and community and hence science is not value free.

Having established these points, we can now reconsider analogy from a fundamental position in philosophy.

CHAPTER THREE

ANALOGY:
A PHILOSOPHICAL POINT OF VIEW

3.1 INTRODUCTION

In Chapter Two a view of science was presented suggesting the argument that science rests to some degree on beliefs and values and on the accepted validity of a self referential rationality. Concepts of analogy have traditionally been criticised from a 'scientific' standpoint and having discussed this basis, it is relevant to reconsider the utility of analogy fundamentally, taking in this chapter a philosophical point of view. The aim of the chapter will be to develop an understanding of analogy and metaphor in order to identify concepts with which to model analogy in Chapter Four.

3.2 THE IMPORTANCE OF ANALOGY AND METAPHOR

In his excellent study on 'The Role of Analogy, Model and Metaphor in Science', W.H. Leatherdale, [1974] stresses the importance of analogy and metaphor in science for a number of reasons, considering these areas worthy of examination on three counts.

Firstly, that the 'metaphorical view of science' is relatively new and interesting enough to merit a detailed consideration. Secondly that the metaphorical framework gives rise to several views which need to be distinguished. Thirdly that the concepts of analogy and metaphor access a body of philosophical and literary work on metaphor which might be of interest in developing our view of science. In this chapter we will consider much of Leatherdale's focus, but with a change of emphasis to concentrate on analogy and the interelated relationships of analogy, models and metaphors.

Leatherdale initially stresses that concepts of metaphor and also that of model are implicitly contained in that of analogy. This will be developed in our systems view of analogy later in the thesis. Leatherdale's analysis of analogy is extremely useful in this respect, proposing that analogy is principally a more simple and fundamental concept than its derivatives of metaphor and model.

3.2.1 *Concepts of Analogy*

The concept of analogy was introduced into general use by the Greeks, notably Aristotle and Plato but has been used in a variety of ways since then. In this sense, Leatherdale attributes a confusion to the ambiguity of usage. Analogy is used to signify both a relation and the things, objects or phenomena which are related by it. Leatherdale suggests that the now commonly used term 'analogue' stands for the latter usage of analogy and he reserves 'analogy' as the term describing the relation.

The prevalence of the notion of analogy in common use raises a further difficulty. Analogy is also sometimes used loosely in the sense of 'likeness with a difference', 'similarity' or more specifically;

> *'...any sort of resemblance provided they do not amount to complete inductions.'* [Mill, 1949, p4].

Evidently both senses of analogy [i.e. including that of analogue] and the associated notion of a relationship of analogy enter into scientific thinking. It is necessary in this sense that the concept of analogy, aside from referring to analogy of relations, be further distinguished. Firstly, the characteristic of modern philosophy has tended to regard the answer to the question *"What is an analogy?"* as obvious and 'unanalysable'. The 'unanalysable' resemblance between two things has been hitherto taken as insignificant to scientific thinking. This issue is intimately related to the discussion of 'literal' and 'metaphorical' language later in the chapter.

On the other hand, hitherto unanalysable but potentially analysable resemblance may well be fundamental to science, either in its early stages or else as part of an original insight at a more sophisticated level. In this context, Leatherdale proposes that we consider the 'intuitive' perceptions of unanalysable resemblances and affinities entering into scientific development. He gives the example of the so-called 'Natural Method' of eighteenth century Botany and quotes Guyenot[1] to illustrate the potential of what is normally taken as unanalysable resemblance to contribute to scientific thinking or possibly explanation;

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[1]Guyenot, E., 1957, *Les Sciences de la View aux XVIIe & XVIIIe Siecles, L'Idee d'Evolution*, Paris.

These classes of plants did not result from a methodical analysis of characters and did not rest on any precise definitions... [but were]... nevertheless, guided by a remarkable intuition, instinctively seizing upon general resemblances, grouping plants by their affinities for the establishment of natural classifications 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.' [1957, p16].

Secondly, Leatherdale describes a conceptualisation of analogy which may be taken as 'resemblance in an ensemble of qualities' or of 'properties or attributes', not relations given in immediate sense experience. In practice, however, he points out that it is difficult to draw clear distinctions between discrete qualities and relations. For example, colours, tastes and sounds seem to count unambiguously as qualities but those of size and shape implicitly contain spatial relations.

The discussion can then be extended by allowing a looser interpretation of 'properties', to include features of size, shape, motion and number under the broad definitional umbrella. With this looser view, Leatherdale is able to interpret, then differentiate between analogy based on such 'properties' and those others including the formulations of esoteric relations. This latter group runs close to the metaphoric case of analogy we shall come to in due course. Leatherdale distinguishes the two by referring firstly to 'manifest analogy', as analogy based on properties given in immediate sense experience or in ordinary perception and secondly to analogy based on more abstract relations as 'imported analogy'.

The differentiation of imported and manifest analogies is a distinction which carries a great deal of importance in our discussion of analogy. This is principally because many critics of analogy have concentrated their efforts in dismissing notions of manifest analogy as outlined above. Leatherdale proposes that this consequence is attributable to the 'British Empiricist' tendency to concentrate on manifest analogy, with the subsequent effect that discussions of analogy have been mainly in terms of logic and induction.

In some ways we should not find this surprising or alarming since manifest analogy is more readily amenable to reductionist symbolism than imported analogy. Further, and for this reason Leatherdale proposes that it has been the exclusion of imported analogy or the failure to make the initial distinction that has contributed to the critical confusion of the concepts of analogy. That proposition is fully supported here.

The confusion of the two types of analogy outlined is very deep rooted and it has caused number of problems which we ought to consider. We may begin the discussion by looking at the role of analogy in Francis Bacon's development of a scientific method in the*'Novum Organum'*. Some commentators [Leatherdale cites Fischer 1857,] have stressed that it consists of a 'systematic tabulation and analysis of analogies'. Despite this, Bacon was clearly aware of the appeal and dangers in the uncritical use of analogies, as can be seen from the passage below;

> *'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 may be things in nature which are singular and unmatched, yet it devises further parallels and conjugates and relatives which do not exist'.* Novum Organum, I, 45, p 50.

It is the concepts of analogy and related 'intuitive ideas' about significant resemblances [manifest analogies] and those procedures of metaphoric illumination that have given rise to a set of issues long discussed in the philosophy of science. These are highlighted in Leatherdale's remarks that to avoid what Bacon identifies as an over-preoccupation with regularity in the world, human understanding must exhaustively list and compare analogous instances, affinities and phenomena to discover the 'true forms' or 'natures'. Bacon regarded these 'forms' as being synonymous with 'laws' and the discovery of the 'laws of simple natures' he considered to be the goal of scientific discovery. Such laws would be describable in terms of qualities and properties found in ordinary experience and in the ordinary languages of description.

Leatherdale points out the important issue in Bacon's work is that he does not seek causal explanation in terms of mathematical relations or extraneous mechanisms, but rather takes some instances themselves as qualitative analyses for the source of explanation. How these privileged instances are initially selected to play the validating role in establishing the basis of insight into 'forms of nature' is not developed, except in the claim that,

> *'Whereas it is most unskilful to investigate the nature of anything in the thing itself seeing that the same nature which appears in something latent and hidden, in others is manifest and palpable'.* [Ibid., I, 88, p86.]

It is his objective of the discovery of 'forms' which led Bacon to place such a heavy reliance on manifest analogy. Although he deals with remoter aspects of analogy ['imported analogy'] in 'Instances Conformable or of Analogy[2]' and 'Supplementary or Substitutive Instances[3]' he does regard them as of little use in the discovery of forms and as a 'last resource'.

Keynes [1921] sums Bacon's contribution in developing the concepts of analogy in the following passage,

> *'Bacon's great achievement in the history of logical theory lay in his being the first logician to recognise the importance of methodical analogy to scientific argument and dependence upon it of most well-established conclusions.'* [op.cit. p 268].

Bacon's method, according to Leatherdale, is correctly linked by Keynes to the processes of scientific argument, rather than to scientific discovery and invention. Hence manifest analogy is taken as a method of validation and Bacon's own belief that it was also a method of scientific discovery is misguided. Keynes goes on to criticise Bacon's conceptualisation, particularly his belief that forms would be describable in the context of everyday language and experience;

> *'Bacon's error was double and lay first in supposing that these distinct elements lie upon the surface and consist in visible characters and secondly that their natures are or can easily be known to us.'* [op.cit. p271]

This significant assumption highlighted here by Keynes is apparent in other treatments of analogy, notably that of J.S. Mill [1949], whose work concerned analogy *'real or natural'* and its relationship with induction. Whewell[4] [cited by Leatherdale, pointed out that Mills' method of experimental inquiry,

> *'... takes for granted the very thing which is most difficult to discover, the reduction to formulae... of the facts of planetary motions, of falling bodies, refracted light rays and when in any of these cases we would discover the law of nature which governs them or if anyone chooses so*

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2 In: Francis Bacon, ed. F.H.Anderson, *The New Organon and Related Writings*, New York, 1960, ii, 27, p156.

3 As above: ii, 42, p216.

4 From W. Whewell, *On the Philosophy of Discovery*, London, 1860, p263.

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'. [op.cit. p9].

Leatherdale hence stresses that if [manifest] analogy and the philosophical mistletoes which spring from it [those of 'Forms' and 'Natures'] are to make a contribution appropriate to scientific discoveries, it is to its earliest stages. The relationship between analogy and intuitive knowledge at the near pre-scientific stage is indeed remarkably close. Leatherdale illustrates this point by using Adanson's[5] account of his method of classification in Botany,

'It was by grouping 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 but on several parts.' [op.cit., p9].

The example implies how such an apparently analytic method depends on intuitive judgements of resemblance despite such characteristics being far from identical in each plant or otherwise, as we shall argue in due course, 'domain'.

Yet overall it has been the manifest analogies which have received the most attention from both scientists and philosophers. As Mill suggested, the possibility of reduction to formulae was a major concern in selecting viable analogies. In other words, manifest analogies are the only types of analogies which are readily reducible to logical treatment in the pseudo-syllogistic format of 'A:B:C::a:b:c'. It is in this format which analogies have been most commonly and insistently described and expanded as below;

'*a, b, c, d,* all have the properties P and Q
<u>*a, b, c,* all have the property R;</u>
Hence *d* has the property R.'

In this context, Leatherdale contends that the logical form is mainly an abstruse elaboration of the 'common sense' level of understanding of analogy, appropriate to our ordinary handling of experience, whether scientific or everyday. He argues therefore that 'straightforward' analogical arguments of this kind do not appear to have aided the development of science. Evidently they do not intrinsically offer any novel insights but can merely act as a broad research guideline.

[5] Quoted in E Guyenot, op.cit, p35.

But the contention of this thesis is that it is not possible to accept the use of analogy as 'straightforward' in the way that Leatherdale implies. The reduction of analogical features to the syllogistic format seen above can be said to rather 'sell the concept short' and does not, in its neglect of the borad concepts of imported analogy deal with analogy to sufficient depth. 'Straightforward' concepts of analogy along with apparently straightforward use in practice do not adequately probe the potential utility in a model of analogy.

To illustrate this important point, we will consider the example of a straightforward analogy, given by Leatherdale. It is Galileo's argument from analogy to show that the *'Earth is a planet'*. In the example, we will be highlighting aspects which show that this kind of approach is not as 'straightforward' as it might first appear. In *'the Assayer'* [6], *'Starry Messenger'* [7] and the *'Dialogue Concerning Two Chief World Systems'* [8] Galileo conducts a skilful argument to show that the earth is a planet, with the Moon playing an important role as a middle term in the analogy.

Hence the argument proceeds:

> 'Earth is analogous to Moon:
> Moon is analogous to Planet:
> Therefore the Earth is analogous to Planet.'

Under rudimentary telescopes, planets were still observed to have visible discs while stars remained as mere points of light even under magnification. Jupiter was the most direct analogy in this context, since following telescopic inspection, it too appeared to have satellites. The Moon again was a significant factor in drawing the analogy, it was fundamental to the straigntforward analogical relationship being proposed in Galileo's argument.

> 'As the Medicean Planets are to Jupiter:
> The Moon is to Earth:
> Jupiter and the Medicean planets move:

6 In; Galileo Galilei, translated by S.Drake, *Discoveries and Opinions of Galileo*, New York, 1957.

7 Ibid.
8 Ibid.

Therefore the Moon and Earth move.'

Thus the Moon, when under examination appeared to resemble the earth with mountainous outcrops throwing shadows across the moonscape. Additionally, because of its changing 'phases', it was proposed that the Moon was not self-luminous. The planets were also perceived to have phases, to be diffusely illuminated and so appeared similar in constitution to the Moon and hence through the analogical relation, the Earth. Venus particularly has phases in the same sequence as the Moon and thus by analogy must circle the Sun. It was suggested that the Earth when viewed [hypothetically] from the the Moon, would also have phases caused by reflecting light and therefore must also circle the Sun and so forth.

Despite the impressive flow of the analogical argument, which we should remember is being viewed from a perspective fully accepting a Galilean conceptualisation of the planetary system, we might feel that the argument belongs at the 'common sense' levels mentioned previously. But there are some important points to notice. For example, although it appears to be an argument closer to manifest analogy and logical progression, there are a number of extraneous theoretical and causal considerations more pertinent to a case of 'imported' than pseudo-logical manifest analogy. Galileo draws terrestrial analogies relating to the diffusion of light and the argument for the Venusian phases is based on the conceptions of the formal Copernican system of thought. These points clearly suggest that there are prevalent elements of 'interpretive perception' indispensable to the effectiveness of the analogical argument.

As briefly mentioned, the reliance on manifest analogy has not proved itself to be sufficient in the development of science to merit its legitimisation as a process of scientific discovery. Leatherdale attributes the resulting 'low status' of analogy to the fact that manifest analogy allows no place for 'scientific genius or inspiration' in deriving novel and creative hypotheses for scientific exploration and development.

To illustrate his suggestion, Leatherdale draws out a number of factors which he presents as characteristic of scientific discovery. Firstly that some 'special ability' going beyond routine method and secondly some novel insight, are required in scientific acts of creation and discovery. Thirdly he also suggests that such creative perceptions are 'instantaneous and comprehensive'.

Leatherdale's first point on this issue raises an interesting issue, namely that if the scientific method is as objective and systematic as the criteria discussed in Chapter Two strive to ensure, why cannot one scientist reason as well as another? James[9], a noted psychologist suggests that,

> *'The flash of similarity between an apple and the Moon, between the rivalry for food in nature and rivalry for man's selection was too recondite for any but exceptional minds.'* [quoted in Blanchard, 1943, vol.ii, p133]

It is hard to deny that some scientists do appear to be possessed of some special quality which lifts them 'beyond the ordinary'. Leatherdale to reinforce his point uses three quotations from famous and respected scientists in which they comment on this ability. Sir Humphry Davy [10] [all three quotations referenced in Leatherdale, op.cit. p13] suggests for example,

> *'Imagination as well as reason is necessary to perfection in the philosophical mind. A rapidity of combination, a power of perceiving analogies and comparing them by facts is the creative source of discovery'.*

And from American geologist G.K. Gilbert[11],

> *'To explain the origins 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.'*

Oppenheimer[12] puts it as,

> *'Science is an immensely creative, enriching experience, it is full of novelty and exploration and it is in order to get these that analogy is an indispensable instrument..'*

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9 W.James, *Principles of Psychology*, Volume II, p343.

10 In: Davy, H., *The Collected Works of Humphry Davy*, ed. J.Davy, 9 Volumes, London, 1839-40.

11 G.K Gilbert, *The Origin of Hypothesis, Illustrated by the Discussion of a Topographic Method*, Science, 3, 1896, pp1-13.

12 R.Oppenheimer, *Analogy in Science*, American Psychologist, 11, 1956, p130.

Leatherdale also points to other writers who have recognised the significant role of analogy in deriving creative hypotheses for scientific development and progress as being Hooke [1961], Kepler [1939], Mach [1943], Maxwell [1890] and Poincare [1958].

As the former correctly points out, it is quite evident that the kind of analogy referred to by these authors is not that of manifest analogy previously discussed, nor is it employed in any usual inductive method. Equally these passages are not referring to an argument by analogy, [a model for which we shall be developing in Chapter Four]. Instead, they talk of 'noticing', 'perceiving', 'suggesting analogy'. This perceptual feature of analogies is referred to by Leatherdale as an 'analogical act'.

In summary, Leatherdale compares an analogical act in process terms to the special circumstances of an 'act of recognition', in which there is a conscious apprehension of an effort of synthesis. In other words, an act of recognition parallels an analogical act in the following way, clearly described by Leatherdale as consisting of;

> '... *the present consciousness putting out feelers to the past* [i.e.another domain] *and when successful in recall, drawing complexes of past experience into the present as an element of the present perception or judgement... An analogical act is rather like an act of recognition... except that it does not concern identification except in a special sense of identifying something similar in certain important respects to something else. It is closer for example to identifying some unknown person as a known person's son, or of recognising a fragment of melody as something by Beethoven because of its style, without perhaps being actually familiar with the melody in question.*' [op.cit. p14]. Bracket added.

Hence the analogical act is one which involves a reformulation. The reformulation is perpetrated by importation of new structures or interpretive frameworks from another unfamiliar domain of knowledge and experience.

To distinguish further, in the analogical act Leatherdale introduces the concepts of 'topic' and 'imported' analogues. The topic analogue is that matter 'in the here and now'. [His conceptualisation of analogy is such that the identification of 'topic' analogue, as that referring to the phenomena under investigation, is concomitant with that of the 'analogue set' to be presented in the model of analogy described in the next chapter]. The notion of 'imported analogue' is referential to the new structures drawn in by some process [not yet described] from elsewhere. Leatherdale's conceptualisation of 'imported analogy' corresponds to that of 'new domain' in the framework of analogy to be explored later, although the broader notion of 'imported analogy' is more accurately identified and developed in terms of the concepts of 'analogical reasoning'.

Leatherdale stresses that it would false to imply that the analogical act can be seen as instantaneously accomplishing at one stroke the criteria for developing a new paradigmatic view of a given topic analogue or hypothetical enquiry. While the *'flash of similarity'*, as James [op.cit] put it, might well be momentary, its effectiveness must additionally rely on other factors. A whole complex of antecedent knowledge and behaviour and a similar complex of consequent implications and associations for example, in order to 'make sense' of the insight. In other words, before the illuminating perception occurs, the topic analogue is ranged over and sometimes one part, sometimes another is brought into focus or juxtaposed with this or that. The topic analogue, proposes Leatherdale, is equally an amorphous entity as that of the imported analogue, ever growing and changing, sometimes crystalising out, only to dissolve again under the pressure of discordant facts drawn from other areas of the topic and imported analogues. It is clear that the syllogistic format by which analogies are typically constrained would not be appropriate here.

Leatherdale's valuable analysis has allowed us to gain some understanding of the controversial issue of analogy by surveying and distinguishing among the notions of manifest or imported analogy and analogical acts. The former conceptualisation has been treated in the quasi-formal style of the logical paradigm and the latter two are normally associated with some kind of 'intuitive' insight above and beyond the usual methods of science.

This issue is considered to be of importance by Leatherdale who sees it as reflective of two features in scientific development which have, as we have seen, been frequently remarked upon by commentators and indeed scientists themselves. It is appropriate that we discuss them a little further.

Firstly, the matter of the apparent 'suddenness' of discovery. In this context many scientists feel that their insights have come independently of former efforts and research. The dramatic instances of these 'flashes of genius' are perhaps among the most well-known events in the progress of science, such that *'Newton's apple'* and *'Archimedes bath'* have passed into cultural fable. A second feature gives high importance to the principally imaginative feature of the intuitive act. Nyman's paper, [1953] also quoted by Leatherdale, discusses the role of intuition in this respect. Nyman strongly repudiates the view that the construction of hypotheses or theories is the result of methodical, not to say mechanical, processes of induction. Further, Poincare comments,

> *'Logic, which alone can give certainty is the instrument of demonstration: intuition is the instrument of invention'.* [op.cit.p23].

It is again important to note the difference between the concepts of manifest and imported analogy. Leatherdale looks at Gillespie's [1966] analysis of Faraday's work. Gillespie was apparently appalled at Faraday's supposed mathematical ineptitude and comments on Faraday's substituting reliance on analogy, by saying,

> *'Analogy, after all, depends on 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.'* '[op.cit. p 375].

Gillespie's remarks rather perversely give us a good example of the basic criticisms levelled at analogy which we have, in looking at the significant discrepancies in different concepts of analogy, been trying to dissipate. Particularly, the assumed linearity of transfer of ideas is not appropriate to the concept of imported analogy. Additionally, when imported analogy makes a contribution to thinking, it is evidently non-linear. The pressumption of linearity is an attempt to introduce 'logical method' and mapping into the creative processes inherent in imported analogy and it is clearly unhelpful and inappropriate. This is because it is more relevant for the analysis of manifest analogy, for whose features such a method was arguably developed. In this remark, I am referring to Bacon's discourse on scientific method. It is an issue with serious consequences and we shall be obliged to comment on this matter in Chapter Five, when discussing the different rationalities which can be introduced via the framework of analogy.

In the meantime, the other side of the issue shows how analogical acts of recognition and concepts of imported analogy can be thought of as a multidimensional gestalt-like insight into new ways of investigation. The analogical gestalt provides more than the limited inference of mere manifest analogy, but new perspectives, horizons and languages of description and interpretation of potentially infinite variety.

The crucial contribution of analogy has been commented on by Hanson [1958 p85ff], Toulmin [1953, p34], Kuhn [1962, Chapter 10] and also Farre [1968,]. The latter's remarks are repeated here,

> *'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 unrealisable without it.'* [1968, p788].

The aim in this research is to identify the utility of analogy in systems sciences and this involves using the concept of analogy in deriving hypotheses to then guide investigation, via a model of analogy. Such a model will be presented in Chapter Four in order that we may subsequently consider the possibilities for theoretical expansion of systems concepts across domains.

Our original point of departure in this discussion referred to notions of analogue and a notion of an 'analogical relation'. So far we have mentioned the latter only in terms of when it is 'noticed' or 'perceived' in analogical acts. In other words, although we have discussed issues of 'manifest and imported' analogy, the nature of the analogical relation needs further analysis to elude the dangers of pursuing analogical features over a potentially inexhaustible number of domains. That quagmire would clearly involve being drawn into the areas of 'hazy and superficial' analogies which are apparently common, but to be avoided at all costs, in order to preserve the building argument for the legitimacy of the concept of analogy. The dangers of 'hazy analogy' demand that we identify the principles by which analogical models and additionally arguments from analogy can be constructed.

An important aside at this juncture is some more precise commentary on what is meant by references to *'hazy and superficial analogy'*. We have previously described manifest analogy as cases of obvious resemblance across domains and it is to certain cases of manifest analogy that the label 'hazy' typically refers. But in fact this is rather a misnomer, for it is suggested that far from being 'vague' and 'hazy', what we identify as superficial analogies have a very clear, and hence 'obvious relationship' linking topic and imported analogues.

Leatherdale makes a similar point by looking at the prevalence of such analogies in the history of science and finds that more 'obvious' analogies between things, commonly analogies of colour, shape or number for example, have 'no particular significance' and demonstrate a lack of a creative basis for inference. He cites the example that the Sages of Greece, the planets as they were known to the Greeks and the gates of Thebes, [and the hills on which Rome was built, for that matter] were all seven in number, and for some time this superficial numerological analogy was taken to be of some special significance. [H.Metzger[13], as noted by Leatherdale, op.cit., has made a study on the role such analogies have played in the history of science].

Similarly, on this subject Achinstein [1968] argues that it is the formal, complex/abstract analogies which are the basis for creative inference in the scientific use of analogy;

> *'... they are more abstract by contrast, say, with similarities of colour, shape or size... 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.'* [op.cit. p 206].

We have noted that in philosophy an analogical relationship is a relationship of likeness or similarity but with the implication that the likeness in question is in some sense systematic or structural. To argue with reference to an analogical relationship, then, is to infer from the fact that is one thing is in some respects similar to another, the two 'things' will 'correspond' in other as yet unexamined respects.

The idea of correspondence is worth a closer look since it is one which we are familiar in terms of the correspondence theory of truth discussed in Chapter Two. The Oxford English Dictionary[14] gives the definition of correspondence as *'...the relation between things that answer to each other in some respect'*. Hence as a notion, it is loose enough to be heuristically appealing although because of this, difficult to attempt to model. In due course a rigorous model will be presented in which an analogical correspondence will be developed in terms of positive, negative and neutral analogy.

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13 H. Metzger, 1926, *Les Concepts Scientifiques*, Paris.

14 Oxford Shorter English Dictionary, Seventh Edition.

Analogy represents a comparison of one thing with another in order to indicate similarities and differences between them and thereby to increase the understanding of the lesser known of the two. Hence it is in the establishment of the relationship of ['imported'] analogy that Hoffding [1905] suggests any *'sophisticated ordering of phenomena is possible'.* There is a clear link through here to aspects and conceptualisations of General Systems concepts, hence, from Boulding,

> *'... for each purpose and at each level of abstraction... there must be an optimal degree of generality.'* [1952, p1].

It is timely also to recall Bacon's reminder of the dangers of analogy arising from the idea that the nature of the human intellect might make us too prone to 'seek order'. Hoffding's further remarks draw us away from the superficial quest for 'obvious analogy' which can contribute nothing in deriving creative hypotheses and inferential bases to work upon. Having said, as Leatherdale stresses that *'... analogy is likeness of the relations of different objects, not likeness of single qualities'* [properties], he goes on,

> *'As there are important differences between the domains of experience, the facts not being homogenous 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 homogenous and analogy is therefore a necessary way to understanding'.* [op.cit.p 28].

In other words, a relationship of analogy acts in such a way as to bring discrete areas of knowledge and experience together and enables one to see that for example, that two things are alike in exhibiting a relation. In this way, the identification of the analogical relation is clearly connected with a process of abstraction. Leatherdale suggests that it is analogy's property *of* and relation *with* abstraction, that accounts for its power and utility in mathematics. This thesis argues that this property is potentially useful to systems concepts also but that it must be treated with considerable caution.

Leatherdale's case is drawn from mathematics in which we have, he suggests, an extremely complex system of abstract relations capable of illustrating analogical relations for the most heterogeneous domains of knowledge. Hence while there are, he suggests, some implied 'descriptive' features of mathematical symbolism, for example, some symbols refer to variables, some to constants, some to the 'proper name' set of cardinal numbers, these meanings are contingent upon a symbolism which is fixed and unambiguous.

Leatherdale develops his point by quoting Hawkins [1958] whom he credits with recognising the Gestalt-like analogical feature of mathematics. The point is that one set of equations [he gives Laplace's equations as a case in point] serves as an analogue for processes of change in a variety of diverse domains such as gravitation, electrostatics, electricity, elasticity and the flow of liquids. Hawkins expresses his point,

> *'...mathematics changes its position in the world of intellectual culture... from that of tool of economic and technical interests to that also of a tool of intellectual growth to be prized and advanced and therefore for some to be regarded as an end in itself.'* [op.cit.p149].

There is reason to suggest that a developed model of analogy will allow similar benefits for the development of systems thinking also. We might now close this area of discussion in expressing the hope that it has been satisfactorily shown that the value of analogy relies on significant distinctions being drawn in a variety of analogical concepts apparent in scientific thinking.

As we have seen the most fruitful is evidently the concept of 'imported analogy' and in the subsequent perceptions of analogy implied in Leatherdale's notion of the 'analogical act'. These latter aspects of analogy involve the importation of analogues from discrete areas of experience and knowledge with a resulting reconceptualisation of the area under investigation. In our discussion we have also decoupled the more common concepts of manifest analogy which typically involve the unwelcome introduction of a quasi-formal syllogistic framework. These have also been shown to be misleading and in some circumstances as invoking concepts of 'superficial analogy' which have acted to discredit the concept of analogy as a whole.

3.3 ANALOGIES AND MODELS

We began this chapter with Leatherdale's assertion that notions of model and metaphor inherently contain a concept of analogy. Since we have scanned some notions of analogy, we might now broaden the debate and consider the relationship between analogy, models and metaphors.

The concept of a 'model' aside from that of 'scale model', has this century spread throughout many more disciplines than those of physics and mathematics, in which the discussions and controversies on their utilities originally developed. Equally, the somewhat renewed concepts of metaphor are also enjoying a re-birth of interest in many fields of contemporary research and the significance of both model and metaphor is to do with their role in formulating a scientific explanation.

Hesse [1963] raised an interesting point when she enquired as to the nature of 'scientific explanation'. Is it necessary, she asked, that a scientific explanation of experimental data should be understood in terms of a model or analogy with phenomena already familiar? Does an 'explanation' imply an account of the new and unfamiliar in terms of the familiar and intelligible or does it only involve a correlation of data among other criteria, mathematical elegance, for example?

A debate arises from these questions centring largely and inevitably on what is required in an 'explanation', that is, a debate on the criteria of explanation tackles one of the fundamental and traditional goals of science. It refers particularly to a dichotomy in the relative attractiveness of the two kinds of theory relevant to explanation, '...abstract and systematic' theory versus explication in a 'model'.

3.3.1 *Developing the Debate on Analogy and Models.*

Hesse [op.cit.] develops complaints made by the French physicist Pierre Duhem [1954, p80ff], whose debate with English physicist N.R.Campbell [1957, Chapter 6] is still taken as a significant starting point in discussion of the role of models in science in the context of their relationship with analogy. Although the debate took place at the begiining of the century and was very much concerned with the development of physical theories it is still worth consideration as a general discourse on the nature and role of theory.

Duhem [op.cit.] had cited some severe objections to the introduction of 'mechanical models' which were based on a 'physical analogy' advocated by Kelvin and Maxwell and which the latter prescribed as a complement to the scientific method 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 another.' [15]

The broad implication in the terms discussed earlier in the chapter is that there is a role for 'imported analogy' in the development and explanation of scientific theories and hence in the philosophy of science. Among the difficulties Duhem identified and attacked was the claim that the development of scientific models was somehow intrinsic to the parallel development of theories. He complained that such models could mislead the investigator by being taken too literally as explanations of the matter in hand, thus acting to distract the mind from the 'search for logical order'. In other words, Duhem claimed that the 'mechanistic', [although this is being used with an implied sense of 'systematicity' and is divorced from the functionalist attributes by which the term is normally now recognised] character of models at the ontological level would lead to much more subtle epistemological and paradigmatic consequences for the processes of scientific investigation.

Duhem instead proposed by way of antidote, that this uncomfortable situation could be avoided and that scientific explanation can be more fruitfully described, if models are developed in terms of purely formal, deductive systems. For him, the notion of a physical theory comprised a system of a small number of extremely general hypotheses referring to abstract ideas and also of laws which could be strictly derived from these hypotheses by deduction.

Some of the features of such a theory can be interpreted into observables and hence empirically tested, but his main assertion was that theory, as a whole, does not require explication by means of any model, and thus the possible epistemological problems are eradicable. For Duhem [op.cit.], the ideal physical theory would be a deductive mathematical structure, *'unencumbered by extraneous analogies or imaginative representations'*, leaving theory abstracted and ideally formalised.

Not surprisingly, Duhem's stand did not pass without challenge. N.R.Campbell [1957], strongly disputed the view that models were 'mere aids' to theory construction which could be thrown away once the theory had been

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[15] In: J.C. Maxwell, ed., W.D. Niven [op.cit], Vol.1, p156.

developed on the grounds that they were not logically necessary nor under some circumstances, as described, desirable. Campbell argued that the formal deductive criterion of scientific theories was not satisfactory because in addition to meeting logical criteria, theories must also display an analogy with already established laws. Campbell based his counter argument on two main points.

His first point was that we, as investigators, require to be 'intellectually satisfied' by a theory and that this satisfaction implies that the theory has an intelligible interpretation in terms of a model, as well as having in some cases a mathematical intelligibility.

Secondly, Campbell goes on to presuppose the dynamic character of theories. In other words and in sympathy with the context of this thesis, Campbell stressed that a theory in its scientific context is not a static museum piece, nor the abstract formalism Duhem suggests, but is always being extended and modified to account for new phenomena and circumstance.

Campbell illustrates his argument with reference to the kinetic theory of gases which was developed with the aid of the *'billiard ball'* model. He goes as far as to suggest that the analogical base of the model played an essential part in its extension and without it, any extension would have been 'merely arbitrary'.

More will have to be said on the dangers of the arbitrary nature of theory extension and it will be argued that it is possible to introduce analogy as a rigourous and parsimonious process in research. This strand is very much in concert with Campbell's second point on the intrinsic importance of models, namely, that without a model, it is impossible to use a theory for one of the essential purposes we demand of it, namely to make predictions in 'new domains' of phenomena. He stresses,

> *'...analogies are not "aids" to the establishment of theories; they are an utterly essential part of theories without which theories would be completely valueless and unworthy of the name. It is often suggested that the analogy leads to the formulation of the theory, but that once the theory is formulated the analogy has served its purpose and may be removed or forgotten. Such a suggestion is absolutely false and perniciously misleading.'* Campbell [op.cit., Chapter 6].

Campbell clearly suggests that analogies are of high significance when developing theories and we can gain more understanding of how Campbell expected analogies to contribute by quoting from him further. Of special interest in this context is his illustration of the importance of analogy in developing the kinetic theory of gases and the nature of a putative [*imported*] analogy between gas molecules and billiard balls.

The important point lies in identifying those conditions of analogy which have significance to an investigation and isolating those properties for full exploration according to the objectives of the study. Those features of a 'misleading' character in an analogy are not pursued and merely ascribed as irrelevances of that particular case, although they may well be of interest or significance under other circumstances. For example,

> *'When we take a collection of billiard balls in random motion as a model for a gas we are not asserting that billiard balls are in all respects like gas particles (for billiard balls are red or white, hard and shiny) and we are not intending to suggest that gas molecules have these properties. We are in fact saying that gas molecules are analogous to billiard balls and the relation of analogy means that there are some properties of billiard balls which are not found in molecules.'* [Quoted by Hesse, Ibid., Chapter 1].

It should be noted that the debate between Campbell and Duhem on the significance of models in the development of scientific theory as a mode of explanation was much influenced by the state of their own contemporary physics. Some of the details of the argument have not survived actual emerging evidence. Certain physicists and theoreticians might take this as an ultimate refutation of Campbell's position, since 'modern day Duhemists' propose there is a marked absence of intelligible models in quantum physics.

But in this situation we can identify a degree of paradox. On the one hand it suggests that the march of physics goes on regardless of the availability or development of 'picturable models', and this shows them superfluous to the scientific process. On the other hand, the idea of 'picturable models' is misleading, since a model need not be intrinsically picturable [i.e. it may be metaphoric]. Hence, *'a three dimensional space curved in a fourth dimension'* is taken as an acceptable model in relativity theory, but it is certainly not easy to conjure up in the imagination. [See Figure 3.3.1a].

Bhaskar, takes the example of the transition from Newtonian to Einsteinian physics as symptomatic of,

> *'... the current trouble with micro physics, such that our imagination cannot find an adequate pictorial representation for it and yet we have*

every reason to believe that it is true. Hence a new scientific ontology or a fundamental change in scientific concepts may transform our conceptions of what is plausible'. [1975, p155].

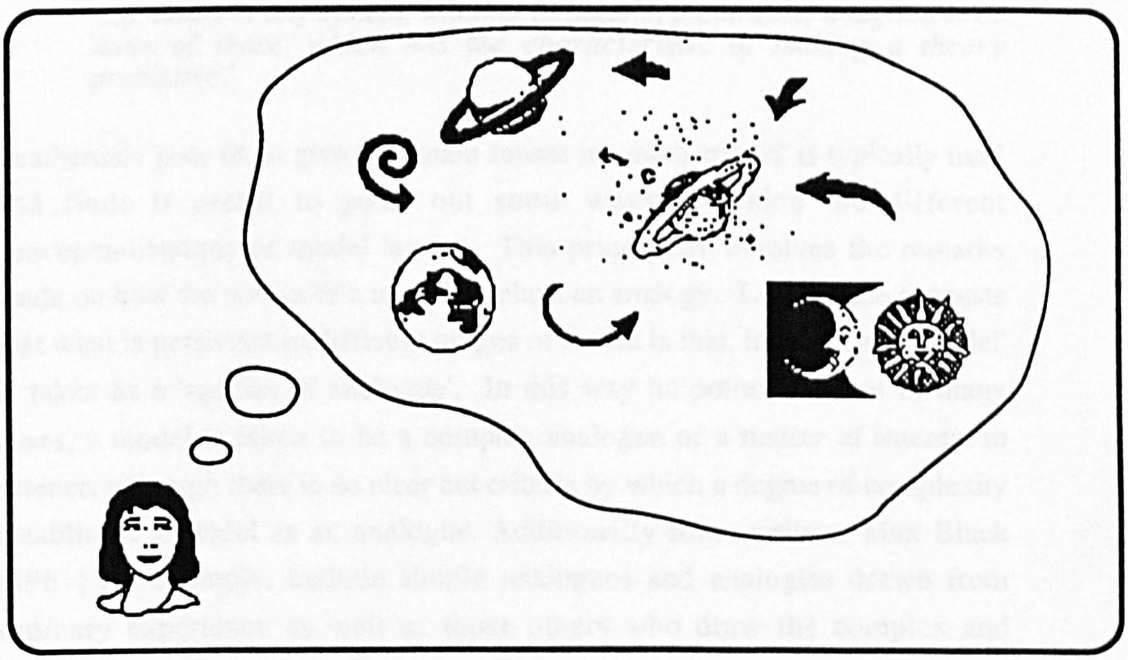

FIGURE 3.3.1a: A Three Dimensional Space Curved in a Fourth Dimension?

Nevertheless, Hesse [op.cit.] in her essay maintains that this debate on the role of models is not fully closed. Something to interest us remains in Campbell's conviction that without models, theories cannot fulfil all the functions required of them; they cannot 'explain' and in particular, they cannot be genuinely predictive.

Thus, by implication, models in some sense are 'essential' to the logic of scientific theories, although this assertion must be qualified with a rigourous definition of 'model'. Leatherdale points out that Campbell does not use the word model directly in his discussion on theory and that Duhem uses it only in the sense of 'imaginary' model, so clearly some analysis of the use of the term 'model' in science is in order. Leatherdale notes in this context that,

'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.'
[op.cit. p 40]

Hesse [op.cit. Chapter One] credits Campbell with giving this definition of 'model' and evidently it is highly general,

> *'...a model is any system, whether buildable, picturable, imaginable or none of these, which has the characteristic of making a theory predictive.'*

Leatherdale goes on to give four main senses in which 'model' is typically used and finds it useful to point out some ways in which the different conceptualisations of model 'agree'. This principally involves the remarks made on how the notion of a model displays an analogy. Leatherdale suggests that what is persistent in different usages of model is that, in each case, 'model' is taken as a 'species of analogue'. In this way he points out that in many cases, a model is taken to be a complex analogue of a matter of interest to science, although there is no clear cut criteria by which a degree of complexity establishes a model as an analogue. Additionally some writers, Max Black [1961] for example, include simple analogues and analogies drawn from ordinary experience as well as those others who draw the complex and systematic analogies from other areas of science.

Hesse distinguishes two main senses in which model is used in her discussion of 'Models and Analogies in Science', those of Model1 and Model2. Model1 refers to 'physical' analogy or those external analogies which are 'imported' to formulate or develop laws and theories. Model2 corresponds to a theory or part of a theory which is under investigation and development.

In Leatherdale's discussion, Model1 refers clearly to an imported analogy and Model2 he claims to the formulated topic analogue. In the context of the model of analogy to be developed in the next chapter, Model1 refers to the 'new domain' as qualified by the deductive/inductive/analogical relationship with the analogue set and Model2 to the process of [hypothesis] development concatenated into the dynamics of the formal analogical model.

In more general terms, Models1&2 refer respectively to a *'model of'* and a *'model in'* theory. We have in Chapter Two already noted the problems associated with development of concepts *in* and *of* science and Leatherdale suggests much of the confusion surrounding the role and meaning of models is rooted in failing to make this distinction. The matter becomes clearer when considered in terms of the role of 'models in' and 'models of' a discussion of analogy in this way.

Leatherdale in his reference to four concepts of model claims that these are the result of there being two variants of each sense of model just outlined. The variants themselves depend on the degree of formality given to the conception of 'model' and broadly caricatured by Duhem's and Campbell's imaginary discussion described by Hesse [op.cit. Chapter One]. Leatherdale clarifies the point,

> *'In the case of either sort of model* [Model1, *of* or Model2 *in*], *the model may be conceived of as either a set of assumptions about, or actual, entities, processes, structures or causes and relations or alternatively as a set of propositions or statements connected together in a deductive system.* [op.cit., p43]. Bracket added.

Therefore clearly of importance in the discussion of models and analogy must be the two-way relationship between theory and modelling. Braithwaite, quoted by Leatherdale, [op.cit., p43] has given a much discussed definition of a formalised conceptualisation of what Leatherdale sees as Model2, hence,

> *'A theory and a model for it... have the same formal structure since theory and model are both represented by the same calculus...*[16]

Or further,

> *'If two deductive systems are interpretations of the same calculus and if in one interpretation the initial formulas of the calculus containing theoretical terms are epistemologically prior to the derived formulas not containing theoretical terms, whereas in the second system the derived formulas are epistemologically prior, then... the former system is related to the latter as a model is to theory. In other words a theory and its models are formally isomorphic'.*[17]

Braithwaite is proposing that a theory and its models are formally isomorphic and that we come to know most of the propositions of the theory starting from the lowest levels and working up to those propositions of the greater generality, and also those propositions of the model starting from the highest level statements and working down to increased specificity.

The ideas have been further developed in the so-called 'B-N-S' model [after Braithwaite, 1955; Nagel, 1961; Suppes, 1957]. The advantages of deriving a

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16 R.B.Braithwaite, Models in the Empirical Sciences, p230f In: E.Nagel, P.Suppes & A.Tarski, (eds.), *Logic, Mathematics and Philosophy of Science*, Stanford, 1962.

17 Ibid.,

B-N-S 'model' for a theory is that the model interpretation is usually more straightforward and familiar than the theoretical one and consequently serves as a stimulus to thought with the added bonus that any discoveries made using the model will have the same logical structure as parallels in the theory. The claim for formal isomorphism between model and theory is, as Girill [1970] has pointed out,

> *'... a good description of, and an even better reconstruction of the relation of models to theories in science, because such formal models fulfil two important functions;*
> *i) an informative function- aids in understanding concepts and their relationship in the theory modelled:*
> *ii) an heuristic function- aids in the process of extending and developing the theory'.*

Figure 3.3.1b is a speculative interpretation and illustrates the proposition that extending the theoretical domain is not inherent in the B-N-S model [evidently itself a Model2] of the theory/model relationship, although this feature is at a later stage attributed to the function of analogical models.

Achinstein [1964, pp328-349] criticises the B-N-S conceptualisation severely on the grounds that the formal similarity between corresponding sets of propositions is considered as the only relationship required between theory and model. Braithwaite had in fact already put it quite bluntly, saying that,

> *'...the similarity in formal structure is all that is required of the relationship of the model to theory'.* [op.cit.p93].

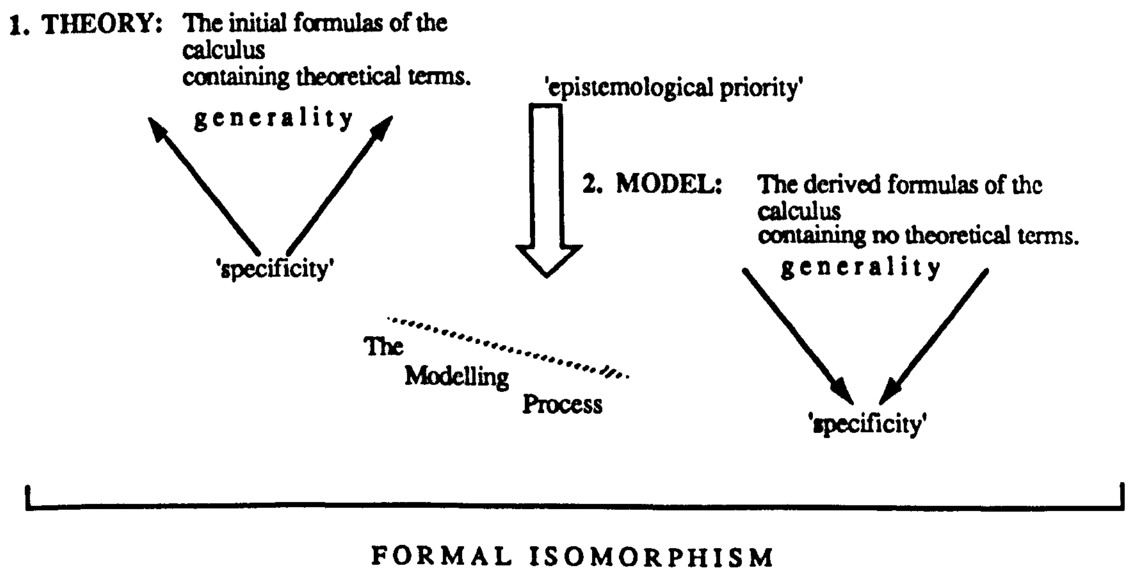

FIGURE 3.3.1b: An Interpretation of the B-N-S Model.

BNS originators employ the terms 'model' and 'analogy' interchangeably. Achinstein [1964] however chooses to distinguish between them thus;

> *'...the term model is frequently used by the scientist in the expression 'model of an x' to refer to a set of postulates or assumptions describing certain physical objects or phenomena of type x. Accordingly the prepositions comprising the model of an x are the same ones constituting what may also 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.) though it obviously is not true that all sets of assumptions, even those called theories will be classified as models.'*

Achinstein asserts that whatever the precise nature or number of such assumptions, it is important to recognise only that the terms model and theory are often used to refer to the same set of assumptions. Alternatively he suggests that,

> *'... an analogy is drawn between certain objects or phenomena described in a model (of an x) or theory and other objects or phenomena which may be more familiar (i.e.corresponding entities are alleged to be similar in certain respects but nevertheless are to be distinguished).'* [Ibid].

Achinstein's argument is that there are other bases for a relationship between the two concepts namely that of physical similarity.

Obviously it is crucial not to confuse the scientist's model or 'theory of an x' with analogies which may be invoked in explaining its features and which may have also aided its construction. Hence Lord Kelvin in his Baltimore Lectures[18] offers a certain analogical description of the ether;

> *'The luminiferous ether we must imagine to be a substance which so far as luminiferous vibrations are concerned moves as if it were an elastic solid. I do not say it is an elastic solid.'*

In this context we can appreciate Leatherdale's note that it is difficult to find a brief and precise definition of a model in the sense of Model2 and this is relevant to the points of Chapter Two when we attempted to explore the concept of science via the construction of a model in science, that *of* factual science.

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18 Lord Kelvin, *Baltimore Lectures on Molecular Dynamics and the Wave Theory of Light*, London, 1904, p8-12.

But if we disregard the variety of uses of the term model identified here in terms of Model1&2 viewed formally or informally, the main focus of the argument returns to two fundametally opposing views of theory. On one side of the debate are those writers who believe that theoretical statements and the theoretical entities to which they refer, are to be understood in terms of, and derive their meanings from, references to their logical connections within a deductive system of propositions with other statements which refer only to observations and experiment procedures.

Leatherdale suggests that to this group belong, non exhaustively, operationalists and positivists. The first because they believe that all extra-logical terms are definable with operations, that is, actual observations or empirical procedures. The second because they believe that all theories and laws are merely predictive, summarising formulas which serve to link elements derived from direct experience. The view is summed up by thinkers like Braithwaite who, although describing himself as a contextualist, asserts 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... thinking of a model of a theory is quite unnecessary for a full understanding of the theory.*'[op.cit., 1962, p230f]

On the other side of the debate we find the 'modellists', as Leatherdale suggests they are known, who believe that theories are more than calculi, or predictive and summarising devices or deductive systems which generate theorems. The modellists' claim is most cogently taken as that put forward by Campbell [op.cit., p129f]. Although the latter did not write specifically on the role of models, his work is taken as a clear precursor of the modellist viewpoint.

Campbell's point is such that a theory must have a 'meaning' and that it has a meaning by virtue of the analogy it displays with established scientific laws. To take our leave from this debate for a while we can close with Leatherdale's summary of the spectrum of modellists' views,

> '*Achinstein sees the role of a model as attributing inner structures compositions or mechanisms to objects or systems so as to explain various properties... moreover it is supposed to provide at least some approximation to the actual situation [i.e. it makes ontological claims]... Hesse claims that models supply... a programme for the extension and development of a theory. Spector claims that a model gives semantic rules by which we may attribute observation predicates to theoretical*

terms and hence to observable objects... Gotlind sees models as providing intermediary links between mathematical equations and empirical phenomena. Nash makes a similar but stronger claim that a model is an essential link without which the mathematics is not a scientific theory at all... Hutten regards one of the chief functions of a model as being able to supply a descriptive terminology for theory...' [op.cit., p48].

3.4 ANALOGY AND METAPHOR

We began this chapter by stating that analogy is a special, simpler case than either model or metaphor and that it is implicitly contained in each one. Metaphor is an area of study which has enjoyed a considerable amount of attention in recent years although again, it is hardly a new idea.

3.4.1 *Concepts of Metaphor*

There are a number of views on metaphor which are worth discussion. Leatherdale [op.cit.,p104] quotes Cassirer's [1942] concept of 'radical metaphor' as illustrating a relatively early interest in the subject paralleling the contemporary and noting the influence of the eighteenth century debate on the feature of metaphor as being a critical part of language and thought.

Mueller is quoted by Leatherdale as expressing the modern view, in which far from being regarded as being a merely literary embellishment, metaphor is seen as key to understanding or at least 'easing the expansion of understanding' beyond its current limits. Mueller puts it this way,

> *'It was completely impossible to grasp and hold the outer world, to know and to understand, to conceive and to name, without... fundamental metaphor... Metaphor in this sense was much less the carrying over of a word from one concept to another than the creation of a new concept by means of an old name.'* [Leatherdale, op.cit. p126[19]].

Another view of metaphor stresses the significance of metaphor in promoting an 'interanimation' of one field of thought with another. This interesting notion is put forward by I.A.Richards [1936]. In the latter's book, 'The Philosophy of

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19 From W. Urban, *Language and Reality*, New York, 1939, Chapter 9.]

Rhetoric', he argued for a conceptualisation of metaphor in which the importance is described as,

> *'We all live and speak only through our eye for resemblances... As individuals we gain our command of metaphor just as we learn whatever else makes us distinctively human.'* [op.cit., p93].

Richards' view on metaphor evolved following his rejection of the eighteenth century assumption made by writers on rhetoric. It suggested that metaphor is something special and exceptional in the use of language, a deviation from its normal mode of working. Richards goes on,

> *'... metaphor has been treated as a sort of happy extra trick with words, an opportunity to exploit the accidents of their versatility... In brief a grace or ornament or added power, not its constitutive form.'* [20]

Richards' interest was in showing metaphor to be an omnipresent principle of language and that this principle could be established through observation. He said that,

> *'We cannot get through as much as three sentences of ordinary fluid discourse without it even in the rigid language of the settled sciences we do not eliminate or prevent it without difficulty'.* [Ibid. p92].

The mechanism of metaphor is especially fascinating because it implies that what we take as fixed words are somehow shifting in their senses. Richards describes it in its simplest form such that when we use a metaphor we have two thoughts of different things active together and supported by a single phrase or concept whose meaning is a result of their interaction.

Richards queries the traditional theory of metaphor which describes only a few of its multifarious facets and thereby has rendered metaphor a seemingly verbal issue whereas Richards stresses that metaphor is fundamentally a borrowing and intercourse of thoughts and a transaction between contexts. To paraphrase, 'thought is metaphoric and proceeds by comparison and the metaphors of language derive therefrom'.

Richards introduces two terms and a number of steps in which he attempts to reveal the complexities of the metaphorical configuration, see Figure 3.4.1a given below.

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20 Quoted by E.Partridge, 1963, Usage and Abusage, Penguin, Harmondsworth, p180.

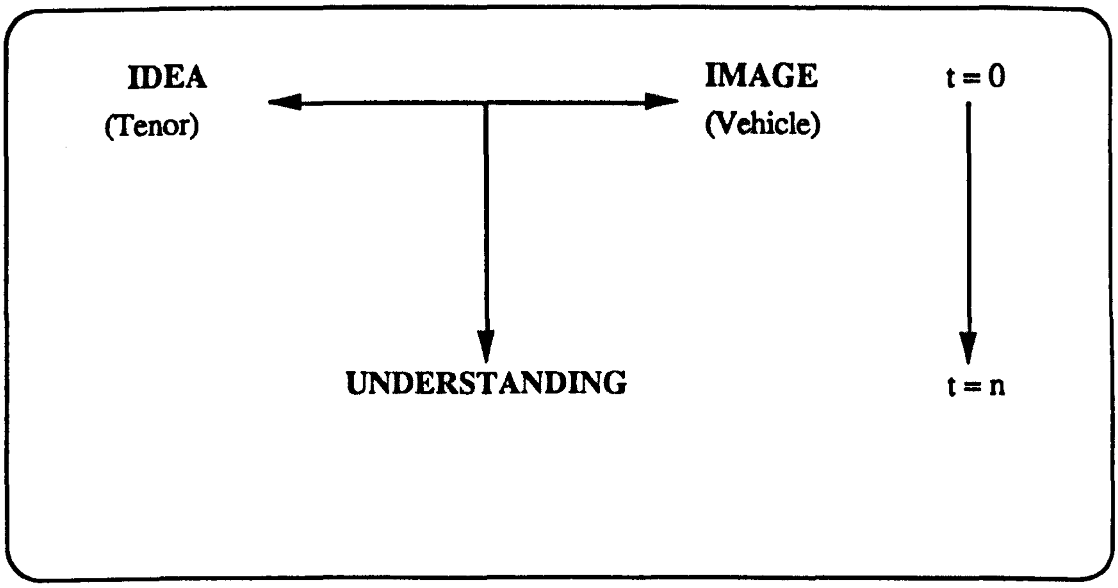

FIGURE 3.4.1a: Tenor, Vehicle and Understanding.

Richards specifically introduces 'tenor' and 'vehicle' as the two ideas that any metaphor in eliciting conditions of co-presence supplies. Tenor and vehicle [see Figure 3.4.1a] were intended to tighten up the clumsy descriptions of the metaphorical process implied by the usual phrases of,

'... the original idea & the borrowed idea...
...what is really being said & what it is compared to...
...the underlying idea & the imaginary idea...
...the principle & the resemblance...
...the meaning & the metaphor...
...the idea & the image...
...the tenor & the vehicle...'

Given the above, the potential for confusion can be easily appreciated. In his objection to the view that metaphor was 'mere embellishment' or 'added beauty to the plain meaning', Richards insisted that the many important issues are raised by metaphor. He argued that the co-presence of tenor and vehicle resulted in a meaning clearly distinguishable from that of the tenor [that is a meaning, 'richer', 'subtler', 'fuller', i.e. in systemic terms possessed of emergent properties] than that of the tenor alone and that meaning is not attainable without an interaction with the vehicle. This applies equally to the concept of vehicle.

Overall, Richards suggested that our skill with metaphor in poetic, prosaic and literary thought is one thing, prodigious and inexplicable but that our awareness and understanding of that skill is quite another, very incomplete and distorted, fallacious and oversimplifying. Hence he claims the business of reflective awareness is to protect our natural skill from the interferences of our crude views about it. We can also note in this context the remarks made in Chapter One on the dangers of the ad hoc use of metaphor in systems thinking and methodology.

In this respect we might further consider the 'shifting senses' of words in metaphorical use. The issue is such that the first thing we notice about metaphor is principally that word meanings are not straightforward and invariant, as in the views on uses of language in which words are taken in their 'literal sense' only. This term 'literal', is defined by Leatherdale [op.cit., p98] as the unambiguous usage 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. This is obviously a broad definition and collectivisation of a whole range of issues in linguistic philosophy, but nevertheless a definition which will suffice for our illustrative purposes here.

In the metaphorical use of language there is a disharmony between words and contexts. We are using language descriptively in unfamiliar ways and ascribing to words attributes, properties, nuances and relations that are not located among their literal meanings and which at first sight might seem inappropriate.

Essentially this view implies that there is a mixing of contexts in which far from exhibiting a failure to communicate effectively, we strive to make sense and to maintain coherence in such situations. This is done by *'... trying to find some unity between discordant contexts...'* and by exploring, in Leatherdale's words [op.cit., p98], *'... the less immediate associations of imagery, sensual and physical experience and inference'* or as Black [op.cit.] has it, (although he does not enlarge on how these might arise), on the basis of 'received commonplaces'.

In this way Black [1979, p27] remarks that metaphor 'creates' novel insight, but this is, on reflection slightly misleading, since it appears to suggest that metaphor formulates 'something out of nothing', when there must be at some level, however esoteric, some contextual parallel of similarity. The is a fundamentally analogical component.

The tension between metaphorical and literal uses of language has caused some writers to express the view that, after Leatherdale,

> '... *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 and old literal meanings and there seem to be only two ways of doing this. One by ostensive definition and two, by use of metaphor or simile.*[21]' [op.cit., p104].

The area of 'ostensive definition' is clearly problematic when we are attempting to introduce, communicate or express new ideas. Indeed the philosophical utility of ostensive definition has been illustrated by some writers as being best observed when one attempts to point out a bone to a dog. The dog merely responds by sniffing one's finger. Leatherdale [op.cit., p107] further notes that it is characteristic of the learning process that early on, we begin to supplement mere indication by verbal exposition and this necessarily involves metaphor, which is shown to be an indispensable auxiliary to an ostensive process.

Van Steenburgh [1965] has also suggested that literal and metaphorical usages of words can be distinguished in terms of ostensive definition and proposed that literal language could be identified by the fact that it is learned ostensively by observation and that metaphorical usages are learned otherwise.

A rudimentary scan at the processes of learning and the accumulations of vocabulary by the individual and culturally, would seem to emphasise the predominant role of metaphor in this context. Consequently the etymological and philosophical claim that languages began 'literally' is clearly misguided,

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21 Metaphor and simile enjoy an intimate relationship but unlike a metaphor, a simile is usually characterised by a particular form of words, 'is... as', for example since it is a clear comparison and uses the form appropriate to this. However this definition is not really sufficient to encapture all similes, which we should also note as being unusual, and non- 'matter of fact' comparisons, but ones suggestive of some novelty. The important point is to notice that similes differ from metaphors in that they do not elicit the 'fusion or absorption' of conceptual areas of comparison in the way of the latter and that throughout analysis similes maintain a conceptual distance between the objects of comparison which remain characteristically distinct.

because this would suggest that they were learned through ostensive definition. Leatherdale illustrates the opposing point that literal language is in fact an 'emergence' from non-literal uses with a set of conclusions taken from Barfield, [1960],

> *'Literalness is a quality which some words have achieved in the course of their history; it is not a quality which the first words were born. And let us be clear about the consequences. We mean by '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 the objects of the outer world would have at some stage of their life been vehicles without tenor and if that is so, it follows that they began life as vehicles with a tenor.'* [op.cit., p106] .

But if we take 'literalness' as an emergent feature of language we ought then to consider its relation, suggests Leatherdale, with science, principally because we often uncritically equate the literal with the scientific and vice versa, which is evident from our observations on the nature of science as made in Chapter Two.

This is a view which is prevalent with writers and thinkers who tend to regard *'common sense language'* and *'material-object'* language as 'literal' and therefore somehow more important [even perhaps more 'true'], in scientific terms, than metaphorical and hence by implication, complex language. The viewpoint serves to rather paradoxically reinforce an already discredited view that metaphors are parasitic and exceptional to language in its *normal* mode of working. It also leads to the confusion of the values of 'subject matter' and 'style' of expression and while of course, certain styles might be pragmatically more suitable in certain contexts this assumption is dangerous, if accepted 'a priori'.

This is because again it reinforces notions that metaphor and poetry are in some way intrinsically distinguishable by means of their use of language [and in fact, in this sense, there is an implied ontological distinction] which is clearly divorced from scientific usage and literalness. Leatherdale reiterates this highly significant point,

> *'This goes with the equally specious but pernicious assumption that literature is merely 'fancy' talk and that science is 'plain' talk. While there may be important differences between the metaphors of science and those of literature, there is really no reason a priori why metaphor should not be appropriate to science and it is equally perfectly possible for literature to be literal.'* [op.cit. p107] .

The links between metaphor and thought and thus language are an important influence on Max Black, whose famous book *'Models and Metaphors'* [1962] contributed the following to the theory of metaphor. Firstly, that contextual tensions between the vehicle and tenor could not be resolved and hence 'understood' without an awareness of the implications of the metaphor. This is a reference to the significance he attributes to the concept of 'received commonplaces' and hence he posits that a metaphor can be concerned with 'systems of ideas'. This led him to suggest that [to paraphrase], the metaphor 'selects', 'controls' or 'organises' features of the 'principle subject' [latterly the tenor, or as we shall see in the following chapter, the 'analogue set'], by implying statements about it that normally apply and refer to the secondary subject, [or vehicle]. Secondly, Black maintains that the system of associations, that is, the 'received commonplaces' need not be a 'natural system' but may be specifically 'constructed'. Black explains it this way,

> *'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.'* [op.cit. 1962, p40].

Black's view implies that models are in fact extended and systematic metaphors and this idea had also previously been developed by Hutten [1956], whose strong views on the integral relation between theories and models we briefly heard in the preceding section in connection with our discussion on the role of analogy. In this context, we might again stress that it is analogy which is the fundamental concept, supporting metaphor although the views on the relationship of metaphor with science can spring from a variety of perspectives.

The diversity of origin is a result of that described earlier as two opposing views on metaphor and its relationship with language and thought briefly introduced earlier in this section. Ortony [1979] in his description of 'Metaphor: A Multidimensional Problem' updates and enlarges upon the eighteenth century view of metaphor as embellishment and the modern view presented through the description of Richards' interest in the area.

Ortony [op.cit., pp1-19] labels the two sets of views on metaphor as 'constructivism' and 'non-constructivism'. The division is based on the opposing views on the role of a scientific model which was briefly described in the previous section. Ortony sets the terms for enlarging the debate on metaphor and its relationship with science, which appears to be high on the philosophical agenda. He points out that science [as we were obliged to describe it in Chapter Two] is supposed to be characterised by precision and the absence of ambiguity and he makes it clear that the language of science is often thought to be correspondingly precise and unambiguous, in short, *literal*. We have broadly explored this theme but overall, it can be seen that scientific faith in literal language as the only adequate and appropriate tool for the objective characterisation of reality has manifested itself in many ways; culminating, Ortony suggests, in the 'heyday of logical positivism' in which the idea of a literal language reigned supreme.

Ortony however goes on to suggest that another approach is possible and the central idea of this approach is that cognition is the result of mental construction,

> *'Knowledge of reality whether it is occasioned by perception, language, memory or anything else is a result of going beyond the information given. It arises through the interaction of that information with the context and that knower's pre-existing knowledge.'* [op.cit. Introduction].

This obviously parallels with the notions of 'analogical acts' which were described earlier and it is clearly a relativist position to adopt. In other words, Ortony is suggesting that the so-called objective world is not directly accessible but constructed on the basis of the constraining influences of human knowledge and significantly, language. This view hence provides no basis for a rigid differentiation between 'scientific' languages and other kinds, those of perception or imagination, for example.

Thus the constructivist sees metaphor as an essential characteristic of the creativity of language, entailing an important role for metaphor in thought. But their concern is also to break down the distinction between the literal and the metaphorical. From the above quotation it is clear that constructivists evidently accept that meaning is constructed rather than merely 'read off' reality and hence the meaning of non-literal uses of language do not constitute a special problem [for science]. Use and comprehension of language are essentially creative they maintain, and this means that the constructivist repudiates the distinction between the languages of the poet and the scientist.

Meanwhile the non-constructivist sees metaphor as deviant and parasitic upon normal use of language and that if metaphors require an explanation then that explanation will be merely in terms of violations of linguistic rules and conceptual frameworks. Thus metaphors characterise rhetoric and not scientific discourse, being fuzzy, vague, inessential frills which are not appropriate to objectivity. The two views clearly reflect similar objections to models raised by the modellists and the positivists/operationalists in the preceding section.

One idea clearly in sympathy with that of the constructivists is presented by Buchanan [1962], whose underlying belief is that analogy, as a resemblance of relations, is fundamental to knowledge and understanding [notably in art or science]. It is by way of analogy that we have the related concepts of simile, allegory, poetry or symbolism of many other sorts, [including mathematics, as Hawkins outlined]. Hence,

> *'Analogical thought is so common that we are surprised... any history of thought might begin and end with the statement that man is an analogical animal.'* [op.cit., p81].

Leatherdale goes on to give Buchanan's 'open' definition of analogy as follows,

> *'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 C to D... relations may be of any degree of complexity provided the identity or similarity is not violated... I shall call this property of analogies their expansiveness.'* [Ibid, p82].

The concept of *expansion of analogy* is central to Buchanan's conceptualisation, as it is to Black's view on the open-endedness of metaphor. As Ortony has pointed out, there is no definitive answer to the basic question, *'what is a metaphor?'* and he suggests that Black [1979], is *'not really concerned with establishing what a particular metaphor means'*. Rather, he is interested in giving an account of metaphor that satisfies an intuition that many writers in the area have had, [see Mueller's remarks at the beginning of the section] that there is some special, emergent 'new thing' created when a novel metaphor is understood, something new that is attributable to the metaphor itself, rather than to the emerged novelty.

Boyd [1979, pp356-409] presented what he dubbed as the three most widely held views on metaphor as the substitution, the comparison and interactive viewpoints which each form a broad framework for the putative solution of metaphorical statements. Black [op.cit.] has rejected the 'substitution view' of metaphor which proposed that every metaphorical statement was equivalent to a 'literal' statement, which here we have shown to be impossible.

Black also criticised the comparison viewpoint, taking it as a special case of the former, since it entails notions which propose that successful communication via metaphor involves the hearer understanding the same respects of similarity or analogy as the speaker. We can, incidentally, plainly see here the identification of metaphor as principally a 'verbal' phenomenon.

Instead, Black adopts the 'interaction' view of metaphor which suggests that metaphors work by applying to a principle [literal] subject [the tenor] a system of associated implications characteristic of the metaphorical [vehicular] secondary subject. Black's position rests mainly on an important distinction from the comparison theory. In contrast to the latter Black denies that successful communication via metaphor depends on the communication of quite definite aspects of either the vehicle or tenor and maintains that metaphors are open-ended.

Another important divergence from the comparison theory is that Black also denies that any analysis, 'however elaborate' of an interaction metaphor in terms of making explicit the analogies between the tenor and vehicle, can capture the cognitive content that a metaphor is capable of conveying.

Further, Black suggests that in some respects it is revealing to understand metaphors as creating ['calling forth'] the analogies on which they depend. Black sees these features of metaphor as indicative of an important difference between metaphorical uses of language and those uses which have the features of explicitness characteristic of scientific usage. In this context, he remarks,

> *'We need the metaphors in just those cases where there can be no question as yet of the precision of scientific statements. Metaphorical statement is not a substitute for formal comparison or any other kind of literal statement but has its own distinctive capacities and achievements.'* [Black, 1962, p46].

Boyd suggests that his view implies that if metaphorical language is used in a scientific context then its function should lie in the pre-theoretical ['pre-scientific', in the Kuhnian 1962, sense] stages of the development of a discipline. Or in the case of mature sciences it should occur in the realms of heuristics or in notably 'informal' exegesis.

In forming his view Boyd [op.cit.] found it useful to compare and contrast his understanding of scientific metaphors broadly with Black's account of metaphors in general. Boyd consequently suggests that there exists a class of metaphors which play an important role in the development and articulation of theories in 'relatively mature' sciences. These are used to introduce theoretical terminology where none previously existed. [This point could clearly be included in the argument for the non-literalness of scientific language.]

Such 'scientific metaphors' possess several though not all of the characteristics of what Black has called 'interaction metaphors', in particular their 'success' does not depend on them communicating specific areas of similarity or 'analogy' but rather on the heuristic value of the conceptual 'fusion' they proffer across contexts. The open-endedness that Black sees as the most significant aspect of the interaction metaphor, is their contribution to scientific development.

Boyd thus argues that metaphors can be of significant importance in scientific development and the impression that the inclusion of metaphor must somehow involve a lack of precision and accuracy, rests on a mistaken view of the role of precision in science. We can reflect on Davies and Ledington's [1988] argument for the pragmatic inclusion of metaphor in SSM [Checkland 1981] here. Boyd suggests that the use of metaphor is one of many devices available to the scientific community to accomplish the task of the 'accommodation of language to the causal structure of the world'. That is, the task of introducing and modifying terminology so that we can establish terms of reference which 'cut the world at its joints'.

What I see Boyd as suggesting here, is that the employment of metaphor in science and in 'reference fixing' [this relates to our points on literal and metaphorical uses of language] serves as a 'non-definitional' mode of reference fixing. This is especially well suited to the introduction of terms referring to complex relations which we have not yet been able to approach by other means. Thus the employment of metaphor provides the basis for making explicit much of what is tacit and inexplicit in scientific work.

Boyd goes on to describe a considerable spectrum of sorts of metaphor which he considers play a role in science or in the terms of his points on 'theory change'. Firstly he parallels them [rather dangerously] in their exegetical context with aiding explication and teaching of theories which have 'non-metaphorical' formulations. [This ascribed role for scientific metaphors runs dangerously close to the debate we introduced in terms of a similar role of models in theory development and it is difficult to see what could be gained from re-entering that quagmire save to notice the 'Warning' signs.]

Boyd [op.cit.] gives the examples of the metaphors of*'wormholes in general relativity'* or that of an *'electron cloud'* or the description of atoms as *'miniature solar systems'*. Boyd points out that while the first two examples are merely descriptive, the third example does betray a theoretical insight. Although in this example the theory does not arise from the open-endedness of the metaphor since Boyd argues that one could say 'literally' in what sense Bohr thought that atoms were like solar systems. [Nevertheless these examples do not discredit the role of scientific metaphor, since Boyd has noted Kuhn's conceptualisation of the establishment of fundamentally new theoretical perspectives rests significantly on persuasion, recruitment and indoctrination. Clearly then, a number of pedagogically effective and convincing metaphors is pertinent to development and progress.]

The most interesting metaphors, he suggests, are those which constitute, if only temporarily an irreplaceable part of the linguistic machinery for a scientific theory. These metaphors are constitutive of the theories they express and are important for their contribution in expressing theoretical claims for which there is no 'literal' paraphrase. [Again this argument could be used to undermine the substitution theory of metaphor]. Boyd suggests that such metaphors are prevalent in young sciences and gives the example of *'the brain is a computer'* as a theory constitutive metaphor for developments in cognitive psychology. Hence *'theory constitutive metaphors represent one strategy for the accommodation of language to as yet undiscovered causal features of the world'*, [op.cit. p362].

Leatherdale comments that in sum, Black's [and hence Boyd's work which was based to some degree on the latters'] influence on the metaphorical view of science is not at all straightforward. Black's contribution was to give, he suggests, accounts of both models and metaphors [which were originally published separately] and which are very suggestive of their similarities. He comments, *'Certainly there is some similarity between the use of 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 transferability of metaphors... Those who see the model as merely a crutch are like those who consider a metaphor as mere decoration or ornament. But there are powerful and irreplaceable uses of metaphor that are not adequately described by the old formula of "saying one thing and meaning another".* ' [op.cit.., 1962, p236].

We have only briefly discussed the potential role for metaphor in science at a macro-level. We have already stressed that a fundamentally less complex concept of analogy, [less complex because it does not 'elicit fusion', but co-presence of contexts] is contained within it. Thus before closing this section I would like to make some further though brief remarks on the utility of analogy for science at a micro-level, that is, in hypothesis formation.

3.5 ANALOGY AND HYPOTHESIS FORMATION

In the previous chapter we had cause to note Gilbert's remarks on the 'analogic suggestion of hypotheses' and now will pursue a brief discussion on the role of analogy with reference to hypothesis formation. Hypotheses are normally taken to form the structure of scientific research and govern the deductive observation of scientific models. As we have argued, there are dubious claims that scientific knowledge is true and we prefer to suggest that it is merely hypothetical.

Hypotheses are sometimes formulated by way of induction, that is, as generalisations on the basis of a number of observations. Hypotheses may be arrived at by way of analogies, conceptual or mathematical. Bunge [1959, p68] suggests the former is illustrated by Huygens [1690], who originally found it useful to compare waves of light, sound and water,

'I call them [light and sound] waves from their resemblance to those which are seen to be formed in water when a stone is thrown into it, and which present a successive spreading as circles, though these arise from another cause and are only in a flat surface.' [op.cit.p4]

Further, the latter are exemplified by Maxwell's [1873] work in which he predicted the existence of electro-magnetic waves on the basis of a formal analogy between his field equations and the known equations of elastic waves. This also illustrates the important point that analogies in science are not limited to the comparison of theoretical objects with more or less familiar objects from everyday experience. They might also evidently be derived from an analogical involvement between two theoretical objects or phenomena.

Sometimes an investigator might be led by philosophical considerations influenced by analogy. This was how Oersted [1820][22] proceeded, deliberately seeking a connection between electricity and magnetism on the a priori conviction that the structure of every existent is polar, and that the forces of nature are organically linked. A similar philosophical conviction that there is no limit to the complexity of nature also led Bohm[23] to speculate about a sub-quantum mechanical level, on the basis of an analogy with classical Brownian motion.

Hypotheses are arrived at, in short, in a number of ways. Many heuristic principles are at work, the sole requirement being that it should be in some way 'testable'. The nature of the test and the direction from which it is approached highlight a variety of interests. Logicians would stress the validity of the hypothesis as a basis for inference to general and singular propositions. The psychologist would be concerned with the process by which the exploration of hypothesis-testing yields somehow creative models. The methodologist with the development of the appropriate test and the philosopher with the nature of the framework generating the hypothesis.

Our interest in analogy regarding the process of hypothesis formation is, at this juncture, in the initial stages of the perception of an analogy, that is, 'observation' and subsequent 'supposition'. We have already discussed the perception of an analogy as an 'analogical act' and it may also be described as a secondary step of supposition in that moment when the investigator regards the observed analogy as having an heuristic utility. So what conditions, then, bring about this transfer from mere observation to a level of supposition which is to become a hypothesis, and how do these conditions come into existence?

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22 See S.F. Mason, *A History of the Sciences*, London Routledge Kegan Paul, 1953, p386.

23 D. Bohm, *A Proposed Explanation of Quantum Theory in Terms of Hidden Variables at a Sub Quantum Mechanical Level*, in Colston Papers, London, Butterworths, Scientific Publications, 1957, 9, p33.

De Groot [1968, p29] suggests that in such a translation in the social sciences there is always a substratum of 'fact'. But we have already discussed that this cannot be said to be the case with analogical models or any other form of model based on assumptions concerning 'literalness' of science to which 'fact' must be seen to refer. The substratum of fact is taken to result from either direct or processing of, observational data which is then said to form a factual underpinning as an empirical resource base for the investigator.

Turbayne [1962] has observed,

> *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 with the literal truth'. But can this actually be done? We might just as well seek to provide what the poet 'actually says' ... we can never know what the facts are... We cannot help but allocate, sort or bundle the facts in some way or other.'* [op.cit. p 64]

Turbayne is pointing out that whatever the apparently empirical basis, it is always being viewed [in the processing of observational data] from a particular angle, i.e. in relation to the problem with which the investigation is concerned. This point is of a wider scope than the mere explanation of 'given facts', since the inductive ideal must be to generalise. There is evidently a theoretical framework in which we are working and the theoretical framework in this procedure provides the terms of reference for any 'data' observed and is of common significance to traditionally empirical and analogical investigations. Further de Groot points out,

> '*... one clearly discernible characteristic of the formation of a new supposition [hypothesis] is that it is based on a new interpretation of the factual data within the theoretical framework'*. [op.cit.p 29].

Turbayne concludes that science is irreducibly metaphoric and that the only way to remedy the situation is by working to develop ever more effective and powerful metaphors, but always with the knowledge that we are using metaphors and hence analogies.

Leatherdale describes convincingly a corollary to Turbayne's claim that the unwitting use of analogy in its complex form of metaphor is more widespread than we might reason, even to the stage where it has already penetrated scientific method. [The argument that metaphor is a more complex form of analogy is persuasive, since it is not only based intrinsically on the latter but even when bisected by the criteria of analogy, then as Black has suggested, we still are not able to predict its full implications.]

In the second chapter, we discussed, under section 2.4 'The Nature of Science' the powerful influence of the mechanistic world view and its implications. Leatherdale [op.cit. p138] goes on to suggest an article by Laudan, 'The Clock Metaphor and Probabilism' [1966] contains an argument to show how the metaphor of a clock and its invisible mechanism has effected 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 the 'true' 'certain' conclusions derived by induction or rational deduction. He concludes that Descartes suggested that the scientist must be content with hypothetical principles and conjectures rather than true and valid inductions/deductions and this has been an important stimulus. He illustrates his point by quoting from Descartes,

> *'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...'* [op.cit., p81].

Also 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,

> *'The hypothesis gives the real meaning of the theory and involves analogy which confers on it value; and the dictionary uses the analogy and the propositions contained in it are usually suggested by the analogy...'* [op.cit., p133].

The significance of analogy to hypothesis formation has been briefly illustrated. In the framework of analogy to be presented in the next chapter we will develop the connections between processes of analogy and the exploration of hypotheses further.

3.6 CONCLUSION

In this chapter we have drawn out two main points. Firstly that analogy is a form of comparison that has traditionally been conceived as a loose identification of resemblances, but which can also be shown to be rigourous and vital to theory development. Secondly, that the validity of the argument from analogy, which has much concerned philosophers, can be assessed in terms of an argument relating to any form of model, since models are vital ways of developing appreciations of theories and concepts.

The following chapters will hence consider two main areas; What is a useful model of analogy for systems thinkers? When is an argument from analogy a valid argument, that is, under what circumstances can the practical explorations of analogy be useful for theoretical development in systems theory?

CHAPTER FOUR

A MODEL OF ANALOGY

4.1 INTRODUCTION

In the preceding section we have established the potential value of analogy as a concept. In this and the following chapter we will take the study further by developing an explanation of how a model of analogy can be conceptualised. In the development of the model we will consider particular implications of three types of analogy, 'positive', 'negative' and 'neutral' analogy in Chapter Five. The objective of the chapter is to introduce a model of analogy for systems thinkers.

4.2 WHY MODEL ANALOGY FOR SYSTEMS?

We noted in the first chapter that systems and cybernetic research has drawn, and continues to draw, upon the concepts analogy and metaphor. General Systems Theory, for example, was built upon an analogic/homomorphic framework [see, for example, Bertalanffy, 1968]; cybernetics has traditionally been based on mechanical and biological analogies [Flood and Jackson, 1988] and is itself of a metaphorical nature. When using Soft Systems Methodology [conceived by Checkland, 1981], Atkinson [1984], Davies and Ledington [1988], Stowell and Allen [1988], all draw upon the concept metaphor. We have already suggested that the use of these concepts has been inadequately discussed and that the debate ranging from philosophical to practical issues must be explored further.

The aim in this chapter is primarily to present a case for these concepts on philosophical grounds and by developing a model of analogy, also uncover some principles by which they may be incorporated into practical efforts. A commentary on these matters will be presented in the concluding Chapter Seven.

The structure of the discussion in this chapter is as follows. Initially a formal distinction between analogy and metaphor will be drawn, for purposes of developing a conceptualisation of use to systems thinkers. Turning attention to analogy for a while, we will briefly consider its relationship to General Systems and Cybernetic Theories, uncovering the relevance to systems thinking of the underlying principles of analogy and of analogical modelling. This provides the basis for our argument for analogy as a meta-concept which in processes of analogical reasoning, can overcome the difficulties of inductivism and deductivism.

We will then tackle some 'more on metaphor' and discuss at once metaphor and analogy within the functionalist and interpretive paradigms. The lessons of this debate provide the basis for considering analogy and metaphor, under the umbrella of interpretive tenets, to be on one spectrum from the 'creative metaphor' to a more formally modelled approach of analogy.

4.2.1 *Distinguishing Analogy and Metaphor For Modelling- A Starting Point.*

From the broad discussion of 'analogy and models' and 'analogy and metaphor' in the preceding chapter, we might now distil a concept of analogy into that of the Oxford Dictionary of Current English; hence an analogy may be firstly defined as *'agreement, similarity (to, with, between) parts'* [A1]; secondly as *'a process of reasoning from two parallel cases'* [A2]; thirdly, *'a resemblance of form or function in entities essentially different'* [A3]; and fourthly, *'an inflexion or construction of words, in place of others'*, [A4].

Additionally, metaphor can be understood as; *'an application of name or descriptive term or phrase to object or action where it is not directly applicable'*, [M1]; and that 'figure of speech' in which *'a name or descriptive term is transferred to some object different from, but analogous to, that to which it is properly applicable,'* [M2].

These apparently straightforward definitions are somewhat circular, i.e. the term 'analogous' appears in the definition of metaphor. We have already argued for a conceptualisation in which the concepts of metaphor and model are understood to implicitly contain the simpler, binding concept of analogy, and in which the latter represents a fundamental mechanism. In the origins of the word, there is a notion of 'transference' in the sense of one word to another [Partridge, 1973], which is also circular since in definition [A4] above, for analogy, we find *construction of words* is a central notion. A completely different method for uncoupling these concepts is therefore required for systems thinkers. In other words, the linguistic issue of the 'literal' and 'metaphorical' use of language has again become apparent and must be dealt with before we can consider modelling analogy.

On a pragmatic basis, one clear distinction that does arise and is evident across disciplinary literature is the predominant use of the notion of analogy in the 'sciences' and metaphor in the 'arts'. We have already remarked in Chapter Three that the distinction between these two areas has much to do with the role of analogy and particularly metaphor in language. But even at an arguably superficial level, it is noticeable that 'analogy' is more widely found at the methodological level in science. Analogy has been used as a means of constructing models and theory, often through diagrammatic and equational representation. In this respect we may think of the process of 'analogical reasoning', 'manifest' and 'imported analogy' as forming a broad framework in which general discussions of aspects of similitude, dissimilitude or identity between phenomena may be postulated for, perhaps, empirical research.

By contrast, our description showed metaphor to involve 'fusions' of conceptual areas and in the open-endedness of interactive metaphor, a 'perceptive struggle'. Metaphor is thus more acceptable and indeed, more commonly found methodologically in the 'arts', where such issues are seen as of central importance to the effectiveness of poetry, plays and so on. There can be no doubt that a fruitful study of Shakespeare revolves around the use of metaphor and additionally its importance in terms of rhetoric has been well documented elsewhere [see Richards, 1936].

At a conceptual level pertinent to systems thinking, we can propose that metaphor be thought of as a dynamic 'act of comparison', taking place within a 'framework of analogy', but that is not to imply that it is always enlightening to distinguish metaphor as a 'subset' of analogy. Rather, metaphor is 'romantic', in Atkinson's [1984] sense of being 'imaginative' and 'visionary' although it can also be analysed in terms of more formal theory-based considerations, [in terms of the linguistic theory of reference for example]. This notion of 'formal theory', 'romanticism' or an 'analogy-metaphor' perspective, constitute the end points of a spectrum. This conceptualisation has some support in the work of Kuhn [1979], who has written, among others, on the relevance of a higher level transference of concepts in terms of 'Metaphor in Science'. In this paper, Kuhn argues for an investigation of a 'metaphor-like process in science', and one which accepts Black's [1962] important contribution of the 'interaction' view. Kuhn places much significance on the assertion that,

> *'... however metaphor works, it neither presupposes nor supplies, a list of the respects in which the subjects juxtaposed by metaphor are similar.'* [op.cit. p409].

Such an assertion might easily cause consternation among scientists who are more sympathetic to the formal explications of analogy. In addition, Kuhn adds further fuel by suggesting that the open-endeness or inexplicitness of metaphor has an important and precise parallel in the process by which scientific terms are introduced and thereafter deployed. The issue is clearly most intimately connected with the theory of reference to which we have alluded previously in our discussion on truth, [in Chapter Two] and the assumed 'literalness' of language developed in scientific usage [Chapter Three].

There are a few additional points to note on this issue at this stage. Kuhn [op.cit.] stresses that, as in the case of Black's interactive metaphors, the juxtaposition of examples in science call forth the similarities upon which the function of metaphor depends and the establishment of referents for scientific terms relies. Further, the 'end-product' of the metaphorical interaction between examples is nothing like a definition, nor a list of characteristics exclusive to that term alone. No list of that sort is possible, suggests Kuhn, although this does not lead to a loss of functional precision. Thus,

> *'...natural kind terms and metaphors do just what they should without satisfying the criteria that a traditional empiricist would have required to declare them meaningful.'* [op cit., p143].

Kuhn's points are clearly commensurate with analogy and metaphor conceptualised in terms of a continuum. Further, this conceptualisation can bypass, to a limited extent, some important philosophical considerations of the so-called theory of reference that romantic metaphor [in terms of its qualities of fusion and 'visionary' aspects] could be accused of ignoring. In this I am referring to linguistic arguments to establish meaning terms and the ways in which we described metaphor as soliciting the 'shifts in meaning', outlined in the previous chapter. The significance of the theory of reference is illustrated in the following passage:

'Juxtaposing a tennis match with a chess game may be part of what is required to establish the referents of game, but the two are not in any usual sense metaphorically related. More to the point, until the referents of game and other terms which might be juxtaposed with it in metaphor, have been established, metaphor itself cannot begin. The person who has not yet learned to apply the terms 'war' and 'game' correctly can only be misled by the metaphor, "war is a game" or "Professional football is war" ', [Kuhn, 1979, p413].

Additionally, we must also note that concepts of 'natural kind terms' to which Kuhn is linking metaphor in his argument, are also significant. Kuhn takes metaphor as an essentially more complex version of the process by which 'ostension' enters into the establishment of reference for natural kind terms. In this way he stresses the actual juxtaposition of examples of games allows the term 'game' to be applied to nature. The metaphorical juxtaposition of the terms 'game' and 'war' highlights other features, ones whose salience has to be reached in order that actual games and wars in nature can be conceptually separated. Kuhn refers us to Boyd's point that nature has 'joints' that natural kind terms aim to locate. Hence Kuhn suggests that metaphor acts to remind us that another language might have located different joints and cut the world up another way.

This matter is the crux for enthusiastic systems methodologists keen to adopt metaphor in practice. Fundamentally, that even a pragmatic use of metaphor must inevitably pay court to some sort of referential theory is clear from Kuhn's example. In our discussion we will take the spectrum extremes of metaphor being the romantic, and analogy as formal or documentary, within a framework of analogy which can at a 'meta-level' discuss the aspects of similarity that a particular metaphor may call forth or, as Black [1979] suggested, 'create'. In some respects this taxonomy also deals with the issue of definitional circularity between concepts of analogy and metaphor experienced earlier.

Some other practical points are worth a mention here. Metaphors and analogies are not uniform in the 'insight' they might elicit, some are illuminating and others obscure, or incongruous, or what is otherwise called *'dead'*. Significantly, it must be pointed out that the continuum described in Chapter Six between formal analogy and 'romantic metaphor' is not one which parallels 'effectiveness' but merely the sliding scale of 'analyticity'. As Leatherdale has pointed out most lucidly, in metaphor,

'.. 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'. [op.cit. p 110f].

Additionally, Boyd [1979] has emphasised that literary metaphors/analogies tend to reside in a specific work of a specific author. If the same metaphor is used by another author, a reference to the original source and use is usually implicit. When applied often, by a variety of authors or under minor variations, a metaphor becomes trite and hackneyed, or frozen into cliche. In other words literary metaphors tend to lose their insight and creative worth through overuse. In fact some writers would claim that when such uses reach a certain stage [of 'overuse'], the words are not metaphorical at all, they are now literal. Wheelwright[1], gives the example in this context of the term *'skyscraper'*.

By contrast, scientific metaphors or analogies, if 'successful', as in terms of providing useful insight become the property of the whole scientific community and variations may be explored by many without their interactive quality being lost. The wide usage encourages the discovery of new features of both the primary and secondary subjects and new understanding of the theoretically relevant aspects of similarity and or dissimilarity between them. This is specifically relevant to the interest to develop a model of analogy for systems thinkers.

At this juncture, then, we will consider metaphor as a dynamic intercourse of thoughts expressed in speech and/or written words which gives rise to new thought images and directed perceptions. Analogy will be considered as an intercourse of thoughts based on observation, expressed in diagrams and/or equations (and/or other language and non-language structures) that gives rise to new formal theories/models and enhanced understanding and knowledge. The contrast of the 'looseness', (or as Black puts it,) the 'open endedness', of metaphor with the more formal nature of analogy presented here will be examined later in the chapter.

Using the understanding developed in the preceding chapter and above, we will now consider analogy as participating in a *'framework of analogy'* and later metaphor in that framework.

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1 Wheelwright, P., 1960, Semantics and Ontology, in: *Metaphor and Symbol*, L.C.Knights and B.Cottle, eds, London, 1960.

4.2.2 *Analogy, General Systems and Cybernetic Theory*

The definitional assumptions [A1-A4] offer a basis for scientific model building but they are also a conceptually attractive 'method' to General Systems theory, which takes as its goal the notion of a 'unified science'. We may note in this respect, Peter Caws'[2] address to the Society of General Systems Research on this theme and especially his emphasis that the form the goal of a unified science might take remains unresolved. Similarly cybernetics, which also purports to favour a self-contained set of concepts that are equally applicable across disciplines, has had recourse to turn to biological and mechanical analogy.

The idea of analogy or metaphor as a basis for legitimate inference is, as we have noted, contentious on both philosophical (in a general discursive sense), and practical grounds. The pernicious influence of their poor status in 'factual science' and the importance of these issues to a broad class of systems thinking make natural enemies. The battle has penetrated systems thinking to the point where analogies have lost their status as suitable models in the investigation of General Systems Theory and their premises have been used as the basis of criticism, not only of the concept of analogy, but in an area of their use, General Systems, also. In fact a so-called 'devastating attack' on General Systems Behaviour Theory by Buck [1956] was based on this view. Buck's case was cited by von Bertalanffy [1972] as follows:

> '*...its essence is the 'so what' argument. Suppose we find an analogy or formal identity in two systems, it means nothing. Compare for example, a chessboard and a mixed dinner party; a general statement expressing the alternation of black and white squares on the one hand and of men and women on the other can be made. If one is tempted to say, "Alright so they're structurally analogous, so what ?" My answer is "So nothing..."* '

This so-called devastating attack, however, is hardly devastating. We can all identify weak analogies [see Bunge, 1979], so what! Buck's argument merely represents a superficial understanding of analogy, clearly limited to that of mere manifest analogy, which we have discussed and largely dismissed in the preceding chapter. Skill and courage is required in identifying useful analogies and we might also note in this context that von Bertalanffy [1972] also stressed

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2 Caws, P., 1967, *Science and System: On the Unity and Diversity of Scientific Theory*. Presidential Address to the Society for General Systems Research, December 1966, at Washington, D.C. Reprinted General Systems, 12:3.

that Buck had missed the point of a 'general theory of systems science'. Its basis is not more or less hazy analogies but the establishment of principles applicable to entities not covered in conventional sciences.

Less wryly amusing than Buck, but more enlightening, is the critique of General Systems Theory as conceived by von Bertalanffy, by the Soviet authors Lektorsky and Sadovsky [1960],

> *'Bertalanffy emphasises the idea that GST is not an investigation of hazy and superficial analogies... analogies as such have little value, since differences can always be found among phenomena as well as similarities. Bertalanffy declares that the kind of isomorphism with which GST is concerned is a consequence of the fact that in some respects corresponding abstractions and conceptual models can be applied to different fields.'*

If analogy is to maintain credibility in our argument, then the concerns of Lektorsky and Sadovsky will have to be answered. We mentioned in our preceding discussion that in certain circumstances, perception of analogy in science had proved *'too recondite for any but exceptional minds.'* If we are now going to attempt to use analogy, without assuming the privilege of an 'exceptional mind', our model must specifically address at least two issues. Principally, that,

a) the danger of hazy analogies requires identification of principles by which analogical models must necessarily be constructed; and
b) the approach adopted must represent the concept of analogical reasoning as a means for identifying 'the kind of isomorphism' by which corresponding abstractions and conceptual models can be applied to different phenomena.

These concerns are discussed below.

4.3 PRINCIPLES UNDERLYING A MODEL OF ANALOGY

Later in the chapter the two concepts metaphor and analogy will be drawn together to form the basis for a single powerful addition to systems 'problem solving' methodology. Yet there is much to clear up before we reach that happy position. For now we will continue to explore analogy and metaphor in separation.

Essentially, we have established that analogies involve a set of objects or entities real or imagined which are familiar. We will label these *'the analogue set'*. In the preceding chapter we suggested that this conceptualisation might map onto Leatherdale's conceptualisation of *'topic analogue'*. In the sciences the analogue sets are typically associated with a theory [or hypothesis]. This marks a clear development of a model of analogy and separates the use of analogy in this context from analogical argument in 'the ordinary sense', which I take to refer to the quasi-formal structure outlined in Chapter Three. The role of theory and hypothesis in this model directs the analogy although remains principally heuristic. We might again refer to Leatherdale's confidence in the utility of analogy in science,

> *'I have argued that the basis of progress in science is not an analogical act in the ordinary sense, but an analogical perception which involves the importation of analogues from discrete areas of experience into areas of experience under investigation,* [the role of theory or hypothesis is clearly evident here] *with a resultant reformulation or re-ordering of the area under investigation so that hitherto unremarked analogies are seen and novel inferences suggested'.* [op.cit. p32]. Brackets added.

The above quotation is illustrative of the particular interest we, as systems thinkers, are assigning analogy. The passage and the model explicitly introduce and emphasise the matter of theory extension across domains as facilitated by the use of analogy.

It is not being suggested that scientific development rests solely on the exploitation of analogy and analogical thought and that induction and deduction are not additionally important in their contribution to the model. It is proposed that the theory and the analogue set together in this model constitute the necessary ingredients for a *'type of inference'* from the analogue set to another, 'real' or imagined, set of objects or entities.

In the analogical model, we will label this set *'the new domain'*, and the type of inference, analogy. It is suggested that Leatherdale would name the process of analogical iteration and the new domain together, *the imported analogue* because he finds it unnecessary to separate out the specific analogical process and the area of the new domain. It is preferred in this discussion, that these two areas be delineated, as we have established above the requirement to make clear the process of analogical modelling for systems thinkers and also to meet the concerns of Lektorsky and Sadovsky cited. This does not infringe on the acceptability of Leatherdale's conceptualisation of the dynamics involved in a model of analogy. In this thesis the delineation is marked to stress the dynamism of analogies. This is crucial to the theoretical exploration of analogically juxtaposed domains and rationalities discussed later in the thesis. Leatherdale stresses,

> *'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 topic and imported analogues. How much or how little is contributed by the imported analogue 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 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...'* '[op.cit., p22].

This passage implies several features on the model of analogy we are to develop for systems thinkers. Firstly, that in optimising the contribution which the imported analogue [I use here Leatherdale's term because I am referring to the concatenated analogical act] is to make, then we must make explicit the areas in which we hope to learn by developing the analogy. Further, Leatherdale talks of 'a new relation' and this in our model is identifiable in the framework of the 'central theory/hypothesis'. The latter links analogue set and new domain, although because of the 'expansiveness of analogy', the investigation is not restricted by the paradigmatic implications of the theory/hypothesis itself. [This is a matter of rationality and will be discussed in more detail in the following chapter].

Lastly, Leatherdale also talks of 'new descriptions and causal details' with which systems thinkers, in developing their systems world view are naturally concerned, and again this will be specifically pursued in the following chapter on 'Analogy and Rationalities'. For the moment we may very simply state that, in this conceptualisation, a model of analogy will contain three elements; a theory, an analogue set, and a new domain.

Clearly, a model of analogy may perform in a variety of ways. Carloyle, [1971] suggests that analogies operate within what he calls a range of analogy;

(a) as revisions of the new domain;
(b) reinterpretation of the associated theory;
(c) a basis for inference to new facts about the new domain

(d) a mediation between the analogue set and the new domain
(e) as a result of (a), (b), and (d), the extension of the theory to the new domain.'

In other words analogy may work 'forward' to revisions of, or to new relations or thereby 'facts' about, the new domain, or under certain circumstances, I would also suggest the analogue set. It is also hence possible analogy to act in such a way for re-interpretation of the associated theory to result. In a similar context, Leatherdale has commented,

> *'...the use of imported analogy ...is likely to be concerned with analogy of relation and hence with comparatively abstract properties and relations and with mathematics in particular.'* [op.cit., p32].

In a broader sense analogies mediate between the theoretical language, and the observation language initially used to describe the new domain. This, it is suggested, is a fundamentally important aspect to the utility of analogy in systems sciences. It is at such a stage that it becomes quite clear that in mediating between 'theoretical language' [we might assume such to be taken as literal] and observations taken in an unfamiliar domain, metaphorical use of language will be inevitably involved. We have previously argued that the use of metaphorical language in science inherently involves using terms in new ways leading to novel insights and interpretations and the extension of vocabulary and also subsequently, conceptual boundaries. Nevertheless, for now the important result is the 'extension of theory'. With this notion of extended theory we have made an important distinction between models and analogies.

Within the listed functions of analogical models just presented two aspects warrant further discussion. First are the principles which are used in analogical model building, discussed in the next section. Secondly, obviously, concerns the grounds on which we are asserting that analogical models promote theory extension into new domains [discussed in the three sections following the next].

4.3.1 *Principles Of Analogical Modelling.*

We have already identified the following three components of analogical modelling quite simply; the analogue set, the theory/hypothesis and the new domain. It is suggested that the analogue set and the theory are to form the basis of iterative inference to the new domain. Such iteration in analogical modelling is shown in Figure 4.3.1a. The following commentary on that figure explains in detail the principles of analogical modelling.

a) By processes of deduction and induction postulates of the theory allow the initial deduction of the analogue set.
b) From the analogue set, properties of positive, negative and neutral analogy are induced to the new domain.
c) Investigation of the new domain identifies areas of positive, negative and neutral analogy induced from the theory and the analogue set.
d) From the investigation, the information concerning the new domain, and by analogical reasoning also, the theory and the analogue set may be deduced allowing extension of the theory in the form of a new hypothesis/postulate.

The process may begin again from the postulates of the extended theory. It is therefore iterative.

Figure 4.3.1a appears on the following page.

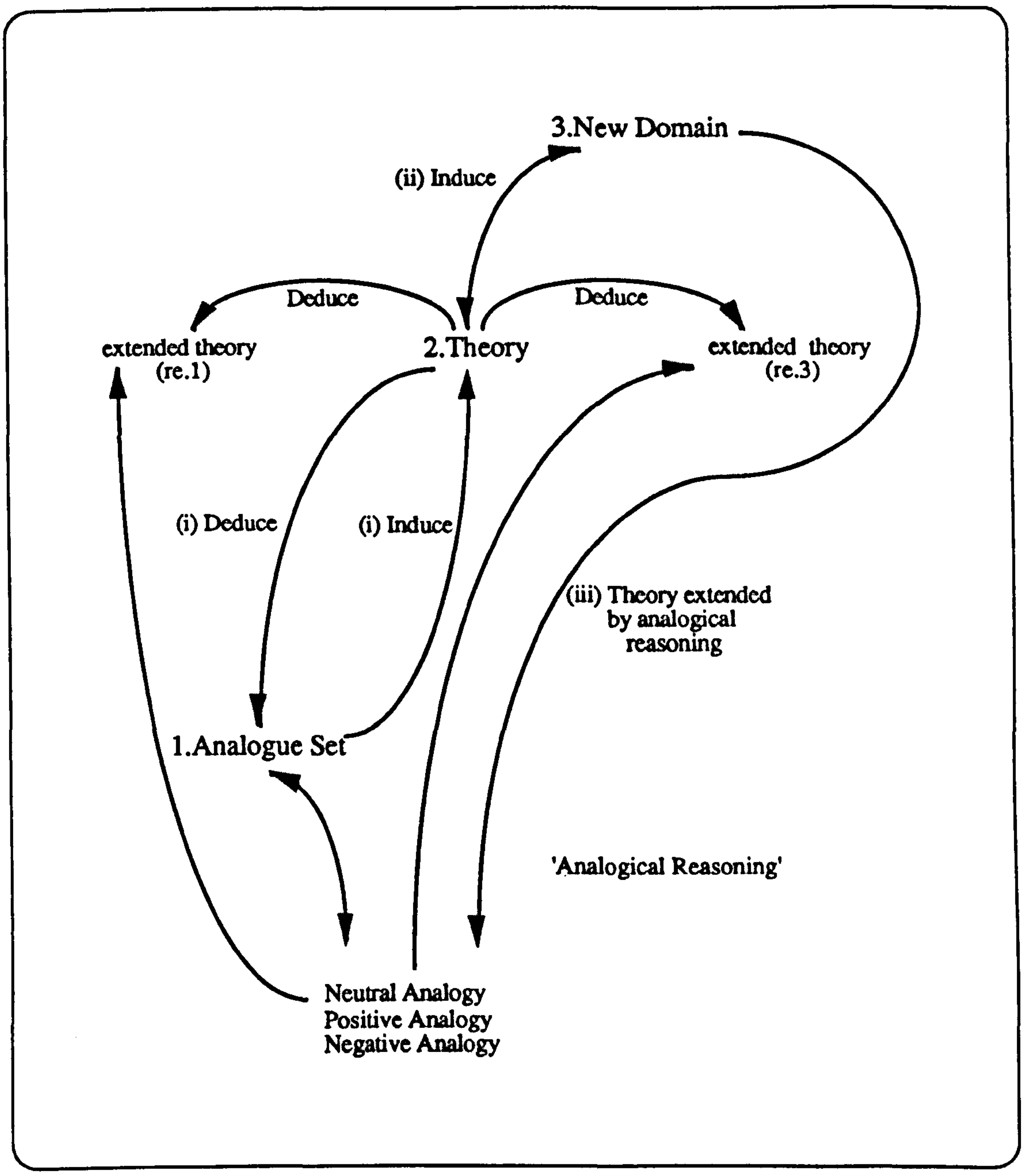

Figure 4.3.1a: Iteration in an Analogical Model.

4.3.2 *Analogy As A Metaconcept: Overcoming Difficulties Of Deductivism*

One function of analogical models, discussed above, concerns their role as a mediation language between those of observation and theory used initially to describe the new domain. In systems terms this function suggests that argument from analogy may be a type of meta-argument [reasoning], i.e. analogy is reasoning of a higher order, in which propositions of inductive and deductive reasoning may be discussed. This may be taken as a fairly radical proposal since the principles of deduction have a sound base in logic and one in which critics of analogical models have established their arsenal. The essence of this notion of meta-language is captured in Beer's [1981] understanding of metasystem which is;

> *'...a system over and beyond a system of lower logical order and therefore capable of deciding propositions, discussing criteria, or exercising regulation for systems that are themselves logically incapable of such decisions and discussions... because metalogic is inaccessible to the systems logic...'* [op.cit p57].

We need only to replace the word 'system' with 'reasoning', and 'regulation' with 'theory extension' to find the passage reading as follows,

> *'... a reasoning over and beyond a reasoning of lower logical order and therefore capable of deciding propositions, discussing criteria or exercising theory extension for reasoning systems that are themselves logically incapable of such decisions and discussions...'*

By such an interchange the implication is that we are proposing analogy can operate over and beyond induction and deduction. But note, the 'dependence' of analogy on induction and deduction, is made clearly evident in the systems viewpoint, which would emphasise that analogy cannot exist without induction and deduction [i.e. the necessary support of a lower order system]. In this context Hesse [1966] reflects the same point when she noted that it is not possible to extend theory to a new domain by deduction only, since no theoretical term links the axioms of the theory to the terms describing the new domain.

4.3.2.1 Grounds Of Theory Extension

Those who agree with the significance of analogy as a basis for the inference of new facts about the new domain, take the so-called argument from analogy as their supporting evidence [suggests Carloyle, 1971], i.e., what we have identified as the pseudo-syllogistic inductive inference from one set of particulars to another. Usually the premises assert that a set of observed instances have certain common properties that presumably are identified by the role of theory in the analogue set. Conversely different properties may also be taken as of interest under certain circumstances.

A scientist may work on the notion that an ascribed domain exhibiting some aspects of similarity with the analogue set could have the other features also. Hence the probability of the conclusion being true is exclusively determined by the conditions of parallel identity of properties, [or conditions of 'positive analogy', which will be fully discussed shortly].

This view clearly pre-supposes that the basis of inference from the model to the new domain lies only in the identity of properties between the new domain and the analogue set. In this argument I would be inclined to support Carloyle's position, by contrast, which in emphasising 'similarity' of properties, is necessarily more concerned with analogy of relations. The argument from theory to new domain, in this model of reasoning, is a modification of the 'classical argument from analogy', covered earlier.

We can usefully discuss iteration of this analogical model further by considering Campbell's view [also analysed by Hesse, op.cit.Chapters 1&2] on whether the utility of analogy holds for theory extension. Campbell's illustration is the linking of water, sound and light phenomena through the concept of 'wave'.

In this discussion it is sufficient to concentrate on water and sound only, since the intention is to show what happens when we attempt to make use of the known theory waves in the *'analogue set'* of water, and the putative analogical relationship with the *'new domain'* of sound. The process of analogical reasoning summarised in Figure 4.3.1a will be drawn upon in the example of Figure 4.3.2.1a. Here we see the generalised model on the left hand side, and our example on the right, where features of water have been taken for the analogue of sound. The theory of waves in water is used to reconceptualise sound, i.e. the formal theory of water waves is extended to those assumed of sound.

The diagram is given on the following page.

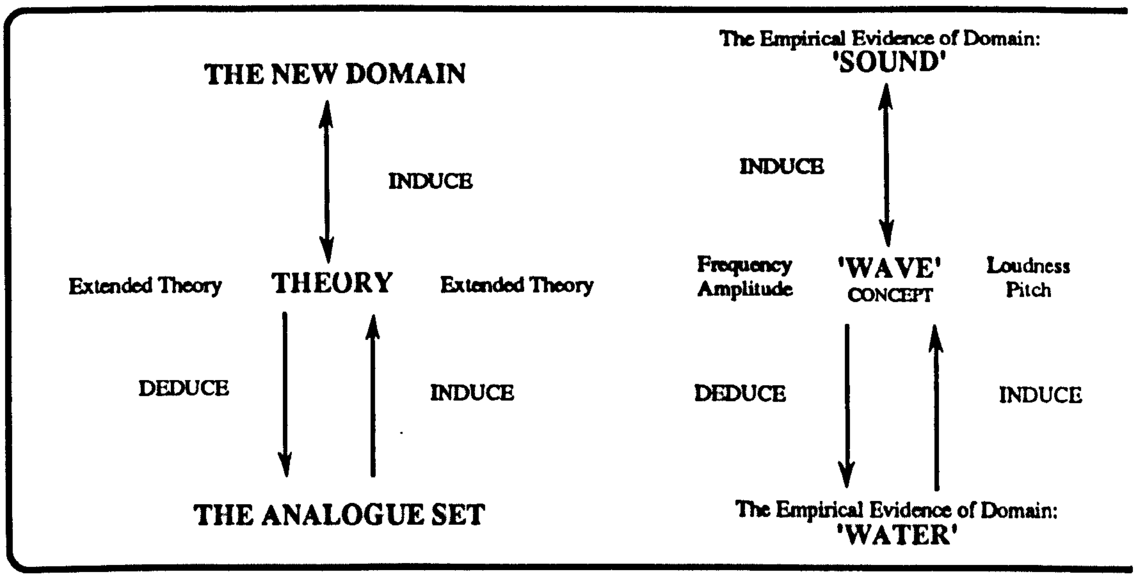

Figure 4.3.2.1a: An Analogical Model of water and sound.

By looking at the water wave theory, the analogies would suggest that sound is produced by the motion of air particles propagated in concentric, spherical waves from a centre of disturbance. We know that increasing the central disturbance of water will give rise to higher waves and so the analogy suggests that the greater the disturbance of gongs, hammers and so forth, the 'louder' the noise produced. Hesse [op.cit.] supports Campbell's assertion that it is then a 'short step' to identify the loudness of sound with the amplitude of sound waves and empirical experiences with strings of various lengths to persuade us that 'pitch' of sound waves could be mapped to the frequency of sound waves.

In other words, frequency and amplitude, part of our analogue set, were not initially reflected in the theoretical concept of sound wave [which would be derived from empirical evidence of water]. Significantly, a re-description of the original analogue set is shown as necessary for empirically based theory extension to the new domain, i.e. in terms of sound phenomena. This example is hence clearly illustrative of the introduction of the metaphorical use of language, facilitated and guided by the model, and a divergence from the literal meaning of the terms 'frequency' and 'amplitude' into metaphorical senses of 'loudness' and 'pitch'. Re-description thus requires a 'shift in the meaning' of these terms, which is the particular feature of metaphor as outlined in part with reference to I.A. Richards, in the preceding chapter. Although in the context of the argument being developed in this chapter, this is clearly evidence of meta-reasoning or analogical reasoning.

We ought to consider the criticism, rooted in that expressed by Duhem [op.cit.] who suggests that such a presentation of the model glosses over the issues centring on matters of observation, since he would raise the point that the 'disturbance' of air particles are eminently less 'observable' than the disturbance of water. Hesse [op.cit.] cites what she dubs a 'Duhemist' viewpoint as follows,

> *'To 'observe' a similarity between the behaviour of ripples at the edge of the bath and the behaviour of sound in a mountain valley is far from a superficial observation... It requires a very sophisticated framework of physical ideas in which, for example, the phenomena of echoes are described in terms of a train of physical causes initiated by a shout rather than in terms of an imitative spirit of the mountains.'* [Ibid].

While we were exploring notions of analogy in the previous chapter we noted that although the utility of analogy might appear to come 'in a flash' or to be in some way 'intuitive', it very often depends on a great deal of preceding research. Preceding work is not altogether negated by the sudden insight the perception of an analogy can facilitate. But more importantly, the Duhemist position can be used to add clarity to a point we have raised to some degree already. Campbell uses the criticism to highlight the important philosophical point that contrary to what empiricist philosophers seem to maintain, observation statements are not written on the face of events to be transferred directly into language. Instead, they are already interpretations of events and the kind of interpretations they are, depends on the assumptions of a language community.

In other words Campbell is going so far as to posit that there can be no descriptive statement which does not go beyond what is given in the act of observing. He is implying that language is inescapably metaphoric, a view we too have been unable to avoid elsewhere.

Further, there are more aspects to emerge from noticing that the observation language of the analogue set [of water] refers to observable properties which cannot be derived visually in the domain of sound. Hence, in some way a correspondence between the observable properties of the analogue set and those properties being investigated in the new domain of sound has been set up. One might easily see this as evidence that it is the analogy of relation, rather than that of property, which should take precedence.

Indeed, Campbell's suggestion that, at this point, we are then in a position to test the mathematical theory of water waves as a theory of sound supports this view. Any subsequent tests will show or not show that the theory is satisfactory. The importance point is not that the use of analogy can lead us to an infallible theory but that it can be most useful in suggesting areas to be investigated.

The theoretical component 'wave' has been seen to promote the re-visualisation of sound as waves and as a result, the formal theory of water waves may be extended to that assumed of sound. This is significant in that the argument also indicates that some of the characteristics of, for example, frequency and amplitude have been abstracted away through induction to a more abstract and idealised level [which is also typical of systems conceptualisation] in order to describe the new domain and hence expand the theory.

To return briefly to Duhem's line of argument, he would suggest that the theory of water waves, for example, was arrived at by making an hypothesis about the effects of a disturbance in the medium. The investigator then expresses this in mathematical language and deduces from it the observed properties of water waves. In other words, there has been no mention at this juncture of models or analogies.

Hence in the case of sound, a one-to-one correspondence between properties of water and properties of sound could be set up and then the mathematical wave theory is transferred to sound. The Duhemist would suggest that this might be a way in which theories are arrived at in practice, but that nothing in this example shows that reference to the water model is essential, or that there is any difference in principle between the relations of the theories and observations in each case. This assertion attempts to undermine the quality of re-interpretation of theory by analogy that we have been striving to establish.

Put another way, the Duhemist line is such that if we had never heard of sound waves, we would still be able, with our collection of observations in the sound domain, to obtain the same result. The 'information' obtainable is derived from the observed production of 'sound' by the collisions of solid bodies, the relations between the magnitude of these collisions and the loudness of the sound, and between lengths of string and pitch of note, echoes and 'reflections'. The Duhemist would then suggest that all of these observations could be deduced from a mathematical wave theory with the appropriate interpretation and without any reference or mention of the water wave model, and without supposing that there is any connection between the transmission of sound and ripples in a pool.

Hesse [op.cit.] proposes that Campbell at this stage would make an important point in defence of models in general and suggest that the reason for this apparent lack of necessary 'mappings' was not that there is no wave model at all, but merely that ripples are the wrong wave model. This highlights what many [e.g., as we have seen in the context of General Systems Theory, Buck 1956], consider to be a most dangerous side to building theory with the aid of analogical models. Namely that reference to a model is not part of the logical structure of an explanatory theory and not even always a useful device in deriving such theories because it can positively suggest the *wrong* theory.

It has already been suggested that analogy and inferences made on the basis of analogy are among the most basic judgements that we make in distinguishing between objects; that is, we identify similarities and dissimilarities between them. In this vein, Bunge [1979] has noted,

> *'Judgements of analogy underlie a number of other types, in particular, collecting or grouping and inductive generalisation.'* [op.cit., p210]

Bunge also suggests that a danger of analogy is such that although initially stimulating, reliance on analogy can 'block' further research. Hence,

> *'We tend to remember only the fruitful analogies. The list of misleading analogies, if it could be drawn, is likely to be more impressive.'* [Ibid.]

Of course, this assertion need not take us by surprise and does no more than reflect the superficial understanding of manifest analogy. It is typical of criticisms which, quite rightly, are directed to the unstructured exploration of analogy, which at best can be said to lead to a growth of knowledge based on circumstances of coincidence and at worst to a highly chaotic research programme. [It is also in some ways appropriate to the adoption of analogy and metaphor by systems methodologists.] Bunge goes on to point out that the significant contribution of analogy rests on a strict adherence to the developed concept of analogical inference rather than mere analogical 'judgements'.

In fact, if anything, this criticism is more appropriate to the syllogistic format in which analogies have often been examined in the past. Analogical inference, although involving analogical judgement goes beyond the latter,

> *'...from the fact or assumption that two objects are alike in some known respects it 'concludes' that they may be analogous in some unknown respects also.'* [Ibid, original emphasis].

Naturally, the clear danger in analogical judgements, because of their syllogistic format, is that logical and 'rational' implications are in danger of being taken wholesale from one domain to another. With this in mind we can examine an example given by Bunge, who asserts,

> *'... organicists, likening society to the human body conclude the workers, being the muscles of society, ought to obey the brains of society, namely its ruling class. And Copernicus, likening the solar system to a monarchy, argues just as the monarch is surrounded by his vassals and courtiers so the sun must be at the centre of the solar system.'* [Ibid.]

Bunge argues that these analogies are 'faulty' because they rest on 'wrong analogy judgements'; society is not like an organism and the solar system is not like a monarchy. These he gives as 'implausible analogy judgements'. By contrast Bunge cites Aristotle's classical analogy, *'Gills are to water what lungs are to air'* and suggests that this is an example of a 'plausible' analogy and therefore fruitful, because it rests on a 'genuine' analogy; lung-gill, water-air.

It is suggested that Bunge's attempt to identify 'genuine analogies' or to distinguish between 'plausible' or 'implausible' analogies both imply a literal, realist position untenable with the description of analogies given here and not helpful in developing a useful model of analogy for systems thinkers. Rather it is much more valuable to examine Bunge's examples of 'implausible' analogies. We can note that in either case a form of interpretation of the analogy has been carried into a new domain without making clear the rationality that held the interpretation to be useful in the original analogue set. The syllogistic format is shown to be quite unable to identify or penetrate the rational basis for interpretation in either case. It is in this context which we will be examining the systems view of analogy and rationality in the next chapter.

We might leave this point for now and reiterate Bunge [1979] whose remarks hint at the role of rationalities in the interpretation of useful analogies. Thus, *'The fruitfulness of an analogy depends not only upon its degree but also on the problem at hand and the brain that uses it. Many people had noted the analogy between man and other primates; in the middle ages it was a common belief that monkey was a fallen man. However it took a Charles Darwin to use this analogy to place man and ape in an evolutionary perspective.'* [1979, p213].

Similarly, from Leatherdale

'... 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 with are resemblances of motion and these in turn are analysed into resemblances of change and motion and relative directions in space and even more abstractedly still, into a number of relationships or geometric ratios.' [op.cit., p25].

We evidently need to further discuss the roles of models, specifically analogical models in issues concerned with the establishing the grounds of theory extension. Clearly, the problems outlined by the Duhemist perspective reflect the relationship between theory and model. Therefore we will return to Campbell's description of the role of the billiard ball analogy in the development of the kinetic theory of gases, first mentioned in the preceding chapter.

Campbell in his analysis, introduces a 'range of analogical conditions' which illustrate the potential to explore an 'imported analogue' in terms far more insightful and complex than ordinary forms of inductive inference. In systems terms, the concepts indicate the emergent potential of adding analogical reasoning to a traditional inductive and deductive basis. Campbell posits,

'Let us call those properties which we know belong to billiard balls and not to molecules, the negative analogy of the model. Motion and impact on the other hand, are just the properties of billiard balls that we do want to ascribe to molecules in our model and these we can call the positive analogy. Now the important thing about this kind of model thinking in science is that there will generally be some properties of the model about which we do not yet know whether they are positive or negative analogies and these are the interesting properties, because as I shall argue, they allow us to make new predictions. Let us call this third set of properties, the neutral analogy'. [Quoted by Hesse, op.cit., Chapter One].

Campbell asserts that the terminology of positive, negative and neutral analogy avoids ambiguity in the iteration of an analogical model. I will suggest that the contribution made by these three concepts goes somewhat further than this for systems thinkers in the next chapter on Analogy and Rationality. For

Campbell, the delineation of these concepts, mark a point of distinction between analogue and theory so that should the investigator conceptualise of a model of gases, s/he can distinguish between billiard balls, and the *'picturing of gas molecules as ghostly little objects having some but not all the properties of billiard balls'*. In other words in referencing the theory, we can distinguish between the model and the theory itself.

Additionally, the need to introduce a discrete terminology illustrates an attempt to control the 'shifts in meaning' in the use of language in this field. Campbell stresses the importance, at times, of using the words to give a meaning to the mathematical symbols. His principal concern, as we saw in the preceding chapter, is in making theories interpretable. Hence, for Campbell the importance of the model is such that under certain circumstances the words then derive their meanings from the position of corresponding symbols in the deductive systems. It is a peculiar account of meaning giving an indirect meaning to any word that can be inserted in a deductive system.

The Duhemist commentary on the point is to suggest that for any theoretical term to have scientific meaning, it must occur in a deductive system which has many observable features. The identification of the nature of the 'observable features' of a deductive system is entirely a question for empirical research and not one for the logician, so that a theoretic term has meaning only in terms of an empirical interpretation and that meaning cannot be logically formalised.

This arises because the Duhemist stresses there are cases where the theory may not be describable in terms of models at all and hence in order to assert the existence of a theoretical entity, we must either coin new words or give old words a new significance by the method of indirect meaning... The concept 'aether' was adopted and given significance in this way; there were some physical theories which 'the aether' had a defined place in a deductive systems and hence apparently observable, empirically testable properties. This assertion could be interpreted to serve either side of the argument! Either for the necessity of analogical models to facilitate the introduction of metaphorical language, or for their dangerous consequences. The Duhemist position appears rather paradoxical.

But again, we find ourselves discussing the nature of observability which we have already discovered is interpretable only through a framework and hence, we can argue, quite clearly capable of continually shifting its ontological and epistemological boundaries. In this context we are obliged to ask ourselves whether concepts founded in solely in empirical, experiential frameworks are of particular interest in systems inquiry. Bunge [1959], in this vein notes that;

> *The scientist is fully entitled to speculate about non-experiential facts, that is, facts that at a given level in the development of knowledge be beyond the scope of human experience, but then forced to point out what experience warrant the inference of such unobservable facts, that is he is obliged to anchor his factual statements on experience somehow connected with the assumed transempirical facts.'* [Chapter One].

We should also, after Bunge note in this context, the theoretical and practical value or consequence of a few distinguished 'unobservables', such as consciousness, history, class struggle and public opinion.

We can further elucidate Campbell's position in the following way, by looking more closely at the conceptualisation and definition of model in terms of Model1 and Model2 introduced in the previous chapter. Hence, Campbell asserts that when we speak of a model in its primary sense, [M1] that is, we are not speaking of another object which can, as it were, be built or imagined alongside the phenomena we are investigating.

The idea of an analogical model is not one of a more or less imperfect 'copy'. We are thus expurgating from philosophy, the unwanted legacy of the concept of analogue inherited from the physical sciences. Rather, Campbell's view on the utility of analogical models is generally that they are a way of imagining the phenomena 'themselves'. In other words M1 is the imperfect copy [the billiard balls, in the analogy discussed above] minus the known negative analogy, so we are only considering the known positive analogy and the probably open class of properties about which it is not yet known whether they are positive or negative analogies.

This stance will bring us to a point of contention with Campbell's analysis of the utility in theoretical development of conditions of analogy. Campbell in his conception of an analogical model places most emphasis on positive and neutral analogy and goes so far as to suggest;

> *'When we consider a theory based on a model as an explanation for a set of phenomena, we are considering the positive and neutral analogy, not the negative analogy which we already know we can discard'.*

This suggestion will be disputed later in the following chapter. It concerns the utility of analogy in making distinctions between 'theory' and 'model' and the tendencies of Duhemist critics and metaphysical philosophers to, after Campbell, use the term 'theory' to cover [only] what is called positive analogy. This, it is suggested, leads to neglect of the potential of the neutral analogies in a model to serve as growing points in a theory and heightens the necessity to consider, as we shall, the additional validity of negative analogy. The thrust of the argument rests on a conceptualisation of theory as in perpetual cycles of growth and development, not as static and formalised theories corresponding only to known positive analogies across domains.

A further point in this context can be emphasised if we return for a moment to how we described analogies as behaving as a mediation language between those of observation and theory used initially in describing a new domain. From this came the argument that analogy may be a kind of meta-argument, that is a reasoning of a higher order in which the propositions of inductive and deductive reasoning might be discussed. An important point in this context is the distinction between theory and models which analogies facilitate. The word 'theory' is used by Duhemists to cover quite a wide field, especially the formal deductive systems which have only partial interpretation into observables.

Clearly, analogical models are mechanisms to ease this process, because they offer at least two sources of information to allow theoretical development. Firstly the observed or investigated properties of the phenomena in hand, [the explication of the analogue set dictated by the hypothesis] and secondly, the observed/investigated properties of their analogies. The philosophical appeal to observable events is a matter which must be addressed at a further level and its exploration would be a task well beyond the context of this thesis.

The issue is significant, however, in that Hesse [op.cit.] and indeed others have stressed that analogies may not be noticeable until they are 'spelt out' and it is unlikely that this would be possible without rendering some properties at least observable in some sense. But when analogies have been pointed out, no esoteric insight, no specific scientific knowledge is needed to recognise that they exist.

But matters are different for the Duhemist and is evidently not the case with mathematical formalisms since obviously in these areas 'background knowledge' is prerequisite. The point to notice is that while trigonometric knowledge might be required to understand a mathematical theory of water waves, there is no intrinsic difficulty in comprehending the terms 'height of the water', 'frequency of the waves', into which the mathematical symbols are interpreted. It is in this sense that it can be said that the mathematical system in this example is about or has its interpretation in terms of observable events.

But the principles by which analogies are identified in this way remain fixed and are concerned with the attempted construction of a one-to-one correspondence between the 'observable' properties of sound and those of water waves. [This is the role of the theory/hypothesis in the formal analogical model...]. At that stage it becomes possible to test the mathematical theory as a theory of sound. Hence the process of testing will show or not show that the theory in that domain is satisfactory. And again it is necessary to seriously note that it is not claimed that the use of analogy can lead to the development of an infallible theory, all that is claimed at this stage, is that it is of value in the formulation of hypotheses.

Hence in an analogical model, the implication is that there are at least two types of theory extension going on. Firstly theory extension as in explication of the analogue set in particular and secondly in the iteration of the analogical model in general. Horizontal expansion of theory to the new domain was shown in Figure 4.4.2. The development of theory along the horizontal plane in the model represents information resources released by drawing an analogy between two domains. The supporting argument, however, suggests a vertical shift in theoretical terms to a more abstract level and this represents theory development along the vertical plane of the model. This kind of theory extension describes the [metaphoric] *'shifts in meaning'* of explicated theoretical concepts to accommodate an increased domain of appropriateness in the 'universe of applicability' for the theory or hypothesis explored via the analogical model.

Analogical models maintain their integrity in this 'two dimensional' shift only if we introduce the new concept of neutral analogy, that is, in addition to the classical view that analogy comprises only positive and negative aspects. Hesse

[1966] argues for this theoretically important concept to provide a proper basis for inference,

> *'...the properties on which the inference is based are neither the same, nor different, but similar.'*

Until the model has been confirmed as a basis for inference, which, for instance, Buck's [op.cit.] manifest black and white analogy could not, significant analogical 'resemblance' cannot be classified as either positive or negative, and must therefore be considered neutral. We can thus support Carloyle [1971] in linking these points with Dewey's thesis that identification of properties is based on their evidential role in inquiry rather than the notion that they are an ontologically prior fact. It a further point to establish language as inherently metaphoric.

Therefore we might sum up this section by asserting that analogical models, then, are able to support an interpretation of theoretical terms which acknowledge their referential function. Additionally analogical models can cope with the difference in logical [or more broadly 'rational'] character across the logical gap, by co-ordinating and guiding the revisualisation of the analogue set and hence the expansion of the theory by analogical meta-reasoning.

As we have seen, the argument is supported in Campbell's [1957] theory of models in which he tackles the deductivist's view of the structures of scientific theories. We have noted that for Campbell, a theory must contain two things. First, the 'dictionary' correlating some [but not necessarily all] of the concepts with empirical terms. Secondly, a model for the theoretical statements of a theory so that its hypothetical subject matter may be imagined to be like in some [but again not all] respects the 'real' empirical subject matter of some field that is already known. This is unequivocally analogical in approach.

Campbell [1957] inevitably, also sees the

> *'... driving force of science as the exploitation of analogies in new fields without which neither theory, nor the range of facts could grow nor the language in which to express them develop'.*

Essential to this view is the idea that for a range of phenomena [say, 'E'] to have occurred, the relationship between E and the theory [say, 'T'] must be supplemented and informed by another relationship, i.e. analogy, rather than deduction. It is through analogy that we render T intelligible to ourselves, i.e.

models have something irreducible to the experiences they are, in some way, intended to embroider or explain.

The above discussion has looked at some of the grounds for theory extension in a model of analogy and outlined some inadequacies of deductive reasoning. We will now focus our attention on the associated difficulties of inductivism.

4.3.3 *Analogy As A Metaconcept: Overcoming Difficulties of Inductivism*

Traditionally, analogies have been recognised as a part of inductive procedures akin to generalisations, that is, induction by simple enumeration. Bacon [op.cit.], whose view of analogy we discussed in the preceding chapter, was opposed to the use of generalisation on the basis of their uncertainty, which rests on the smallness of the sample, compared to the infinite sample of all possible cases, although one might suggest that this objection has more to do with the problematic nature of induction.

We proposed earlier that the classical positivist view insists that models may only be regarded as heuristic devices, i.e. they are inductive. This is refutable because analogue models cannot be replaced by generalisations from instances. In analogy, there is only one instance referred to in the premises and hence the probability of the conclusion being 'true' is determined by the conditions of the analogy only. Returning to Carloyle [1971],

> *'If there is a causal connection between the positive analogy observed to be common to premises and conclusion on the one hand and the remaining properties inferred to hold for the conclusion, the probability is increased.'* [op.cit p562].

In this sense analogies may be presented as superior to generalisations in the attainability of a higher degree of certitude for their conclusions.

Agassi [1964] follows this argument to a point, but prefers instead to examine analogies as either 'generalisations proper', perhaps higher level ones, or ad hoc. His examination suggests that analogies are not seen as 'more certain' than generalisations but are equally legitimate. This line of argument is worth some further examination. We have seen that induction plays a significant part in the iteration of the analogical model we have described, indeed it is intrinsic to the processes which take us from the analogue set to the new domain.

Traditionally, however, extensions of known observation reports to unknown cases are taken to be of two kinds. Firstly from the sample to the whole 'ensemble', and Agassi [op.cit.] takes these to be 'generalisations proper'. Secondly, the extension is from the sample to the next case to be observed. Agassi hence notes,

> '*... all intermediate terms between these two extremes are possible, but inductive philosophers have hardly paid any attention to them. The reason may be this: the principle of paucity of assumptions leads us to reject the generalisation, i.e., the extension of an observation report about the observed terms to the whole ensemble in favour of a less bold extension. The least bold extension is not to report at all... but as this will not do, the extension to the next case to be observed is advocated as a second best.*' [op.cit.p351].

This line of argument, if we think of it in terms of Carloyle's comment above serves to show the usefulness of modelling by analogy. Agassi himself suggests the mode of thinking he has described is erroneous and that any assertion about the 'next case' should be interpreted in the following way. Broadly, the next case to be observed will agree, or as we would suggest, correspond with the sample on the basis of the hypothesis that the sample is representative. In terms of our model of analogy, this is the same logical basis by which the analogue set was initially selected, [deduced].

A standard example given to illustrate the problematic nature of induction concerns the *'whiteness of swans'*. This example can also be used, however, in the context of supporting Hesse's assertion given earlier on the linking of axioms of the theory to the new domain. Although Hesse's point referred to deductive axioms, it also indicates the problems inevitably associated with a model of this sort, that is one geared to theory/hypothesis extension, based on inductive and deductive processes only. Hence, in attempting to establish the whiteness of swans, the hypothesis that the next swan to be observed will be white, cannot be an extension of the hypothesis [that 'all swans are white']. Rather it is a disjunction among infinitely many conjunctions about all the unobserved swans in the universe, and about our luck with the next observation. Agassi enlarges on the issue,

> '*... if we do not claim that all unobserved swans in the universe are white, and if we deny that our next encounter with a swan is of any special cosmic significance, then we cannot but ascribe the whiteness of the next swan to mere chance.*' [op.cit., p352].

The iteration of the analogical model, however clearly avoids the arbitrary extension of the theory by 'mere luck or chance'. The processes by which we might range over the analogue set and new domain are formalised in terms of the heuristic exploration of concepts of positive, negative and neutral analogy, concepts which rigourise the processes of analogical reasoning, as we shall be discussing at some length shortly.

Interestingly, in this context, Agassi makes a further point in favour of the utility of analogy over generalisation, although I do not think it is deliberate. He suggests, [op.cit.] that generalisation is often claimed to be more reliable when certain conditions are met. Although he admits that these conditions might change from author to author, he distils three conditions which he claims are 'traditional'. Firstly, that a sample should be taken at random, [a]. Secondly, that it should be relatively large, [b] and thirdly that the ensembled populations be characterised by a stringent set of properties, [c].

Agassi goes on to argue when the second condition is met, we tend to speak [loosely] of generalisation. Additionally, when the third condition is met, we would tend to speak of having recognised an analogy, although we would have emphasised by contrast that the relations of analogy play a more significant role than that of properties. But lastly, the most important point to note, is that we can never know that any sample is taken 'at random'. Agassi suggests,

> *'In any case, the form of a generalisation which meets [b] more readily than [c], is the same as the form if one goes the other way. Hence, one cannot ascribe more reliability to the one or the other and objections validly applicable to one are equally applicable to the other'.* [Ibid].

It is strongly suggested that because condition [a] is intrinsically problematic, the validity of apparent generalisation derivable from [c], with the caveat that analogy of relation is more significant than analogy of property, is of a higher order than inductive generalisation based specifically on the requirement that a sample is [relatively] large, whatever compromises that might mean or empirically involve.

According to the inductivist, science starts with observation since observation apparently supplies a secure basis on which to build scientific knowledge, i.e. it is derivable from observation by induction and described by literal use of scientific language. We have already argued that science is more than the collection of observable fact and in this context we are obliged to ask at the rudimentary level whether the principle of induction can be justified? Inductivists themselves attempt to show that it can in two ways, by recourse to logic and by appeal to experience.

In the first instance, inductive arguments have a dubious connection with logic. Repudiation of this naive inductivism is well documented. As we described, Agassi [1964], for example, highlighted the difficulty by referring to the whiteness of swans. The inductive statement that the next swan observed will be white, because all swans so far observed are white, was seen to be too arbitrary, giving *ad hoc* a special significance to the next observation.

On the second account of appeal to experience, repudiation is equally simple since the argument purporting to support inductivism is, in itself, inductivist. In systems terms we could say that it is the inaccessibility of the metalogic to the systems logic which causes this problem.

There is also a further difficulty in determining the number of observation statements that are required but on more complex grounds than merely deciding what constitutes a 'relatively large sample'. For example we must also consider the variety of contexts to be explored, before an inductive inference can be derived. It is here that analogical modelling can contribute to the inductive method. Chalmers [1978] suggested that we could overcome this expansionary difficulty [that is, in making decisions about interesting phenomena] inherent in induction by appealing to our theoretical knowledge of the situation. To do this however would be to admit that theory plays a vital role prior to observation. Such an admission would be alien to inductivism but is explicitly recognised in the analogical approach.

4.4 INITIAL CONCLUSIONS OF THE PHILOSOPHICAL DEBATE

The preceding sections have shown how analogy may be used as a method of meta-reasoning, which is important for the development of systemic ideas while a model of analogy retains the advantages of induction and deduction.

It is further suggested, however, that such an argument requires a 'new paradigm' to accommodate a different ontology and premises. There is, for example, an interpretative view that refutes the notion of 'real' structure and form in social contexts and hence might be fruitfully examined through the framework of analogy. Thus the whole process of analogical modelling may be used, not only in 'real world' thinking but also, in the abstract world of systems thinking, a point to which we will necessarily return. This latter area is meta-disciplinary and therefore lends itself to meta-reasoning.

Another key point that emerges is that the role of theory is greatly emphasised and if we consider this in the context of the systems paradigm then the theory extension, the broad unifying goals of cybernetics and General Systems Theory must take the fore [Flood and Robinson, 1989].

We are therefore interested in metaphor in systems thinking, and this will be discussed in the next section. Following that discussion we will move on to consider analogy and metaphor outside the functionalist paradigm [next but one section].

4.4.1 *More On Metaphor For Systems Thinkers*

In the preceding chapter we introduced a helpful discussion on metaphor and its relation with analogy, drawing upon the work of Richards [1936]. Therein, we were informed that much of our everyday life is metaphorically based. We all live and speak only through our eye for resemblances. As individuals, he suggested, we learn our command for metaphor in the same way that we learn whatever else makes us distinctly human.

Metaphor was described as a borrowing and intercourse of thoughts, a transaction between contexts; thought is metaphoric and proceeds by comparison whereupon 'meaning' is achieved. The metaphoric mechanism of meaning transference, as we saw, is a source of complex theorising in recent years and there is evidently an important contribution to be made by the systems concepts of 'emergence' and 'synergy'. Additionally, there is support for the idea of metaphor as a meta-concept, working in the same way as analogical reasoning. Further, metaphor reflects Campbell's points made earlier on the role of models for theory development and may be analysed as the 'language' required for the exploitation of analogies which he saw as the 'driving force of science'.

For our purposes, metaphors can be distinguished into two classes;

a) those that work through direct resemblance and
b) those that occur through some common attitude.

These involve an act of comparison. There are many different conceptions of comparison, of which, four are presented below;

a) putting together of the two ideas to let them work together;
b) a study of ideas to see how they are like and how they are unlike;
c) process of calling attention to likeness of ideas; and
d) a method of drawing attention to certain aspects of one idea through the co-presence of another idea.

At one extreme there is likeness, at the other extreme there are striking clashes where two remote ideas are forced together. This may be somewhat exaggerated, however, in terms of Burrell and Morgan's [1979] recognition of 'radical change' such disparity may be appealing for systems and cybernetic 'problem solving'.

4.4.2 *Metaphor And Theory Change*

At several earlier points we identified a contrast between the 'loose', open-ended structure of metaphor with the more formal features of analogies. Having demonstrated how the construction of analogical models can contribute to theory extension, some discussion here on the ways that metaphor may similarly act is appropriate.

In this context, Boyd [1979] proposed that there exists an important class of metaphors, that is, *'theory constitutive metaphors'* which play a role in the development and articulation of theories in relatively mature sciences. Such metaphors may be used to introduce theoretical language where none previously existed and their success does not depend on their conveying quite specific respects of similarity or analogy.

This is suggestive of some important relationship between the concepts of metaphor with neutral analogy. In this way metaphors can act rather differently to analogies by using theoretical terms explicitly with reference to the secondary subject [the *'new domain'*]. Analogical models, by contrast, were demonstrated as seeking evidential support, prior to a possible reinterpretation of the theoretical concept.

Typically users of this kind of scientific metaphor are unable to precisely specify the relevant respects of similarity [i.e. aspects of positive or negative analogy] and the utility of these metaphors in theory articulation and change depends very much on this *'open-endedness'*.

The initial open-endedness, or looseness, necessary in scientific metaphor does not mean that they may permanently resist complete explication. Such explication would indicate conversion of aspects of neutral analogy to positive or negative analogy and be seen as the essential consequence of successful research. The significance rests in the operational rationality. It is suggested that such conversion indicates when a scientific metaphor has become a scientific model.

Boyd further suggested that the most interesting metaphors are those which, for a time at least, constitute an *'irreplaceable part of the linguistic machinery of a scientific theory'*. These are the metaphors which are not merely exegetical but constitutive of the theories they express, those which scientists use in expressing theoretical claims for which no literal paraphrase is known. Theory constitutive metaphors may be seen as fundamentally pre-theoretical, yet as he has argued they are prevalent in new sciences.

We have also noted that scientific metaphors of this sort undergo the public articulation and development uncharacteristic of literary metaphors. They do share one thing, however, there is general agreement that their utility does not depend on the even tacit availability of an explication; in science their importance rests on the fact that they provide a way to introduce terminology for features of the world whose existence seems probable, but many of whose features fundamental properties have yet to be discovered. To paraphrase Boyd [1979], *theory constitutive metaphors represent one strategy for the accommodation of language to as yet undiscovered causal features of the world.* In this respect, metaphors are a necessarily creative complement to the formality of analogical models, preceding their construction yet incomprehensible outside the framework.

In this sense, too, metaphor and language development offer us a means by which the soft systems language of the interpretive paradigm may be formally developed, [as called for by Flood 1988 and to some extent answered by Flood and Robinson, 1988].

4.5 INTRODUCING RATIONALITY

By now it is hoped that the reader will be sure that analogy and metaphor are themselves similar in their developmental processes, yet traditionally different in their application domains and that they represent useful tools for systems thinkers.

It has also been suggested that the traditional theoretical grounding in the use of analogy was a fundamentally functionalist framework. On this basis, analogy was thought to be concerned with the identification of parallel properties across domains, and its potential for development has evidently been made ridiculous by an over-preoccupation with manifest analogy. As an understanding of analogy has emerged, these assumptions have been shown to be questionable. In this subsection we can introduce the notion of developing parallel rationalities in exploring analogy and metaphor in systems thinking and in the remainder of the chapter work towards bringing together analogy and metaphor through, for the moment, interpretivist and/or radical humanist world viewpoints.

Traditionally, systems and cybernetic research was developed within the functionalist paradigm. Briefly, this is defined by Burrell and Morgan [1979] as assuming;

a) a realist ontology, (in systems terms, 'what is, is systems');
b) a positivist epistemology (how we can represent and disseminate knowledge about those systems);
c) nomothetic methodologies, (investigated by systematic means/end approaches to systems identification, representation and implementation);
d) a deterministic systemic ('machine-like' metaphoric) view of the 'nature of man'.

Management cybernetics, incorporating as it does the fundamental notion of 'black-box' (which is, of course, itself metaphorical), has been criticised for treating (through analogy) man and organisations as if they were machines, i.e. where there are externalised control parameters and deterministic elements [Flood and Jackson, 1988].

To some extent Beer's [1979, 1981, 1985] Viable Systems Model overcomes these difficulties, that is, in principle people may be involved in democratic decision making [Flood and Jackson, 1988]. This does not, however, answer Checkland's [1980] critique where he asks whether organisations cannot legitimately be seen as a social grouping, an appreciative situation or a power struggle, also discussed in Morgan's 'Images of Organisation', [1983]. Each of Checkland's points can be examined metaphorically after Morgan [1980] under the auspices of, for example the *'theatre metaphor'*, which highlights the fact that organisational members are essentially human actors engaging in various roles and other official/unofficial *'performances'*. Alternatively, he proposes that the *'culture metaphor'* may approach aspects of an appreciative situation, since it draws attention to the symbolic aspects of organisational life, and the way that language, rituals, stories, myths etc., embody networks of subjective meaning which are essential for understanding how organisational 'realities' are created and sustained. Lastly, the metaphor of a *'political'* system emphasises the conflicts of interest and the role of power in organisations.

These latter ideas work towards the development of an interpretative view or rationality in drawing analogies or metaphors. Briefly this is defined by Burrell and Morgan [1979] as having, by contrast, to the functionalist framework:

> a) a nominalist ontology (claiming that reality is a product of individual consciousness, a product of one's own mind or of individual cognition; and hence onto such perceptions systemic frameworks can be mapped, but they cannot be mapped onto an 'external world');
> b) an anti-positivist epistemology (such that knowledge is 'soft', subjective, based on experience and insight and hence essentially of a personal nature);
> c) ideographic methodologies (which investigate the world by facilitating an understanding of the way an individual creates, modifies

and interprets the world in which understanding is obtained only by acquiring first hand knowledge of the subject under investigation); and d) a non-determinate view of the nature of man.

By using the functionalist or interpretivist world viewpoints, but de-emphasising regulation, a radical rationality may be found. There is a particular interest here with human radicalism, which draws upon the interpretative ideas, although it extends them to deal with the need for change where inequalities of power are evident and where human emancipation could be called for.

The debate on analogy to date, however, and as stated a little earlier, has been restricted to the possibility of physical/structural, processural and form similarities, that is variations of manifest analogy. We surveyed some of them in Chapter One but more broadly, this also reflects the 'unified science' vision of von Bertalanffy's General Systems Theory and the earlier, prodigal cyberneticians. If it is true that there is form and function to know, then the role of analogy is supposedly to help us find those relations which allow us to understand that form and function. Looser concepts of imported analogies however can be explored via a model of analogy as has been suggested in this chapter. In this, we are able to translate the modelling rationality and in social contexts by concentrating on the relation of analogy, it is proposed that analogy can also be of use to the interpretivist systemist who would rather uncover many perspectives and conceive of these through a systemic form.

From an interpretivist's viewpoint, therefore, there is a necessity to reformulate the classical [by this I refer to its assumed functionalist character] role of analogy. So let us reconsider how a radical adaptation of the classical view can be shown to be of use to the interpretivist.

Of the four definitional components of analogy presented at the outset, only [A2] and [A4] are suitable to interpretive thinking in social contexts- [A1] and [A3] referring explicitly to physical/structural similarities. Neither [M1] or [M2] are suitable definitions since both refer to (concrete) objects. Overcoming this, we will 'combine' the two distinct concepts, metaphor and analogy, as defined at the end of the section 'DISTINGUISHING ANALOGY AND METAPHOR- A STARTING POINT' in the following way, alluding to methodology and forming the basis of the 'framework of analogy' spectrum.

Let us look more closely at the latter. In general in an analogy, we have a process of reasoning from two parallel cases, through inflexion or construction of words, diagrams and so on. In systems thinking the process of analogy takes place in terms of holonomics, in order to develop new images and/or theory and/or models that enhance our understanding of situations and thus promote the basis for well-reasoned intervention.

Holonomics refers to the laws of wholes. With this nominalist viewpoint, the possibilities for analogy and metaphor to be used by the interpretivist and other 'soft' researchers has been opened up within a theoretical framework. But finally, we ought still to recall Bunge's [1979] warning that it is;

> *'... necessary to assess the respect and degree in which an analogy holds, and to check the conclusion of an analogical argument.'*

This spirit must be adhered to whether it is structure and form, or issues, to which we address our attention in discussing relations of analogy in the framework of analogy.

We have now discussed aspects of analogical modelling and the use of metaphor within the field of systems and cybernetic research in philosophical and theoretical terms. This discussion has raised several issues. Most important is that the significance of analogies to systems thinking suggests that they be reconsidered according to the principles outlined, and further, that metaphors be valued as offering some potential to 'see' phenomena in ways eluding traditional empirically based methodologies. For the future, some methodological analysis will be required in order to establish how the ideas presented here on metaphor and analogy can translate into practice. Some early efforts in this respect are documented in the appendices although it is intended that such analysis could also be concerned with the practical development of metaphor and analogy in terms of a soft and critical systems language.

4.6 CONCLUSION

In this chapter we have drawn out three main points. Firstly we have described and discussed a model or method of exploring analogy. Secondly we have shown that rigour can be established and that there is a potential for further analysis of the framework by developing the concepts of positive, negative and neutral analogy. Thirdly we have clearly distinguished between analogy and metaphor although suggesting that the framework of analogy represents a continuum from analogical isomorphy to romantic metaphor.

In achieving this, we have stumbled upon a very important characteristic of the framework of analogy, that the three types of analogy, positive, negative and neutral analogy can be in fact related to types of rationality. In the following chapter we will be exploring this further.

CHAPTER FIVE

ANALOGY AND RATIONALITY

5.1 INTRODUCTION

In the previous chapter we introduced important characteristics of the framework of analogy for systems thinkers. Those are the concepts of positive, negative and neutral analogies. We will now discuss the suggestion that they reveal possibilities for exploring different and contrasting rationalities; these issues will be discussed looking at the relationship between analogy and rationality and in this context the validity of the argument from analogy. The objective of the chapter is to increase understanding of the relationship between the model of analogy and different rationalities with which it might be explored.

5.2 EXTENDING AND EXPLORING ANALOGY

It has been established that analogy and metaphor have a significant role in systems thinking and the historic perspective was surveyed at greater length in the first chapter. We have considered a model of analogy which has potential utility for systems thinkers and at the end of the preceding chapter we argued that the profile of analysis in this area has enjoyed a boost since systems thinkers began to think interpretively. In this context, it has been previously noted elsewhere that,

> *'... interest in either of these sister concepts [analogy and metaphor] was hardly noticeable until a quite natural revival occurred in the new epistemological position of soft systems thinking. Such an interpretive mode of reasoning attaches importance to communication and understanding that metaphor might contribute to. A conceptual-metaphorical component does potentially offer a means of raising relative positions and developing mutual understanding, key principles of a theory built on a notion of human subjectivity.'* [Flood, R.L. & Robinson, S.A. Unpublished Paper, 1989].

Nevertheless, exploration of analogy and metaphor has tended to concentrate on the potential areas of pragmatic methodological usage, without, it has been argued, sufficient analysis on the potential to develop these concepts for specific use in systems thinking. Overall, the broad utility of these sister concepts of analogy and metaphor has been demonstrated and discussed increasingly in the last few years as we have noted in the work of Davies and Ledington [1987], Atkinson and Checkland [1988], and Flood and Robinson, [1989a, 1989b].

In this chapter we will be considering the process by which, having based our studies on variations of the 'open system' concept, systems thinkers then project the *'metaphor of the open system'* across the variety of domains which sustain the interest of systems thinkers. It will be argued that the fundamental concept which facilitates this process, is the relationship of analogy.

It was stressed in the previous chapter that this research has been an analysis, not of 'hazy and superficial' analogies in themselves, but much more specifically how analogies are observed and made useful, how 'analogical hypotheses' are derivable and in this process, the emphasis has been theoretical looking at how theories can be developed.

In this context, it has been stressed that using analogies implies intrinsic notions of looking at phenomena afresh, re-interpreting old ideas and of trying to think in new ways. It is hence a very appropriate tool in the systems framework since the systems thinker will necessarily be concerned with deriving processes to aid reconceptualisation and the recognition of processes of development and change.

The core of systems thinking is representative of a driving necessity to develop theory and methodology from an ontological/epistemological world view that, even at its most compromising, can only be said to treat 'reality' as if in a state of 'dynamic equilibrium'. In this sense it could be argued that systems thinkers and philosophers are particularly sensitive to the difficulties inherent in any attempt to deal with the shifting nature of 'reality'. In Chapter Three we noted the importance of differentiating between analogy and the associated concept of 'analogue', the latter representing as Leatherdale suggested, those things or phenomena which are related by a relationship of 'analogy'. This distinction was also made evident in the model of analogy which shows both an 'analogue set' and a relation of 'analogical reasoning'.

Taking into account the dynamic systemic viewpoint, it is rather disappointing that systems thinkers have understood and accepted the concept of 'analogy', merely in terms of a one-dimensional concept of 'analogue'. The situation could be taken to represent an unwanted present or 'white elephant' from the factual science paradigm. As a result we might further note that systems thinkers have undervalued analogy in the wild goose chase for isomorphisms [see section

1.2.2.1, Analogy and Isomorphism] across domains that were apparently promised by General Systems Theory and consequential to a narrow systems understanding of analogy in terms of analogues.

This matter is reflected in the main exertion to develop systems theories that will establish the attributes of the *'open system'* metaphor in terms of identities and similarities across domains. An example of work at the forefront of this aspect of the systemic endeavour is that of Troncal [1988] who has attempted to establish nomic isomorphism in various areas of natural science. However, the thinking behind the interest in indicating isomorphisms of this sort is static and isolationist, because it is concerned with developing a *'literal'* definition of the characteristics of a principally metaphoric notion of system and we have already noted that this is not useful, nor arguably, possible. Vickers [1972], showed his concern for the matter in the following way,

> *'The view of entities as both systems and constituents of systems raises intriguing questions about identity and continuity. When does something, or somebody, retain its identity and continuity through change? When does it cease to be its old self and either vanish or become something new and different? The question is not frivolous or metaphysical but may be of great practical concern.'* [op.cit.p20].

The concept of analogy entails ideas of structural and systematic similarity and this points to the theoretical significance of different kinds of analogies apparent in different frameworks, such as material ['manifest'] and conceptual ['imported'] analogies. Additionally, exploring the concepts of positive, negative and neutral analogy pointed to the potential to explore differing rationalities in the context of the framework of analogy.

Analogy explored via 'the framework' notably reflects a transition from considerations of possible analogues for a systemic concept in the 'real world', to the dynamic analysis of analogical relationships. Clearly this development parallels a similar transition from 'hard' to 'soft' systems thinking and potentially beyond this to involve this model of analogy with a dialectical and critical scope.

In describing an analogy, what needs to be borne in mind is the identification of the analogical relationship which we have already argued we are in danger of fruitlessly pursuing in a potentially inexhaustible number of domains. The validity of the model, and the goal of theory development must be carefully tested and guarded in order to avoid the explorations of 'merely trivial analogies'. This requirement behoves us to make clear what we, as systems thinkers, consider as 'analogical', and how we intend to model it.

5.2.1 *More on Positive, Negative and Neutral Analogy*

We have previously noted that an 'analogy' represents a comparison of one thing with another, in order to indicate similarities [although *with a difference*] and differences [although in the context of a proposed similitude] between them and thereby to increase the understanding of the lesser known of the two.

Bunge [1979], in his discussion of analogy stresses that there are several ways of developing an analogy. Verbatim, Bunge is attempting to 'exactify' analogy which, in this analysis, we would not consider a useful objective. But, for Bunge, one way in which an analogy might be explored involves modelling of the analogous objects as sets.

In this case one can elucidate, he suggests, a number of analogy strengths, ranging from *'mere some-some correspondence to homomorphism to isomorphism or perfect similarity,'* [1979, p211]. Alternatively, a second way is to count the number of properties shared by the analogous things and lastly he suggests that a third way is possible when the things concerned are 'systems'. In Bunge's terms a system is definable *'as an object with a definite composition, environment and structure.'* [Ibid].

In the latter case Bunge goes on to argue that two systems can be modelled for comparison in terms of their composition environment and structure, each of these components themselves constituting a set. Bunge develops his argument thus,

> *'In comparing the... two systems we compare sets (of components, environmental items or relations). Therefore we must start with the concept of similarity or analogy of two sets. We stipulate that two sets are similar with respect to a third set if they overlap with the latter. More precisely, if A, B and C are nonempty sets, then A is similar to B with respect to C if, and only if,* $A \wedge C = 0$ *and* $B \wedge C = 0$*.'* [Ibid]. [Where ^='Intersects'].

On this basis Bunge also derives descriptions of two systems being '*substantially* analogous', if their compositions are analogous. Or they might be '*environmentally* analogous' if their environments are similar or they might otherwise be '*structurally or formally analogous*' if their structures are analogous. Further yet, by quantitatively modelling these criteria of similarity Bunge comes up with a 'degree of total analogy'. Hence,

> *'Finally we can invert the process to obtain the qualitative concepts of analogy in terms of the quantitative ones: Two systems are (a) analogous iff [that is, 'if and only if'], their degree of total analogy is greater than 0; (b) weakly analogous iff their degree of total analogy, though non-vanishing is close to 0; and (c) strongly analogous iff their degree of total analogy is close to 1.'* Ibid.

Bunge, in arguing this corner, is implying that in an analogy, it is possible to formalise a degree of logical 'truth', although we have already argued in the second chapter, and by implication in subsequent chapters, that this conceptualisation of truth is at best a chimerical notion. Bunge maintains that the judgement of an analogy will be '*superficial*' if the systems in question are weakly analogous: or that the judgement will be '*deep*' if they are strongly analogous.

The modelling of analogy in this way fails to develop an understanding of analogy in any but one dimension. In Chapter Three, we discussed Bunge's points on 'plausible and implausible' analogical inference and noted that Bunge argued that certain analogies are 'faulty' because they rest on 'wrong analogy judgements'. We have described here Bunge's method of identifying and modelling 'genuine analogies' and clearly his view reflects a realist view which quite naturally, a systems thinker, viewing the world interpretively would find hard to accept.

It was previously suggested that a predominant difficulty with this realist view of analogy is such that different possible forms of interpretation of an analogy, are not incorporated into Bunge's method and thus it is possible to view the model of the analogy, in terms of only one rationality.

The paradoxical nature and the limitations of this realist conceptualisation of analogy are summarised by Bunge himself, who in offering two methodological rules for the use of analogical reasoning, admits the necessarily significant role of interpretation. Hence, *'Because the fruitfulness of an analogical inference depends on the actual degree of analogy as well as on the imagination of the user, there can hardly be a general theory of analogical reasoning. The most we can do is to formulate some useful methodological (not logical) rules, such as:*

R1 Use only deep analogies-i.e. make sure that the degree of (substantial, environmental, structural and if possible total) analogy is significantly greater than 0.

R2 Regard all analogies as heuristic devices that may have to be discarded eventually.' [op.cit., p213].

The first rule is 'obvious' without being helpful and the second, proposes by implication that an 'analogy is no substitute' for investigation of the peculiarities of the thing of interest. Not only does this second rule rather defeat the point of Bunge's struggles to derive a realist description of analogy, it also neglects the vital contribution that analogies might make, namely that they allow the investigator to speculate in areas which are as yet unexplored by discussing matters which are rather more familiar.

Overall, we have argued that in philosophy and in practical terms, an 'analogical relationship' is a relationship of likeness or similarity, but with the implication that the likeness in question, is systematic or structural. 'Positive' analogy is that case when similarities are found across domains and it should be noted that as we have seen, usual conceptualisations of analogy inherently favour the quest for 'positive' analogy. Hence in this sense, to 'identify', 'recognise' or even propose an analogy is actually to make an inference of a quasi-inductivist nature. It is the inference from the fact that if one thing is in some respects similar to another, the two things will also 'correspond', [that is, have aspects of identifiable similarity] in other, as yet unexamined, respects.

As an example of positive analogy, we can consider Bunge's [1959, Chapter One] discussion of this phenomenon. Bunge clearly defined analogy in the terms of positive analogy, by suggesting that an analogy may be said to exist between two objects by virtue of their common properties. We can consider as an example, he proposed, an analogy between the Earth and the Moon. Both, to variable degrees, are large solid, opaque, spherical bodies receiving heat and light from the Sun, revolving on axes and so on. These are the areas of commonality.

Yet, although large, the Moon is smaller than the Earth, it revolves but it does not rotate, it has no atmosphere, water, nor vegetation. These are matters of dissimilarity, or as we in our terminology would say, properties of negative analogy.

Having identified these two sorts of relationships, Bunge argued, the most interesting part of assigning them 'positive' or 'negative' analogies between the Earth and the Moon, is the subsequent inferential basis from which we could explore a hypothesis that human beings could exist on the Moon. At Bunge's time of writing, this was of course, a most intriguing and highly speculative matter.

By taking such principles further, the intention is to use the inferential basis of analogies between domains to derive hypotheses and consider the possibilities for theoretical development. It is suggested that from our analysis of analogy so far, the potential for the development of theory in an analogy is of at least two types.

In one way, by investigating unknown territory on the basis of making analogical judgements with 'conceptual weaponry' developed in an already familiar field, we cannot help assimilating information at a basic identificative dimension. In this way, it is inevitable that a 'fund of observational knowledge' will be accumulated in the new domain, in terms of a specific, though not always explicit, rationality.

Additionally, in a second way, as we gather knowledge, it is then suggested that a further dimension of theory development is added, enabling us to assess the appropriateness of the original concepts in a new theoretical environment. In making this assessment, the rationality of the analogical model, or the form of interpretation it is given will evidently play an important role.

While this, to a degree, is in common with Bunge's [Ibid.] assertion that in the process of exploring an analogy, it is the common properties which are of most interest, other areas of concern must also be scrutinised. There is a danger that a concentration on the positive analogy will limit the investigation and that it will develop into a search for analogues. Therefore, the 'positive' emphasis imposes inhibitions on the second of the potential utilities inherent in exploring analogy outlined above. This is caused by the fact that in concentrating merely on commonalities, the power of the model to penetrate 'cross paradigmatic' implications of theories and hypotheses is neglected.

Therefore in this inquiry, it is maintained that there is much more of interest in an observed analogy than merely common properties. By introducing other conceptual frameworks with which to view analogical phenomena it is more likely that the utility of analogies implied, even at the 'common sense' level, in 'seeing things afresh' will be formalised to lead to theoretical development. Hence a commentary on the value of neutral and negative analogy as rational frameworks with which to view analogical relationships is appropriate.

We have established the any basis for inference in science must be closely scrutinised. The process of reasoning in this model as we have seen principally relies on the introduction of the three concepts, those of 'positive', 'negative' and 'neutral analogy'. To reiterate, the first refers to common properties of 'similitude', and the second, to properties of 'difference'.

The introduction of the third concept of neutral analogy was originally made by Campbell [1920] and quoted in the preceding chapter. Neutral analogy is that concept which allowed the exploration of areas of similarity in which it was thought possible to pursue two contexts of theoretical investigation, the vertical and horizontal, in the analogical model given here and illustrated in its complete format below.

Therefore, we can consider theoretical development by taking seriously Black [1962] and Toulmin's [1953] claim, summarised by Carloyle [1971], that,

> '*... models are used in ways similar to the use of metaphor in ordinary language... models help scientists to see things in new ways by changing the perception of the thing... Linguistically this is expressed by describing the perceived thing in language not ordinarily appropriate to it.*' [op.cit., p563].

Figure 4.3.1a is reproduced on the following page to add clarity to the developing argument.

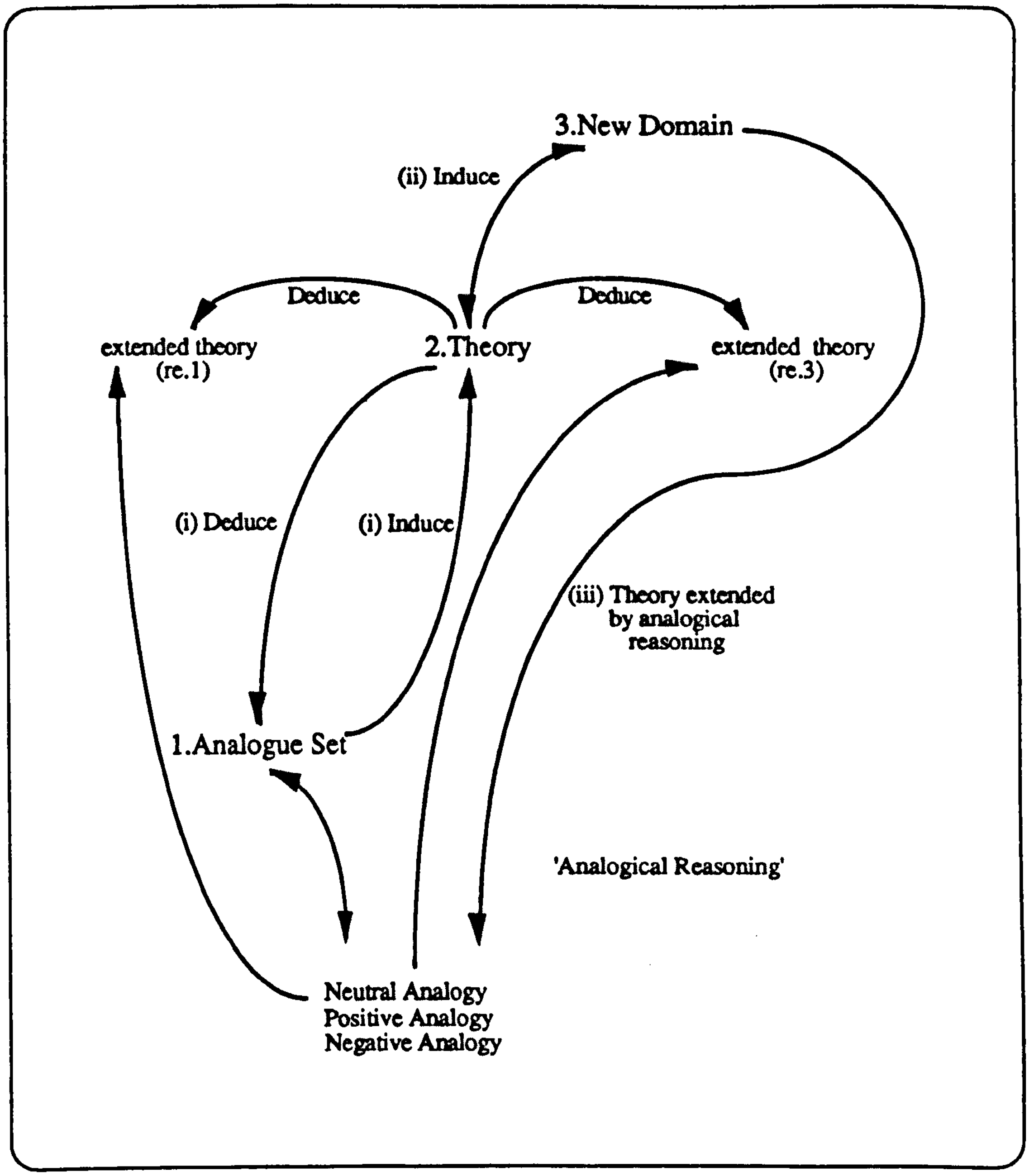

Figure 4.3.1a: Iteration in an Analogical Model.

Referring again to Figure 4.3.1a we can note that the development of theory along the horizontal plane in the analogical model represents information resources released by means of drawing an analogy between two domains; that is, the first kind of theoretical development described here. Theory development along the vertical plane of the model is that of 'the second kind'. By this, I mean theory extension in the full sense, since it describes the 'shifts in meaning' of explicated theoretical concepts to accommodate an increased domain of appropriateness in the universe of applicability, for the theory /or hypothesis explored in the analogical model.

Hesse [1963] followed Campbell, [op.cit.,] in arguing for neutral analogy as 'the proper' basis for inference in reasoning by analogy . This argument rested on the idea that the most theoretically profitable concepts were those which were neither the same, nor different but similar across domains. The basis of similarity of properties means that the argument from the analogue set to the new domain is based on a modification of the traditional argument from analogy which relies on conditions of positive analogy, implying 'identities'. Carloyle [1971] has also held that this condition of 'resemblance' has hitherto been identified as the most significant aspect of an analogy;

> *'Clearly the neutral analogy of the analogue set is the key to the mediating function of the analogue model... Hence it is intermediate between the exactness of the theoretical language and the more indefinite inexactness of ordinary observation terms.'* [op.cit. p569].

It is prescriptions of exactness and inexactness in theoretic and observation languages which belie a set of metaphysical assumptions by which concepts are being identified and the nature of the methods of their explication are being developed. Here it is evident that these metaphysical notions are based fundamentally on processes of 'abstraction', intended to provide theory with an exactness. The conflict inherent in abstraction as a literal or metaphorical tool makes this area wrought with difficulty. Hence 'metaphysical abstraction' particularly in attempting to provide a literal exactness, will necessarily render concepts 'static'. From abstract concepts, metaphysicians propose theories can be built although in the very process of abstraction they will stand in isolation, indifferent to the experiences the theories themselves are somehow intended to embroider or explain.

In this way, it is evident that the metaphysical procedure of abstraction is residual from a philosophy of science rooted in reductionism. Intuitively, it must be seen as wholly inappropriate in the systemic world view which cannot recognise dissociated, static concepts as of significance to, or indeed reconcilable with, the contrasting systems conception of the flux and interplay of interpreted reality.

It is therefore postulated that the emasculation of concepts by abstraction is not useful, or indeed possible, in a subject whose conceptual core is characterised by notions such as 'emergence', 'equifinality', 'communication', 'autopoesis', to name a handful reflecting the systemic concern with development and change. As such, the theoretical core is representative of the driving necessity to develop problem-solving approaches integrating theory and more importantly methodology, in an ontological world view that, even at its most compromising, can only be said to treat reality as if in a state of 'dynamic equilibrium'. Analogy, with its intrinsic concept of 'multi domain theoretical development' and in metaphor 'cross paradigmatic semantic shifts' is not only suitable for discussion is clearly highly significant in the development within Systems Sciences.

Analogical relations, of course can only have a potential utility if the argument which springs from them can be shown to be 'valid'. And this raises the question; 'When is an argument from analogy... a valid argument?'

5.2.2 *The Validity of the Analogical Argument*

Carloyle has suggested that those who agree with the significance of analogy usually take as their evidence the 'philosophic argument from analogy'. This is usually described as having the form of an inductive inference from one set of particulars to another. The premises of such an argument would assert that a set of observed instances, having certain common properties, prompt the scientist to assume that if an ascribed domain exhibited some aspects of the positive analogy, the domain might usefully be probed for others as well.

The validity of the basis for reasoning rests on the combination of the three types inherent in the model. Principally the three types, deductive, inductive and analogical reasoning, work systemically and via the structure of the framework to some degree systematically. Deductive reasoning works to allow the identification of the analogue set, inductive reasoning in part accounts for the projection of the hypothesis into the new domain. Lastly, we invoke what we have called 'analogical' reasoning to suggest that analogies can be observed and acted upon to allow theoretical development.

The introduction of 'analogical reasoning' is a necessity because we have acknowledged Hesse's note that it is not possible to extend theory to a new domain by deduction only, since no theoretical term links the axioms of the theory to the terms describing the new domain.

The logical problems associated with inductivism and deductivism have been well documented elsewhere and it would be profitable to consider whether the addition of analogical reasoning does anything to salvage this model from criticisms levelled at the aforementioned methods of argument.

Significantly, the model of analogy presented has the advantage of being distanced from the usual inductive arguments concerning the validity of generalisations. This is because we have argued that the probability of an assertion being true is determined by the conditions of the analogy only. In other words, we are concerned principally with the 'singular' at the 'object level' although potentially with the 'universal' at the methodological level. In this case most concentration in the investigation should be directed towards the validity of the model of the conditions of the analogy.

Bunge [1959], placed a far greater emphasis on conditions of positive analogy than we are prepared to accept in this thesis, although he also stressed that the validity of the argument from analogy depended on two factors. Firstly, he suggested that validity in an argument from analogy is a function of the extent to which a positive analogy exceeds a negative analogy. Secondly it depends on the relation between the new property, that is, the predicate of the hypothesis and those properties already known and attributed a 'positive' or 'negative' status. In other words, Bunge is implying that the validity of an argument from analogy also has something to do with the utility of the heuristic element in the model, the neutral analogy.

But we might also think of the introduction of negative analogy as a form of systemic dialectic. This thought follows the adaptation of the classical argument from analogy which requires that the basis for inference be in the identity of properties between the domain and the analogue set. In this, and Carloyle's position, the emphasis is in perceived similarity, giving within the goals of the thesis, more importance to the theoretical rather than methodological angle.

We have previously noted that both analogies and metaphors, [the latter conceived as a subset of the former] can be readily incorporated in methodology. Davies and Ledington, [1988] as we saw specifically use metaphor to enhance the conceptual processes of the Soft Systems Methodology. I have argued that the introduction of metaphoric material in a methodology and attempts at implementation in an unreflective, randomly-driven fashion, can only give a methodological illusion of problem solving. It is not possible to derive in this way a theoretical explanation of the utility of metaphor which can be used for a transference of utility beyond the immediate context. The metaphors are used to facilitate creative 'revelation' and without a fuller development arguments derived therein cannot be seen as having a valid status. More can be said to justify this position.

Analogies and metaphors used in methodology carry with them their own dangers, by virtue of their apparently ubiquitous utility. I would also give as an example, the 'problem-solver' using Soft Systems Methodology who is often encouraged [Checkland, 1981] to think of himself in the role [this is itself analogical] of a 'therapist'. The situation has some parallel with Laing's [1968] points on how the 'problem of madness' is treated in psychiatry.

> *'I started to try to see through the dense opacity of social events from the study of situations and not simply of individuals. It seemed that the study ... was arrested in three principal ways ... in the first, the behaviour of such* people *['the mad'] was regarded as signs of a pathological process that was going on in them, and only secondarily of anything else. The whole subject is enclosed in a medical metaphor ...[conditioning] ...the conduct of all those who were enclosed by it, doctors and patients. Through this metaphor the person being isolated from the system could no longer be seen as a person. If one does not act towards the other person as a person, one depersonalises oneself.'* [op.cit ., p28.]

In other words, Laing is indirectly pointing out a methodological danger. We have previously noted in chapter three that some commentators argue that much of 'reality' is itself governed by metaphor, in this instance a metaphor which elicits behaviour in the form of 'role playing'. Hence any problem solver with an interest in using metaphor [or analogy] in methodology to facilitate 'understanding', must pay strict attention to processes by which these tools operate, to avoid pasting one metaphor over another, introducing many layers of consciousness.

The necessity to penetrate processes of analogy and metaphor is evident in this passage. It is further suggested that until these are explicated, any 'revelation' brought forward by analogic or metaphoric methodology is theoretically non-productive in any but the most contrived circumstance, because it can do no more than 'abstract' and 'isolate' the processes of most interest, i.e. methodological processes accounting for analogical and metaphoric illumination. In such isolation the concepts and processes are eminently unsuitable for inclusion in methodological intervention.

What is plainly required is a tool which will supersede the processes of abstraction inherent in the reliance for inference on conditions of neutral analogy. The tool should also be developed in such a way as to approach the persistent impetuosity of the reality in which we are attempting to communicate, to intervene and most urgently, in which to learn. Hegel [1892] most eloquently put a case for a method of inquiry that will be considered in some detail here,

> *'wherever there is movement, wherever there is life, wherever anything is carried into effect in the actual world, dialectic is at work. It is the soul of all knowledge which is truly scientific.'* [Sec. 81z p148].

Dialectic is not unfamiliar to systems thinkers. Churchmanian dialectics derived from the cyclical Hegelian concept of a heroic struggle between thesis/antithesis for synthesis has been developed methodologically in the Strategic Assumption Surfacing Testing methodology [Mason and Mitroff, 1981].

This methodology was seen by Jackson and Flood [1991] as suitable for ill-structured problem contexts where differences of opinion over which strategy to pursue are preventing decisive action being taken. The methodology works by confronting problem owners' 'most cherished' systemic assumptions with plausible counter assumptions; this adversarial trial leading, in due course, to a synthetic agreement concerning the systems goals and implementation of agreed changes can proceed. The cyclical process of the dialectic means however that any manufactured agreement that is sustained is purely temporary.

Bryer [1979] criticises the basis of the Hegelian conception on the grounds that it implies, *'the absence of any real structure in the whole'*. Jackson stresses that Churchman's methodology is therefore regulative, since his process of dialectical inquiry is a continual adjustment of ideological consensus.

These ideas of *'continual ideological adjustment'* and the fleeting nature of *'synthetic agreement'* represent a rather narrow appreciation of the process of dialectic itself. Both criticisms are directed at an end state, in the first case a misplaced perception of a literal *'structured whole'*, [which as we have argued is entirely metaphoric, in any case] and a state of ideological consensus which is not appropriate with regard to a continually shifting process with synthesis becoming thesis imperceptibly as the cycle rotates.

In the discussion of the formal analogical model, however, it is proposed that the concept of 'negative analogy' may be usefully evaluated as a heuristic device in iteration of an analogical model. The emphasis is in the processes of learning, derivable from the juxtaposing of domains, since the dissimilarity identified by conditions of negative analogy are clearly representative of a dialectical, rather than an abstracted, metaphysical world view. In other words in the exploration of a negative analogy, we are conducting our inquiry by the criteria of an entirely different rationality.

In the *'Republic'*, Plato presented 'the Analogy of the Divided Lines', in which he proposed dialectic as a procedure of rational disputation which, by careful consideration and resolution of opposing arguments, works to attain what he regarded as the highest form of knowledge, the *'Form of the Good'*. Although we will no doubt find it necessary to make the distinction between the Platonic realism of the Theory of Forms and the non-universal nature of systems epistemology, the process of dialectical inquiry is still of significant interest.

In the nineteenth century, Hegel adopted the view that reality itself is dialectical; that opposing rational views, a thesis and an antithesis, resolve into a synthesis which then becomes the thesis of a further dialectical process and so on. For Marx, the dialectic was made material and the process seen in terms of the struggle between material and economic forces towards their resolution in a 'better society'.

Of the Platonic, Hegelian and Marxian conceptions, the view which most represents a current interpretivistic climate in systems is that of Georg Wilhem Friedrich Hegel, who believed that 'Mind or Spirit' was the ultimate 'reality'. Significantly this position is connected with our discussion on analogy, especially the dialectical basis for inference which it is suggested as supplying. Hegel was also a philosophical monist positing that everything is interrelated with one vast complex system or whole. He called this the *'Absolute'* in which seemingly distinct parts have reality only in so far as they were parts of the whole.

The proposition is that analogy may be considered a form of dialectic, and to avoid challenges on the grounds of pragmatism, we must clarify several points;

> 1) the relevance of dialectic to the development of systems;
> 2) the status to be accorded to insights derived by a process of analogical dialectic;
> 3) the potential and limitations of 'analogical dialectics' should be outlined.

Approaches which seek to explore reality by juxtaposing different domains of inquiry are not new to the systems movement. Checkland [1972] is aware of the potential it offers and strongly delineates his methodology into the arenas of 'real world' and 'systems thinking'. Thomas [1980] suggests that this separation represents an early systemic attempt to generate a tension between *'what is'* and *'what might be'*. This is done in order to perform analysis and conceptualisation in parallel and that the 'tension' precipitated will form a source of creative change.

Thus, the conceptualisation developed here parallels the interest in generating tension. But tension of a very specific type represented by identification of negative analogies across domains. There will not be an excursion into a metaphysical abstract, since it is suggested that such analogies will betray the conditions for a dialectical analysis. Dialectics rest apparently paradoxically on 'the law of the unity of opposites'. This notion is postulated by dialecticians as

> *'... the fundamental law of the universe ... [operating] universally, whether in the natural world, in human society, or on man's thinking. Between the opposites in a contradiction there is at once unity and struggle, and it is this which impels things to move and change.'* Zedong, [1961, p91].

Dialectic has at least one property in common with systems science. Its propositions are easy to state, but not so easy to grasp and understand. This is partly explainable by the fact that dialectical logic and systems science attempt to deal with things at their most general level and to provide the most universal principles of thought.

Also, it is worth noting from Norman and Sayers [1980] that,

> *'... the dialectical way of seeing things seems to fly in the face of all the traditional philosophy and common sense. The idea of contradictions existing in things seems absurd and impossible -a metaphysical and*

mystical extravagance and the very opposite of scientific and rational thought.' [op.cit., p2].

Hence, what fundamental view of the world does dialectic represent?

Norman and Sayers [op.cit.,] discuss the dialectical method as a philosophy, a logic and a way of seeing things. In other words we have introduced a supplementary layer in the procedures of reasoning intrinsic to our model of analogy, explicitly denoting a conflict between two rationalities, the dialectic and in this case what may be called the *'metaphysical world view'*. The latter is best summarised by Bishop Butler's gnomic phrase, *'Everything is what it is and not another thing'*, [1736].

This view is presented as a formal logic which isolates 'obvious and evident truths' about the nature of reality concerning issues of identity. Butler's phrase is compounded by the implied philosophic truth of 'A=A', i.e. that everything is identical with itself. Hegelian dialectic does not deny this apparently rather trivial position as an important starting point for philosophers attempting to deal with the shifting nature of 'reality'. Significantly, neither do systems thinkers hesitate to contemplate the issue of identity, and have considered the immanent importance of denoting identity as a fundamental starting point. We have already noted Vickers' [1972] timely concern with this matter;

> *'The view of entities as both systems and constituents of systems raises intriguing questions about identity and continuity. ... The question is not frivolous or metaphysical but may be of great practical concern.'*

Hegel meanwhile sums up the metaphysical view of identity as

> *'The subsistence or substance of anything that exists is its self identity, for its want of identity ... would be its dissolution. But self identity is pure abstraction.'* [1807 B, p113; M p33].

Notably, this initial point on identity can be used to illustrate the potential of the formal analogical model in the context of exploration of the dialectical world view. The model is, by virtue of the deduction and induction of set and domain principally concerned with the expansion of the theories used to identify the analogue set. This is done via analysis of the 'meaning' of concepts, terms or interpretations in a variety of contexts, that is the new domains. Thomas [op.cit.,] has noted that problems in using systems models to generate tension for constructive change arise because generating tension is difficult, due to the fact that the two sides of a comparison must be far enough apart without making an 'unbridgeable gap'. This difficulty is clearly avoided in the formal analogical model in which analogue set and new domain are linked by processes of reasoning as outlined, and additionally by the nature of the heuristic inquiry being explored under a hypothesis.

We could go as far as to suggest that the processes which precipitate metaphoric or analogic revelation, as previously mentioned will be made apparent in explicit exploration of properties of negative [and to a lesser degree neutral] analogy. Conditions of self identity, evident in positive analogy, cannot aid explication of a conceptual hypothesis on the vertical plane of the model, although they are able to contribute to horizontal expansion. [Please refer to the Figure 4.4.1a for clarity].

Norman and Sayers [op.cit] have stressed the significance of the dialectical view and their discussion precipitates a renewed consideration of dialectic in the systems movement. Analogies, as we have noted throughout the thesis, prompt reconceptualisations. We can also see how the most contemporary issues in systems; methodological validation, penetration of organisational culture and particularly those raised by Critical Systems theorists, for example, Flood, [1989c], Ulrich, [1983] discuss matters of subordination, subversion, inequalities in communication and power relations, which are approachable by dialectic. Hence,

> *'Everything has self-identity, being-in-itself, but the matter does not end there, for nothing is merely self-identical and self-contained, except what is abstract, isolated, static and unchanging. All real, concrete things are part of the world of interaction, motion and change, for then we must recognise that things, are not merely subsistent but exist essentially in relation to other things.'* [Norman and Sayers, op.cit., p3].

Before interpretivist systemists respond to descriptions of concepts being 'real' and 'concrete' as anathema, an explanation of the specific meanings of these terms in either context is most urgently required. They, like all terms existing within a given rationality carry particular interpretations there which are not altogether externally visible. In other words, what do dialectic philosophers mean by *'concrete?'* Superficial analysis implies abstraction, but the notion is not fundamentally metaphysical nor is that the original rationality. As we have noted in that arena, Plato regarded objects considered ***in-themselves*** as abstractions, the perfections of the 'Forms' at the core of the 'Theory of Forms'. Hence, an object regarded 'of itself' and in concert with Hegelian dialectic, is abstracted in the literal and precise sense that it has been *taken out of context,* and is viewed in isolation.

Thus in the metaphysical rationality, terms are considered merely in themselves, merely as what they are, as static and subsistent, a view arguably impoverished in approaching the complexity of the real world. In treating things as isolated, all movement and development especially in terms of theory itself is arrested, merely a 'given', indifferent and inactive in relation to all other terms, 'things' and theories.

Dialectical thought proposes, by stringent contrast, that real and concrete things cannot be abstracted in this way. Rather they are fundamentally embedded in the world and essentially related to other objects and in perpetual interaction with them. Hegel's monism is implied clearly here,

> *'A determinate, a finite being is one that is in relation to an other, it is a content standing in a necessary relation to another content, to the whole world,'* Hegel, [Science of Logic, 1929 p86].

Hence usage of the word 'concrete' carries a particular meaning in discussion of dialectic, that is, that all concrete things are in a process of movement, of becoming and of developing and change. Essential to all concrete things, is that

> *'...we are aware that everything is finite, [but] instead of being stable, is ultimately rather changeable and transient'.* [Hegel, Ibid, p442].

The metaphysical view that abstracted concepts are somehow pure and unchanging has generated many useful models for the advance of science and other forms of human knowledge and experience, [we may, arguably consider religion here]. But its infiltration as a residuum in systems thinking is stifling. Its presence in philosophic and scientific inquiry is so deeply involved, that processes of abstraction have been accorded a status virtually impenetrable to challenge.

To the dialectical inquirer, the ascribed status of metaphysical abstraction presents a situation of some wry irony. In fusing questions of philosophy and science into the hybrid area, 'philosophy of science', we have done nothing but reinforce the isolationist procedures of the metaphysicians and perpetuated the conceptual, rather than historicist emphasis in philosophy. This also allows, of course, associated ethical concepts, *'Justice'* or even the nature of *'Human*

Rights' to be defined and treated as somehow unchanging and static. In considering their nature 'out of context' as abstraction demands, very urgent ethical issues demanding attention *right now*, can be comfortably relegated to the attention of further generations of metaphysical philosophers. Hence one is in serious danger, metaphorically, of contemplating the state of the stable door, after the horse has bolted.

Further, a logical progression of introducing 'abstraction' as a starting point, is the subsequent effect is that 'reality' too, comes to be regarded as abstract, unchanging i.e. metaphysical, not in this case dialectical. We can identify evidence in our own nonsensical and pre-emptive attempts to establish the literal features of the 'open systems' metaphor. Additionally this tendency has clouded systems thinkers sensibilities and sensitivities, presenting us with epistemological and indeed ideological problems. Among others, the tendency to abstract within a one-dimensional rationality has presented us with a disturbing phenomenon. Namely the pervasive 'common sense' views that *'you can't change human nature'*, or that *'human beings are naturally competitive'*. This view fails to see that it is the dominance of certain kinds of prevalent social relations, that produce, and are required, by certain kinds of social behaviour. I will say a little more on this in the context of general systems theory in the next chapter.

Abstraction of this sort is also a feature of investigations into the explanations of the architecture of our language. The philosophical problem is not with clarifying the effects of abstraction but with accounting for them. The point is seen in the question of how far meanings are forced in us by the nature of our experience or by the requirements in this context of 'successful communication'. Ayer [1969], puts it like this,

> *'We cannot detach ourselves from every point of view. If we abandon one, we have to occupy another. The idea that we could prise the world off our concepts is incoherent for with what conception of the world should we then be left?'* [op.cit , p67].

In his analysis of the problems of the social sciences in the thirties, Lynd [1939], makes a point that has a resonance in the development of the systemic view today,

> *The time outlooks of the scholar-scientist and of practical men of affairs who surround the world of science tend to be different. The former works in a long leisurely world in which the hands of the clock crawl*

slowly over a vast dial; to him, the precise penetration of the unknown seems too grand an enterprise to be hurried and one simply works ahead within study walls relatively sound proofed against the clamorous urgencies of the world outside. In this time universe of the scholar-scientist, certain supporting assumptions have grown up, such as impersonal objectivity, aloofness from the strife of rival values, the self justifying goodness of new knowledge about anything big or little. Such a setting has tended to impart a quality of independent validity and self sufficiency to the scholar-scientist's work. The practical man of affairs, on the other hand, works by a small time dial over which the second hand of immediacy hurries incessantly. "Never mind the long past and the infinite future...but do this, fix this now, before tomorrow morning...".' [op.cit., p7].

It has been taken for granted that there has been no need to synchronize the two time worlds of Lynd's 'scholar-scientist' and the 'practical man'. Immediate relevance has not been normally regarded as so important as the 'ultimate' relevance, whatever that should be or mean.

Necessarily, dialectics confronts the uncomfortable and difficult thesis that all dynamic and concrete things are contradictory. There are tensions and conflicts within all things and in relations between things. A distinction must, however, be made between the dialectical conceptualisation of contradiction and the conceptualisation used in formal logic. In a nutshell, dialectical contradiction is a 'concrete contradiction', existing not just between ideas or propositions but in things. It is in strict contrast to the metaphysical view, in which things are regarded as self contained existents, indifferent to other things, after Hegel,

> *'The different diverse things are each individually what they are, unaffected by the relation in which they stand to each other. The relation is therefore external to them.''*[1892, sec.117, p216].

Dialecticians, indeed systems thinkers are fundamentally unable to recognise this vision of reality as representative. It is the nature of binding, perhaps 'existential' relations, that the systems thinker looks for support of the truism that *'the whole is more than, and less than, the sum of its parts'.* In the rationality of these dialectic relations, we uncover forces of opposition and negation, leading to development and change. It is not a case of mere connection but of constant processes of conflict and pro-active interaction. Hence, dialectical reason,

> *'.. sharpens the blunt difference of diverse terms into essential difference, into opposition. Only when the manifold terms have been driven to the point of contradiction* (i.e. by identification of negative analogies) *do they become active and lively towards each other,*

receiving in contradiction, the negativity which is the inherent pulsation of self movement and vitality.' [Science of Logic p.442.] Brackets added.

It is hence suggested that this accounts for the potential of the analogical model to comprehend things in their movement, that is,

'...contradiction is the root of all movement and vitality; it is only in so far as something has a contradiction within it that it moves, has an urge and activity'. [Ibid p.439.]

'Contradiction' in the dialectical sense then, is emphasised in the development of notions of negative analogy for two primary reasons:

a) to stress that concrete things are not indifferent to one another, but rather in interaction and conflict with each other, i.e. a thing is determinate and has its own identity only by maintaining itself distinct from, by opposing, other things.

b) Contradiction is required as a concept in order to stress that concrete opposition is not external, accidental, but essential and fundamental 'opposition within a unity' The dialectical conceptualisation of contradiction is that of a 'concrete unity of oppositions'.

The law of contradiction applies, as its rationality dictates, to all things; and its neglect is perhaps attributable to the revolutionary enigma it represents in Marxism. Duhring, a critic of the process of dialectic, but particularly of the concept of contradiction, is quoted by Engels [1962],

'Contradiction is a category which can only appertain to a combination of thoughts and not to reality .... contradiction accepted as reality in itself is the apex of absurdity'. [op.cit ., p164].

As a logical defence against the tenets of dialectic, metaphysical philosophers propose, almost ironically, 'the law of non-contradiction'. That law states, according to Popper [1940],

'... no self contradictory proposition or pair of propositions can be true, that is, can correspond to the facts. In other words, the law implies that a contradiction can never occur within the facts, that facts can never contradict' [op.cit., p416].

What is said, by critics of dialectic, is that the concept of contradiction is unacceptable and that conditions of opposition do not yield significant insight. The basis of the criticism, in point of fact, rests on a misconception of the notion of concreteness as it has been outlined here, and can be largely dismissed in the following way.

Metaphysically rooted criticisms of dialectic are based in an understanding of a condition of 'self identity' [or a rationality recognising self-identity or positive analogy in the formal analogical model] As we have seen, this is static and indifferent to 'existential relations' and further one in which an object or thing is taken out of context. The criticism exhibits a gross shortfall since the assumption is made that attempting to express a contradiction is a 'self-annulling' proposition, characterised by the capacity to imply anything and everything and thus able to assert nothing. Popper [op.cit.,] summarises the point,

> *'From two contradictory premises we can logically deduce anything and its negation as well.* [The formal analogical model is founded on principles of induction, deduction and analogical reasoning]. *We therefore convey with such a contradictory theory - nothing. A theory which involves a contradiction is entirely useless, because it does not convey any sort of information.'* [Ibid].

But a simple defence is readily available in making a rationality-based distinction between formal and dialectical conceptualisations of contradiction. The former is indeed such that a contradiction represents *'self-annullment'*, a formal impossibility and hence able to assert nothing. In other words if a theory of 'A' is simultaneously, and of itself, a theory of 'not-A', it is no more than a failed theory. Alternatively, formal contradiction can be considered as a mere assertion; if a theory of 'A' is simultaneously, and of itself, a theory of 'not-A', then it is an assertion of anything and everything as regards 'A'; in other words, an absolutely indeterminate theory. Thus, in sum, formal contradiction is an indeterminate and abstract assertion, a condition of which Hegel was well aware, in 'Logic', where he stated, that whatever has only abstract and indeterminate being, is pure nothingness.

Dialectical contradiction, as has been demonstrated, must be seen to represent not formal, but by the terms of its own rationality 'concrete contradiction'. From the analogical model, negative analogies offer potential for 'theory expansion' on the grounds that the juxtaposition of the analogue set and any number of 'new domains', force a re-interpretation or re-conceptualisation of the initial hypothesis/concept, as the cycle revolves.

This chapter has attempted to discuss a number of points, firstly the value of the formal analogical model presented initially, in terms of an heuristic in systems thinking. The argument has presented a case for a theoretical model of analogy to be explored via notions of positive, neutral and negative analogy in three modes, as a method of abstraction, as an heuristic device and as a process of dialectic respectively. In discussing the shortfalls of processes of 'metaphysical abstraction' implied by neutral analogy, we could pinpoint the contemporary interest in 'ethical methodology'. In introducing negative analogy in a dialectical context we can add to thoughts in the field on the value of juxtaposing domains as adversaries in the critical mode. Additionally it is hoped that each of these modes clearly offer the potential to develop in both analytic and particularly although not at this juncture, methodological terms. It is hence suggested that the initial intuitions of the analogical and metaphorical basis to the systems approach retain much to offer the systems movement as it continues its development.

5.3 CONCLUSION

In this chapter we have drawn out two main points. Firstly that it is the case that characteristics of analogy, the positive negative and neutral can indeed be shown to lead to different rationalities. Secondly that this may have implications for systems sciences in terms of different rationalities which may interpret systemic and other concepts, for example a pluralist rather that isolationist position.

The consideration of a rigourous approach to analogical reasoning in these last two chapters was pursued following the establishment of some optimism for analogy as a concept. We have now seen in detail how analogy can work and that we must recognise, through this conceptualisation the importance of differing rationalities. We are now in a position to consider analogy specifically in the domains of systems thinking and systems practice.

CHAPTER SIX

USE OF ANALOGY IN SYSTEMS THINKING: UTILITY

6.1 INTRODUCTION

We have established that systems thinking should not shy away from explicit use of analogy, but we now need to show how analogical reasoning can be drawn upon in systems thinking as well as highlighting some ways in which it should not. The objective of this chapter is to illustrate how we can use the framework of analogy to re-conceptualise systems concepts.

6.2 ANALOGY AND SYSTEMS THEORY

Any neglect of theory, specifically ontology and epistemology in the theory/practice relationship in systems activities will lead to much confusion. There are various, although familial, conceptions of 'system' which could be said to form an embryo of 'systems theory', but more noticeably there has been a proliferation of aspiring systems methodologies. This rapidly expanding body of methodology has been attaching itself in an increasingly vague manner to a proportionally diminishing theoretical 'core'. This thesis considers that this situation should be a matter of some concern.

Therefore it is of interest that theoretical issues on the matters of what constitutes our 'research programme', in a general sense, both within systems science and by drawing upon theoretical lessons from elsewhere [Flood and Gregory, 1989] be raised. This will contribute to the development of ontological and epistemological stances to establish overall consistency, credibility and integrity and the importance of different aspects of this debate can be found elsewhere, [Flood, 1989a, 1989b, 1989c].

The preoccupation of 'self-reflection' in the systems movement has tended to concentrate on methodology, together with some tentative forays into metaphysics [De Vries and Hezewijk, 1978] and aspects of our apparently ideological nature [Lilienfeld, 1978]. The methodological emphasis is understandable. It is a natural consequence of the benefits that basic systems concepts offer as a supplement to the methods, and concepts, of the objective/reductionist natural science paradigm and its 'crisis' in the face of 'complexity'. In other words, systems practice tends to concentrate on methodologies for contemporary societal difficulties.

Further, incorporation of systems ideas in methodologies in practice is fundamentally easier than considering how these ideas might change our views on reality. Mattesich [1978], among others, makes it clear that the methodological disposition should not be seen as the most fruitful one and he makes a strong call for theoretical development,

> *'The excessive worship of short run needs and the failure to see that in the long run the theoretical is the most practical is symptomatic for the mood of our immediate past. Such an attitude hardly falls short of killing the proverbial goose that lays the golden eggs.'* [op.cit., p6].

We have already noted in the Preface that discomfort with the current situation shows a second face. Systems practitioners such as Eden and Graham [1983] have complained about the lack of guidance that systems theory offers them. They have even suggested that an 'unnatural' stress on systems theory has been to the detriment of methodologists working in practical, transactional worlds. To reiterate,

> *'Theory tells ...about the nature of complexity but the tools presented for handling it do not seem related to the theory.'*

Further that,

> *'Systems assertions are often understandable, undeniable and matters of fact. The substantive difficulties for the practitioner arise in establishing the relevance of any particular systemic description of a problem as being relevant to the issue under consideration.'* [op.cit., p723].

It is acknowledged that there is a weakness in systems theory and the thesis contends that this is at fault, rather than the stress on being theoretically referential. Vickers [1970] has drawn a parallel with the so-called fate of the social sciences, lamenting the lack of substantive theory to which practitioners can turn.

We must conclude, therefore, that a prior neglect of theory has led to much confusion in the systems community. Even worse perhaps, is the neglect that the systems community suffers as a consequence of this [Mattesich, 1978, suggested that this is true, for instance, in the case of philosophers of science]. The unique turn, then, that challenges systems and cybernetics is to show the way toward practically relevant theory, combining social and systems theory and practice [Flood, 1989c].

6.2.1 *Using A Framework of Analogy.*

The following question therefore arises- 'what can be done in this urgent situation?' Mattesich points to one way forward;

> *'If this notion [system] is something beyond a passing fad, it must be possible to relate it to the contemporary view about the structure of the universe. Thus the systems approach might be enhanced by giving it an ontological grounding (i.e. by stating assumptions about the existence of certain entities and events) and by showing the system notion as being rooted in the foundations of science'.* [Ibid].

The issue of ontology has, of course, arisen more usefully elsewhere. Checkland, celebrated systems methodologist, in referring to the theory/practice relationship has indirectly tackled ontological matters. Checkland [1985] suggested that;

> *'A set of linked ideas in an intellectual framework F, embodied in a methodology, M, applied to an area of application, A, yield learning about F,M,A. Neither theory nor practice is prime, each helps create the other in a process which is groundless.'* [op.cit., p757].

It is Checkland's points on the 'grounding' of systems concepts within a groundless process which are interesting with respect to the development of systems theory and these philosophical issues can be surveyed at greater length.

One relevant perception of theory is consonant with the Kuhnian concept based on historical and sociological evidence that theories may be thought of as a *'structured whole'.* Chalmers [1978], however, stresses that another more general philosophical argument is closely linked with the theory-dependence of observation. It has been established that observation statements claiming to pertain to facts must be formulated in the language of some theory. Consequently the statements and the concepts figuring in them, will be as precise and informative as the theory in whose language they are framed. There is clearly an implied role for analogy and metaphor in both of these conceptualisations of theory.

Such a point was declared by Suppe [1978][1], in his call to dismiss the Weltanschauungen conception of the development of scientific knowledge, but to retain an emphasis on language and theory.

With a similar emphasis, the objective has been to devise a framework to tackle the issue of 'theoretical language' as it shifts in terms of rationality. The notion of the framework incorporates, implicitly, aspects of ontology, epistemology and other matters of theory. These require sensitive treatment since the important thing in philosophical inquiry is asking the *right* questions. It is an issue on which systems thinkers might usefully distinguish themselves. This is not only a matter of deciding what it is we want to know, but also of seeing what kind of inquiries make sense, how the problems or the questions may most usefully be posed and whether we are to acknowledge that there might be some things which it is just 'not possible' to know. All these matters are inherently decided with reference to a given rationality.

Additionally, a primary concern in formulating philosophical questions is having some idea about how to look for a suitable answer. This is treacherous ground which is why it must be dealt with explicitly.

In this context Emmet [1964] has identified three sorts of question. Firstly, he suggests there are a very large number of questions for which the appropriate method of forming an answer is 'going and seeing'. These are questions about the world around us and we attempt to answer them by looking at the 'facts of experience.' Answers to this sort of question are generally empirically testable and we have already discussed whether these are the sorts of question in which systems thinkers are interested. I do not think that they are.

Secondly, there are other questions which are best answered by a process of argument, deduction and putting together knowledge we already possess. These are questions more of interest to systems thinkers although I would not be happy with the emphasis merely on knowledge that we already possess, since this implies a reliance on past empirical collection. It has been previously suggested that there are other types of knowledge and equally other ways of collecting it than the traditional empirical methods. Thirdly, there are those questions which might be described as philosophical.

1 Suppe (1978) made a damning attack on the weltanschauung view of the history and progress of knowledge and in this broke away from such theorists as Kuhn.

But some further qualifications are necessary to reinforce this point. Principally they concern the first set of questions, those to which the answer is empirically testable. Significantly, although an answer may be empirically tested in principle, it may not be so in practice, either now or ever [although this does not, of course, necessarily render it metaphysical]. Emmet gives the example of trying to count how many people there are at a particular moment in Greater London. Although this is clearly possible in principle, it is evidently [almost] impossible in practice.

Further, as we have discussed, when we ask questions about 'matters of fact' we normally have some idea about what sorts of answers to expect, that is, what sorts of answers would be 'acceptable'. These latter qualifications emphasise the theory-practice relation that, while common to all scientific activity, is especially important to systems. In pursuing an investigation we must have some idea of what it is we are seeking, for the nature of what is being sought is likely to determine to some extent the method of investigation. Emmet puts it rather well,

> *'...in Lewis Carroll's poem, it is a snark that is being hunted, thimbles and hope may be appropriate; a nonsense hunt requires nonsense methods. Some people might suggest that metaphysical hunts in the past have sometimes had a snarkish flavour about them.'* [op.cit., p64].

It is important then to consider, when questioning the 'grounding' of systems in a 'groundless process', what kind of question we are asking. Is it something that will be empirically testable? Should such testing be required in principle, in practice, or both? Or is it something we may deduce, reason by argument or confine to the realms of the 'unknowable'.

The 'Framework of Analogy' is intended to incorporate factors of each of the stipulations given above in methodological development to approach matters of theory. In the 'model' questions of all three categories are asked. Incidentally, the descriptive term 'framework' is rather deceptive, it is more fruitful to conceptualise this as methodological, and in terms of sliding on a continuum, see Figure 6.2.1a.

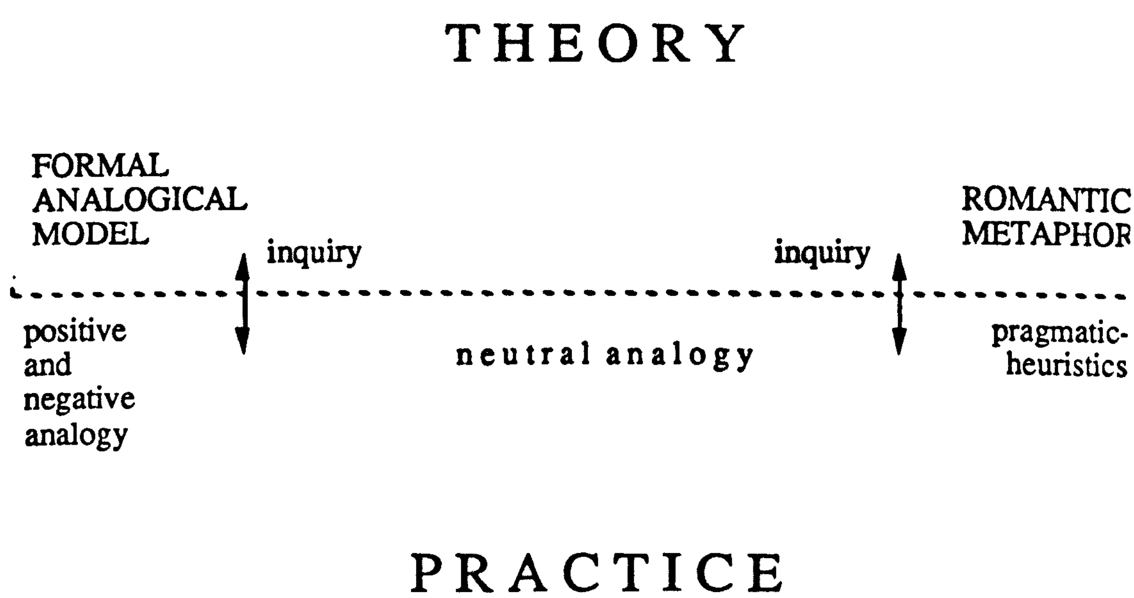

Figure 6.2.1a ;
'The Framework of Analogy': A Methodological Continuum.

At one end of the continuum we can find a Formal Analogical Model, the framework of which was explored in the preceding chapter. The Formal Analogical model is largely an ephemeral notion representing a highly unlikely 'ideal case of analogy' consisting of nomic isomorphisms between analogue set and new domain, still explained through aspects of positive and negative analogy [it is unlikely that a fully expounded Formal Analogical Model, that is an 'ideal type' will contain neutral analogies]. At the other extreme we can see the proposition of 'Romantic Metaphor', [Atkinson, 1984] which lends itself to metaphysical explication and pragmatic heuristics of the type used by Davies and Ledington [1988].

In the intervening spectrum, we can conceptualise the analogical models based on aspects of positive, negative and neutral analogy using reasoning by argument [deduction and induction] and supplemented by analogical 'metareasoning' with reference to rationalities exposed by the heuristic tools of the model.

The idea of a Framework of Analogy can, as suggested above, be presented in methodological terms. We go some way towards this in the two examples of the use of the framework presented a little later in the chapter. As yet, however, a full methodological development has not been achieved and the case studies illustrated in the appendices necessarily present a rather broad perspective of the use of analogy. But this factor should not surprise us since, as with Soft Systems Methodology [Checkland, 1981], interpretively based reasoning of this sort is not a systematic procedure, nor is there likely to be a clear starting point.

Other work in this area has also come across methodological difficulties. Clemson [1984] drew upon the somewhat systematic four stage approach of Beer [1966, 1968], adapted and illustrated in Figure 6.2.1b.

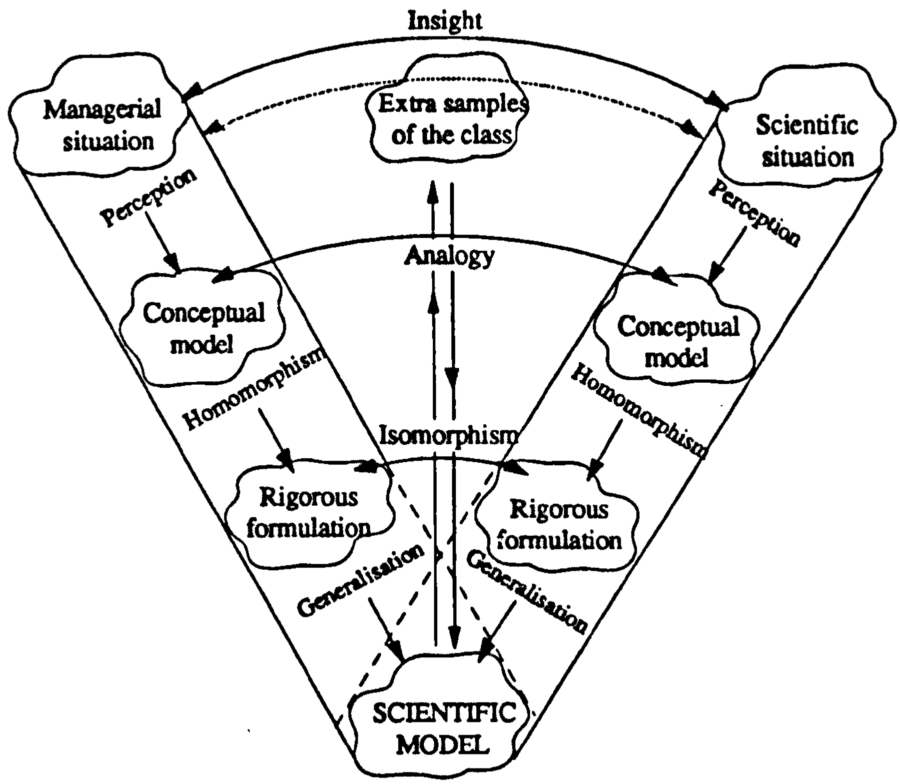

Figure 6.2.1b;
The Process of building a 'scientific' model of a Managerial situation [from Beer, 1966].

This initially involves insightful or metaphoric investigations into a managerial situation, then through perceptive and analogical reasoning a conceptual model is developed which leads to homomorphism and isomorphism in a more rigourous formulation, ending up with generalisations and a scientific model. Yet this is iterative and, although akin to systematisation, has led to some difficulties in practical use. For instance, Clemson says that when trying to explain the process to *'sharp, graduate students (it somehow) just didn't make any sense to them'*. His way forward was simply to do it rather than explain it [the curse of the methodologist!].

Clearly, although difficulties lurk in praxis the Framework of Analogy is intended to serve us for two practical purposes. One is as an aid to systems analysis in a practical context. Secondly, we are using it to re-conceptualise systems concepts in terms of theory, in order to revise theoretical systems concepts and to promote links to methodological developments and implications. An additional and related aim in this secondary context, is in using the Framework of Analogy and its associated methodological approach is to develop a substantive soft systems and critical language.

Flood [1988c] has tackled the need for a substantive soft systems language, considering the translation of theory based concepts under groupings of ontological, epistemological, methodological and additionally those concepts relating to the nature of man. Lastly 'Type Z' translations were considered, these being a general grouping dealing with matters of misuse, abuse and ambiguity which emerged as a group encouraging clarity, and a more exact understanding of intended meanings.

The Framework of Analogy is intended to deal formally with this matter of explication of inferable theory. Analogical reasoning in this sense can release certain systems theoretical concepts from paradigmatic 'entrapment' although also following the criteria of a Lakatosian [Lakatos, 1970] research programme, i.e. by protecting the 'hard core' and offering to some degree positive and negative heuristics.

At this stage we can demonstrate the methodology in its re-conceptualisation mode with necessarily simplified examples. These relate to the area of the continuum where positive, negative and neutral analogy play a role. Reference to both theory and extended theory are made.

6.3 TWO EXAMPLES OF THEORY DEVELOPMENT

We will consider two examples; the concepts 'validation' [see Figure 6.3.1a which includes the general simplified framework from Flood and Robinson, 1988b] and 'complexity' [see Figure 6.3.2a].

6.3.1 *Reconceptualising 'Validation'.*

In thinking about validation it is important to consider aspects of the 'theory' of validation. How can we set about exposing the paradigmatic implications which may need to be challenged or refined? Traditionally, the theory of validation resides in the functionalist paradigm, together with the objective empiricist characteristics this paradigm ascribes the concept. In other words, validation commonly expects demonstrable proofs carried out through empirical methods and functionalist criteria.

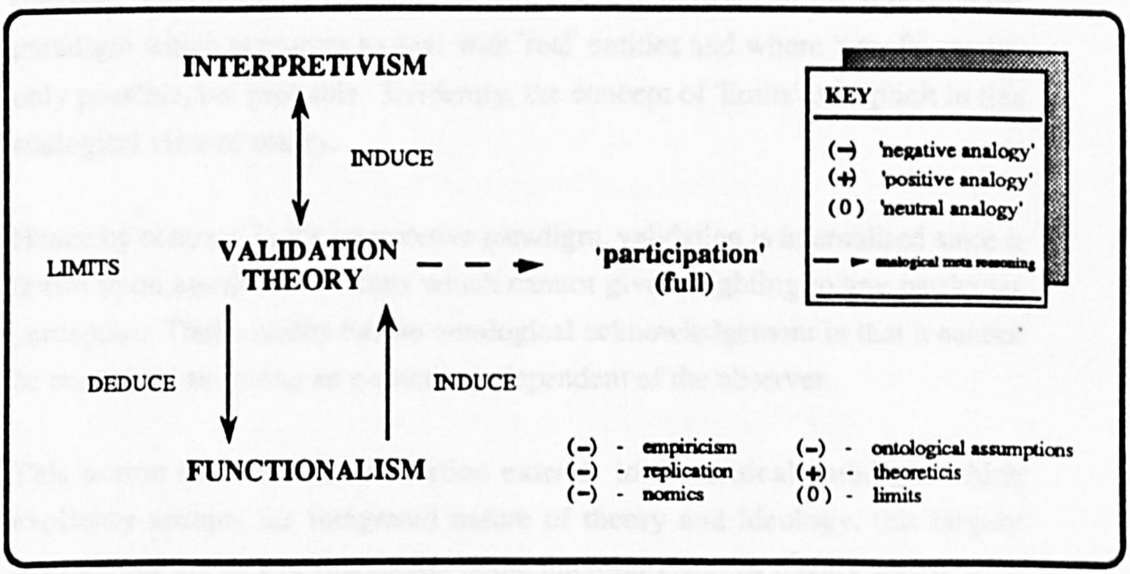

Figure 6.3.1a;
Example 1; Reconceptualising Validation

We can consider the properties attributed to the concept 'validation' by the rationality of the analogue set- functionalism. These are, principally, empiricism, limits, replication, theoreticism, nomics and certain ontological assumptions, i.e. that *'reality is real'*. By observation of the features of the paradigm we can use the concepts of validation and functionalism to infer features about the identified new domain, interpretivism, in the terms of positive, negative and neutral analogy since our intentions are heuristic. Within the rationality of the new domain we can identify empiricism, nomics and replication as aspects of negative analogy. This implies that there is strong disagreement on the importance attached to these concepts in each paradigm.

Theoreticism in the model is described as a positive analogy, i.e. there is similar regard for the concept in both paradigms. Most significant, however, is the identification of 'limits' as a 'neutral analogy'. This conceptualisation would suggest that validation has meaning in the interpretive paradigm which requires revelation. Validation in this area could be understood at least in part through the notion of *participation*, and this does relate to the idea of limits, e.g. full or part participation. In this way we are examining a rationality which aims to validate with respect to the underlying assumptions of the interpretive paradigm. The essential notion is similar, i.e. validation is by satisfactory/unsatisfactory criteria. Validation is, therefore, seen to be externalised in the functionalist paradigm which presumes to deal with 'real' entities and where 'proofs' are not only possible, but probable. Evidently, the concept of 'limits' is implicit in this analogical view of reality.

Hence by contrast, in the interpretive paradigm, validation is internalised since it draws upon nominalist notions which cannot give weighting to any particular perception. There, reality has no ontological acknowledgement in that it cannot be conceived as having an existence independent of the observer.

This notion of immanent validation extends to the critical paradigm which explicitly accepts the integrated nature of theory and ideology, this largely overcoming the lack of foundation of the interpretivists', by clearly stating what ought to be achieved [i.e. emancipation]. The practical relevance of methodologies can be assessed or *validated* in terms of the possibilities that it offers in bringing about desired change to the 'social order'. Here we are beginning to reveal the notion of validation in the critical paradigm which could be further explored with the Framework of Analogy. Evidently, the concept of validation must explicitly change in the process of modelling to be meaningful in either the analogue set or the new domain. It changes, of course, relative to the prevailing rationality. The initial concept cannot retain its integrity without appropriate reinterpretations.

6.3.2 *Reconceptualising 'Complexity'.*

A second example would be to consider the concept of 'complexity', through the analogue set, 'objectivity' and the new domain, 'subjectivity'.

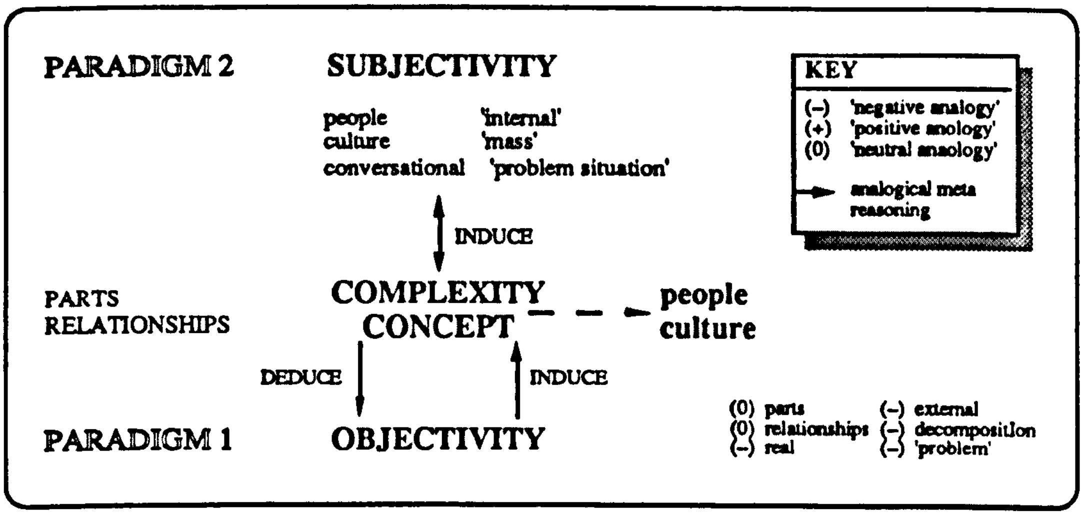

Figure 6.3.2a;
Example 2; Reconceptualising Complexity

In this example we review the objective assumptions of the concept of 'complexity'. These are notions of parts, relationships, 'real, external systems' [i.e. complexity and systems are synonymous], which may be understood by decomposition. The notion of 'problem' is readily acceptable here.

The original concept complexity is understood initially in terms of the analogue set *objectivity* in which complexity reduces to numbers and types of *parts* and *relationships*. All other characteristics in this example are identified as 'negative analogies' since the concepts do not make meaningful transfer to the new domain 'subjectivity' which rests only on individual perceptions. Hence a concept such as the 'decomposition of problems' is logically possible only if it is taken with a parallel view of 'objective reality', that is within the criteria of a single rationality and which can give validity to a particular decomposition of a particular problem at any given time. Some broadly complementary characteristics of the 'subjectivity' paradigm have been illustrated in Figure 6.3.2a. These evidently reflect the rationality of paradigmatic assumptions on a subjective, notional reality.

In the iteration of the model we can, however, identify 'parts' and 'relationships' as phenomena recognisable in terms of 'people' and 'culture' in the new domain. In this recognition we have identified a neutral analogy and hence a heuristic for the development of a theory of 'complexity'. This re-conceptualisation also supports Flood's [1987] notion of reconceptualising Weaver's [1948] famous three types of complexity.

The example further indicates analogical reinterpretation [and notably the mediation of the observation language] of analogue set and concept together, [Flood and Robinson, 1988b]. Here again the initial concept of complexity has explicitly changed.

The long term intention in developing a methodology within a Framework of Analogy is to establish a grounding of systems theory and practice by attaching an explicit interpretation on the continuum. It is, hence, still possible to incorporate 'hard' and 'soft' viewpoints depending on whether the interpretations are in proximity to the Formal Analogical Model [realist isomorphism], or the area of Romantic Metaphor [nominalist romanticism]. Indeed the pluralist aspect is brought unavoidably to the fore since it is inherent in the framework of analogies' relationship with matters of rationality and how they come to be exposed through the exploration of the model.

The most significant aspect of any emerging methodology is the potential methodological output. In the examples given above we have seen the explicit change in interpretation of the hypothesis which should, in a research programme, affect practice. With respect to paradigmatic/ conceptual 'release', Checkland [1981] has remarked that he cannot 'validate' the output of Soft Systems Methodology. We are now considering that 'validation' is possible, if we change the conceptual framework it previously implied. The essential notion, however, remains similar. This also holds true for the conceptions of 'complexity', where the similar notion is 'difficult to understand'.

In the discussion of the foundations of a methodology that generates output explicitly in terms of theory and practice, extending these ideas to 'work' for the newly emerging Critical Systems Thinking and methodologies thereof, is clearly a most promising opportunity.

But because we have raised the issue of implied rationalities in a subject of investigation we must be aware of those views on the utility of analogy which are held here. Thus what range of views could philosophers hold of analogy? Philosophy aims at yielding knowledge and has many branches and many objects of study. We have argued that the empirical sort is not the sort we are interested in, but what else is there?

The various directions from which philosophers approach their subject are described by Ayer [1973], who concentrates on the differing interests of each sub-group. Hence, metaphysics investigates the structure of reality, ethics the basis of rules of human conduct. The logicians investigate the canons of valid reasoning and the epistemologists consider the theory of knowledge not in forming an encyclopaedia, but the criteria which may set the limits to what can be known. Philosophy hence has a lot to do with criteria it is concerned with the the standards which should govern the use of concepts.

In this thesis I have argued for the utility of analogy principally on the grounds that analogical reasoning offers a potential framework for guiding investigation and re-conceptualisation of concepts and/or phenomena in diverse domains and that this is a contribution to systems sciences. Nevertheless, before we can move to summarise our discussion on the utility of analogy in the systems sciences we must also briefly point out some further dangers inherent in neglecting to develop our understanding of the role of analogy and metaphor in the systems sciences further.

6.4 ANALOGY IN SYSTEMS THINKING REVISTED

6.4.1 *Discussion.*

Analogy is an important structural feature in human language frameworks. The use of analogy and the implied ability to reason from two parallel cases is a fundamental feature of human language most obvious in child language in which children are apt to form plurals such as 'foots', 'mouses', 'mans' having heard the previous plurals like 'cats', 'dogs' and 'horses'.

Metaphor too is a most important and influential factor in linguistic communication. Boyd [1979] Black [1961, 1979] and Lakoff and Johnson [1980] make the assertion that the 'open-endedness' or 'inexplicitness' of metaphor has an important parallel in the process by which scientific and other conceptual terms are introduced and thereafter deployed. Indeed metaphorical occurrences are probably the most easily identifiable of the features of analogy and *'meaning asymmetry'* with non metaphorical instances is obvious even to persons who have not yet found a way to describe them.

In the complexities of linguistic theories, how contrasting possibilities within a medium take on expressive actuality we cannot even guess but we do find it useful to believe that what can be thought in words, like what can be said, is limited to the expressive capacity of the symbols. The expressive capacity of words can be modulated by new collations of words, that is, by differentiation [i.e. the *concrete contradiction* of the previous chapter]. In fact poets and other creative writers arrange words to exploit evolving capacity with new thought and this must account for why systems thinkers are interested in using metaphor creatively in methodology, although this thesis has additionally argued we can use the concepts of analogy and metaphor to take us much further than that.

In the following diagram the relationships between analogy [including the concept of metaphor] and diverse rationality are illustrated in an example showing suggested rational priorities or criteria guiding investigation across domains. The diagram [Figure 6.4.1a] builds on the preceding examples of the re-conceptualisations of systems concepts this time emphasising the interests prevalent when analogical features across diverse domains are being identified as positive, negative or neutral analogies even though they are apparently linked by the neutral heuristic of the hypothesis. By confronting these notions and attempting to conceptualise in this way, a systems thinkers is clearly not operating one dimensionally in the manner implied by our discussion of factual science in chapter two.

Figure 6.4.1a is presented on the following page.

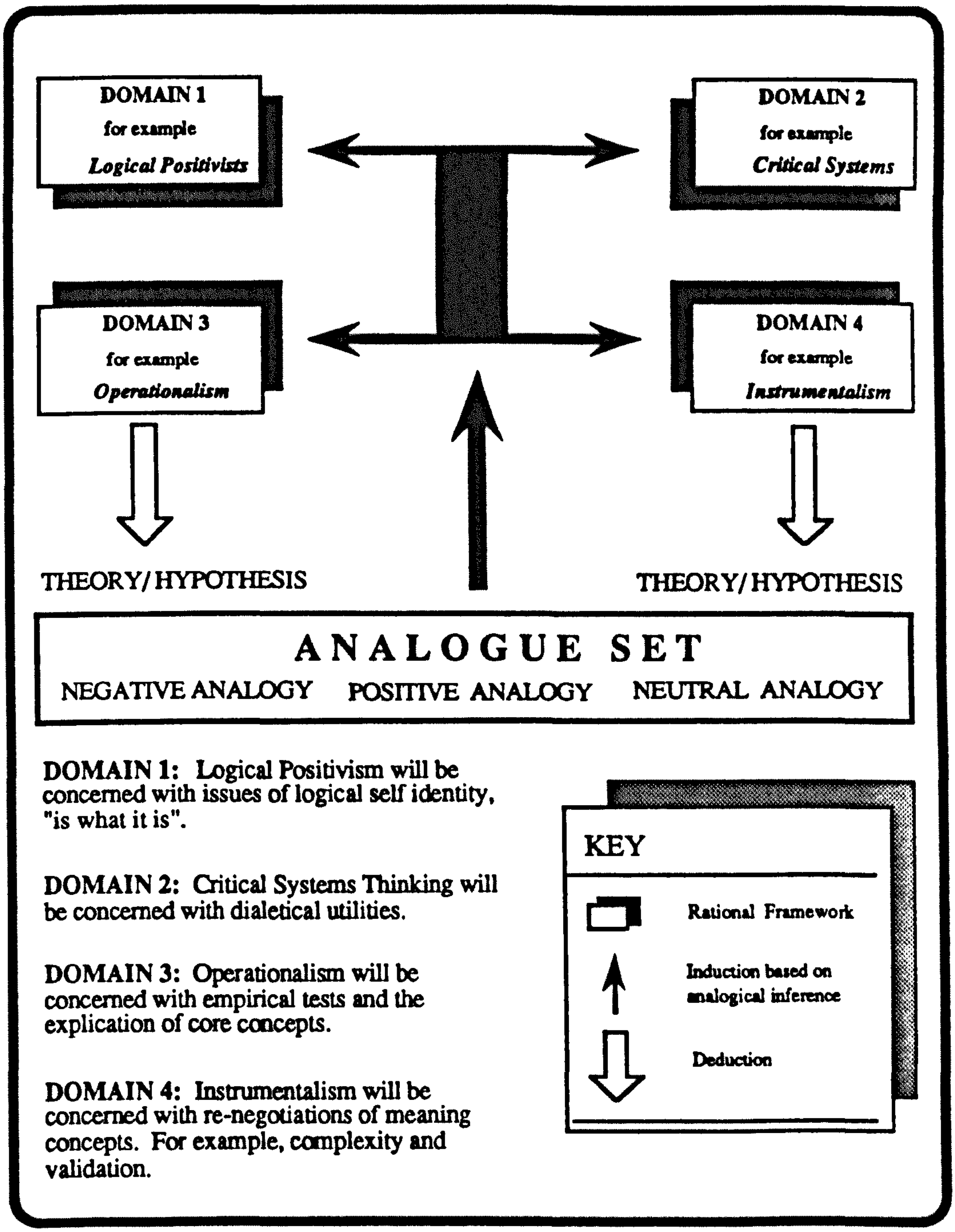

Figure 6.41a. Analogies and Rationalities.

On first inspection, the polemics surrounding the linguistic significance of analogy did not seem to bear much relation to the role we assigned it in building models. We have attempted to face up to this debate in systems thinking and gain sufficient understanding to legitimately use these models for deriving hypotheses and considering subsequent possibilities for theoretical expansion across domains. What in essence appeared to have been considered most urgently was the pragmatic exploitation of the 'analogical relation'. Therefore we have concentrated on developing theoretical rigours [and to some degree practical frameworks] to avoid investigating analogy in such a way as it appeared as merely an exploration of hazy and superficial analogy. Rather the dangers of hazy analogy demanded that we identify the principles by which analogical models could be constructed.

The 'process of argument', that is, the procedures by which we are attempting to uncover what we will accept as *'systems knowledge'* have been emphasised, because of the influences of inherent rationality in these kinds of decisions. In other words, we need to make clear the nature of the basic assumptions we make, especially in the face of a fundamentally dwarfish theory of what kind of knowledge constitutes *'systems knowledge'*. We must face these discomforting issues because we have painfully little epistemology on which to base our postulates. Further, we have to question the prominent status accorded to methodology and this we have already queried. But also, Flood [*Liberating Systems Theory,* 1989] has stressed the necessity to do more than question or analyse, but to act,

> *'In systems theory there is a need to develop a view on truth and meaning in order to promote coherent argumentation* [dialectic] *which is fundamentally satisfying and of general utility.'* [op.cit.,] Bracket added.

That area is problematic and it could be argued that it is made even more difficult by the analogical and metaphorical characteristics of our [small] and perhaps misunderstood, theoretical base.

6.4.2 The Methodological Problem of Abstraction

The matter of abstraction is most pertinent to investigation into the methods and views of science. When we view culture and institutions as the behaviour of individuals, we claim we are able to assign a normal place to deviations from *the normal way*. But a chronic embarassment of social science theory is the explanation of exceptions to the general rule. Some of these exceptions are so egregious as to defy explaining away by such qualifying phrases as 'by and large' or 'ceteris paribus'. These are the sorts of deviations which refuse to cancel each other out and are such striking deviations from the assumed *norm* that they often become independent research enterprises. As a result, social science is full of dichotomies composed of a 'norm' and a prominent deviation from it. Lynd [1930] gives many examples 'competition *and monopoly* ', 'voting *and non-voting* ', 'lawfulness *and crime* ', 'marriage *and divorce* ', 'employment *and unemployment* '.

Even this overt recognition of departures from the norm belies the situation. For the Aristotelian emphasis upon classes and paired opposites hides the fact that one is dealing not with two contrasted poles but with a distribution of frequencies ranging from one extreme to the other. It was for this reason, that is, in the attempt to circumscribe the methodological problem of abstraction that the framework of analogy has been presented on the continuum ranging from the formal analogical model to the perplexing vagaries of Romantic metaphor.

The point of view is succinctly put forth in the opening chapter on 'Aristotelian and Galilean Modes of Thought' in Kurt Lewin's 'A Dynamic Theory of Personality', [1935]. Lewin speaks of,

> *'... the loss in importance in modern physics of logical dichotomies and conceptual antitheses. Their places are taken by more and more fluid transitions by gradations which deprive the dichotomies of their antithetical character and represent in logical form the class concept and the series concept.'* [op.cit.,].

What is now important to the investigation of the dynamics of systems thinking is not to lose potential knowledge in any methodological abstraction from the situation under investigation. [This implication does not carry implicit realist luggage as it may be confronted via notions of diverse rationalities across domains]. In other words, instead of a reference to the abstract average of as many historically given cases as possible, there is a reference to the full concreteness of the particular situations via a model of analogy. There is systemicity as well as systematicity.

In embracing analogy and metaphor without sufficient theoretical consideration given to the full theoretical implications of such an encounter, systems sciences have another potential cuckoo in their nest; and clearly this is not a healthy habit for the development of indigenous offspring. The problems raised by this kind of behaviour are not isolated to the particular difficulties of appropriation of metaphor in systems methodology described in chapter one. Rather, they are *symptomatic* of a problem that has been around a while longer. I would argue, for example, that there is a problem methodologically in systems abstraction and that a misdirected emphasis on abstraction has at times compromised systemic objectives.

We have, for example, previously noted that systems sciences hold at their core concepts which attempt to acknowledge the flux and interplay of reality, hence concepts not appropriate to mechanisms of formalisation and abstraction. Any attempts to use them for this purpose can be seen as an attempt to rationalise or make literal concepts by one set of criteria across what are essentially paradigmatic [rationality] boundaries. This would only serve to take the concepts away from their meaningful context inhibiting, by resort to static and isolated formalisms, the possibilities of developing the dynamic epistemology of a systemic world view. Significantly, this metaphysical influence made manifest in attempts to 'abstract' is a feature of a number of rationalities, not least of which is the 'Hard Systems' paradigm.

Metaphysics is concerned with reality as a whole. In some senses is in competition with the natural sciences in its attempts to deal with the 'underlying nature of reality'. While everyday experience and common sense teaches us that appearances can be deceptive, when we look more carefully at this assertion we find that there is fundamental conflict in the various appearances. Further that we have interpreted some observations in such a way that further observations do not follow. In a similar way, it is the differing interpretations, made with references to different frameworks and rationalities that make it such that,

> *'Sometimes scientific knowledge is disagreeable, often contradicts the classics, at times it tortures common sense and crushes down intuitions.'* Bunge, [op.cit p32].

Further it is from this abstracted basis of metaphysics that criticisms have been levelled at the systemic world view represented by General Systems theory. Since it springs from the stable of philosophy, the metaphysical viewpoint is not always recognisable as merely one, of many possible angles, on reality. A metaphysical abstraction can in this way come to dominate, however, as the static and isolated model of *the underlying nature*. A given abstracted concept can become a defined area for analysis and exploration but also a sitting duck for criticism. We have, in systems thinking, first hand experience of the latter with shots aimed at an arguably fundamental systems concept, General Systems Theory. It will be argued in the rest of this chapter that a dynamic concept of

General Systems Theory, if we release it from the unnatural shackles of metaphysical abstraction can be defended in terms of a systems rationality.

6.4.3.1 A Defence of General Systems Theory

The term 'General Systems Theory' disappeared from the name of the flagship organisation of the systems movement over thirty years ago. Almost as soon as it was established, the Society for the 'Advancement of General Systems Theory' became the 'Society for General Systems Research.'

But in this context a neglected question is why the notion of General Systems *Theory* has since been largely abandoned by the systems community. The question is more philosophically based than might first imagine and in asking it, we are also asking;

> 1. Can the notion of 'General Systems Theory' be resurrected and defended now that we have acquired experience of more than thirty years of 'systems research'?
>
> 2. Would it be useful for the movement to do so?

Any progress toward answering these questions begins with an attempt to review how systems scientists think of GST. It is be suggested that while the common conceptualisation contains admissible flaws, the notion of General Systems Theory has been criticised and summarily dismissed, on the basis of clearly refutable, largely abstractive metaphysical criteria. As refutations are examined, it is hoped that the necessity to re-consider the conditions which prompted the historic apostasy will become apparent and the re-consideration itself will highlight new criteria against which to validate a conceptualisation of General Systems Theory.

But what initially do we mean by 'General Systems Theory'? It suffers, in a way, from a surfeit of definition. Here we will only look at the most common views of what GST is supposed to be. It is not claimed that the discussion will lead to a literal definition of what GST is, nor by including some views and omitting others, is it intended to 'weight' one against another.

The name most commonly linked to GST is that of Ludwig von Bertalanffy who argued for [1962, 1968],

> i) a development of *a set of theoretical concepts* based on simplified mathematics of systems,
>
> ii) and also on the assumptions of their *applicability in various spheres of experience* which,
>
> iii) could culminate in a *unification of the sciences.*

Central to this set of concepts was Bertalanffy's belief in isomorphisms. He interpreted the importance of 'isomorphisms' in that the same laws would find expressions in different and apparently unrelated fields. On this foundation, GST then serves as;

> iv) an important regulative device in science. It will make possible the *transfer of simplified conceptual models* from one field to another and
>
> v) lead to *parsimony* in scientific research.
>
> vi) At the same time, by formulating exact criteria, *GST will guard against superficial analogies* which are useless in science and harmful in their practical consequences.

Boulding [1956], went further in establishing a teleology for GST, giving it a definition and an objective. He perceived General Systems Theory as;

> *'a body of systematic theoretical constructs discussing the general relationships of the empirical world. This is the quest of GST'.* [op.cit].

Boulding pointed out that it was the contention of General Systems Theorists that since the *'optimum degree of generality in theory'* is not always reached by the particular sciences, GST could fulfil a role here also. Additionally, Boulding predicted that GST would develop something like a spectrum of theories- a system of systems which may perform the function of a gestalt in theoretical construction. Such gestalts have been of value in directing research. General Systems Theory, in Boulding's conceptualisation is clearly teleological and emphasises a theoretical output;

> *'It is one of the main objectives of General Systems Theory to develop these generalised ears and by developing a framework of general theory to enable one specialist to catch relevant communication from others.'* [Ibid].

Later he hints, in defining a further objective of systems theorists, at the development of a philosophic framework;

'If the interdisciplinary movement is not to lose that sense of form and structure which is the discipline involved in various separate disciplines then it should develop a structure of its own. This I conceive to be the great task of general systems theory.' [Ibid].

Hence a structural development for GST was suggested by Boulding. One such way to meet this objective would be to look over the empirical universe and pick out certain general phenomena and to seek to build up general theoretical models relevant to these phenomena. This would be a *'GENERAL* General Systems Theory'. A second approach would be to arrange the empirical fields in a hierarchy of complexity of organisation of their basic 'individual unit' of behaviour and then try to develop a level of abstraction appropriate to each. This would be a *'SPECIFIC* General Systems Theory'.

For others, GST begins with a philosophical, almost metaphysical emphasis. To T. Downing Bowler [1981], General Systems Theory represents;

a) the quest for relational universals that are true for systems in general, *relational universals that emerge at new levels of complexity,* and

b) *a model of the whole of existence* as the interaction of entropic and negentropic processes.'

Clearly we have defined a number of different conceptualisations with different emphatic objectives [shown in italics] which should bode of the difficulties inherent in *making literal* a concept which like a metaphor, contains and promotes *shifts in meaning*. The strategic importance of General Systems Theory, its appeal and ambition, rests in this conceptualisation as fundamentally a dynamic description of relationships that may be represented by mathematics or other symbolic methods.

Before looking at complex defences of the concept from philosophical and paradigmatic standpoints, we will begin with a brief resume of areas of criticisms of the notion of GST.

6.4.3.2 Three Areas of Criticism

Firstly, General Systems Theory suffers from criticisms launched from a position that is shared by many so-called 'general theories' [Jones 1978], in that they are too vague, hard to prove, disprove or use in any practical or convincing way. Jones [op.cit], gives Freud's theory of psychoanalysis and Marxian economic theories as examples. While it is possible to use such general theories to formulate specific, testable predictions, an outcome contrary to the prediction does not really shake the theory. Jones explains,

> *'This is because both reality and theory are rich enough to allow for constant reinterpretation'.* [op.cit].

Real world practitioners, however, commonly prefer more rigourous guidelines. It has often been said that GST somehow failed to bear the fruits it offered. In this respect however, we should decide whether such criticism pertains to the *theory* or the *practice*. Good theories have been rejected in favour of poor data in the past and as a heuristic, or possibly gestalt, General Systems Theory may still have something to offer.

Secondly, from simple historical analysis it is clear that it was the practical offshoots of the notions originally heralded as the forbears of a 'GST' which took precedence. There is evidence that this is true of Information theory, Cybernetic principles, Organisation theory, Control theory, and even Management Science. Specific development of practical methodology was emphasised in these and other related spheres.

It is further suggested that the pragmatic emphasis left these exemplary theories together with the possibility of a meta, 'General Systems Theory' philosophically immature. In this respect, General Systems theory as an abstract concept appeared ephemeral and of small relevance to practical activity. This view has been perpetuated; it is static and marooned. How many so-called 'systems scientists', for example, could give a satisfactory answer to even a casual enquiry as to 'What is General Systems Theory?' How many of us could specify with surety how the everyday business of 'systems science' differs from the longer term teleology prescribed to GST?

Lastly, the full development of GST required that the torch be taken up by other disciplines. This has not been the case, and it has been lamented elsewhere although ultimately we have only ourselves to blame. Additionally, many other disciplines have taken GST as a platform from which to snipe at subsidiary systems activities [Lilienfeld, 1978].

6.4.3.3 Reviewing Philosophical Criticism

There have been three [at least] philosophical attempts to dismiss the concept of a General Systems Theory which we will consider here;

a) an attempt by the positivists;

b) an attempt by the neo-positivists;

c) an attempt by the interpretivists.

These three arguments, two functionalist, one evidently interpretivist, are relevant to the period elapsed since the formulation of the concept and which may be said to reflect paradigmatic implications for the systems rationalities during which GST has lain virtually largely dormant.

Attack and criticism from the positivists is perhaps attributable, Peter Caws suggested in 1966, to the unwillingness of the Society for General Systems Research to 'dignify' with the label theory, a *'mere working hypothesis'*. The tone reflects paradigmatic connotations concerning the rational basis for legitimisation of a notion of a General Systems *Theory*.

To summarise the 'Positivist Attack', we may follow Ayer's [1971] criticisms of the positivist adoption of conclusive verifiability as a criterion for significance. Conclusive verifiability is taken as the logical framework for the rationality of positivism. Before looking at the propositions of GST in this context, it is helpful to look at how that rationality copes with the concept of other, more 'obvious', general laws. These may take a broad, syllogistic empirical form, such that *'all men are mortal'* or that *'arsenic is poisonous'*.

With reference to the intrinsic rationality, it is in the very nature of these propositions that their truth cannot be established with certainty by any finite series of observations. This criterion, however, has already been accommodated by GST since it acknowledges that it is naturally impossible to describe all systems. General Systems Theory proposes to merely deal with a 'typical' example in each class. Hence a 'phenotypic system' is likely to omit, of course, certain aspects found in the 'genotypic system', although the task of GST was to recognise this limitation and construct a theory that was a logically consistent set of propositions concerning a wide variety of systems models.

But the positivist, in pursuing his own argument, must acknowledge that if such *'general propositions of law'* are designed to cover an infinite number of cases, then it must be admitted that they cannot, even in principle, be verified conclusively. Then, in adopting the logic of 'conclusive verifiability' as a criterion for significance, the positivist is logically obliged to treat any general propositions of law in the same fashion as they treat the statements of the metaphysician.

This is clearly a most impractical position. In the face of this difficulty, some positivists have adopted the heroic course of saying that these general propositions are indeed pieces of nonsense, albeit an essentially important piece of nonsense. But the introduction of the qualifying term 'important' is simply an attempt to hedge and marks a recognition that the positivist view of any 'general proposition' is philosophically paradoxical, without in any way attempting to deal with the paradox.

The contention is then that no general proposition, apart from a tautology, can possibly be anything more than a probable hypothesis. Conclusive verifiability as a criterion for significance cannot make distinction then, between the concept of a general systems theory, against other, clearly practical general propositions, such that men are mortal or that arsenic is poisonous.

Secondly the 'Neopositivist' attack, along with its attacks on metaphysics, sought to impose restrictions on hypothesis formation. The neopositivist rationality saw this as a way to rule out, or at least to curb those broad, sweeping generalisations and 'explanations' in the sciences and humanities which defied what it considered to be the validating rigours of empirical testing.

The roots of a neopositivist criticism of General Systems Theory lie in a conceptualisation and an proscribed importance of, isomorphism. While we acknowledge that this is one of the areas requiring reconceptualisation in GST, diverting the neopositivist attack is still quite straightforward.

An important facet of von Bertalanffy's GST [op.cit.,] rests in the concept of isomorphism. Indeed, the isomorphism found in different fields is based, he suggests, on the existence of general systems principles of a more or less well developed general systems theory. On the basis of the existence of isomorphism across disciplines, GST can hence be expected to play useful roles in the meaningful transfer of models from one field to another, while weeding out the meaningless similarities. Von Bertalanffy has been criticised in this respect for not specifying the criteria by which general systems theory will distinguish the 'meaningful' from the 'meaningless' similarities. Such a task is eminently suitable for resolution by definitive abstraction.

In pursuing their criticisms that General Systems Theory fails to distinguish between 'meaningful' and 'meaningless' statements, neopositivists made an inappropriate assumption pertaining to the status of GST as an abstract scientific theory. The assumption was inappropriate because GST in this respect it is not a scientific proposition, even by the criteria of falsificationism, which defines scientific statements as falsifiable in principle. Since this is not the case with General Systems theory, logically the principles of GST must move into the area of philosophy.

Pursuing the falsificationists argument, after De Vries and Hezewijk [1978], Popper [1959] has been credited with showing the impossibility of the neo-positivist attempts to demarcate 'sense' from 'nonsense' and hence the 'meaningful' from 'meaningless' in philosophy. We have encountered this difficulty already insofar as it pertains to analysis of metaphor. Popper attributes their attempt meanwhile, to a 'naturalistic fallacy'.

A separation of meaningful and meaningless should or would be coterminous with the distinction between science and metaphysics. Popper has satisfactorily shown that the difference between science and metaphysics is not of a naturalistic character. Instead, there are conventions, stipulating norms that may enable us to come nearer the ascribed goal of science [and in this respect GST], that is, explanation. This goal is of course, a normative convention, not given by nature as we considered in the second chapter.

Once again the proposition of a General Systems Theory supports a refutation on these grounds, in that the strategic importance of GST, its appeal and ambition, rests in its conceptualisation as fundamentally, a description of relationships. From our discussion on analogy and rationality it is possible to recognise that other definitions than Bertalanffy's could be constructed leading to *another kind* of GST. For example, Miller's [1965] classification of generalisation suggests that there are at least two kinds, those pertaining to uniformities across a given class, and those pertaining to different classes.

Further even from within the systems movement, attack has been made by interpretivists who are paradigmatically unable to accept traditional concepts of isomorphism.

General Systems theory rests easier by far within the context of the natural science paradigm, and notably it has not been seen to struggle here as elsewhere. This may be partially explained by the empirical emphasis in this area, together with the benefits this method of theory development offers its paradigmatic host. Hence serious problems first grated with GST when the attempt was made to 'transfer models' into the social sciences. The repercussions were felt throughout the systems movement [Hoos 1972]. In the social sciences the difficulties begin even in the identification of a 'system', which is then only acknowledged as an organising structure or conceptual tool to aid real world intervention. It is clearly impossible in this area to recognise any concept of nomic isomorphism.

However, perhaps we have, once more, mis-represented the spirit of GST. General Systems theory has been previously conceptualised as referring to ontological *'matters of fact'*, otherwise, so called *real systems in a real world.* We have, in this context, already established that no general proposition referring to a so-called matter of fact can ever be shown to be necessarily and universally true. It can, at best, be a *'probable hypothesis'*. This is so, as we have seen, of all propositions which pretend to a 'factual' content.

Although that argument failed to dispose of the possibility of a GST in the 'factual', natural science paradigm, it appears that the criteria of that argument persist in the Interpretive paradigm despite the identificative condition that facts have no status in the second systems world view struggle. Further, interpretivists and social scientists are arguably only justified in theoretically rejecting the notion of nomic isomorphism. In a GST description of a 'system' it is the business of the related methodology to demonstrate that these 'phenotypes' are representatives of 'real systems' evidenced in empirical data.

Since the naive argument that GST is only able to handle the 'real systems' evidenced in natural science is thus dismissible, why should it not be extended into perceptual or communicative systems. An awareness that there might be such concepts as issue-based, 'contextual isomorphisms' might prompt interesting research.

Therefore the vehemence with which GST has been denied by systems scientists is misdirected and the energy it takes should be perhaps concentrated on rooting out the metaphysical residuum we have unconsciously taken into our fold. As a result, everyday systems scientists who wholeheartedly accept and use the concepts and features of the open system metaphor; autopoesis, equifinality, feedback, and homeostasis, for example still find themselves unable to seriously contemplate the possibility of a metaphorical [i.e.non-literal] General Systems Theory.

This peculiar and confusing situation is partly explainable as the side effects of the rationalities involved in some of the functionalist philosophical arguments described. Positivism largely precludes that statements can ever become logically certain and this conclusion must be accepted by every consistent empiricist. It is often thought to involve the positivist in scepticism and we have seen this scepticism in the fact that Systems scientists go so far as to shrink from general theory concepts. Examples easily spring to mind Ackoff [1963], Naughton [1981], Lilienfeld [1978].

But in our world view this should not be the case. That the validity of a proposition cannot be logically guaranteed in no way entails that it is irrational for us to 'believe' it. What is irrational is to look for a guarantee where none can be forthcoming and to demand certainty where probability is all that is obtainable.

In the interpretive paradigm we have acknowledged that 'system' is not a static thing but a dynamic heuristic referring to a particular relationship among things that can be actualised in a number of ways. The development of GST was not intended to end with the last full stop of von Bertalanffy. The genius of Bertalanffy, suggests Battista [1977], does not depend on the validity of the classical systems theory he devised but rather that he was attempting to devise an altogether new kind of theory. It would not be in the spirit of Bertalanffy to attempt to develop a theory that could integrate all of science, or to accept his version of GST as the final form, any more than it would be in the spirit of Freud's attempt to develop a general theory of psychology, to accept the concept *id* as the final version.

With reference to the claim that GST has the potential to unite science, this has been interpreted in many ways. Does unification of science refer to a reduction to a common language, a synthesis of concepts or a form of encyclopaedic theory among disciplines ?

In this context we may lastly return to Ayer [1971] who emphasised a point which appears to have relevance to GST within the systems movement. Namely that the most pressing matter is not so much the unity of science but rather the dynamic coupling of philosophy with science.

> *'With regard to the relationship of philosophy and the empirical sciences, we have remarked that philosophy does not in any way compete with the sciences. It does not make any speculative assertions which could conflict with the speculative assertions of science... and, of course it is impossible by merely philosophising to determine the validity of a coherent system of scientific propositions. The function of the philosopher is merely to elucidate the theory by defining the symbols which occur in it... science is blind without philosophy.'*

CONCLUSION

In this chapter we have drawn out two main points. Firstly that analogy is generally useful when used explicitly and rigourously. This has been shown specifically in the re-conceptualisations of systems concepts. Additionally it has been argued that issues raised in the analysis of analogy can be deployed in a defence of GST. The second main point has noted that concepts of analogy have flawed usage when used in partial observation, typically to point out aspects of similarity, which we have argued represent the most superficial appreciation of the framework of analogy.

Also we have noted that problems arise when the rationalities of different domains are confronted. In the second half of the chapter we looked at the difficulties caused to systems thinking by a one-dimensional understanding in a framework of abstraction. To alleviate this problem it has been argued that a dynamic systems rationality can be developed using processes of analogy.

Now that we have established the utility of the framework of analogy in systems thinking, we can lastly move on to summarise the lessons for systems science.

CHAPTER SEVEN

LESSONS FOR SYSTEMS SCIENCES

7.1 INTRODUCTION

This chapter will briefly review and summarise the argument for the utility of analogy in systems sciences presented in this thesis.

7.2 A REVIEW OF THE THESIS: Lessons for Systems Science

The aim of the thesis has been to develop a rigourous understanding of the concepts of analogy and metaphor in order that we can more fully appreciate their potential utilities in the growth of systems thinking and theory.

We have noted that much of systems thinking is inherently analogical but that the current enthusiasm to adopt these techniques more overtly in methodology needs to be closely scrutinised to ensure credibility. To meet the objective of the thesis, the discussion has been theoretically based. The analysis developed three main themes; the validity of analogy as a philosophical concept; the nature of *'reasoning by analogy'* and the potential of the latter for use in practice, as well as theory.

In the first chapter, 'USE OF ANALOGY IN SYSTEMS THINKING: A SURVEY' a brief historical survey of the systems thinker's use of analogy and latterly of metaphor was presented. Through the work of early systems thinkers, the highly analogical basis for systems thinking became apparent alongside associated criticisms on the use of analogy. It was argued that there is confusion in the role attributed to analogical processes of reasoning by both systems practitioners and critics of systems sciences alike. Our expectations of the utility of analogy needed further examination before we could begin to develop a clearer understanding of that role and the contribution it could make in systems terms. The rest of the thesis set about enhancing our understanding of analogy.

In this respect we initially found it important to make the distinction between a systems conceptualisation of *analogy* and that of *isomorphism*, stressing that a looser contextual notion of analogy was to be introduced and latterly modelled.

It was similarly argued that the use of metaphor in methodology, although pragmatic is undisciplined and the implications of theories and analyses of concepts of analogy and metaphor have not been seriously absorbed. This situation, it was suggested, endangered the viability of analogy and metaphor as systems tools since the methodological adoption of metaphor was shown to be ad hoc and potentially indefensible on theoretical [and practical] grounds. By the end of the first chapter we had noted that systems thinking has undergone a cynical attack for its use of analogy and thus we were ready to move on to consider the context of that attack on the systems approach.

The opening survey had led us to consider how systems thinkers view science. It was suggested that it is on the grounds that analogy is somehow *unscientific*, that systems thinkers have retreated from a more explicit analysis of its utility in systems sciences. Hence in the second chapter 'A SYSTEMS THINKER'S VIEW OF SCIENCE' we drew out a number of points concerning the legitimacy of four characteristics of a model science.

In the review of the model of factual science, it was argued that a fundamental *negotiability within a rational framework* infests the beau ideal of science. This constituted an important lesson for systems thinkers attempting to build a credible concept of analogy. Analogy is concerned with developing new conceptualisations in unfamiliar domains using concepts that are already either more familiar, or seemingly relevant, or practically useful elsewhere. Using an analogy involves in broad terms a matter of pointing out aspects of similarity *with a difference.*

In other words analogy is, as it has been argued throughout the thesis, connected with notions of conceptual negotiability in *rational frameworks*. In our discussion of a systems thinkers view of science we considered the claims that *'Factual Science is Objective, Cumulative, Success Oriented* and *Value Free'*. Firstly we found that 'facts' cannot be considered meaningful unless you go behind them, that is, the enterprise of science cannot be principally concerned with facts but with building a rationality that gives them status. Secondly we noted that the criteria distinguishing matters of truth and belief cannot be unequivocally established and that arguably, science is based on belief as much as on notions of fact or truth. Thirdly, it was argued that the success orientation of science carries within it potentially compromising anomalies. Lastly we concluded that the values of any scientific rationality and community must inevitably penetrate the enterprise of science; hence science is not value free.

The discussion of the characteristics of science suggested that it was sensible for systems thinkers to reconsider the role of analogy in their thinking and to evaluate it independently. We subsequently revealed that there is a scientific context in which analogy is begrudingly acknowledged to have significant role. That is, in processes of discovery and hypothesis derivation. Systems thinkers had thus found a foxhole from which to defend the utility of their analogical basis. It was hence proposed that the traditional pre-conceptions of science cannot be taken as a relevant source of criticism for the utility and legitimacy of analogy in systems science. There it can be credibly used to aid acts of re-conceptualisation or to guide systems research in unfamiliar domains.

That concepts of analogy have traditionally been criticised from a 'scientific' standpoint and having discussed the unsteadiness of that basis, we reconsidered the utility of analogy fundamentally, in 'ANALOGY; A PHILOSOPHICAL POINT OF VIEW'. The aim of that chapter was to develop a deeper understanding of analogy and metaphor in order to identify and isolate relevant concepts and processes with which to model a concept of analogy specifically for systems thinkers.

The chapter ranged over philosophical issues in an examination of analogies, models and metaphor and their interrelationships. It was suggested that analogy as a form of comparison contains parallel concepts of model and metaphor. Further, that although it can be conceived as a loose identification of resemblance, [as is implied in the vague notion of *similarity with a difference*] it can also be shown to be rigourous and vital in theory development. In other words it would be possible, in a model of analogy which classified the natures of similarities across domains, to also establish and exploit the utilities of the '*with a difference*' component. This was latterly attributed to the relationship between analogy and rationality.

Additionally in that chapter, we began our discussion on the validity of the argument from analogy to systems thinkers. This is a matter which has much concerned philosophers at a broad level and which is of significance to the pragmatic aims of systems thinkers. Without establishing ***reasoning on the basis of analogy*** as a valid argument, systems thinkers will be unable to transfer the knowledge they might accumulate through their use. The situation might also allow another tedious recital of the lament of systems practitioners on the irrelevance or insubstantiveness of systems theory to work in the field. In this way it was stressed that, overall, analogy could be assessed in terms of an argument relating to any form of model, since models are vital ways of developing appreciations of theories and concepts.

The following chapters concentrated on two main areas, asking what characteristics we wanted in a useful model of analogy for systems thinkers and on that basis, under what circumstances we could accept the argument from analogy as a valid argument. In Chapter Four, 'A MODEL OF ANALOGY' we established analogy as a systemic tool. Firstly a model of analogy was described for systems thinkers and the model presented comprised a framework for a model/method of exploring analogy. Secondly, in the development of the positive, negative and neutral features of the model of analogy it was argued that rigour can be established in what was latterly taken as a framework of *'loose resemblance'* . These positive negative and neutral characteristics were the criteria representing the components explaining the functioning of analogy in modelling resemblance *with a difference*. Thirdly, although acknowledging that metaphor represents a particular case of analogy, we made a useful distinction between analogy and metaphor for systems thinkers, maintaining that the conceptual framework of analogy represents a continuum from analogical isomorphy to romantic metaphor.

In Chapter Five, 'ANALOGY AND RATIONALITY' we analysed further the important characteristics of the framework of analogy for systems thinkers. Those were principally the concepts of positive, negative and neutral analogies. The three concepts were held to reveal the potential to explore different and contrasting rationalities. We discussed Bunge's attempt to *exactify* analogy and dismissed his model of analogy as attempting a realist and static map of a dynamic interpretive conceptual framework. In the discussion of Bunge's argument we were also able to establish criteria by which to distinguish notions of strong or weak analogy. These classifications were shown to be irrelevant once we have accepted conceptualisations of analogy acknowledging simultaneously, a number of rational viewpoints. In this context we considered aspects of negative analogy as a form of systemic dialectic.

Hence the framework of analogy as a rigourous basis for analogical reasoning was developed following the establishment of some optimism for analogy as a concept in Chapters Three and Four. In a detailed argument we saw how analogy can work for systems thinkers and that we were able to appreciate the significance of differing rationalities within this conceptualisation.

In Chapter Six, 'THE USE OF ANALOGY IN SYSTEMS THINKING; UTILITY' we have established that systems thinking should not shy away from explicit use of analogy. The re-conceptualisation of two systems concepts using the framework was demonstrated. In the ensuing discussion we were able to draw out two further main points. Firstly that analogy is generally useful when used explicitly and rigourously. Additionally issues raised in a discussion of a model of analogy made visible as in the re-conceptualisations of systems concepts can also be invoked in a theoretical defence of General Systems Theory.

Our second main point was that analogy has flawed usage when it is used in partial observation, typically to point out aspects of similarity. The latter case we have argued represents the most superficial appreciation of the framework of analogy, reinforcing the importance of using analogy to explore a number of juxtaposed rational frameworks. We also were able to note that problems arise when the rationalities of different domains are confronted especially when aspects of similarity only are being sought. We broached this issue in the second half of the chapter when we looked at the difficulties caused to systems thinking in terms of General Systems Theory. We accounted for the critical attacks on GST by exposing the one-dimensional understanding associated with methods of *abstraction*. It was argued that without a framework in which we can propose a juxtaposition of rationalities, critics and systems thinkers alike are unable to establish a valid critique or to appreciate the full utility of a framework of analogy to the theoretical development of a dynamic systems rationality. This must surely highlight the importance of establishing a theoretical understanding of analogy and metaphor before we go further in using these two concepts in systems practice.

7.3 A NOTE ON A SORTIE INTO PRAXIS

In the appendices two case studies are presented illustrating some practical work undertaken in commercial organisations during the research of the thesis.

The organisations concerned have preferred to remain anonymous and their wishes have been respected. The firms showed considerable interest in the nascent concepts of analogy considered in the thesis and set loose project outlines for practical analysis. Although this was initially encouraging, it caused some problems in deriving adequate hypotheses for exploration via the model which as the reader will observe does not feature explicitly in the reports submitted. We have seen in the thesis in the iteration of the analogical model that the initial hypothesis drives the model and consequently the practical studies do not have a sufficiently robust structure to exploit the full utilities implied theoretically by the roles of diverse rationality in the model.

Nevertheless the practical experience was of benefit to the research highlighting if nothing else, the difficulties of conceptualising alternative perspectives within the rigid and largely deterministic conceptual environments of commercial organisations. Further, the emphasis of the research has been principally theoretical and has stressed the heuristic, iterative and dynamic nature of the framework of analogy and therein, I have argued, lie its potential benefits to the systems sciences. As a result, a full translation to methodology has been a task beyond the immediate objective of this thesis although it is hoped that research into the utility of analogy in the systems sciences will continue.

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APPENDICES

APPENDIX A

PRIVATE AND CONFIDENTIAL

CASE STUDY REPORT

for

ORGANISATION AND METHODS
DEPARTMENT

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CASE STUDY REPORT
for
THE ORGANISATION AND METHODS DEPARTMENT

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INTRODUCTION

The approach to this report is systemic and will contrast with the characteristics of the 'Organisation and Methods' approach to problem-solving which breaks down problems into their 'constituent parts'. The latter is the most common method we adopt when we attempt to rationally devise problem-solving methodologies. It is held as scientific and therefore is the basis behind the concept of a 'work measurement' system, to which we shall come in due course.

However, as Klein [1980] points out, ask a typist to think about which finger is being used to type which letter and watch the typing speed slow down. Ask a tennis player how they snap their wrist when serving and watch the strokes become self-conscious and unnatural. The Systems Science perspective attempts to view 'the whole' and because of this the analysis presented in the case study will probably seem quite abstract although I hope as a result, raise some wide ranging issues for further consideration by the Bank in question.

Specifically the report has been undertaken as an assessment of the utility of 'analogy' and 'reasoning by analogy' within a practical, that is, methodological environment. This is an area which has been surveyed by several authors in recent years, for example, Dreyfus, [1979], Klein, [1980], Kahneman et al., [1981]. The study considers the use of 'clerical work measurement' techniques by an Organisation and Methods Department and the additionally the role of the analyst in devising staffing levels and structures.

This paper is intended to serve largely self reflective methodological purposes. Principally, to try to use the over-arching 'framework of analogy' to evaluate the nature of the overall 'methodology' employed on an 'Organisation and Methods' review of a major office. Then to assess the utility of the 'analytic' mode of the 'framework of analogy' to see if present methodological pitfalls, travails and shortcomings can be highlighted and possibly reconsidered.

Hence, the objective of the case study is to examine the methodology used in a 'real world' problem-solving exercise to review a major branch bank which, with its satellites, represents a significant profit centre to the organisation. Subsequent to this examination I will recommend a number of methodological observations which might be of interest to the Organisation and Methods Department.

The operational constraints were; time allocated for the on-site review was three weeks. The team consisted of four analysts at varied levels of seniority and experience.

THE ROLE OF THE 'O&M' DEPARTMENT

The 'Organisation and Methods' Department forms part of the Services Division within the Bank. Other co-habitees in the Services department are the Personnel, Property, Inspection and Management Services [Computer] Departments.

Normally in large organisations there is no uniform pattern for a Services department. In fact some firms do not have such departments at all, with line and departmental managers solving their own problems as they arise and calling on specialists from inside or outside the organisation as they require.

Otherwise, some firms, such as the Bank have preferred to develop large and 'multi-disciplinary' Services departments. These departments typically include permanent specialists, with the structure of the division reflecting the needs of the organisation pertaining to where its activities are concentrated. As any one set of problems appear to dominate at particular times during an organisation's development, so should the structure of a Services Division respond to these dynamic changes.

While specialists within a such a division deal with a variety of the organisation's problems, using a variety of tools and techniques to solve them, the problems themselves will usually surface from similar sources. Most commonly, branch or at times departmental managers become aware of a deficiency or pending deficiency in the performance of their area of responsibility and will ask for help.

In other circumstances, a problem stretching over a number of departments, or a policy matter can be referred for investigation by a higher stratum of management. Where close working relationships or a particular level of perceptiveness exist, the individual departments may themselves take the initiative in identifying worthwhile projects. As a result, in some cases the types of work undertaken will sometimes reflect the way in which the department is perceived by the rest of the organisation, with more responsive and dynamic departments attracting a more varied remit of projects, than departments which have taken the traditional approach more consistently.

THE ORGANISATION AND METHODS APPROACH

Once a problem has been accepted it is allocated to the appropriate specialist and/or project team. From whatever source the problem has arisen, it is convenient to initially use the normal procedures for investigating organisational problems. Hence resolution is presumed to follow from;

i) identification of the problem,
ii) assembly of data,
iii) evaluation of possible solutions,
iv) implementation of the preferred solution.

Each of these stages, of course, raise questions in themselves. In fact, they raise 'methodological' questions and show the need for careful analysis in this area. For example, in the primary stage calling for identification of the problem, we could also ask, for example;

- has the problem been correctly identified?
- what are the goals of the system, or sub-system under investigation?
- is there a chance that any problem solution might interfere with these goals?
- what criteria are used for measurements?
- what methods can be used to help the investigator understand the system and its role in the wider system, which might not be under investigation?

These are the sorts of methodological questions that it is easy to neglect as a result of the pressures of 'real world' problem-solving activity. But the issues they highlight are significant to any form of investigation since they are concerned to increase problem solving effectiveness.

Hence in the usual approach to problem solving given here, we have firstly assumed that a specific problem can be identified and in some sense isolated. However, even this may not be possible in some cases until a great deal of data has already been collected and processed according to some set of what we may call 'taken for granted' criteria. In other circumstances, problems might be 'recognised' as types of problems demonstrating features for which 'solutions' already exist. This gives rise to the idea of 'standard problems'. Equally, other types of problems may not be familiar or indeed amenable to already developed techniques of analysis and for these, methodology may have to be developed from scratch.

Secondly, the data and information required for analysis may not be readily available or collectable. In this context there is much reliance on standard techniques which have been developed for data collection, measurement and retrieval elsewhere. The project leader may, or may not, be in a position to make decisions about the appropriateness, the validity, or the accuracy of these methods and any analysis based on data of this sort will obviously be flawed at source. We will come to more questions in this area in due course.

Thirdly, the formulation and selection of a solution is normally the result of an 'evaluation of possible alternative solutions'. A case is then formally presented, verbally and in writing to support the most appropriate course of action with the obvious aim of relieving the problem symptoms which prompted the investigation. This process may vary on some occasions from a 'rubber stamping' exercise to a negotiated compromise involving the parties interested in the problem solution. Again further commentary on this process will arise later in the paper.

Lastly, the preferred solution is implemented in the problem context which will allow a degree of post-hoc rationalisation and evaluation of the effectiveness of the investigative work in the earlier stages.

The operation of the 'Organisation and Methods' Department in this case derives much of its workload and hence remit with reference to a 'basket of monitors' generated in part via the mainframe data and in part by manual collection of the branch staff. The Department's allocated role within the Services Division is to assess branch [and internal department) staffing levels, in terms of quantity and grading, organisational structures and job functions. On the basis of the analyses, the department hence makes recommendations for change as necessary in these areas.

The statistical monitors are intended to highlight branches/departments where an increase or decrease in staffing levels might be required. This will normally lead to an on-site analysis. Plans for refurbishment and so on also suggest the appropriateness of an O&M analysis, since the basis of workload monitoring must imply, via method study, a concern with layouts.

The Organisation and Methods Department have an advisory function and do not retain executive power to implement their recommendations following their on site analyses.

This report outlines a number of the methodological areas which have come under scrutiny following the review of the Sheffield Group. Significantly, several matters discussed during the review are now receiving close attention, principally the range and implementation of the 'Clerical Work Improvement Programme' system.

This system of work measurement has hitherto been used to generate the statistics which prompt and subsequently structure, any analyses undertaken by the Organisation and Methods Department. Importantly, the version of CWIP in use is now itself under review with regard to 'efficiency and effectiveness'.

CASE STUDY BACKGROUND

Church Street was the principal case to be analysed. No Organisation and Methods review was undertaken with reference to the six sub branches which form part of the 'Sheffield Group'. Five of the six branches were briefly visited and their senior official consulted on the operation of the 'group' as a whole.

Church Street itself has experienced quite substantial growth in recent years. The analysis was undertaken with a broad emphasis on this issue, although Manchester Regional Office had prompted the review after a major and lengthy refurbishment.

The range of the analysis was to incorporate: [Ref: O&M Dept., JLW B],

a) The operating structure and Managerial and Clerical staff numbers at present with a view to establishing capacity to accommodate predicted growth as suggested by statistical trends to date;

b) examine the 'grouping' arrangement of main branch and sub-branches;

c) following analysis of clerical procedures, the development and installation of sectionalised work measurement;

d) an analysis of methodological procedures implied by sectionalised work measurement.

The main problems identified by the Organisation and Methods analysts during the 'on site' review were:

1 Lack of defined job roles at a variety of levels;
2. Excessive workload of the four Senior Banking officials at Church Street indicated by level of work undertaken out of hours;
3. Inadequate capacity for delegation;
4. Inadequate support by and to the Accounts Managers;
5. A need to support the Banking Manager, Branches.

METHODOLOGICAL OBSERVATIONS

This section will form the main body of the report in terms of a methodological evaluation of the overall approach to the review of the branch.

i) Commentary on the 'CWIP' Model

The most frequently used methodology in Organisation & Methods analysis has been the 'Clerical Work Improvement Programme'. This reporting system produces a monthly computer printout summarising data related to a number of 'controls' monitoring the main banking transactions within each branch. The data on each 'control' is collected via the mainframe system and manually recorded registers. A print-out in the form of a 'Standard Hours Calculation Sheet' is analysed to assess staffing levels and organisational/structural implications. It should however be pointed out that CWIP figures refer chiefly to the individual tasks undertaken by staff below management grading. Management activites and work were, at the time of writing, seperately monitored by the 'Managerial Workload Rating.'

The CWIP system produces information used by the Personnel Department for assessment of general staffing and manpower planning activites and by Property department for estimating space needs. The manual involvement at time of writing appeared to be high, onerous and inefficiently organised, although this would have significantly relieved by provision of a Personal Computer in the branch.

In referring to statistics in general, there will always be a degree of distortion and a loss of accuracy in information collected due to the time taken, the reliabilities of sources, integrity of 'controls', sample sizes and so forth. CWIP data is normally analysed with reference to continuous comparison over the previous three months. In terms of staffing however, the 'lag' in the system may have a detrimental impact.

During the course of any on-site study examples arise for which 'standard times' have to be ascribed and there will be distortions, as there were in this particular case study, if reference to standard times is not consistent. For example, in some timed measurements, analysts would include certain tasks as part of the 'Interruption allowances' while others would include the task as part of the job. Further, in generating contextual data, activity sampling was carried out in neglect of fundamental rules [of sample sizes, for example], which served to make data gathered in this way merely 'pseudo-statistical'.

Evidently, there is a loss in accuracy because of inconsistent measurement and also because in the data collection 'information' is being lost or hidden, needing time to trace. Theoretically, if they are to be applied, standardised measures must be applied according to strict procedures.

By applying the work measurement techniques in different ways, in different circumstances and breaking the theoretical rules, there are some strongly practical benefits. These are the benefits which presumably account for the fact that despite the unsophisticated application of the 'broad principles' of work measurement, workable solutions are generated by the analysts. By 'workable solutions', I mean those acceptable to the particular context and circumstances of the branch.

There is, therefore, room for discussion on the capacity of an analyst to 'legitimise' subjective-decisions about candidate organisational solutions by reference to CWIP and other so-called 'statistical' data. For example, following meetings with branch managers, the analyst might agree to incorporate a higher margin of 'flexibility' or 'slack' into the branch work systems measurements in order that any subsequent allocation of an 'Establishment' level of staff, will not restrict branch growth.

Hence it is suggested that the dominating reliance on CWIP data neglects the perceptual, interpretive and interpersonal skills of the analyst in his/her examination of a given organisational situation. Thus any corruption of the accuracy of a 'structured measuring system' is to allow the adaptation of individual organisations to their 'socio-cultural structures', which are invisible to CWIP measurements.

There is a 'degree of objectivity' introduced in the relevant CWIP data processing techniques, that is in the nature of classifying, counting, computing and mathematical manipulation which are broadly considered as 'objective' measures in themselves or are capable of being 'objectified' without too much difficulty. Overall the 'art of processing' organisational data of this sort, is in processing the data in such a way that the output -the value of the variable- not only appears to have been obtained as objectively as possible, but appears to have immediate relevance as well.

What we are really looking at here is symptomatic of a fundamental methodological problem concerning the roles of 'objective' and 'subjective' decision-making criteria. The maxims 'Facts speak for themselves' or 'Data speak for themselves' are commonplaces and these declarations are accepted because empirical facts, particularly measurements appear to stand alone and are therefore labelled as 'objective'. The basic scientific method behind this asserts that personal relations between the world and the investigator do not exist because the world is external to the person.

Nevertheless, Counelis points out,

> *'The problem studied, the questions pondered and the employed data generation and data reduction methods are all results of the scientist's [investigator's] personal interests and thus a subjective choice. Facts, sources and methods are selected through the scientist's personal subjective criteria. The objective and subjective are not polar and therefore they are not divisibly yea/nay affective states. Rather the objective and the subjective are an indivisibly unitary affective state.'* [1989].

In this case, this problem has resulted in the adopted 'objective procedures' of reduced, elemental 'work unit' measurement being used to override subjective decision-making procedures arrived at through discussion, negotiation and accumulation of perceptual material. Why this ostensible disguise should be necessary, and why the arguably inaccurate data should be thought to have a greater validity or legitimacy than expert opinion would be an interesting area for discussion. For example, people grasp paragraphs, chapters, stories and atmospheres, the unit of analysis is rarely a single, isolated sentence.

The scope of this problem stretches further. The CWIP 'measuring instrument' and the decisions which arise from it, it could be argued, are at times 'mutually exclusive'. Also, at other times the relationship of the measuring instrument and the decisions it prompts, could be said to be 'non-reflective' of the dynamics of the environment.

We will be discussing, in due course, the value to organisations of the skills of experienced personnel but at this stage we can consider it as the 'neglected dimension' in the CWIP measuring system. I refer to an organised monitoring of the value of human assets, a most important contributor to the running of the Bank, at all levels.

Of course, this area was explored informally by the analysts during their on-site investigation of the Bank, but in view of the demographic problems that will be facing all organisations in the future, it is recommended that a more structured approach be developed for this problem as soon as possible.

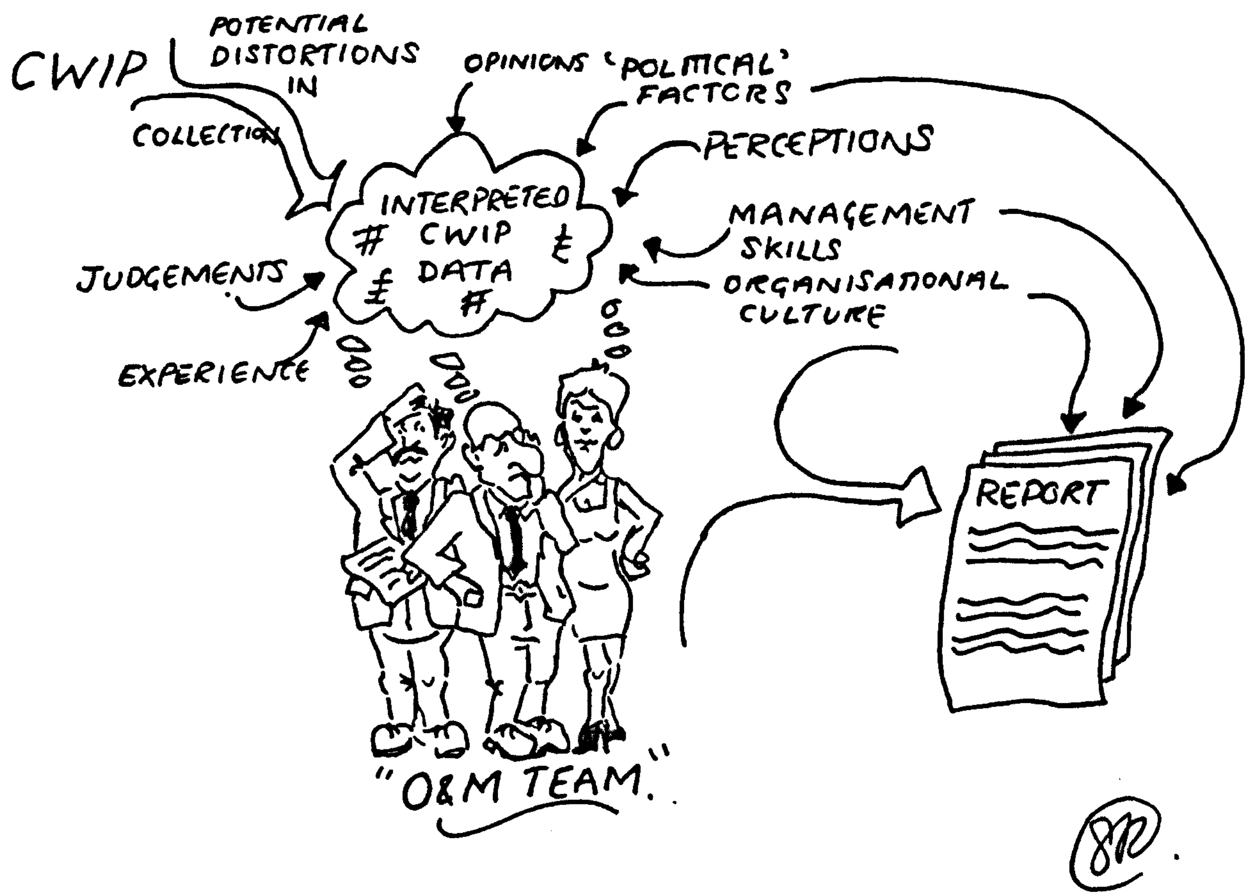
Paton, as long ago as 1962, stressed the significance of this argument;

> *'In business enterprise a well organised and loyal personnel may be a much more important "asset" than a stock of merchandise...Until some scheme is found by which these imponderables of the business enterprise may be assayed and given definite statistical expression, the accountant must continue to prepare the balance sheet as he has been doing... At present there seems to be no way of measuring such factors in terms of the dollar; hence they cannot be recognised as specific economic assets. But let us accordingly admit the serious limitations of the conventional balance sheet as a statement of financial [organisational] conditions.'* p104.

There has been some academic attention paid to this since Paton first made this remark, [Likert, R., The Human Organisation, New York; McGraw Hill 1967, and the same author in 'Human Resource Accounting- Building and Assessing Productive Organisations,' Personnel, vol.24, pp8-34, 1973).

Many authors still believe that there is an unsatisfactory lack of attention to standards for measuring an organisation's human resources. Most organisations are highly efficient at measuring and minimising the costs associated with personnel management in its various phases, but little or no attention is given to tracking the benefits or marginal utility of experience and expertise; e.g. experience with certain systems in environments (this will be referred to later as 'episodic' knowledge), career broadening assignments, routine career progress, training course marginal utilities and specifically, postgraduate education.

It is generally conceded that most employees, particularly managers, find these opportunities valuable, but no general technique is available to measure or record the values that may be obtained and hence expert knowledge (know how) accumulation continues unpredicatbly. This area should be the concern of the Organisation and Methods Department, since although currently their specification is for assessing staff numbers and structure it is also within their present remit to consider 'Managerial Roles'.



The Strategic Role of the Analyst.
Figure 1

The problems caused by the vagaries of workforce dynamics will be exacerbated by demographic changes, and mean that this potential resource of 'know how', unless it is monitored or 'captured' [see points on Technological aids), will very likely be lost permanently to the Bank over time.

Hence the role of analyst in processing organisational information and assimilating wide ranging organisational knowledge must be urgently addressed. Their strategic importance can be usefully displayed in a diagram. In the figure, it can also be shown that there is no direct relationship between the output of the measuring system and the subsequent development of an organisational structure to meet the project's stated objectives.

The measurement system as it stood in terms of the CWIP system is not a direct mapping into the real world issues it was intended to address and thus arguably is inadequately able to offer guidance in these areas.

Further, during the course of the study, the individual analysts stressed that they are aware of the 'grass roots' market and organisational information being generated at the non-formal level in their on site analyses as having importance to strategic issues and policy decisions. Indeed, it is felt that there is a well developed awareness of strategic matters assimilated intuitively in their individual modus operandi. So far this has been a neglected resource and what must be an urgent task for the bank should be to make these matters and processes explicit.

Additionally, there were frequent attempts to extrapolate from past data to indicate present and future trends. This was particularly true with reference to activities pertaining to business development and personal account transactions. Extrapolation itself is not an accurate predictor of future needs which will almost certainly arise from changes in the environment. The internal Organisation and Methods Discussion paper on CWIP [Ref;JLWB/FS] raised the resulting associated problems,

> *'Recent events have however highlighted the inadequacy of using Work Measurement statistics in isolation from other factors...work monitoring in itself will not highlight the need for procedural or other cost saving activity.'* [p 8].

Hence many assumptions have been made at an unknown stage concerning the predictive capacity of the measurement instrument, although we have not established whether the CWIP system is sufficiently robust to give the Bank a market and/or organisational advantage. It is suggested that in this context the role of the Organisation and Methods Department should be reconsidered to make it more pro-active rather than statistically, 'stimulus-response'.

This consequence is entrenched in the fact that the Regional Office does not allow decision-making to be based on anything but reflective, that is, 'backward looking' data. How this condition restricts strategic and operational capacity of the Bank to respond to market needs should be obvious and it will ensure that the Bank can only remain a 'market follower' in the provision of financial services.

ii) Recommendations on CWIP

Since the Sheffield review was also intended to consider 'an analysis of methodological procedures implied by sectionalised work measurement' an internal discussion paper on CWIP has been already produced [Ref: Organisation and Methods JLWB/FS].

Some brief remarks may still be timely at this stage. The six major proposals of the discussion paper have been reproduced in Appendix 3.

That paper reflected a number of the points implied in the previous section. For example examination of the Standard Hours Calculation Sheet indicated that 95% of recorded workload was accounted for by 30 of the 120 controls. Further analysis postulates that a very large number of the controls collected and recorded are virtually redundant in terms of the value of the analytical information they contain. Additionally closer examination of the recorded controls revealed that their information value could be enhanced by merging it with another.

This suggestion clouds the issue of the fundamental reasoning behind a basically dichotomous decision for the Bank concerning the CWIP measurement system. Either the Bank can continue with the systems in a revised format; including additional controls such as 'Word Processing Measurement' to replace 'Letters Produced', 'Advances measurement' or Business development allowances. Or the Bank can radically rethink its method of internal review based perhaps, on the kinds of analogical principles incorporated in integrated databases, as will be discussed later in the paper. This would re-direct the focus of any investigation away from the techniques of quasi work measurement, back towards the analytical and perceptual skills of the Organisation and Methods analyst.

Possibly the most valuable lesson to be learnt from the CWIP review is the realisation that the significance of information gathered under CWIP in its current format escaped many branch officials.

Although I have not tackled the associated issue of the Managerial Workload Rating in this analysis, that figure offered an example of a deliberately distorted [by use of a 'hidden' constant] workload measurement. This was done to prevent the managers concerned becoming aware of the meaning constituents of their subsequent rating. This must be seen as having a detrimental effect since it would reinforce an unnecessary 'conceptual boundary' between the figures collected by the Organisation and Methods Department and what is going on in the actual branch bank.

In other words, there appeared to be is no obvious connection between the purposes of the 'monitoring and the monitored' in this respect. I would recommend a rethink of the format of any work measurement figures, both to de-bunk their presentation and to make them much more 'User Friendly'. Clearly, the situation described would actively encourage under-utilisation of potentially useful and relevant information. [In this context, please see the distinction that will later be made between 'Knowledge' and 'Information'].

Nevertheless, it is likely that any change in the CWIP reporting procedures or overall format would hopefully lead to releasing skilled 'Organisation and Methods' analysts from much of the unskilled filtering of paperwork that the present system entails. This would release substantial time and expertise to make other contributions to the overall 'efficiency and effectiveness' of branch and departmental areas.

iii) Commentary on the Role of the Analyst;'The value of experience'.

I have already mentioned that the role of the Orgnaisation and Methods' analyst is worth a closer analysis. Morgan's commentary provides a useful prelude to the points to follow;

> *'Effective managers and professionals in all walks of life...have to become skilled in the art of "reading" the situations that they are*

> *attempting to organise or manage. The skill usually develops as an intuitive process, learned through experience and natural ability. Though at times a person may actually declare that he or she needs to "read what's happening at X", or to "get a handle on Y", the process of reading and rereading often occurs at an almost subconscious level. For this reason it is often believed that effective managers and problem solvers are born rather than made, and have a kind of magical power to understand and transform the situations they encounter.'* [1983]

We may now take this point somewhat further. The experience of Organisation and Methods' analysts plays a significant role in the respect of allocating staff and with branch management, devising organisational structures capable of responding to expanding workloads and hence business. Again and again during the on-site analysis reference was made by the Project Leader and other members of the team to previous branch reviews in both written and recall forms.

Overall, it was found that the individual skills and experience of the analysts and the project leader were of high significance in devising the new structure for the branch and this involved incorporating what appeared to be intuitive behaviour at times.

This brings us to a necessary analysis of the methods by which experience aids the practical problem solver. How do they know what they know, how do they use their experience to reason and think and create new from old? For example, in this study, the process of analysis appeared to use the measured CWIP data not in the reductionist fashion it was arguably intended, [that is in reducing the various tasks to their components and then re-assembling them in new structures enhancing their component's 'efficiency and effectiveness'].

Instead, it is suggested that the data gathered was utilised interpretively in quite the opposite way, to immediately attempt to build a bigger picture, in order to understand the particular context of work flows, control and co-ordination processes and management information flows, holistically.

The broad contextual kind of understanding that was being sought in this way, psychologists would argue, is that involved in establishing the closest description available to explain a particular process or situation and then discriminating between the previous experience and the present case under examination. In other words, by eliminating the 'negative analogy' between the two cases and preserving what is correct, that is, the 'positive analogy'. Thus 'understanding' is thus a process that has its basis in memory (human or documented), particularly memory for closely related experiences accessible through analogy.

The wide occurrence and usefulness of reasoning by analogy in managerial thinking is accepted by many organisational theorists [Morgan, 1986]. It normally begins by making an analogical/metaphorical link between two situations, reflecting on each [otherwise 'abstracting a model' of each] and then looking at the possibilities of importing information and knowledge across the two boundaries.

In other words, the experienced 'O&M' analyst when s/he is confronted with a problem, compares it with what was done elsewhere, or in the past with a similar problem. This is clearly an informal attempt to use 'analogical reasoning', that is, using what is known about one situation to investigate an unknown situation, the present problem under scrutiny. Interestingly and indicating the value of the approach, much of what is known about analogical reasoning is to be found in the literature on 'intelligence research'. Psychologists have a high degree of confidence in the suggestion that 'analogical reasoning' is a distinguishing feature of what we call 'intelligent' behaviour.

We are all familiar with the use and utility of analogies, but the establishment of useful learning analogies of this sort is a non-trivial exercise. Although not fully agreed upon in the field, there is a basic 'information processing' model of analogical reasoning which has some relevance here. In this model, the analyst must begin solution by analogy by 'encoding' analogy terms, that is translating them into an internal representation [a model] upon which other mental operations can be performed.

This behaviour was clearly evident in the progress of the case study. The project leader made repeated sketchings of potential organisational structures and their evaluation as candidate solutions appeared to take the form of incremental adaptations to an organisational structure developed in this form quite early on in the study.

De Groot [1965] suggests that 'experts' differ from 'novices' in their capability to recognize individually a very large number of different problem situations, and Klein [1980] argues that experts reason holistically, by analogy with previous similar experiences, rather than by explicit analysis and computation.

In other words the 'expert' might produce creditable judgements of problem situations and components which he has seen before but that a less experienced individual would need to analyse into more familiar elements. Further Klein and De Groot argue that 'expertise' arises in this way from perceptual abilities; from the paradigm of the Gestalt tradition of instant recognition. The capacity to recognise 'whole situations' is refined to the point that predictions or decisions learned through experience intuitively accompany situation recognition.

Theorists disagree about the roles of three intermediate comparison operations that occur in recognising situations 'analogically' in this way. The stages themselves are called *inference, mapping* and *application*. They take place between the analyst's model building and indicating 'a response', that is, decision-making about the validity of past experiences as useful analogies upon which to draw in the current situation.

In systemic terms the 'encoding' process is a factor of the 'attribute identification' between the two domains, [Previous experience and the current problem]. *'Inferring', 'mapping'* and *'application'* constitute an attribute comparison process between the two systems of interest. [Previous experience and the current problem].

Thus, to briefly enlarge on these terms;

> **'Encoding':** In the process of encoding the analyst begins identifying a feature of the situation [e.g. a particular facility for business development) and then retrieves from long term memory the attributes (e.g. political factors, skilled management support routines, rapid responsiveness characteristics in a particular branch) that may be relevant for a solution by analogy. The result is then stored in the 'working memory'. Potentially relevant attributes are those that experience has indicated are useful in relating one concept to other concepts.

'Inference': Next the subject infers a relationship between all values of corresponding attributes between the two sets.

'Mapping': The relationship is then mapped onto the unknown domain of the current situation.

'Application': A decision is made pertaining to the validity of the experience to the present situation.

It thus appears safe to conclude that the expert's representation of knowledge in memory, retrieval and verification processes will play a vital role in establishing alternative candidate structures.

An expert is defined in this analysis as one who possess two types of knowledge; deep structure semantic knowledge and significant accumulations of concrete, episodic knowledge. 'Semantic knowledge' concerns that human ability to construct an internal representation of 'reality'; to take in and interpret perceptual impressions to combine them with the products of previous experience, to draw inferences, implications and predictions and to reshuffle them into novel combinations. Episodic knowledge captures the temporal and spatial context of a person's past experience.

This definition is consistent with findings that maintain that experts are those individuals who possess such wide 'autobiographical experience' that they need not rely on the methods of formal analysis to infer the requisite information. This is arguably the case in this study, in that the CWIP material is used to a degree, for post hoc purposes. Novices, by contrast cannot recall either deep structure semantic nor obviously, substantial episodic knowledge.

But experts too are subject to error and in this context we must look at the consequences of 'intuitive' modelling and judgements made under the influence of memory and the pitfalls of the use of analogical reasoning with reference to previous experience.

There is a large body of psychological literature available in this area comparing the descriptive behaviour of analysts' 'intuitive problem-solving activity' involving information processing to the normative results that would prevail if people followed an optimal procedure. Silverman [1983] distils from psychological literature six, [of twenty seven,] 'cognitive' biases that affect information processing. They are presented as having particular reference to the analyst's task of processing data and information on-site in branch reviews.

Firstly, **the availability of data;** the decision-maker tends to use relatively easily available information and the possibilities of not easily collected information are ignored. This tendency is subject to strongly emphasised post-hoc rationalisation. Further an event or situation is believed to occur frequently if it is easy to recall similar events.

Secondly, **ease of recall;** data which can be easily recalled or assessed will affect perception of the likelihood of similar events occurring again. More easily recalled data and experiences are weighted more in decision-making than the reverse case.

Thirdly, **fact-value confusion**; strongly held values may often be presented as facts. That type of information or, in this case measurement, is sought which can confirm or lend credibility to, one's own views and values. In this sense, contradictory information tends to be ignored. There is in other words, 'selective perception'.

Fourthly, **'gamblers fallacy'**; the analyst or the decision-maker falsely assumes that a 'run' of some events enhances the probability of a change in the series of events.

Fifthly, **representativeness;** when making inference from data, too much weight is given to results of small samples. [This tendency has already been raised in the preceding section]. As sample size is increased, the results of small samples are taken to be representative of larger populations. The "laws" of representativeness differ considerably from the laws of probability.

Lastly, **selective perception;** issues are structured on the basis of personal experience and wishful thinking.

More generally there are likely to be a number of human frailties in 'information processing' and even before that stage when data is susceptible to the identifiable biases given above, others are evident at a broad level of 'retrieval'.

These include, when too little information is generated in response to a query requiring creative supplementation, or estimation. Too much information is a problem when not all collected data is relevant or necessary, and then there are additional problems of 'false recovery'. These occur when in recalling experience, there is *'overshoot'* or an item is correctly recalled but taken out of context' and *'fabrication'*, the incorrect reconstruction of a desired piece of information' and *'self correction'*, in which a sudden mind change occurs in the absence of information or hints.

iv) Recommendations Pertaining to the Role of the Analyst

There is a popular 'common sense' view among analysts and in general, that experience in a particular field engenders 'intuitive' problem solving skills Hopefully, it will be agreed that the potential utility and effectiveness of reasoning by encoding analogies in this way, is too important and complex a process to be left to the peculiarities of human judgement alone.

For example, forgetting/remembering and then analogising experiences will be affected by the degree to which the original organisational system was studied with respect to gaining an understanding, or for short term recall purposes. Of course individuals differ in short and long term memory capacities, familiarity with the subject, sensitivities, semantic abilities, opinions, biases and purposes.

The view presented clearly rests heavily on the suggestion that analysts tend to use their own experience of their job in forming broadly based mental models of the situation. For example, there is much talk, on arrival at the branch, of 'getting the feel' of the environment, and also a healthy degree of informal speculation on what soft systems thinkers would call 'cultural', 'social' and 'political' sub-systems to the visible organisational system under investigation.

In a sense, the mental models constructed with the aid of situational analogy will lead to a degree of selective perception of measured CWIP results. Subsequently, whatever utility to the analysis the CWIP data might originally represent is susceptible to the problems of interpretation and decision-making under the influence of memory outlined above.

Nevertheless a number of prescriptions can be offered to relieve the effects of biases of this sort.

- information should be sampled under a broad data base, including data bases which might contain disconfirming information.

- avoid the 'hindsight bias' [i.e. the temptations of post-hoc rationalisation] by providing access to information at critical past times.

- the use of structured methodological frameworks based on logical reasoning in order to avoid confusion of facts with values.

- analysis from time to time of the type and size of the sample that data has been gathered from, to avoid the representativeness bias.

- record and document potentially relevant comparison cases to aid the establishment of proto-typical formats and examples, e.g. the restructuring of the 'Sheffield Group'.

- record and document what the various decisions are intended to accomplish in their individual contexts to maintain broader perspectives.

- record and document options generated in comparative cases.

- evaluate options for their implications in a broad, 'strategic' context. In this way conflicts and incompatibilities can be identified early on.

Overall I hope that it will be agreed that the individual's development of these complex-intuitive analytical skills deserves some help. Sternberg [1975] in his studies of how people behave in problem solving activities, found that the difference between high and low scorers appeared to be the result of a lack of a consistent, systematic strategy on the part of the low scorers. Other major sources of error were lack of time, idiosyncratic knowledge gaps and inferential errors, i.e. just the sort of real world constraints analysts would normally encounter.

In this context I would like to present a brief discussion on some ways in which the benefits of the analogical approach propose the development of areas for computer aided analysis by comparison guidance and other management decision support possibilities. Artificial intelligence of this sort has moved towards the development of 'expert systems'. The intention of these systems is described by Counelis [1989],

> *'Such [expert systems] purport to provide the systematic use of logical thought within some given area be it chemical analysis, college student selection, engineering design, medical diagnostics or symbolic*

mathematics. The intent for expert systems is to replace the human art of logical and scientific thought [which are not errorless] with a relatively higher level of subject matter competence and a greater degree of consistency in thinking'.

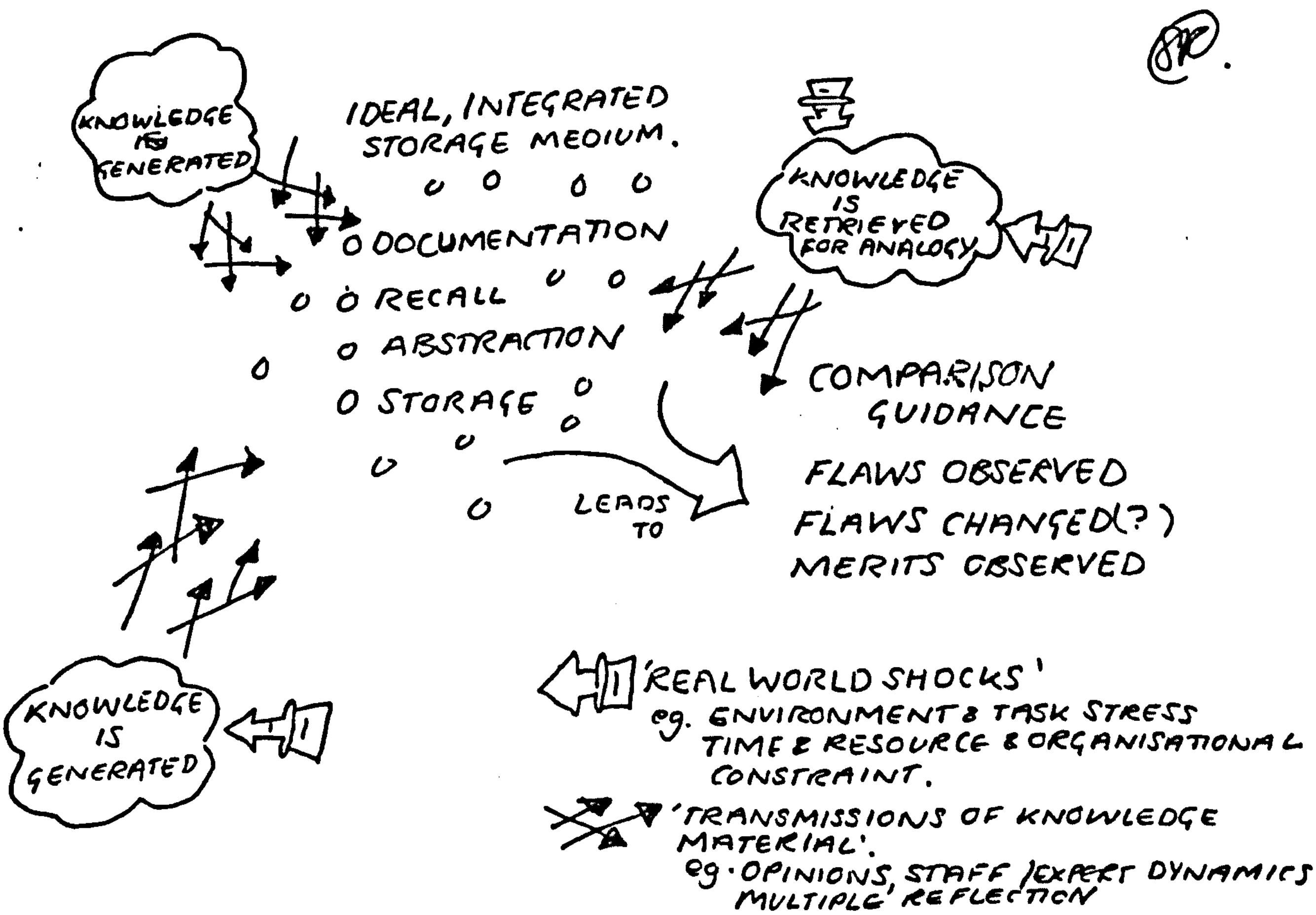
Although I would emphasise the usefulness of expert systems as being in supplementing rather than the replacing human decision-making processes, the above description outlines the direction of their present development.

v) Technological Possibilities for Analysis Support.

Silverman [op.cit.] has stressed that the metaphor of the *'analyst as an information processor'* holds great promise for helping to pinpoint precisely what kinds of integrated information systems and adjuvants (e.g. handbooks, software packages, expert systems, and so on) will best support the practitioner working in the field for whom bounded rationality, time limits to achieve a solution and organisational constraints all affect the analytical process.

In accepting the significance of the analogy between the analyst and the human 'information processing' view, there are a number of promising potential outputs. The value of this metaphor is twofold. Primarily, it delineates a basis for determining the types of information systems and analytical aids that an organisation should consider providing by way of support for analytical reasoning [and planning by analogical methods]. The usefulness of analogy is in providing the theoretical basis for the design of expert systems. Expert systems are 'single-purpose conceptual tools which provide knowledge based problem solving. The operational premise of expert systems is that belief in the results is credible because the data base is a source of 'true facts' and the 'logical engine' is appropriate and precise in rendering results. Overall, the perspective of the approach goes beyond the boundaries of the Organisation and Methods Department.

Secondly, in this case the metaphor clearly provides a research direction towards a generalised analogical reasoning system [although whether this could ever be automated or would require methodological enforcement would need to be further investigated].



Elements of System Knowledge
Figure 2

The view of the descriptive emphasis in establishing analogical frameworks for investigation of branch structures suggests that a great many practical aids to the analyst in the field could be developed. The entire knowledge generation process is highly dynamic, for example, what was originally felt by one set of analysts to be a 'good structural design' is later observed by a different set differently. Knowledge generation is hence continuous and in flux i.e. what is at one time believed to be true, will change over time.

Nevertheless, fairly strong and specific requirements for knowledge and information systems are beginning to emerge in most forward-looking organisations, if they have not already done so. It is felt that this is an issue that the Bank should address. There is at present a chronic under-utilisation of technological resources at branch [free access to PCs would have aided compilation of much of the normal data records kept at the branch concerned, e.g. personnel attendance files and also aided monitoring of the manually recorded CWIP figures]. This lack of utilisation at branch level must also raise questions as to the optimality of broad Data Processing and retrieval policies in the context of Organisation and Methods.

Hence a possible remedial option in this context would be to change the remit of the O&M Department to allow them to have a formal input to computing and systems analysis and design. This is normal within some conceptualisations of a Services Division and such responsibility would fit most comfortably into the O&M department, since they are already versed to some degree in data, branch and strategic issues. Overall, the relationship between computing and organisational design should definitely be enhanced since there is much of value to be exchanged between these two areas. [See 'Horizontal' knowledge integration].

There are various forms of information integration; e.g. 'vertical', 'horizontal' and 'longitudinal' (temporal) systems within the information field. Vertical knowledge integration is needed to avoid the situation of high level analogy practitioners having to re-collect information previously collected at lower levels. Horizontal knowledge integration across operating groups and and managerial functions is necessary, due to the multi-faceted nature of systems design through operation. Longitudinal (across time) knowledge integration is necessary due, on the one hand to the turnover, inexperience and/or availability of what we have named 'experts', and on the other to the sheer bulk of 'relevant information' that might be generated. We refer to 'knowledge' integration in this respect in order to highlight the distinction between 'information' and 'knowledge' being that the latter is 'full of expertise'.

In other words, management information systems could be designed with specific information retrieval goals, i.e. to facilitate analogical comparisons. Currently most data is stored principally as historical records. The prescriptions for expert de-biassing call for documentation that would form easily accessible input for such a database. The implication is that if decision aids are to exploit the individual capabilities of each potential user then a variety of adjuvants, with different functions and at different levels of aggregation should be made available.

This policy would involve building relational databases and establishing *'memory-modelling'* capacities to allow an increased use of computer-aided analogical planning and design. Once developed, this kind of expert system could guide the selection and/or generation of the 'ideal' analogy for planning or design problems, rather than the 'most recent' analogy which could be selected by analysts lacking technological support.

This approach would avoid the old problems of non-integrated information systems for example, the same knowledge would not have to be collected many times for various levels or segments of the organisational hierarchy. There would be, necessarily, increased communication between these parts such that collection of replicated and hence redundant, relatively non-interfaceable knowledge systems would be avoided.

Notably, the integration of information systems in such a large organisation as the Bank is to some degree inevitable but it will have radical implications for the structure of the entire Services Division and possibly further afield than that. In this context, it might be wise to consider the possible affects on control and co-ordination processes now.

These could be affected, it is suggested by Silverman [op.cit], in two major ways. Firstly in the control and co-ordination of technical aids; cost growth, systems user take-up/satisfaction, level of technical performance and significantly the perpetual problem in building expert systems, the capture, storage, retrieval of accumulated 'Know-How'.

Secondly, but arguably more difficult to manage, the organisational changes involving redefinition of certain jobs, or elements of jobs requiring analysts to become much more computer literate in order to facilitate the development, maintenance and usage of *'comparison guiding'* analytical aids.

CAVEAT

We have not in this discussion mentioned the opposing argument from a school of psychologists [e.g. Dreyfus, 1982] who argue for the existence of a non-verbalisable mental capacity which is called 'intuitive experience'. This experience is only actualised by situations and it may not be described by the expert. Hence, Dreyfus [op.cit] conjectures that,

> *'Believers in information processing... must hypothesise an unconscious decomposition into facts and recomposition by means of rules. Be that as it may, research shows that experience created typical examples, and the facts and rules, if any, that produce them are unavailable to the conscious mind of the expert. Consequently, they are non transferable to the mind of the modeller of expertise.'*

The twin horns of the dilemma are that unaided intuition is subject to bias at lesser or greater degrees and that the information processing analogy will not lead to replication of human behaviour in analogical reasoning.

One way out of the dilemma would be to stress that the 'quality of service context' in which we would hope to build an expert comparison-guidance system is not a system which is anxious to retain diversity which may be considered as a by-product of 'intuitive' problem-solving.

SUMMARY

This report has considered a broad perspective on the current role and potential directions for the development of the Organisation and Methods Department of the Bank.

At the time of writing, there was already some debate within the department concerned with improving the quality and relevance of the information being generated by the O&M methods of analysis and in this context extending the range and utility to the User.

We also mentioned initially in discussion, how in some cases the types of work undertaken will sometimes reflect the way in which the department is perceived by the rest of the organisation, with more responisve and dynamic departments attracting a more varied remit of projects. The dynamism is already there, and this report has, it is hoped, suggested where new projects might arise.

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APPENDIX A-1

O&M REPORT CONCLUSIONS

Ref: Church Street Branch; O&M Department JWLB/March 1989.

The management recommendations of the O&M analysts were as follows;

'This report recommends a change in structure to strengthen and support the Management Team, in particular by the creation of an Advances Support Unit, together with a unit providing personal customer services in the new open plan Banking Hall. The existing separate functions of Securities and Foreign should be merged together and the number of Accounts Units reduced from three to two.

The proposed structure should serve to reduce the pressure on the senior officials and enable greater time to be devoted to Business Development activity at a variety of levels. The other problems recognized of [sic] lack of capacity for proper delegation, inconsistency of staffing on the Accounts units and confused areas of responsibility will also be resolved.

The reorganisation is possible within the existing accommodation although the need to create an additional Banking Manager's room in the short term is evident.

An overall increase of five staff (4 officials, 1 clerk) is recommended as part of the package together with re-evaluation of all officials positions with the exception of the Senior Manager'.

APPENDIX A-2

THE UTILITY OF WORK MEASUREMENT

Work study and Organisation and Methods are often talked about together since many of the principles and techniques involved are common to both areas. Generally work study is more concerned with work methods and Organisation and Methods with the content of work systems. Both should be, however, concerned with the systematic examination of human work in all of its contexts, the presentation of the facts and the making of recommendations in order to improve productivity and/or service.

Work measurement follows the gathering of data processed in order to identify individual components of the work system. Work measurement analyses the content of a task in order to make more effective use of workers, control materials scheduling and labour costs, establish performance indicators and so on. The necessity for each unit is examined as to whether modification, elimination or combination is possible together with the appropriateness of new technological applications.

At the highest level, the procedures of work measurement are just as relevant to the planning and design of new systems as they are to the update and augmentation of the existing systems. At the lowest level, the basic of analysis tool is the structured determination of an appropriate time for a defined task to be completed by a specific method.

Of course, different methods for carrying out the same job need to be compared so that members of staff, machines and their total cost can be evaluated. Similarly, when new methods are introduced work measurement will help to measure improvements in 'performance'. These 'measures' can subsequently be used to highlight a poor performer, bottlenecks and ineffective work distribution. It will aid the matching of the man/machine capacity with the work to be performed. Peaks, panic overtime and delays should all be uncovered and eventually eliminate by work measurement.

BH Walley [1973] gives points below as necessities in work measurement assignments.

1. Preliminary method study to evaluate the 'best way' of carrying out the job being studied.

2. Decide the method of work measurement to be used, this is specifically important in clerical work measurement since the method must match the situation.

3. Plan the operation. Decide which jobs need to be measured. Lay down an appropriate timetable and inform all concerned (management and staff) of what is going to happen.

4. For each job which has to be measured, a breakdown into appropriate elements is required. An element is a part of the job which can be measured as a part separate from the whole. There are, for example, repetitive elements, random elements, constant elements, variables, manual and machine, constraint and foreign elements.

5. Carry out the study which will be concerned with two factors. Firstly, the rate of working, i.e. what rate of working is actually being achieved. Secondly, the time taken monitored by reading element completion times from clock or watch. [in this instance the Organisation and Methods Department habitually refer to the ubiquitous Mulligan manual]. Walley suggests this can be assessed by accumulation of numerous observations and the results averaged.

Various element times are then added together and an operational time results. To obtain a 'normal' time for the operation it is necessary to relate the observed time with the observed rating using the simple manipulation;

$$\frac{\text{Observed time * observed rating}}{\text{Desired level.}}$$

A number of allowances are normally included in the 'normal time', e.g. fatigue (energy output), posture, motions, personal needs and general environmental conditions. The 'harder', the job the longer the fatigue or relaxation allowances need to be.

APPENDIX A-3

SUMMARY OF MAJOR PROPOSALS

Six major proposals are set out below. Whilst each can be seen as an individual exercise, each will impact on the others. It is therefore suggested that they be treated as six elements of one integrated project.

1. Susbstantially reduce the number of controls reported, both manual and computer captured.

2. Introduce a small number of new controls for Advances and Business Development work and dispense with the Managerial Workload Rating system.

3. Reformat the Standard Hours Calculation Sheets to improve user understanding of the information provided.

4. Eradicate paper from the procedures by taking advantage of the introduction of P.C.'s to branches.

5. Update Supervision, Error and Special Job Allowances through Activity Sampling.

6. Examine other organisations' methods of setting staffing levels in a sales orientated environment.