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# LOOKING AT THE MANAGEMENT SCIENCES THROUGH THE LENS OF AUTOPOIETIC THEORY

by
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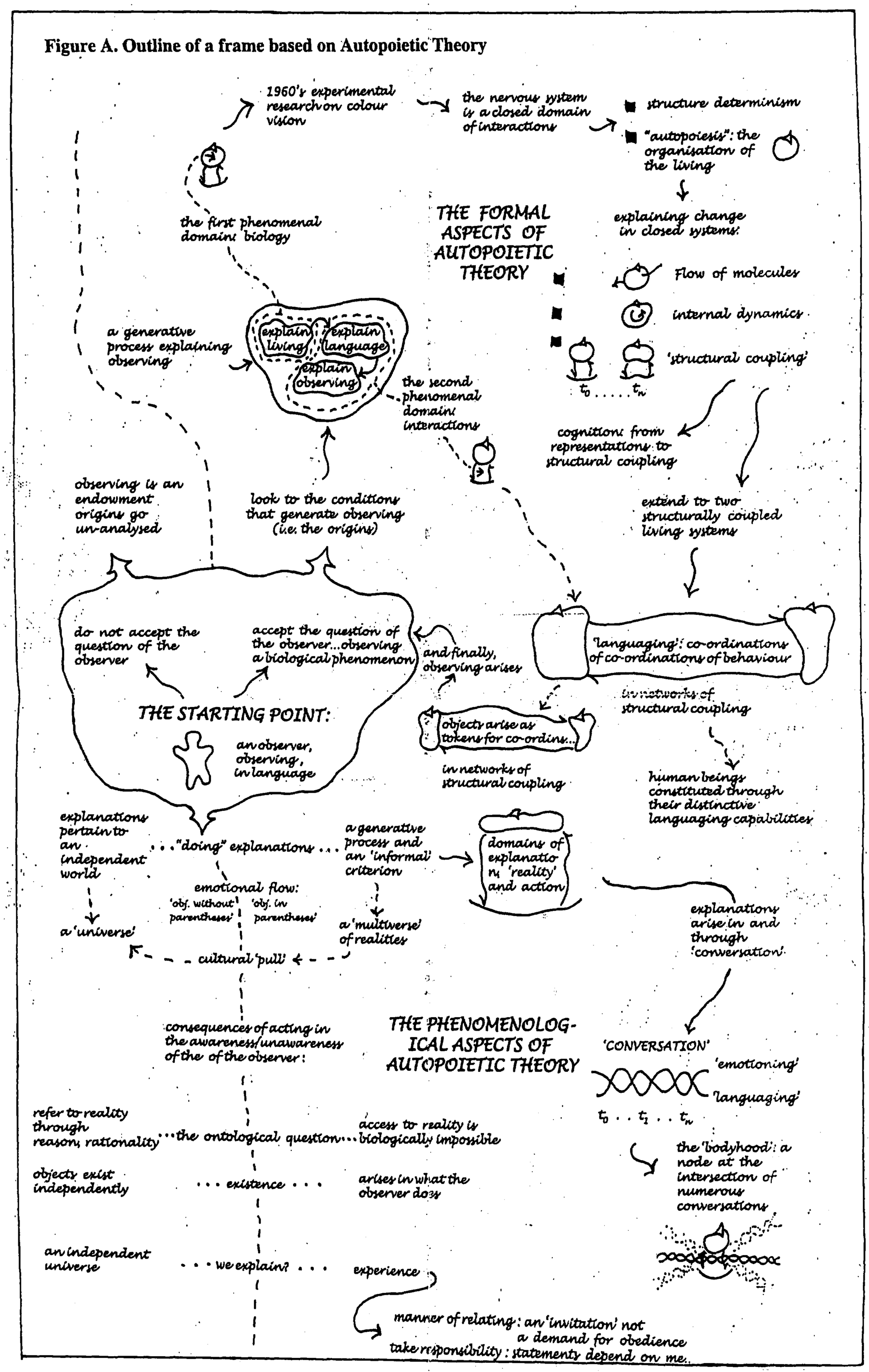
**Figure A. Outline of a frame based on Autopoietic Theory**



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# SUMMARY

The broad aim of this research has been to employ the various propositions of Humberto Maturana, as derived from his and Francisco Varela's central concept 'Autopoiesis', as a lens or frame through which to critically reflect on both Management Science and major traditions within it. Such reflection has been carried out with both a 'critical' and a 'developmental eye'. From a critical standpoint the research identifies gaps in existing understandings, and suggests ways in which these may be plugged. Used in this mode, the research shows that Autopoietic Theory is a body of knowledge that management scientists, especially inexperienced ones, can turn to as a means of enriching and/or enhancing their practice in distinctive ways, or allowing them to better prepare for it.

Used from a developmental standpoint, the research shows how Maturana's epistemological propositions invoke a particular kind of critically reflective Management Science practice, and, how Autopoietic Theory more generally, can stretch the limits of existing practice.

In developing these lines of argumentation the main contribution of the work is to remind members of the various Management Science communities that theirs is an activity that is carried out by real human beings first and by impartial scientists second; moreover, that Management Science is an activity that takes place in human, social and organisational contexts. Acknowledgement of this has far reaching ramifications. In that regard, the main contribution of the research can be taken to be an argument in favour of repositioning 'humanity', in all its various facets, much more centrally within the discipline than has been the case hitherto.

# PART ONE: OBJECTIVE, METHOD, FRAME

## 1. INTRODUCTION

### 1.1 Introduction and basic definitions

The broad aim of this research is to employ the various propositions of Humberto Maturana, as derived from his and Francisco Varela's central concept 'Autopoiesis', as a lens or frame through which to critically examine the self images that management science presents of itself, as a set of activities and practices, through its own literature. I further aim to show how the epistemological propositions of 'Autopoietic Theory' invoke a particular kind of critically reflective management science practice and how autopoietic theory more generally can stretch the limits of existing management science practice.

Much more will be said about the logic of the research in Chapter 2. Here I shall begin by offering the suggestion that the marriage of abstract ideas that originate in the life sciences with those that have more to do with the practical matter of how people might go about intervening to improve the functioning of organisational systems, is not as unseemly as it might initially appear. Indeed it brings to mind Feyerabend's (1993) maxim that advances in knowledge tend to arise more from the juxtaposition of ideas across diverse traditions, than they do from incremental developments within traditions. It is also consistent with the idea that speculative research that opens up discussion is valuable in exploiting synergy between approaches (Brown, 1992; Burgoyne, 1989). In

this spirit, the research aims to raise issues about management science that would not otherwise be raised; it does not necessarily aim to settle them.

At the outset it is necessary to provide some broad definitions. Following the usage of the term established by Whitaker (1996), I shall take *Autopoietic Theory* ('*AT*' hereafter) to extend beyond Maturana's biological contributions, which – strictly speaking – is where the term belongs. In total, I shall take it to include Maturana's contributions to the biology of cognition paradigm, to its progeny: *autopoiesis*, as well as to the series of propositions about human cognition and epistemology that comprise Maturana's so-called *Ontology of the Observer*. I shall explain this terminology later.

As for *Management Science* ('*MS*' hereafter), I shall largely trace a path charted by Jackson (1991). According to Jackson, this is a field of inquiry and professional practice that is concerned:

> " . . . *with the development and appropriate utilisation of rational methods of intervention in human affairs* (that includes) . . . *operational research* . . . (and) *is coextensive with hard systems thinking* (and) . . . *more recent systems trends such as soft systems thinking, organisational cybernetics, and critical systems thinking*". Jackson (1991:254).

Again, I shall further elaborate on this later.

## 1.2 The what and the why of the research

As the title and opening sentence of this work suggests, the aim is to reframe *MS* using the lens of *AT*. More generally it could be said that it is to "apply" *AT* to *MS*. But what does reframing as a form of application mean? Why might reframing *MS* be desirable? And why this particular frame? The rest of this introductory chapter seeks to provide answers to these questions.

### 1.2.1 Approaches to reframing

Conventional wisdom (see, for example Morgan, 1986; Davies and Mabin, 2001), has it that reframing involves looking at something in a different light, or as the title of this project indicates, looking at something through a different lens. Look through a different lens, so the argument goes, and you will see something different. Philosophically, this idea extends Husserl's (1900, 1901) proposition that we human beings always encounter 'objects' in the world not as they are, but *'under aspect'*. We structure our worlds, says Husserl, from a vantage point comprising something in the mind called *'intentional content'*. It is only through intentional content, so the argument goes, that an observer can have thoughts about some object under aspect. On this view, framing and reframing involves changing the vantage point to gain a different perspective on whatever object happens to be in the spotlight. All of which raises the possibility that we can take management science to be a pre-existing phenomenon or 'object of inquiry', that *AT* – looking from the 'outside' as it were - can examine, interpret and reflect critically upon.

That having been said, looking differently at a pre-existing object or critiquing an established set of ideas is not the only approach to reframing. For example both Pondy and Boje (1981), and Tsoukas (1991), note that frames (or metaphors) can reveal aspects of a pre-existing reality or create realities of their own. Indeed, as we shall see later, the possibility of creating a reality, or of constituting objects through acts of distinction, better reflects Maturana's own approach. According to this line of thought, frames are

tools for acting in and creating the world. They are not simply ways of reflecting on the world.

Pursuing this 'reality creating' approach to reframing opens up an interesting possibility. Since, as we shall see, *AT* does have its own explicit epistemology, this means that, in principle, it could be used as the basis for a new paradigm for *MS*, or less ambitiously perhaps, as the basis for 'doing' *MS* in a distinctively different way.

Some might say that the former is timely since the recent history of the discipline suggests that the paradigmatic status of *MS* is by no means settled. The soft systems assault on traditional hard systems thinking instigated by Ackoff (1974, 1979a, 1979b), Churchman (1971, 1979), and Checkland (1981), was followed during the 1980's by an equally vitriolic assault from scholars of an emancipatory and critical systems persuasion (see for example, Jackson 1982; Mingers 1984; Rosenhead 1987). Those who were bemoaning the increasing fragmentation of the discipline might have held out hope that the Habermasian-inspired critical perspective promulgated primarily by Jackson (1987, 1991) and Flood and Jackson (1991), might provide an overarching and unifying framework for *MS*. However such hopes were dashed during the late 1990's as a seemingly unlimited number of further 'critical' variants on the new paradigm theme were given an airing through the literature (see Jackson, 1994, 1997; Midgley, 1990, 1995; Flood, 1995; Mingers, 1997; White and Taket, 1997). Today the philosophical underpinnings of *MS* are as fragmented and contended as ever; not that this is necessarily a bad thing.

### 1.2.2 Charting a middle ground position.

Although there are these two distinctive perspectives on reframing, the research reported on here seeks to exploit aspects of both, essentially by 'softening' the extremes and charting a course between them. In a 'reality revealing' sense this means employing *AT* to critique *MS* but with an eye to extending and stretching the discipline. In a 'reality creating' sense it means employing *AT* as a basis for constructing a distinctive approach to *MS* but without necessarily having to grapple with the full discipline-wide ramifications that are implied when the object of the exercise is to develop a new paradigm.

To the extent that softening and integrating these two extremes is possible – and I shall deal with the philosophical difficulties of this later - the question arises as to why it is desirable. From a critical standpoint, reframing is desirable because it can identify gaps in existing understandings, and, where possible, plug them. This argument hinges on the idea that disciplines can be rather inward-looking, and that novel insight often requires stepping outside a community to examine it in the light of something that transcends it. Used in this mode, *AT* is a body of knowledge that management scientists – especially inexperienced ones – might turn to as a means of enriching and enhancing their practice in distinctive ways.

From a developmental standpoint, reframing *MS* involves considering what might happen, and what the consequences would be, should an agent turn to *AT* not merely to plug gaps in existing knowledge, but as a way of doing things rather differently, i.e. doing things according to a different philosophical and theoretical logic. Today there is increased acceptance of the legitimacy of enlisting intervention methods and techniques

in the service of philosophical positions that are different to those of their origin (see, for example, Smaling, 1994; Jackson 1997). Given this, the research challenge is to work out how the philosophy and theory that underpins *AT* might influence the use of particular *MS* approaches in concrete organisational settings.

### 1.2.3 Why autopoietic theory?

Clearly *AT* is not the only potential spur for thinking differently about *MS*, nor is it the only source of theoretical inspiration. So why *AT*, and not something else? The simple answer to this question is that there are a number of reasons for thinking that *AT* might be particularly useful.

Firstly, to the extent that the activity of *MS* is geared towards the production of knowledge, it is axiomatic that management scientists ought to reflect on this facet of their work. But do they? Maturana (1988a:37), remarks that most scientists ". . . *are not aware of the epistemological and ontological implications of what they do because for them science is a domain of practice and not a domain of reflections.*" In Chapter 4, I identify some fundamental differences between basic science and *MS*. However it would be difficult to argue that management scientists are any less pragmatically-inclined than are their basic science colleagues. If this proposition has any validity, I venture to suggest that management scientists can benefit through reflecting on a topic about which *AT* has much to say.

Secondly, *AT*'s rather unusual perspective on human cognition (from which its epistemology derives), provides further justification for its application in this setting. Whereas conventional wisdom has it that cognition is primarily a mentalistic process that

can be seen as a separate and discrete human activity, Maturana and Varela (1987), argue that cognition is involved, engaged, activity in the world. This, one could argue, gets to the heart of what *MS* is about. It is about being involved, engaging with problems, and acting to bring about an improvement in the practical affairs of organisations and their various constituents. In this context, such a perspective on cognition seems befitting.

Thirdly there is the proposition that if management scientists want to have a genuine impact in and on the organisations where they ply their trade, then they ought to have a sound understanding (beyond that which is acquired experientially) of the key challenges that must be faced when intervening in the name of organisational improvement. Paramount among these challenges are those that have to do with the complexities of human behaviour and the functioning of organisations.

Over the last thirty years or so it has become commonplace to hear people speak of the existence of an 'intellectual gap' between management scientists on the one hand and practicing managers on the other hand (see, for example, Ackoff, 1979; Checkland, 1981; Rosenhead, 1989; Senge, 1990; Chapman, 1992; Daellenbach 1995). Much of the gap, it is claimed, results from there being a certain naiveté among management scientists when it comes to transfering insights from their technically sophisticated models to the human and organisational contexts of their application. Thus, Pidd remarks:

> *"To the management science students I want to say, "Your maths and computer models are all very well, but what about the organisations in which you will work? Don't you think that the context of your work is worth some investigation?" management is through people and in organisations."* Pidd (1996:ix, emphasis added)

It would be wrong to convey the impression that *MS* has ignored entirely the human, social and organisational context. Nonetheless it is perhaps natural that management scientists, especially recent graduates with an applied mathematics or

engineering background, might prefer to concentrate their efforts on analytical and technical matters rather than becoming embroiled in philosophical abstractions or grappling with the complexities of human and organisational behaviour. Amongst this group it seems likely that the image of the organisation as a machine that needs fixing is more compelling than that which emphasises the human, cultural and politically contended nature of organisational life. Such predilections aside, if the management scientist is to have substantive organisational impact, then at some stage he/she is going to have to develop a broader appreciation of individual, interpersonal and organisational processes. Again this is a topic about which *AT* has much to say.

Finally over the last two or three decades, *AT* has attracted an enthusiastic group of people in various intervention fields who, through professional practice, have sought to operationalise some of its principles. This opens up the possibility that where there are overlapping concerns, *MS* might benefit through being exposed to these wider experiences.

## 1.3 Summary

In summary the aim of the research is to apply *AT* as a means of reframing *MS* according to two distinctly different perspectives on frames. In seeking a middle ground between the extremes of these two positions, it can be said that the broad aim of the research is to employ *AT* to critique existing understandings about *MS* practice that are promulgated through its own literature, and to do this with a view to identifying gaps, and where possible, by plugging them. I then aim to extend critique by examining what, in this domain, 'putting *AT* into practice' might involve.

# 2. METHODOLOGY AND THE LOGIC OF THE RESEARCH

I shall now switch attention from the 'what' and the 'why' of the research, and focus explicitly on the 'how' question. In this chapter I shall describe the methodology used, then, on a chapter by chapter basis, I shall outline the structure of the research and comment on the style of presentation used.

## 2.1 Methodology

When people describe and extract meaning out of phenomena their descriptions do not come out of nowhere; some cognitive structure is involved. In the literature, various terms have been employed for this purpose. These include 'paradigm' (Kuhn, 1970), 'cognitive schema' (Fiedler, 1982; Fiske and Dyer, 1985), 'personal constructs' (Kelly, 1955), 'habits of mind' (Margolis, 1993), 'mental maps' (Schutz, 1964; Senge, 1990), 'intentional content' (Husserl, 1900, 1901), 'metaphors' (Morgan 1980, Lakoff and Johnson 1980), and last, but certainly not least, 'weltanschauung' (Checkland 1981). Following Morgan (1983), Bolman and Deal (1991), and Davies and Mabin (2001), the concept 'frame' is used in a generic sense to cover such terms.

The idea of using frames in academic discourse has been one of the intellectual growth industries over the last decade, thanks mainly to scholars such as Bolman and Deal (1991), Morgan (1986, 1989, 1993) Bergquist (1992), Quinn, (1988), and Torbert (1989). In *MS*, and in various guises, Checkland (1981), Eden (1989), Flood and Jackson (1991), and Davies and Ledington (1988), have integrated the idea of reframing within

problem structuring methods such as Soft Systems Methodology, Total Systems Intervention, and Strategic Options Development and Analysis.

In simple terms, the idea of reframing is based on the premise that the mindset of a large proportion of the population can be described as *'departmentalised'* (Torbert 1989). This term seeks to convey the idea that many people operate according to a relatively small number of logics as the source of meaning, coherence, and validity. Thus 'reframing' draws attention to the narrowness of people's mindsets, and it arises as a means of looking at situations in new ways, and in opening up creative possibilities for action.

One thing that those who write about reframing tend to agree on, is that creative insight into phenomena requires some sort of 'external' perspective. Maturana, in his own distinctive style, remarks that you cannot observe such a domain by operating within it. You can observe a cognitive domain, he suggests:

> *". . . only through the recursive consensuality of language by consensually specifying another cognitive domain in which the first one is the object of consensual distinctions."* (Maturana 1988a:61)

The contemporary philosopher, Paul Feyerabend, claims that an external perspective is necessary because knowledge development often proceeds, as he puts it, *'counter-inductively'*. How, Feyerabend, asks:

> *". . . can we examine something we are using all the time? How can we analyse the terms in which we habitually express our most simple and straightforward observations, and reveal their presuppositions? . . . The answer is clear: we cannot discover it from the inside. We need an external standard of criticism, we need a set of alternative assumptions . . . we need a dream world in order to discover the features of the real world we think we inhabit . . . "* (Feyerabend 1993:22, emphasis added).

On this view, the critically reflective approach to *MS* promulgated (among others) by Checkland and Scholes (1990), that involves people reflecting systematically on their experiences, and then building this into the learning cycle, is undoubtedly a step in the right direction. But it is a moot point as to whether it goes far enough. Commonsense logic dictates that people have much to learn by reflecting on their experiences. But the amount and quality of learning depends very much on the nature of the reservoir from which they draw their experiences. The trick then is to expand the size of this reservoir.

At the level of the discipline as a whole, a number of management scientists have sought to broaden and extend horizons. Checkland (1981) for example, has sought to bring consideration of broader philosophical and social issues into the prevailing techno/analytic discourse of *MS*, as have more the more critically inclined scholars who were mentioned in the previous chapter. This has provided an opportunity for management scientists to connect with literatures, and to develop appreciations – for example in relation to the role of power in organisational life - that might not have emerged otherwise. Scholars such as these can justifiably claim to have broadened the backcloth against which management scientists reflect on their practice.

The point however is not to debate the relative merits of one reframing structure or another. Rather it is to highlight the importance, from a reflective practice perspective, of giving different perspectives an airing, and to encourage the guardians of knowledge production – especially journal editors – to continue to acknowledge the benefits of connecting with debates occurring elsewhere. Again, as Feyerabend remarks:

> *" . . . a scientist who wishes to maximise the empirical content of the views he holds and to understand them as clearly as he possibly can must introduce other views . . . He must compare ideas with other ideas rather than with experience . . . "* (Feyerabend 1993:21).

Hence the need for an external perspective, or a 'dream-world' as Feyerabend put it in the quotation referred to earlier.

The same theme of employing an external perspective to facilitate reflection and learning comes through in Morgan's idea of *'reflective conversation'*. According to Morgan,

> *"By reflecting on one's favoured research strategy in relation to other strategies, the nature, strengths and limitations of one's favoured approach become much clearer. In seeing what others do, we are able to appreciate much more clearly what we are not doing. In this way, we are able to create a means of developing and refining favoured research strategies in a way that makes them stronger . . . we increasingly realise the limitations of our favored perspective the more we explore with others, learning about ourselves through the other . . . "* (Morgan, 1983:381)

Sir Geoffrey Vickers, whose ideas assume greater significance later in this work, is another whose ideas can be drawn upon to support the logic of the inquiry used here. Vickers (1965, 1967, 1970), emphasises the importance of critical reflection as a basis for action, irrespective of whether this is carried out in the service of basic research or in support of practical agendas such as those pursued in *MS*.

To help researchers orient their activities in ways that attempt to take full account of the relations within which such action is set, the consequences of that action, and of alternative actions, there is a need, says Vickers, for a 'reflective awareness' or an *'appreciation'*. On this view, appreciation involves juxtaposing events and ideas as these develop over time. Such reflection, in what Vickers calls the *'Lebenswelt'*, provides the context within which learning takes place, and it becomes the basis for further 'intelligent' practice.

The logic of the research reported on here, is grounded in the tradition of inquiry promulgated by the aforementioned scholars. It could be said that the research uses *AT* as

a *perspective* employed in *reflective conversation* about *MS*; as a set of *ideas* juxtaposed against the *events* of *MS* in the *Lebenswelt*; or as a *dream-world* to illuminate the *real world* in which management scientists ply their trade. Irrespective of the terminology used, the logic revolves around the idea that since *AT* is an external perspective, i.e. one that would not normally appear in mainstream *MS* discourse, it has the potential to generate novel insight.

## 2.2 The structure of the research

Having outlined the broad logic that underpins the research, we can now summarize the structure of the rest of the manuscript. I shall do this on a chapter by chapter basis.

Because of its scope, its complexity, and my strong desire to present it as I believe Maturana would wish (i.e. as a coherent whole and not as a set of diverse concepts), it is not possible to use *AT* as a conceptual frame to sustain any useful purpose, unless there is a very clear and coherent understanding of exactly what I am taking it to be. The main objective of Chapter 3 is to carefully construct this frame.

Having completed Part 1 of the thesis, the structure of Parts 2, 3 and 4 (Chapters 4 – 9) which contain the main body of the work, broadly mirrors the 'critique' and 'create' perspectives on reframing respectively. Chapters 4, 6, and 8 adopt a largely critical perspective while Chapters 5, 7, and 9 adopt a largely developmental perspective.

The object of attention in Part 2 (Chapters 4 and 5) is *MS* conceptualized as a general class of activity, with particular emphasis being placed on what is often referred to as 'classical' operational research and/or 'hard' systems. From there, Parts 3 and 4

(Chapters 6 – 9) shift the level of analysis down from *MS* in general, to two of the key areas described in Jackson's formulation of the field that I referred to earlier. Part 3 (Chapters 6 and 7) examine the Theory of Viable Systems, an example of organisational cybernetics, while Part 4 (Chapters 8 and 9) examine Soft Systems Methodology, an example – perhaps the best example - of soft systems thinking.

Of the various areas of *MS* that Jackson mentions, only critical systems is not examined here. Primarily this is because of its scope, its breadth and its contested nature. Whereas soft systems and organisational cybernetics are well-established traditions that stand alongside the dominant paradigm of *MS*, the philosophical foundations of critical systems, as Mingers (1992) and Jackson (1991) both admit, are not yet fully worked out. The sheer number of variants of critical *MS* or critical systems makes the research proposed here beyond the scope of what is possible.

In Part 5 of the thesis (Chapters 10 and 11) the emergent frameworks on the VSM and SSM that are developed in Chapters 7 and 9 provide the opportunity of reporting on original empirical research, and on rethinking two interventions that the author instigated, designed, carried out, and subsequently had published. The logic of this retrospective examination of already-completed interventions is very much in the vein of Vickers *Lebenswelt* idea – the ongoing flux of events (the actual intervention) and ideas (the emergent theoretical perspective on the two methodologies). It is further carried out in the spirit of the suggestion that: " . . . *every study . . . .can be mined repeatedly for insights relevant to new concerns in an ongoing programme of research*" (Checkland and Scholes, 1990).

Chapter 12 seeks to draw some substantive conclusions regarding the overall relevance and applicability of *AT* to *MS*. It outlines further avenues of research, and, importantly, it deals with possible criticisms of the research as well as some important feasibility issues that might be involved in application.

Finally, I wish to comment on the style of presentation used. In the following chapters, I frequently and deliberately use the first person in the full knowledge that this is a breach of standard academic practice. However the epistemology employed here gives a generative role to the observer, and in this case I am the observer. As Maturana puts it: *"everything said is said by an observer"*, and this piece of work is something that is said.

# 3. DEVELOPING A FRAME BASED ON AUTOPOIETIC THEORY

*" . . . hmmm, let's have a quick look . . . he opened the suitcase . . . holy Jesus!, he said . . . legs!"* W.S.Burroughs, from "Spare Ass Annie and other Tales".

## 3.1 Introduction

The aim of this chapter is to construct an internally consistent and coherent frame - based on *AT* - that can sustain the purposes outlined in the previous two chapters. The chapter is written with two audiences in mind. Firstly, since a major aim of the exercise is to examine what management scientists can learn from *AT*, and since *AT* is not a widely known or widely understood topic in that community, it is written for those who may have had some, but not much prior exposure to these ideas. Secondly it is written for those who may have an adequate understanding of particular aspects of *AT* but who may be less familiar with the wider systemic context that give these aspects their distinctive meaning.

Since the chapter is simply a frame constructing exercise, I do not rehearse the chronological development of the component parts. I do not delve into controversies, neither do I identify resonances with other literature. And, for the sake of brevity, referencing and direct quotations are kept to a minimum.

It is not possible for *AT* to sustain the purposes for which it is employed here unless there is a very clear understanding of exactly what it is, or more correctly, of what I am taking it to be. Yet providing such an account is not an easy task. Certainly it is much less straightforward than is often the case in putting together a conventional literature review. For a start, Maturana's work traverses an extremely broad territory:

biology, cognition, perception, evolution, language, culture, epistemology, ethics, science and philosophy. This presents a serious challenge to anyone who has been subject to the intellectual constraints that are a by-product of the traditional academic division of labour. In some of the primary literature (Maturana 1970a, 1970b, 1980, 1983, 1988a, 1988b, 1990, 1991, 1992, 1993, 1995, 1997; Maturana and Guiloff, 1980; Maturana and Varela, 1980, 1987; Maturana et al. 1960, 1968, 1995), the translation of terminology and ideas from Spanish to English can further compound the accessibility problem.

Of the secondary accounts that exist of Maturana's work, Mingers (1995) is easily the most comprehensive and digestible. However, it would be imprudent to rely solely upon secondary accounts as a sole source of understanding. A case could easily be made to show that some (Simon 1985, Dell 1985, Berman 1989, Birch 1991) are guilty of misinterpretation. Others (Beer, 1979; Morgan, 1986; Efran, Lukens and Lukens 1990; Ison and Russell, 2000) tend to couch *AT* more in terms of the light that it sheds on their own often narrowly defined area of interest and pay scant attention to the broad system of ideas.

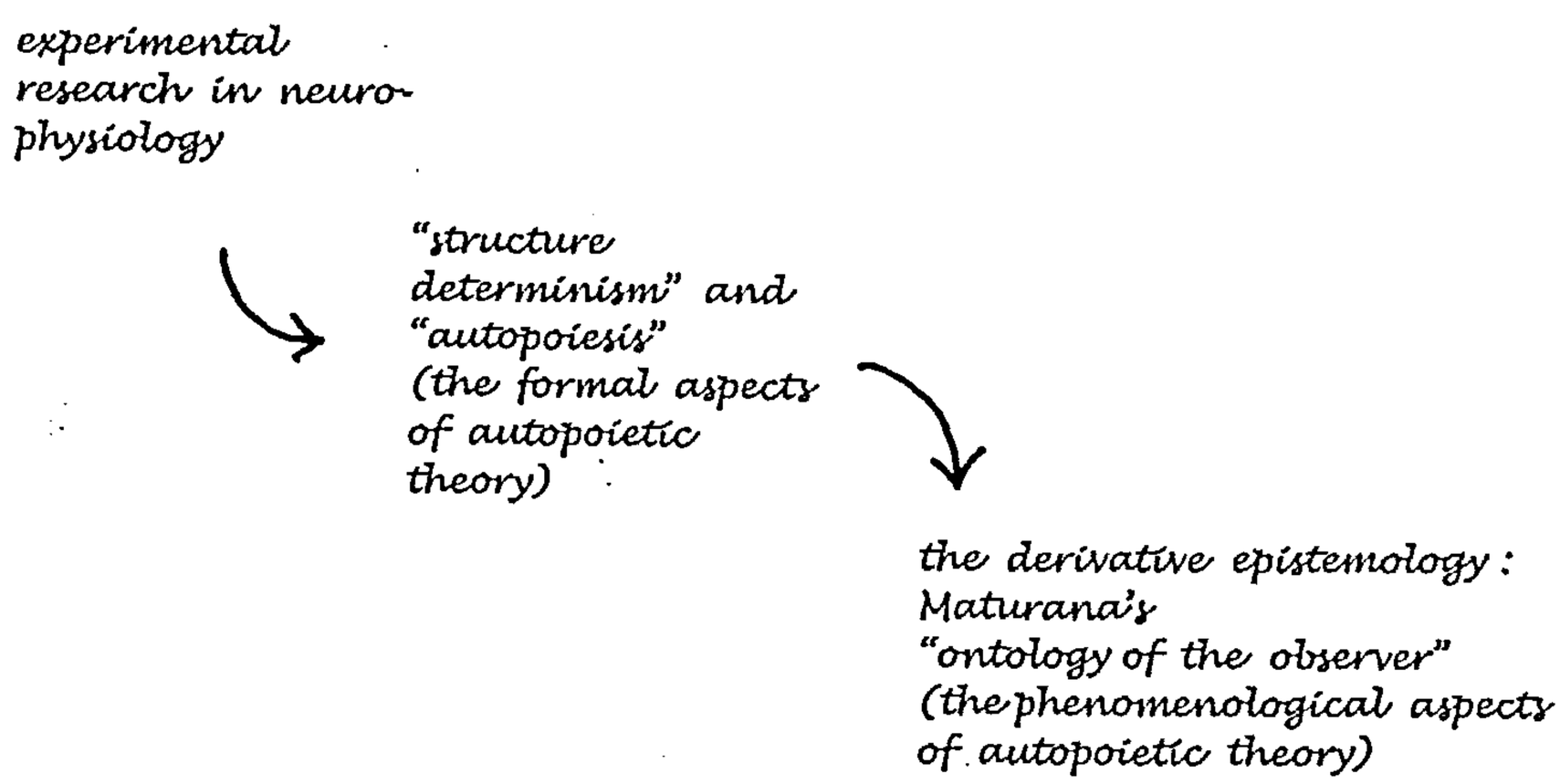
Given the complexity of *AT*, this approach is understandable. It is unfortunate however since Maturana's careful interweaving and inter-linking of ideas around an internally consistent worldview, is - I submit - one of its strongest features. It is certainly how Maturana himself likes to be understood (see, for example, his introduction to the 1980 edition of *'Autopoiesis and Cognition')*. Abstracted from the whole that gives them meaning, the various component parts of *AT* lose much of their explanatory power and hence their attractiveness.

Finally, I have often heard it said, that the inter-connectedness and complexity of *AT* make diagrammatic presentation very difficult, if not impossible. Figure A attempts to dispel this myth by presenting my interpretation of the theory in the form of a 'mind map'. The figure is attached in loose-leaf format at the front of this document in order to assist readers who may wish to refer to it as the description unfolds.

## 3.2 Autopoietic theory – a basic description

At this point it is worth remembering that I am using the term *'autopoietic theory'* in a very broad sense to describe what is, in effect, a system of ideas (see Figure 1) Strictly speaking the term *'autopoiesis'* belongs to the biological component of this work. I accept that broadening it here to include derivative and related contributions might offend purists. However it is done for the sake of brevity and convenience, and because – with the possible exception of Capra's (1997) use of the term *'Santiago School'* - there is no single label that adequately covers the full spectrum of Maturana's contribution.

Figure 1. 'Autopoietic Theory' – a system of ideas



Knowing where to begin with *AT* is no easy task. Partly this is because of its aforementioned breadth. Partly it is because the theory as a whole has an inherently circular quality. Indeed the issue of circularity is never far away in *AT*. As observers we seek to explain observing, we explain the mind using the mind, and we explain our experiences using our experiences. This makes the starting point, as Maturana (1988a:26) himself admits, something of an arbitrary choice.

This being the case, I shall begin with the question of the observer. Although this derives both logically and chronologically from the biological components of Maturana's work, beginning at this point facilitates comprehension and does not, I believe, do severe violence to the complete system of ideas.

### 3.2.1 Autopoietic theory: the general contours of a description

> *" . . the world we see has its reference point in the awareness of the biological and social structure of human beings"*

For Maturana (see 1988a, 1990, 1993a, 1993b), there are two distinctive ways of explaining the manifest capacity of human beings to *'observe'*, by which is meant the capacity of human beings – through the mechanism of language - to make distinctions, to utter cognitive statements and to explain phenomena.

One approach is to regard observing as being the result of innate properties, or as an endowment of the human condition. On this view, language is a characteristic of human beings, reasoning is a primary ability and knowledge is a property of the human mind.

Another possibility, as Maturana puts it, involves *'accepting the question of the observer'*. This entails accepting that the abilities of the observer are not a natural

endowment, instead that they are the result of a generative process (involving other phenomena) that gives these abilities their distinctive character. In a sense it could be said that the *AT* frame, as it is employed in this research project, arises as a comprehensive explanation of the 'something else' that explains observing.

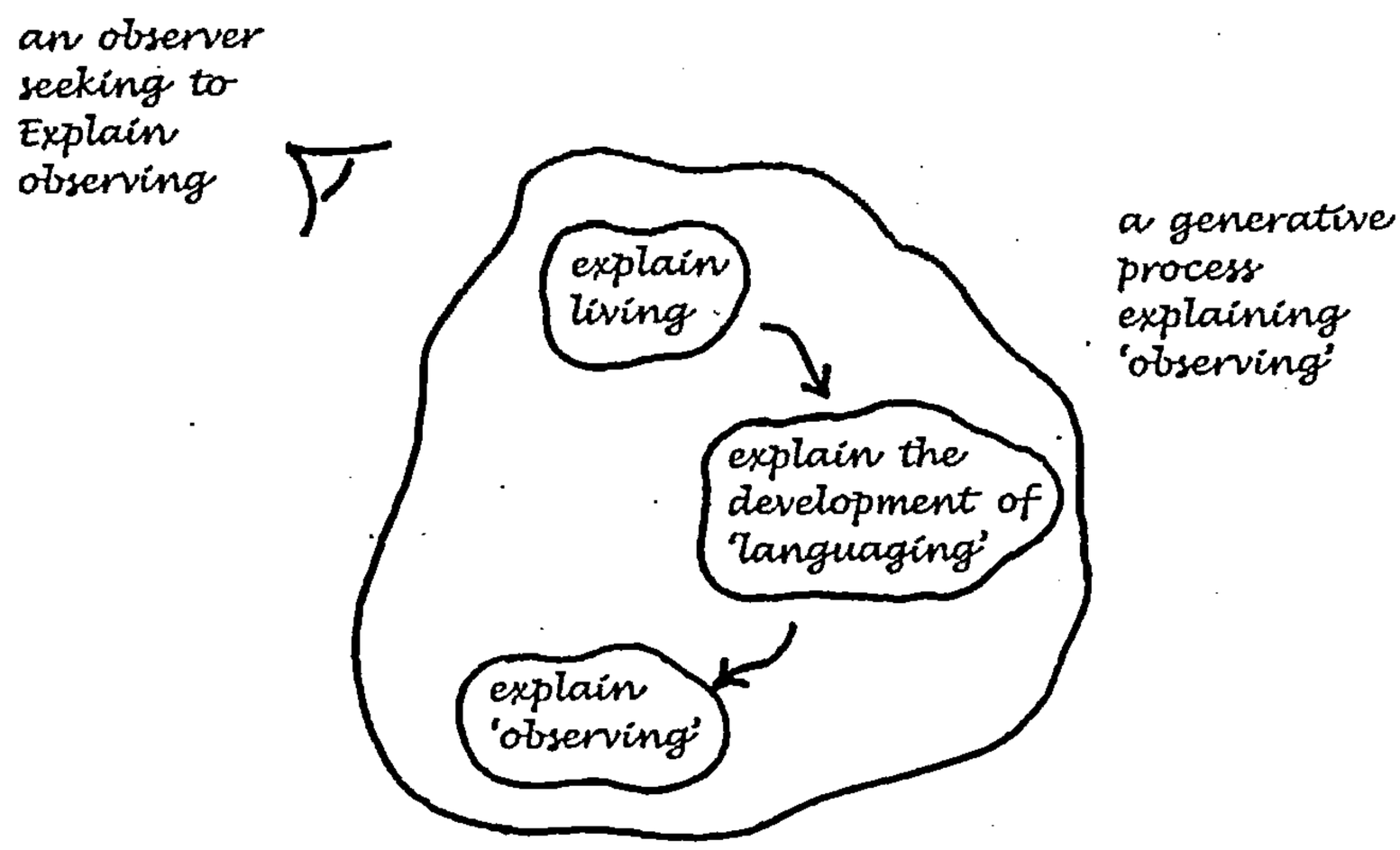
Before I come to the specific details of Maturana's explanation of observing, it is necessary to outline the broad contours of what such an explanation might entail. Here it is worth noting Maturana's claim that if we attend to how people explain in daily life, or *'do explaining'* as he often puts it, we see that this involves two processes. Firstly there is a *'generative process'*, which is structured along the lines of: 'if . . . and . . . and . . . and . . . then, something else (this being the phenomenon to be explained) will result'. Secondly there is, or are, other criteria of evaluation that arise in the listening of the observer as he/she assesses the congruence between his/her experiences and the proposed generative process. Such criteria can be highly informal and personalized, or they can be formalised such as in the case of scientific explanations (see Maturana 1990)

Having outlined the basic form of an explanation, Maturana then proceeds to " . . . *propose a generative mechanism that would give rise to all the phenomena entailed in observing* . . . " (1991:387). In other words, his project is to produce a generative mechanism such that observing, as a phenomenon, results.

In setting the scene for this explanation, Maturana suggests that looking to the conditions that interfere with observing provides a broad indication of what the key components to such an explanation might be. Thus, he claims that since any interference with either biology or language impedes observing, then biology and language are pivotal explanatory components.

Against this background, I am taking *AT* to be a comprehensive and coherent explanation of living systems, of how basic language capabilities arise, of how these become more fully developed in living systems that have access to a nervous system, and, of observing as something that happens *'as a manner of living in language'* (Maturana 1993) in human beings. These elements are presented in a simplified form in Figure 2.

Figure 2. Explaining observing: elements of an explanation and the problem of circularity



Extrapolating from the basic idea that biology and language generate the phenomenon of observing, Maturana claims that there are two *'phenomenal domains'* involved (see Figure 3). Thus, he says:

> *"As living systems, we exist in two non-intersecting phenomenal domains: the domain of our realisation in our bodyhoods (the domain of physiology) and the domain of behaviour (the domain of our interactions as totalities)."* (Maturana 1988a:63)

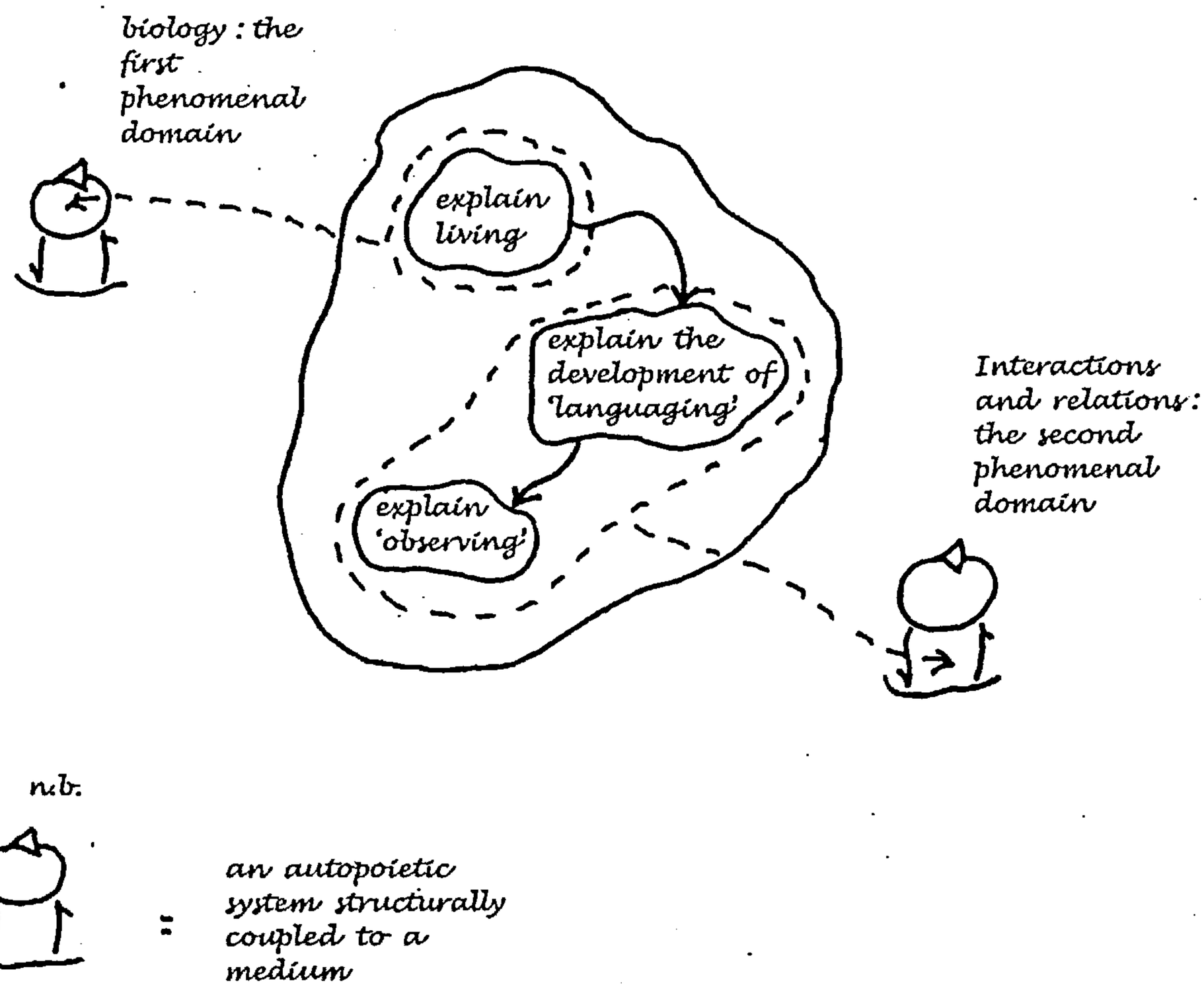
These two domains are non-intersecting in the sense that phenomena belonging to one, should not – on Maturana's view - be explained in terms that are appropriate to the other. To do so, for example to explain observing phenomena such as language or 'the mind' in terms of anatomy and physiology, or explaining behaviour genetically, involves 'collapsing' the two domains into one.

Non-intersecting they may be, but the two domains are inextricably intertwined:

> *" . . . bodyhood and manner of operating as a totality are intrinsically dynamically interlaced; so that none is possible without the other, and both modulate each other in the flow of living. The body becomes according to the manner* (in which) *the living system operates as a whole, and the manner* (in which) *the organism operates as a whole depends on the way* (in which) *the bodyhood operates."* (Maturana 1997:2).

I shall say much more about the precise nature of the interaction between the two domains later. For now it is necessary to flesh out this general outline of Maturana's explanation of observing in more detail.

Figure 3. The relationship between the components in the explanation of observing and the two 'phenomenal' domains




### 3.2.2 Biology: the first phenomenal domain

The biological aspects of *AT* originate in experimental work on colour vision carried out in the early1960's (see Maturana et al. 1960, 1968; Maturana, 1970a, 1970b). Subsequently, this led Maturana and Varela to reject the prevailing idea that the nervous system is open to information from the environment, and that human cognition mimics the symbol-based functioning of computers.

In this experimental research it was found to be possible to identify direct correlations between the configuration of external wavelengths of light and activity on the retina of the eye; further, it was possible to identify direct correlations between

activity in the optic nerve and subjects' verbal descriptions of colour. It was not however possible to establish such a link between cellular activity on the retina and activity in the optic nerve.

These findings led Maturana and Varela to conclude that:

> *" . . . we must stop thinking that the color of the objects we see is determined by the features of the light that we receive from the objects. Rather, we must concentrate on understanding that the experience of a color corresponds to a specific pattern of states of activity in the nervous system which its structure determines."* (Maturana and Varela 1987:22)

The physics of wavelengths of light then are not irrelevant to colour vision, but they do not determine it.

Later, Maturana was to summarise the conclusions of these findings. Firstly he proposes that no external stimulus acting on the nervous system can determine an organism's experience of it. Experience, as the above quotation suggests, corresponds to neuronal activity; it does not correspond to external perturbations. Thus, whereas popular opinion takes the view that the nervous system is open to environmental inputs, Maturana claims that it is closed, autonomous and circular. It operates, he says:

> *" . . . as a closed network of changing relations of interactions between components in which every change in relations of interactions between its components gives rise to further changes in relations of interactions between its components, and in which all takes place in a system of highly interconnected loops of unending recurrent circular processes of changing relations of interactions of different lengths and time constant."* (Maturana, 1988a:54)

Unpacking this further, it can be said that the sensory and effector surfaces of the nervous system have a dual character. That is, they operate as elements of the organism and as elements of the nervous system. Yet their manner of operation depends upon whether they are operating in one or other mode. Acting as components of the organism

they operate in the interactions of the organism in its medium, but acting as components of the nervous system they operate in its closed dynamics. This means that the organism as a totality interacts with the medium, but the nervous system does not. Hence the notion that the latter is closed and circular (see Maturana 1997:6).

An important consequence of this is the understanding that there is no 'outside' to the internal components of the nervous system; there are only internal correlations of neuronal activity. Notions such as 'inside' and 'outside' require the existence of an observer who can see both and explain changes in one in terms of the other. This issue assumes greater importance in Chapter 7 and is discussed in more detail there.

The nervous system, says Maturana, is an example of a *'complex'* or *'composite'* system, i.e. one that an observer decomposes to highlight not just the system itself, but also its component parts. All such systems, he suggests, are *'structure-determined'*. That is:

> *" . . . everything that happens in them happens as a structural change determined . . . either in the course of their own internal dynamics or triggered but not specified by the circumstances of their interactions."* (Maturana, 1990:13).

Logically, the circularity and closure of the nervous system means that it cannot work with representations. Cognition therefore cannot be taken to involve the brain processing symbols that stand for external elements, manipulating representations and then computing a response that is adequate in the light of prevailing circumstances. Although we humans frequently speak as if we know about the 'real world' and can accurately perceive the 'things' in it, biologically this is impossible. Thus, instead of characterising objects (e.g. "that tree is green") according to their intrinsic characteristics (the tree is green), we do so on the basis of what happens to us through our experiences.

This leads Maturana to conclude that we do not exist in the physical space, rather that the physical space exists through us.

Extending the key notion of circularity, Maturana (see 1993b) sought to relitigate the age-old question of what is meant by the term 'living'. Eventually this led him and Varela to claim that all living systems are *'autopoietic'* or 'self-producing', i.e. they are:

> *"networks of molecular production such that the molecules produced, through their interaction, generate the network that produced them and specify its extension"* (Maturana 1993b).

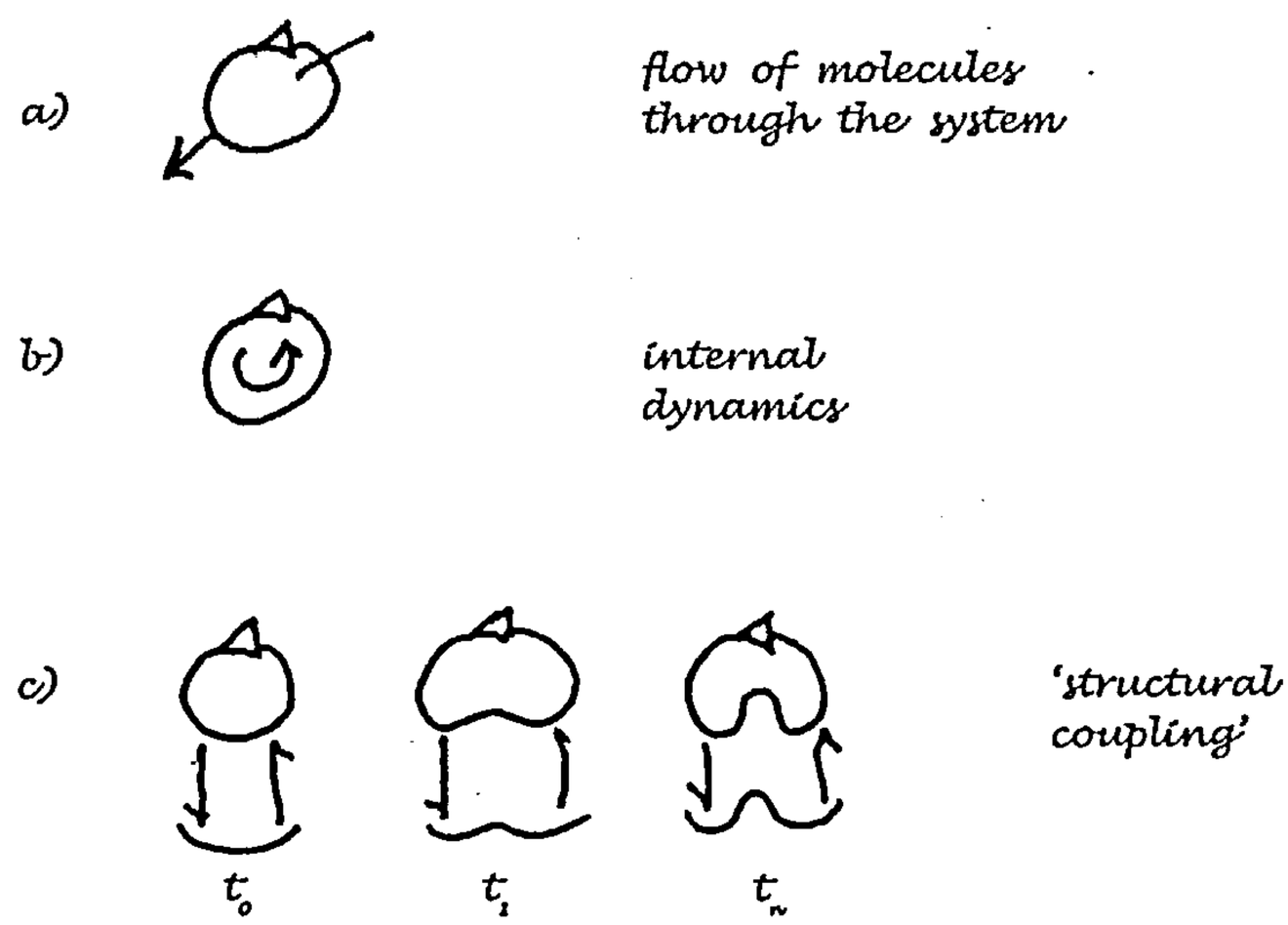
On this view, life is not, as conventional wisdom might have it, constituted through the specific properties of molecules. Instead it is constituted through a dynamic; through a particular *'manner of relating'* of molecules. Thus, the term *autopoiesis* describes the invariant, or the *'organisation'*, which defines all living systems as belonging to this particular class entity. Whereas organisation is conserved as long as the living system maintains its class identity, *'structure'* , the term used to describe the space in which organisation is realized, changes constantly.

Stability and change then, exist side by side. In human beings their exceptional structural plasticity allows for changes in behaviour, and for what Maturana and others (see, for example Winograd and Flores 1987:45), describe as learning.

At first sight, the idea that living systems are structure-determined, that they have an autonomous autopoietic organisation, and, where the nervous system – if there is one - is closed to information from the outside world, presents serious difficulties in accounting for their manifest adaptability. It makes it equally difficult to account for their distinctive evolutionary trajectories.

Conventionally, the environment is taken to be the primary source of adaptation and change. In human beings, as we have seen, adaptation is linked to the capacity of the nervous system to construct and manipulate environmental representations. This is all part of a process the outcome of which is intent to act in a manner that befits prevailing circumstances. Accordingly, rejecting such logic on the grounds of its biological impossibility, begs the question: if the nervous system is closed to external information, then what is the main source of adaptation?

Figure 4. Sources of structural change in living systems




In answering this question, it is helpful to look at what *AT* has to say about the main sources of change in living systems (see Figure 4). Essentially there are three of these. Firstly there is change that arises conditional upon the flow of molecules through the system. In *AT*, the circularity of living systems pertains to a dynamic, i.e. to a peculiar manner in which component molecules relate to each other. It does not refer to the pattern

of movement of specific molecules or components, as it would for example in the case of a tornado where molecules and particles do trace a circular pattern. Living systems are different; molecules enter the system, they participate in the circular dynamic, they then die or they leave the system. As this molecular flow through the system proceeds, its structure changes, as long – of course – as the constitutive self producing dynamic is conserved.

Secondly, there is change that results from internal dynamics. These can be incremental such as those that result from metabolic changes, or they can be more drastic such as those that occur when the flow of molecules in dissipative structures sets up instabilities in and around systems that otherwise appear to be characterised by order.

The third, and - in the context of this discussion - most important source of change, occurs through a process known as *'structural coupling'*. Maturana employs this term to refer to situations in which it is possible to observe a congruence or 'fit' between the living system and medium. When there are recurrent interactions between an organism and what an observer would regard as its environment, or between one organism and another, structure-determined changes occur in both. In other words the two structures change congruently each according to its own structure-determinism. Through this process, the structure of the organism, at any point in time, becomes a record of previous interactions. Or, as Maturana (1988a:55), puts it the structure and operation of a nervous system always embodies " . . *the behavioural present of the history of interactions of the system that it integrates* . . . ".

As long as the system survives, i.e. as long as there is conservation of autopoiesis and adaptation, external interactions trigger structural change. Structural coupling then, is

a key mechanism for change in a living system during its lifetime. In human beings - as I have already said - it is also the basis of learning.

Whereas cognitivism tends to emphasise the response of the system to its environment, and posits a primarily one-way relationship, the idea of structural coupling implies a much more complex interdependent relationship. It says that when one system perturbs the other, two things can happen. Through spontaneous homeostatic processes the system can revert back to its original state. Alternatively, the structure of the system can change. However this is not adaptation as cognitivism construes it. Where a change does occur in response to an environmental perturbation, it is because the particular change that occurs is a pre-existing feature, or one possible state, of the system's structure. The perturbation only triggers it.

Turning now to cognition, structural coupling generated by the demands of autopoiesis plays the role that conventional wisdom attributes to having a representation of the world. Traditionally, as I have said, we have come to regard cognition as a process that has the brain manipulating representations of the external world. For organisms that possess a nervous system this sounds eminently plausible. But what about organisms that are not endowed with a nervous system? How is it that they are often just as well-adapted to their medium? And does the absence of a nervous system mean that these organisms are not 'cognitive' beings?

Of course it is possible to answer the first of these questions using cognitivist logic. Thus an observer might regard physical movement in a single cell unity such as an amoeba - as it surrounds a source of nourishment in its medium - in terms of the amoeba having somehow 'perceived' its environment and having computed an appropriate

response (Maturana and Varela 1987:147). Yet this is unsatisfactory since the absence of a nervous system and the inability to operate in language would seem to preclude the amoeba carrying out the necessary 'perceive and compute' functions.

Despite this the amoeba's behaviour seems adequate in its domain of existence. It is adapted to its medium, and one could say that the amoeba is a cognitive being that has the capacity to 'know'. Obviously it does not 'know' in a conventional sense, rather 'it knows' because it is successfully coupled to its medium, something which is necessary for its conservation of autopoiesis.

On this view, cognition is linked to structural coupling. The physical movement of the amoeba does not involve perception. Instead there is a process in which changes in the chemical composition of the medium trigger changes at the sensory surface of the amoeba. This sets up an internal dynamic that results in the amoeba altering its position in relation to the food source in its medium.

It is this logic that leads to the conclusion that, in its most basic form, cognition is <u>not</u> a mentalistic phenomenon. Rather, it is <u>effective action</u> in a defined domain of existence (see Maturana and Varela 1987:29). This process involves the whole organism and is not limited to what might happen in the nervous system or brain if these exist. If an observer sees a unity behaving adequately in some defined domain then we can surmise that it 'knows' relative to whatever criteria the observer applies in making the necessary judgment. In this sense cognition equates to the whole process of living. The abstract thinking that goes on in human beings is but a special case of what is a much broader phenomenon (see Varela et al. 1991, for a detailed extension of this argument).

Maturana's linking of cognition with structural coupling provides a distinctive take on the increasingly popular idea (see, for example Feyerabend, 1987; Heidegger, 1927; Introna, 1997; Mingers, 1996; Schon, 1982, 1983; Varela et al. 1991) that cognition is an integral part of our normal everyday mindful and unmindful activity. According to this line of thought, action that looks like adaptation to an observer, and which the representationalist perspective would explain in mentalistic terms, is merely the system operating in a relationship of structural coupling with a medium.

The distinctive contribution of *AT* then, is to allege that cognition has to do with the process of living which, in turn, is inextricably linked to structural coupling. This turns on its head the notion that the mind is synonymous with the brain. Instead it asserts that the brain is merely one of many structures through which the mind operates.

Such then are the basic distinctions that I am taking to pertain to the first of the two domains that are central to Maturana's explanation of observing. To summarise, we can say that living systems are self-producing systems of molecular production which operate in a dynamic structural coupling with a medium.

In the case of human beings, the basic biological character of the nervous system is important since it makes observing possible, and it has implications for the what and the how of observing in specific instances. However it is in the second phenomenological domain, that of relation and interactions with a medium, where observing (and where behaviour more generally) takes place.

### 3.2.3 Relations and interactions: the second phenomenal domain

· Although the basic biological constitution of living systems, including the conception of cognition as being tied to the full process of life, is important when it comes to understanding how people operate as observers, this tells only part of the story. The context in which observing is inextricably embedded, involves a second phenomenal domain: that of relations and interactions. While Maturana's work forces us to acknowledge that cognition is more than just conscious thought, it does not deny in human beings the mentalistic cognitive process. The operation, that makes possible this, and observing more generally, is *'languaging'*.

Maturana's explanation of the development of observing in human beings extends the idea of a living system structurally-coupled to a medium to that of structural coupling between two or more living systems. It is in the space between these two structurally-coupled systems that the key process of languaging takes place.

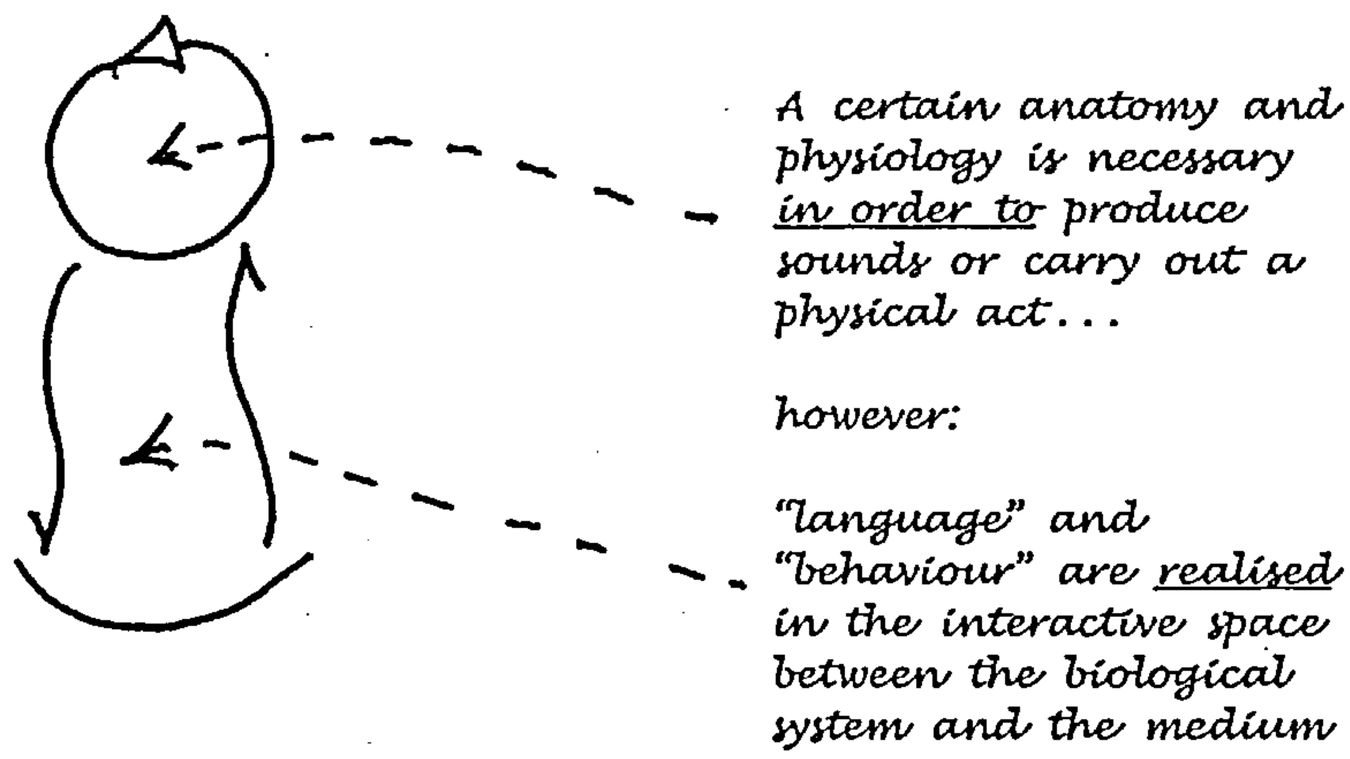
According to conventional wisdom, speech acts are the fundamental element of language, and language is a symbolic system of communication about the world where each symbol corresponds with some aspect of the world (see, for example, Searle 1983).

Citing the various biological imperatives, Maturana rejects all of this. He argues that because human beings are structure-determined systems whose nervous system operates in a closed circular manner, there is no direct access to an independent reality. The conventional view of language, he suggests, presumes an operation that human beings are simply incapable of carrying out.

Once again, Maturana urges that we turn to the conditions which generate that which we observe when we observe languaging. Doing so, he submits, reveals behaviour

to be the cornerstone of language. And because behaviour belongs to the relational domain (physical acts such as walking arise when motor movements produced by the nervous system intersect with the ground), so too is language a relational phenomenon. While in human beings certain anatomical and physiological features are necessary to produce speech or to make gestures, neither are biological phenomena. Language, no matter how it is realised, belongs to the domain of relations (see Figure 5).

Figure 5. Behaviour and language as relational phenomena



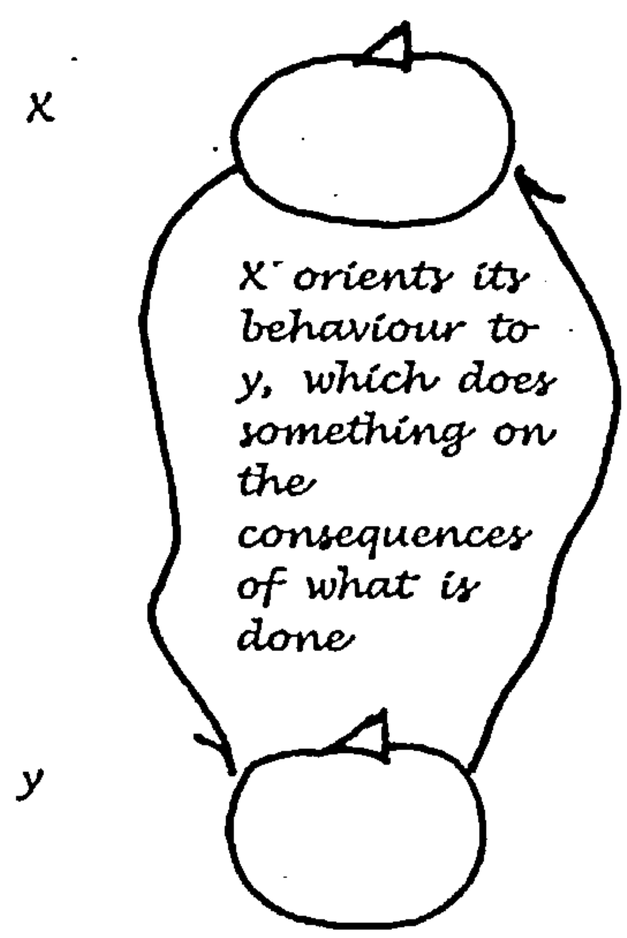

In order to explain this, to explain languaging, and hence to explain observing, is necessary to revisit the concept of structural coupling.

According to Maturana's epistemology (see below), no entity is 'in-itself', and this applies as much to language as it does anything else. So turning to what we observe when we observe two or more entities 'in language' with each other, what we see is a behavioural process. Using Maturana's terminology we see an initial *'co-ordination of*

*action'* between two structurally-coupled entities, followed by a further coordination of the two actions. Consider the following: a parent in the company of strangers looks disapprovingly at her misbehaving child; the child immediately ceases to misbehave. Alternatively consider the owner of a dog whose whistle attracts the attention of a dog that brings it 'to heel'. In both cases, there is an initial coordination of action: the child's gaze falls upon the parent and the dog's falls upon the owner's. Next both the child and the dog does something on the consequences of what is done. In both cases there is a further coordination of the now already-coordinated actions.

Maturana's contention then, is that a *'coordination of a coordination of action'* constitutes the minimum operation that is involved in language (see Figure 6).

Figure 6. The minimum operation of 'languaging' (*"that which happens when there are coordinations of coordinations of behaviour"*)



To the extent that the recursive coordination of an already coordinated action between two entities is the basis of languaging means that a rudimentary form of the latter is possible in organisms that do not possess a nervous system, and/or in those with a nervous system but which are incapable of abstract thought in the manner of human beings.

Much learned animal behaviour such as the courtship, nest building and chick rearing behaviour of birds, or the 'dance' of bees, is of this basic type (see Mingers 1995:78). However, to the extent that there is no abstract thought involved in these activities, and in order to differentiate it from languaging proper, Maturana refers to this as *'linguistic'* consensual behaviour.

However where there is an advanced nervous system, the languaging potential of the organism is increased dramatically. In simple terms, the intrusion of a nervous system severs the direct link between sensory and motor surfaces of the organism. That having occurred, although the sensory surface can and does perturb the nervous system, the nervous system begins to interact with itself (indeed it interacts with itself much more than it does with the sensory surfaces). In other words, activity within the nervous system becomes the object of further activity, which, in turn, becomes – *ad infinitum* – the object of further activity and so on. This process (see Mingers, 1995:73-79, for a fuller description) provides the basis for a massive expansion in the cognitive capability of the nervous system that, in human beings, culminates in abstract thought.

Because we human beings have such a capacity, and because we live our lives completely immersed in the full complexity of language, it is hard for us to see that, in its minimum form, languaging involves a simple coordination of an already coordinated set

of actions. Thus we tend to describe language in semantic terms i.e. involving a transmission of information from one organism to another that embodies some meaning. However, when - in a relationship of structural coupling - one organism coordinates its actions with another and there is a further coordination, then we have the basic dynamic that culminates in languaging. Neither action is necessarily determined by meaning; rather they are both a consequence of the dynamics of structural coupling.

Further recursions of this basic coordination of actions result in language becoming increasingly complex and sophisticated, especially, as I have just said, in organisms such as human beings that are endowed with a sophisticated nervous system. Here, in networks of structural coupling, human beings use words as linguistic distinctions to coordinate actions. Through this process objects become tokens for highly specific behavioural coordinations.

In his classes and public seminars (see, for example, 1993b) Maturana uses a number of examples to illustrate this process. The designation 'taxi', for instance, connotes the actions that are involved in carrying someone from one place to another, in a motor vehicle, in return for the payment of money. The designation 'clock' connotes the various actions that are necessary to read the time. Accordingly, Maturana claims that even abstract entities such as 'justice' or 'democracy', are anchored in concrete behaviours.

Over time, having arisen through the aforementioned process, the anchoring of objects in actions tends to be forgotten. Thus: " . . . *objects take place as distinctions of distinctions that obscure the co-ordination of actions that these co-ordinate.*" (Maturana, 1988a:47). At this point in time, taxis, clocks, and all other objects – to all intents and

purposes - become entities 'in-themselves', which - of course – is how we tend to live them.

Once objects have arisen, other developments are possible. We can make distinctions of distinctions and this allows abstract concepts to arise. Meaning then arises as patterns of relationships among descriptions. And once we have objects, we can carry out the operation that allows us to refer to ourselves. Thus we can become self-aware and self-conscious. This whole process is shown in Figure 7.

Fundamentally, because language is rooted in behaviour, which is a relational phenomenon, and because the agreements to symbolise behavioural coordinations through tokens occur in networks of structural coupling, language is a relational phenomenon. Even our so-called 'inner world' of solitary reflection and consciousness arises determined by our existing structure, which is a record of previous interactions and structural couplings. It could even be said that that which we describe as the mind is a social, not an anatomical or biological, phenomenon.

The key point is that although a sophisticated nervous system is necessary to make it possible, human language is anchored in behaviour which belongs to the relational domain. Fundamentally language is not abstract, neither is it – as conventional wisdom would have it - about symbols, even though that is how it appears to us. Rather it is about doing. Entities and objects correspond to 'doings', and language is a flow of coordinations of coordinations of action.

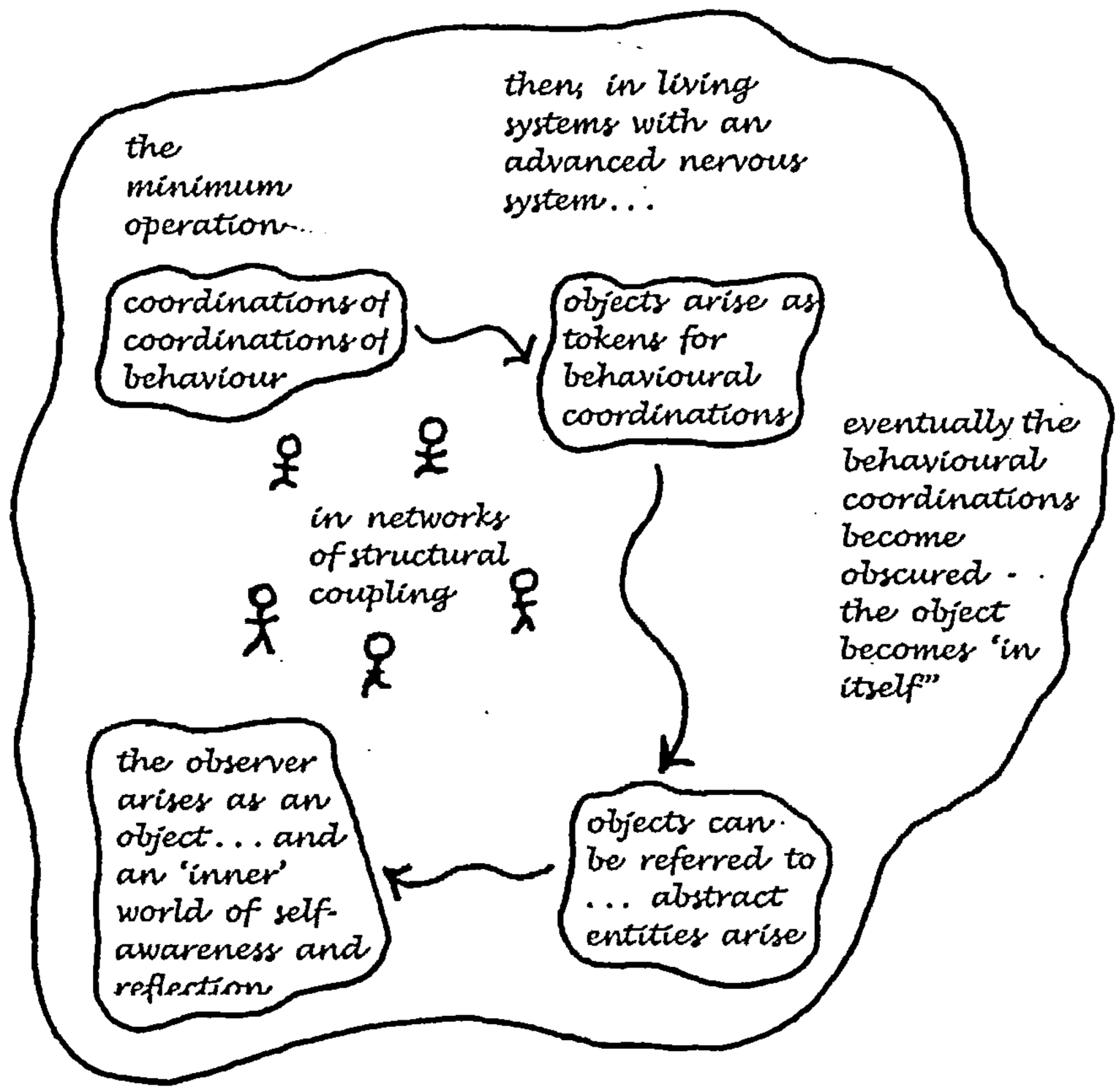
It follows that languaging arises in our network of structural couplings with other people. As we coordinate our actions in different ways, as we make new distinctions, and as we come up with new tokens, we are continually weaving linguistic networks with

other people. And languaging, says Maturana, is what humans do. It is the total immersion in language, as a manner of living together, that constitutes the human being as the class of living system that it is. Human beings, he claims, are not basically different from other animals, " . . . *all that is peculiar to us is that we live in conversations.*" (Maturana 1993a:47).

In summary, combining the two key components involved in Maturana's explanation of observing creates the understanding that when human beings observe phenomena they do so as living systems first and as languaging beings second. As living systems we are two things: firstly we are an aggregation of an infinitely large number of individual autopoietic systems; secondly we are structure-determined entities that exist in a relationship of structural coupling with a medium. As human beings we are living systems that live our lives totally immersed in the social phenomenon which is language. All of this happens whether we like it or not. It is an ever-present backdrop that both constrains and enables.

Having provided a description of the biological and social basis of observing, we can now return to the process of observing itself and consider its derivative epistemology. Thereafter, we can examine some of the consequences that doing this reflection (i.e. of '*accepting the question of the observer*') has for people in their daily and professional life. This is important, because later I shall be examining what all of this would mean for a management scientist who chooses to act in such awareness.

Figure 7. How objects arise through languaging




### 3.24 The derivative epistemology

According to Maturana's epistemology, the manner in which human beings come to 'know' their worlds, depends moment by moment, upon the precise nature of whatever *'domain of explanation'* is operational for that individual. A domain of explanation is an explicit or, more commonly perhaps, an implicit frame of reference that is defined at the very moment in which an observer, actively or subconsciously, distinguishes a particular area of experience, and then explains it. As I have already said, such domains are

constituted through the particular criteria that the observer applies in his/her listening in accepting a generative statement relevant to the area of experience as valid.

Each domain of explanation is *'coherent'* i.e. its elements – as they are experienced – hang together in a regular and predictable manner. Examples of what one might call 'large scale' domains of explanation include free market economics, classical physics, Buddhism, Christianity, and Marxism. 'Small scale' domains of explanation might include those that pertain to games such as field sports or board games. In all of these there is a consensually validated generated mechanism that gives rise to an experience to be explained. For example in free market economics there is a view on what must be done to ensure that an economy is competitive. In Christianity there is a view on what it takes to be a good citizen. In Marxism there is a view on the conditions that must be satisfied for there to be a proletarian revolution. In chess there is a sequence of moves that leads to checkmate. In each domain, the generative mechanism contains various entities and objects, and posits relationships between them. Further, each domain has its own rationality that has to do with the 'rules' and 'truths' that pertain to it.

Although domains of explanation arise only as an abstraction of the regularities of the observer's experiences and do not pertain to an independent reality they are nevertheless often lived as though they are objectively real. There are many reasons for this. In Western cultures we cling to the notion that there is an independent reality whose transcendental truths may be grasped through the process of perception. Another reason is because when we grow into language we do so slowly. With repetition stories harden into realities and so we often fail to see how our language creates a particular way of 'seeing'. Most important of all perhaps, when we accept an explanation we do so because

our experiences not only confirm the explanation itself but they also confirm various deductions that we might make from it. When all of our experiences point to a particular explanation as being valid, it is hard for us to avoid thinking that we have discovered a transcendental truth.

In board games such as chess most people would accept that a 'truth' statement such as "bishops cannot move horizontally across a chess board" is valid only in relation to the rules of the game. In other domains of explanation such as in politics and in religion, such acceptance is less certain. Yet fundamentally these domains are no different to the game of chess; their rationalities and their truths are no less relative to their own fundamental premises than are those that apply in board games.

Because there are as many domains of explanation as there are validity criteria, it follows that there are many in a *'multiverse'* of realities; there is no single transcendental reality (i.e. there is no *'universe'*). Further, a domain of explanation is also a domain of rationality, i.e. it responds to deductive reasoning within its own boundaries. Finally it is a domain of legitimate actions, i.e. it suggests actions that are 'sensible' given its own logic.

Each explanatory domain is coherent i.e. the various components and parts, as they are experienced by the observer, hang together in a regular and predictable manner. These coherences integrate various elements and objects - *'brought forth'* by the observer in language – which, in combination, comprise a mechanism that gives rise the phenomenon to be explained and other phenomena that one might deduce if the mechanism were to operate. The coherences that pertain to the explanatory domain provide a description of how the objects and entities that constitute the generative

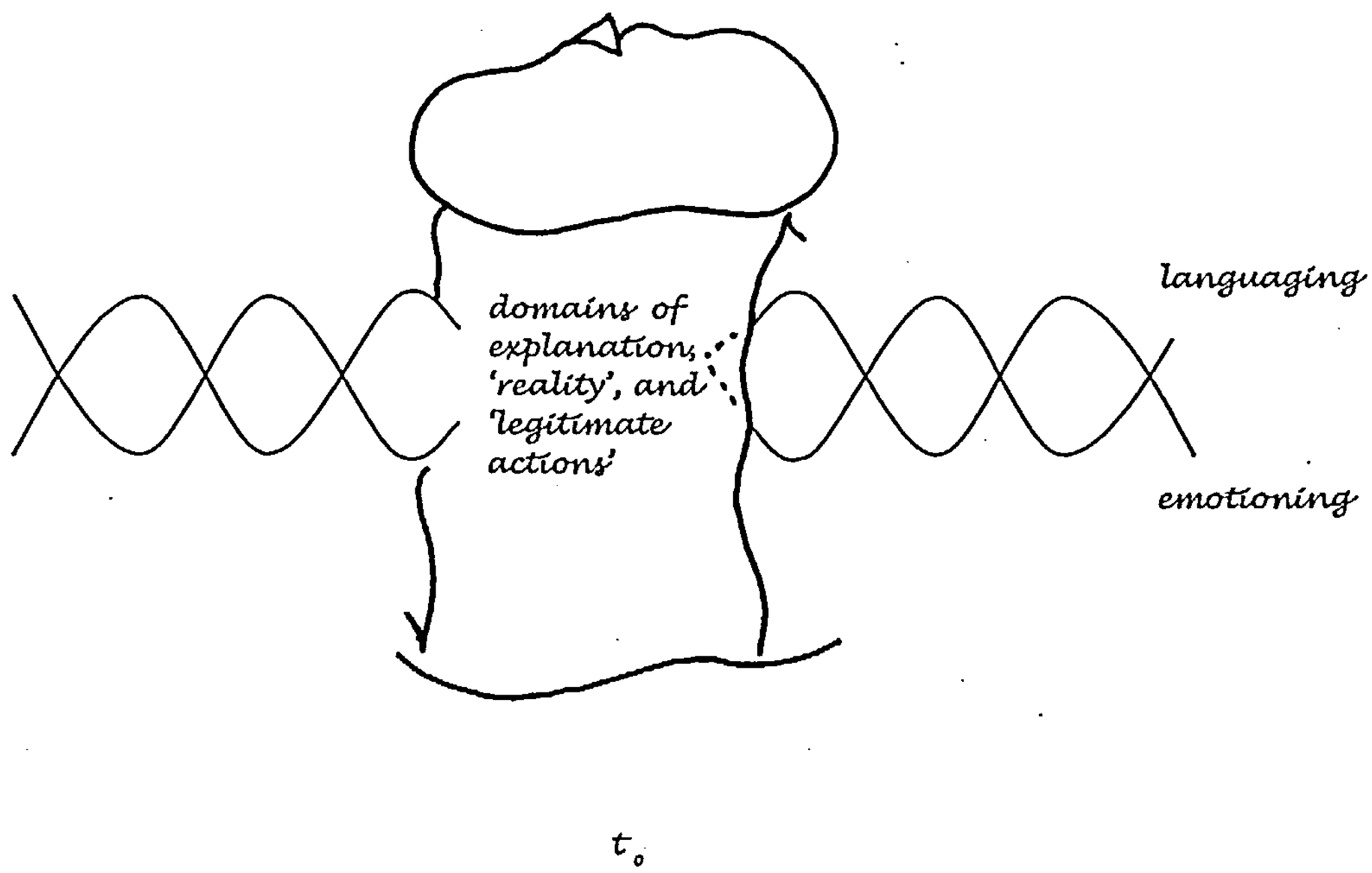
mechanism relate to each other in some manner that, as experienced by the observer concerned, is not arbitrary.

While explanatory domains – in relation to some experiences – might be relatively stable over time, in other cases they alter from moment to moment. Since the observer's experiences are subject to his/her structure-determinism, and since structure is always a record of previous interactions, preferred explanations are partly contingent upon his/her history. However, as we have already seen, observing is not an anatomical/biological phenomenon. Therefore, in understanding the precise nature of the explanatory domain that applies from moment to moment, it is necessary to turn to the domain of social relations and interactions.

Here Maturana's notion of *'conversation'* is pivotal. Much more is said about this later, so I will not go into great detail here. Suffice to say that, in this context, the idea of *conversation* refers to a dynamic braiding of languaging which has already been discussed, and what Maturana calls *'emotioning'*. These, he defines as *'bodily dispositions for actions'* (see 1988a:42).

Importantly, as conversations flow through an interweaving of distinctions and emotions, experiences and explanations alter (see Figure 8). While the structure of the observer imposes limits on what is and what is not possible, it is the nature of the conversation (with self or other), and its flow, that determines the outcome.

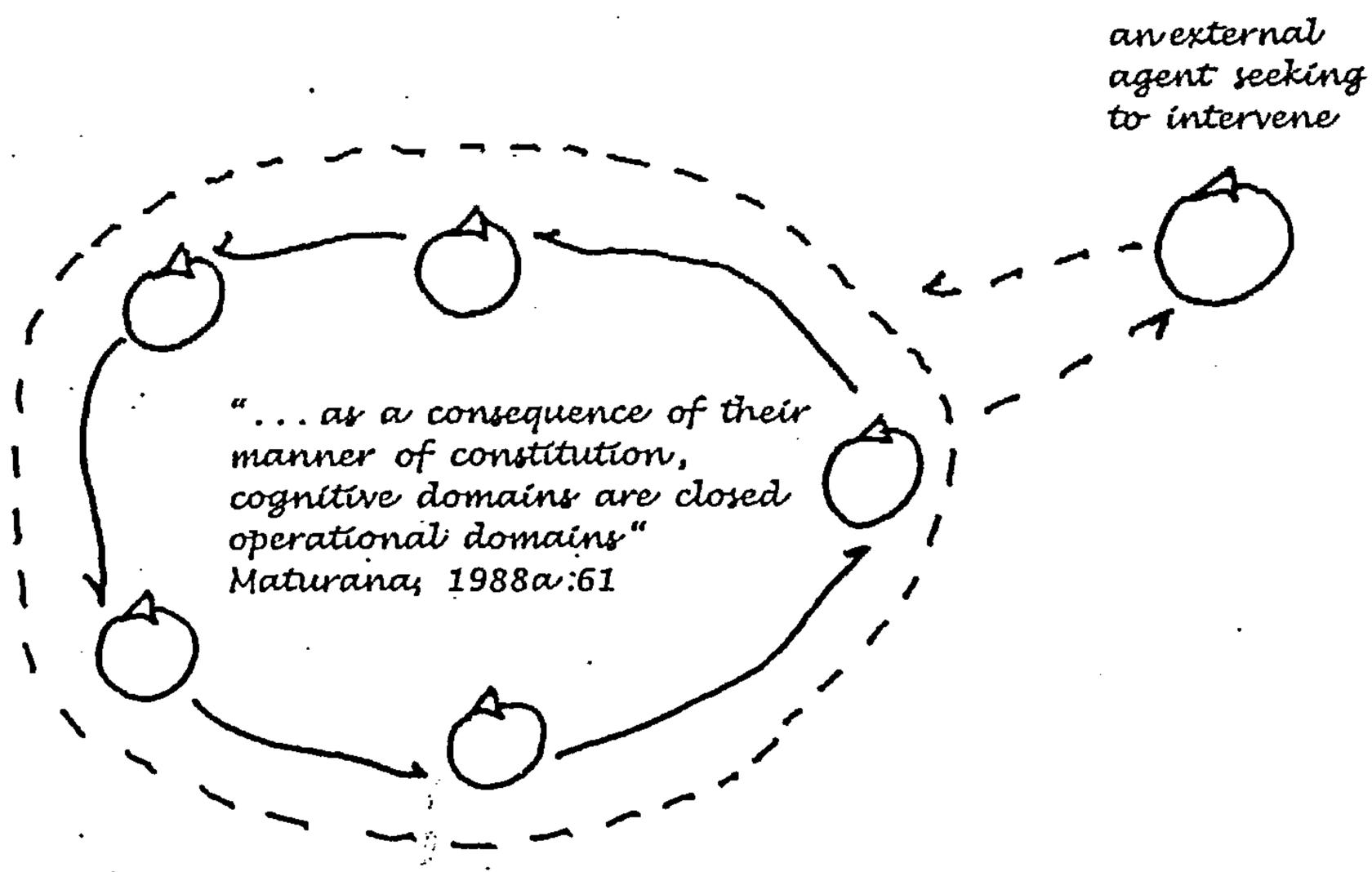
Figure 8. Explanations and actions as a function of the interaction between structure and the flow of emotioning and languaging




As people shift then from one emotional state to another, changes take place in the kinds of things that they will and will not do. People behave differently, they see differently, and, importantly, they describe and interpret things differently according to the emotion in which they do these things. Whereas classical thinking believes that emotion and reason are fundamentally <u>different</u> ways of responding to situations Maturana believes that they are inextricably linked. We think we live as rational animals, he claims, but we do not, " . . . *we are emotional animals who use reason to justify our*

*desires"* (Maturana 1988a:25). Moreover, as Figure 8 shows, languaging and emotioning are braided, each process affecting the other.

Figure 9. The closed conservative nature of social networks constituted through the conversations that define them




Maturana uses the term *'consensual domain'* to describe these networks of structural coupling that are the site for conversations. In these contexts people learn their emotioning and their languaging with people to whom they are structurally-coupled, and,

through recurrent interactions, structural patterns of become conserved. The closed, conservative nature of these conversations (portrayed in Figure 9) renders difficult (but not impossible), any attempt to directly intervene from an external source.

Finally, Maturana's claim that:

> *"every human being usually participates in many different conversations, simultaneously or successively, that intersect each other in their flow through their intersections in our bodyhoods."* (Maturana, 1988a:51)

further complicates the picture.

Figure 10. The 'bodyhood' of the observer: a node at the intersection of numerous simultaneous conversations conversations

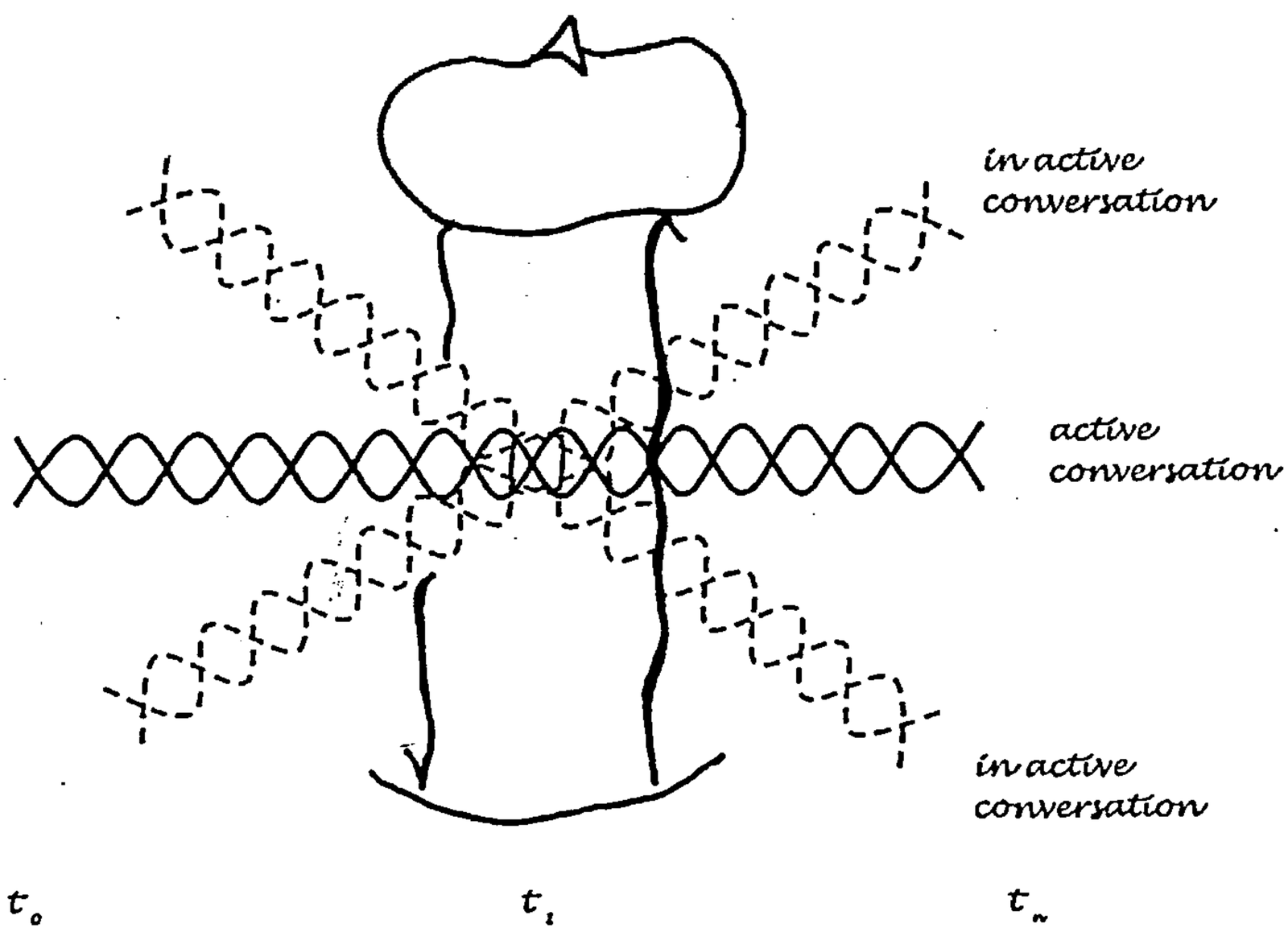



Figure 10 shows that the observer's *bodyhood* is a node at the intersection of many different conversations within and across different structural couplings. Each one

of these has its own braided flow of distinctions and emotions that have been learned through recurrent interactions over time and which alter subject to the dynamic flow of the conversation. This means that just as thoughts and descriptions within a single conversation are subject to change depending upon the flow of the conversation, they can also alter as the observer shifts from one conversation to another.

All of this is an unmistakably stark rendition of some of Maturana's key propositions about observing. It is enough however to provide the basis for reflecting on what the consequences of accepting it might be in daily life. This is discussed next.

### 3.2.5 The consequences of accepting the question of the observer

In simple terms *'accepting the question of the observer'* involves acting in a certain awareness in three key areas. Firstly, in relation to the ontological question of whether or not there is an independent reality; secondly, in relation to the nature of existence; and thirdly, in relation to the question of what it is that we explain. In what follows I shall take a lead from Maturana who presents what one would act in the awareness of if one were to accept the question of the observer, alongside what one would act in the awareness of if one were to not accept this question. It is important to present the two options side by side, not only to identify the contrasting positions, but also to reflect Maturana's view that we frequently shift from one posture to another contingent with the flow of and changes in our emotioning. There will be more on this later, but essentially what he is saying here is that when there is a predisposition for one party to seek agreement from another, then he/she tends to act in the unawareness of the consequences of accepting the question of the observer. In this emotional state, he/she is

acting in what Maturana refers to as the explanatory path *'objectivity-without-parentheses'*. When the relationship between self and other is paramount, then one tends to act in the awareness of the consequences of accepting the question of the observer, or in the explanatory path of *'objectivity-in-parentheses'*.

Maturana's position on the ontological question is unequivocally clear, if not entirely satisfactory. The fact is that he simply refuses to speculate on the possibility that there is or might be an independently existing reality:

> *" . . .even though it seems to us that for epistemological reasons we need some transcendental substratum to support our experiences, nothing of what we might say would apply to it, not even the notion of 'itness' through which we refer to 'it'."*
> Maturana 1991:386

In simple terms, Maturana submits that although the concreteness of our daily experiences and the hegemony of modern western ideology accustoms us to believe that there is an independent material world, the question of whether such a reality does 'in fact' exist is not worthy of serious discussion. Speaking as a biologist (and not as a philosopher) the reasoning behind this is as follows.

Firstly, he claims that if we combine the idea of structure-determinism (no external perturbation can specify its own effect inside a system) with the observation that the architecture of the nervous system is closed and circular, then it necessarily follows that *"an observer has no operational basis to make any statements or claim about objects, entities or relations as if they existed independently of what he or she does"* (Maturana 1988b:30). How, one might ask, "if I am a structure-determined entity and everything that happens in me happens because of me, how can I say anything about something external and independent to me?"

Secondly, in the explanatory path of objectivity-without-parentheses, objects and entities are 'real'; everything else is an illusion. Yet Maturana (see 1988a:29), claims that distinguishing between perception and illusion is biologically impossible. At the point of experience, he suggests that both perception and illusion are lived as real, and are indistinguishable. It is only, he says, through reference to a subsequent experience, which confirms the original that we speak of perception. It is only in reference to another experience, which disconfirms the original that we speak of having experienced an illusion, or of having made a mistake. From this, Maturana deduces that we can never be sure that any image that we might hold about the world is an accurate representation of what might exist independently of us. The possibility always remains that through some contrary disconfirming experience we reconsider what, to that point in time, may have been taken to be a rock-solid perception of 'what exists'.

While, for these reasons, Maturana is not willing to countenance the idea of an independent reality, he does, nonetheless, speak about the nature of existence. Once again, he claims that broadly there are two possibilities that depend upon whether or not one accepts the question of the observer. If one does not accept the question, then existence is straightforward: objects, entities, causal relationships, underlying forces, explanations, living systems, language, emotion, all exist independently. That is, such things are 'in-themselves'. On this view:

> " . . .*the observer implicitly or explicitly assumes that existence takes place independently of what he or she does, that things exist independently of whether he or she knows them, and that he or she can know them, or can know of them, or can know about them, through perception or reason.*" (Maturana, 1988a:28).

If, on the other hand, one accepts the question of the observer thereby attending, for example, to what people do when they notice and interact with 'things', then one

realises that existence arises in some operation that the observer carries out. In other words, existence depends on what the observer does. Rather than things being thought of as existing independently of the observer, here it is a case of objects being 'brought into being', most obviously through the social process of languaging outlined earlier in the chapter. If the operation – the distinction in language - that brings an object into being is not carried out, then, to all intents and purposes for that observer (note emphasis), the object does not exist. Thus, " . . . *the observer and observing, as biological phenomena, are ontologically primary with respect to the object and the physical domain of existence.*" (Maturana 1988b:1).

As we have seen, Maturana has his own reasons for choosing not to become embroiled in discussions on the possibility that there might be an independently existing material world that logically pre-exists the observer. This however, does not mean that we should not speculate, on the basis of what he does say, on what conclusions his line of argumentation suggest. Indeed some commentators (see, for example, Mingers 1990, 1995) have already done this.

My interpretation is that Maturana's refusal to speculate on the ontological position is not tantamount to him denying the possibility that there is a transcendental reality. Rather I take his position to be that of maintaining an agnosticism towards the idea, and instead focusing on the processes through which human beings construct the experiential reality which they live.

In attempting to nail Maturana's ontological colours to the mast, the distinction between 'no things' and 'nothing' is pivotal. In simple terms 'things', as Maturana claims, do indeed belong to the domain of the observer:

> *" . . . outside language nothing (no thing) exists because existence is bound to our distinctions in language . . . all phenomena . . . are cognitive phenomena that arise in observing as the observer operates in language explaining his or her praxis of living."* (Maturana, 1988a:80)

This seems clear enough. Since existence is constituted in the distinctions of the observer, nothing precedes its distinction. It is only through the aforementioned languaging process that people bring objects forth and subsequently live them as 'real'. In that sense 'things' or 'objects' are not there 'in-themselves' and the World, indeed, is observer-dependent.

None of which necessarily means that there is no pre-existing material world. No-thing is not synonymous with nothing. In this sense, Maturana leaves open the possibility that the world does pre-exist the observer. Indeed one could interpret him as saying that the world exists, but the naming and living of it as 'real' depends on the observer.

Maturana's rationale for not speculating on the ontological question is sound. However it has left him open to criticism from those who claim that he is vulnerable to suggestions that there is more than a hint of solipsism running through his work. Critics – Mingers (1995:112) most notably - claim that if there is no underlying reality that sets limits on people's reality constructions, then surely it follows that people are free to believe and do whatever they so desire.

There are a number of reasons why, in spite of his ontological agnosticism, Maturana's constructivism is not, in my view, synonymous with solipsism. Partly this is because as observers we do not have direct control of our experiences. Since our experiences "*just happen . . . out of nowhere . . .* " (Maturana 1993b) we cannot experience anything we want.

This apparently unremarkable observation allows Maturana to counter the claim that if we give up on the idea that we are constrained by the coherences of reality, then inevitably our lives are reduced to chaos, or, in extreme circumstances, to death itself. If we recognise that our realities are in accordance with the coherences of our experiences, and that these just happen to us, it follows that we cannot simply make them up. The lesson from daily life is that providing there is no biological interference, induced, for example, by drugs or alcohol, our experiences are mostly regular and predictable. This leads to the conclusion that the idea of people constructing their realities is by no means synonymous with chaos and/or unfettered creativity.

Of course it could be claimed that the reason why people's experiences are mostly regular and predictable is because although they do 'just happen' as Maturana says, they happen out of our structurally-coupled, structure determined interactions, and not merely by chance. And since for the most part human beings, in their structurally-coupled state, do indeed survive (if not always prosper), then this does more than just hint at the existence of an underlying reality that these states are in some degree of harmony with. Again this is not something that Maturana denies; he merely refuses to speculate on it.

The social basis of language in *AT* provides another way of countering the solipsist charge. Since our experiences take place in language, and since – as we have seen - language is a social phenomenon, then we are also constrained by convention. Experiences and knowledge is participatory, and what we think, and believe, is a product of that participation. As Efran et.al. (1990:76) put it, *"truths do not spring out of our heads as arbitrary inventions – they grow out of communal practices."*

Let us now come to a point that has been touched upon already: the question of what is explained when someone explains. Maturana claims that in Western cultures explaining assumes great significance. We human beings, he argues, " . . . *use knowledge continuously, we claim knowledge, we demand and act upon knowledge, we argue about knowledge, we even kill each other over knowledge*" (Maturana 1993b:tape1)

It is hard to disagree with this proposition. Even children in the early years of their lives are accustomed to seek explanations for phenomena, and rarely are they content with answers which do not describe the conditions that give rise to the phenomenon to be explained, or that do not satisfy other validity criteria that arise in their listening. In daily life explaining is an important activity. And it is equally important in professional and technical life.

The question thus arises: what is explained when we explain? Conventional wisdom, in modern Western society, is that our explanations pertain to reality, to an independent universe. If, however, we accept the question of the observer, if we attend to what observers do when they explain, then we realise that we explain our experiences of reality not reality itself. And when we explain experience we do so using our experience as observers in the happening of our praxis of living.

> " . . . *explanations explain the praxis of living of the observer, and they do so with the operational coherences brought forth by the observer in his or her praxis of living.*" (Maturana 1988b:4)

So domains of explanation arise in the explanation of experience; they do not pertain to an independent reality. And this holds even though they are lived as pertaining to such a reality. Even if we accept that there is an independent reality our explanations are many times removed from it.

If we accept the proposition that we explain experiences and not an independent reality, and if we act in such awareness, important consequences follow. Firstly, such awareness invokes a particular dynamic of interpersonal relations, or as Maturana puts it, a particular *'manner of relating'* with others. Thus, he claims that statements made in the name of objectivity and 'truth' constitute an implicit demand for obedience. Such cognitive statements, says Maturana, " . . . *are implicit claims of a privileged access to an objective independent reality and are, hence, demands for obedience.*" (Maturana 1988a:61).

Once a speaker gives up on the idea of an independent reality, these kinds of demands are no longer tenable. Instead he or she must present explanations or cognitive statements " . . . *as invitations to enter in the same domain of reality as the speaker*" (Maturana 1998a:62), where there is full disclosure of the basic premises and coherences that give the statement validity.

Taking responsibility for one's explanations is just as important. If someone operates under the implicit or explicit belief that forces such as rationality and reason allow them access to an independent reality, then they can make statements knowing that they do not have to take personal responsibility for whatever is said. If the speaker believes that he/she is merely a conduit through which there is a flow of information about some aspect of the world, then the question of taking responsibility does not arise.

On the other hand, if the speaker is aware that statements and explanations pertain only to his or her domain of experience, and are valid only in accordance with some criteria of validity that he or she applies and the coherences of a domain of reality that is actively brought forth through some operation, then it is a different matter. Under such

awareness, the speaker is obliged to take personal responsibility for what is said. Whether the speaker is also responsible for the consequential actions taken by others in the name of his or her statements and/or 'knowledge', is another matter. Since this is a matter that bears upon the practice of *MS* I shall put it on one side pending further consideration in later chapters.

## 3.3 Summary

In this chapter I have attempted to construct a frame, based on *AT* , that can sustain the research purposes described earlier. The frame is based on Maturana's detailed explanation of observing in human beings that covers two key phenomenal domains: biology and relations/interaction. From this, there is a derivative epistemology and some very important consequences for those who are prepared to accept its various propositions.

# PART TWO: MANAGEMENT SCIENCE

Having settled on what, for the purposes of the research, I am taking *AT* to be, we are now in a position to begin following the logic of inquiry outlined in Chapter 2. This involves applying the *AT* frame at two levels: firstly that of the field as a whole (Chapters 4 and 5), and later in relation to two of the three major traditions that I have earmarked as being central to the field.

The next two chapters look at management science as a whole but with a particular focus on the two dominant *MS* narratives: hard systems and classical operational research; and, on models that seek to represent reality rather than models whose purpose is to facilitate interaction and learning.

## 4. MANAGEMENT SCIENCE: A CRITICAL PERSPECTIVE

### 4.1 Introduction

Having first issued the caveat that we must be careful about what we read into traditional classifications of *MS*, this chapter goes on to construct a rudimentary definition of its own. In depicting the essential *organisation* of *MS*, it places particular emphasis on the process of modeling and, even more, on the person or people who do it.

Fundamentally, the distinctive *AT* perspective on the agent casts him/her in the role of a biological entity structurally coupled to a medium and an observer. The chapter

contrasts this with other perspectives that may be discerned through the literature before going on to examine the issues that are involved in managing the tensions between norms of objectivity and impartiality on the one hand, and the agent acting as an adjunct to management on the other. The remainder of the chapter examines these issues from the angle that while the structure of the agent is important in terms of what actions are and are not possible, it is the nature of the relational circumstances that is critical in terms of determining specific modeling and action outcomes.

## 4.2 Traditional classifications of Management Science

At the outset, Jackson's (1991) rendering down of *MS* into discrete sub-fields provides an opportunity to make what is perhaps the most obvious point of critique. This is that since these kinds of distinctions belong to the domain of the observer they have something of an arbitrary quality about them. They are arbitrary in the sense that they are the result of the kinds of questions that we ask of our experiences; in this case about *MS*. They are not arbitrary in the sense that although as observers we decide what categories we wish to apply, we do not empirically determine which of them will be instantiated.

To that extent *MS* is a construction. The 'carving up' of *MS* that goes on through the literature, is done from an external perspective, and cannot therefore be taken to provide unambiguous statements about its 'true' state. Relatedly, labelling someone 'management scientist' does not convey anything like a complete picture of how that individual practices *MS*. Since they have all undergone different ontogenies, no two management scientists are completely alike. And since some element of novelty is present in almost all practical *MS* (see Pidd 1988), all interventions are different in some

regard. To say for example, that someone practices 'hard' or 'soft' *MS* defines them only at the grandest epistemological level. At the level of practice this categorisation is less useful because here the situation is infinitely more complex. A rather straightforward basic assumption about the nature of the world, and/or knowledge, can generate extremely rich and complex behaviours. In short, the practice of *MS* arises out of the specific circumstances and interactions of the actual people who practice it, and it is shaped by their lived experiences, concerns and interests.

Even if what people do does appear to converge on favoured descriptions, we must be careful about what we conclude from this. Maturana invites us to question the utility of semantic and/or contextual explanations of behaviour, particularly those which take the form - *"he is doing X because of. . . , she is doing Y in order to. . .'*.

> *"Each scenario is choreographed against the background of action provided by the others. An observer . . . viewing things from the balcony, as it were, can distinguish particular system "properties" in the choreography. But this in no way means that the individual dancers are being forced into doing what they are doing by some sort of systemic force . . . The choreography is a description from above and not a series of promises, obligations or certainties."* Efran and Lukens (1985:28)

Semantic descriptions then, ought not to be confused with system operation, and we must treat customary and traditional distinctions with care. The view *'from the balcony'*, as Efran and Lukens put it, is but one possible view, and a restricted one at that. Ultimately, what any management scientist does is an outcome of their current and historical interactions, which arise out of a complex triggering of structure-determined behaviours. It follows that there is infinite variety in how we might go about distinguishing management science.

## 4.3 The basic form of Management Science

Despite the arbitrariness of its definition and the complexity/diversity of practice at the operational level, the terms 'management science' and 'management scientist' continue to crop up in popular and academic discourse. We can only assume therefore that there is something distinctive about who management scientists are, and what they do (i.e. distinctive ways of co-ordinating behaviour in Maturana's terms). If there were not, then there would be no reason for differentiating management scientists from applied mathematicians, organisational development practitioners, or action researchers, all of whom, from time to time, do similar things.

Given its variety, the immediate task is to find a way of simplifying management science such that it may be discussed in concept rather than as an infinitely complex and diverse set of activities. Fortunately, in seeking an answer to this question, we need look no further than *AT* itself, for Maturana faced exactly the same sort of conundrum in his quest to ascertain what it is about living systems that allow observers to describe them as such.

To this end, as we have seen, he uses the term *organisation* in reference to the dynamic features of any *complex* system (i.e. one that an observer decomposes into component parts), which define it as a member of a particular class of system. These include the parts of the system and the manner in which they relate. He uses the term *structure* to refer to the way in which a system's organisation is realised in a particular case.

Applying this logic, I submit that there are four elements that are invariant across management science which have a particular *manner of relating*, and which can be taken

to *exist in the dynamic space* (Maturana 1993b). These elements are: a situation that at least one stakeholder (often a 'manager' of some sort) deems to be problematic; an 'organisational' context in which the problem is located; a process of modeling (carried out by one or more 'agents'); and, an intervention that occurs in the name of 'improvement', and which involves instigating some sort of change.

Of these activities, I submit that model building is pivotal to the organization of 'management science'.

> *model building is the core process of the profession of management science"* (Mason, 1994:194, emphasis added).
>
> *" . . . few people would disagree with the notion that some kind of formal model is involved in most management science . . . "* (Pidd, 1996:25 emphasis added)
>
> *" . . . learning to model, or to think in terms of models, is one of the most important motives for studying management science."* (Eppen, Gould and Schmidt, 1988:2, emphasis added)

In what follows, the focus is very much on the verb (modeling) and not on the noun (models). This enables a broader perspective to be taken, and is in line with usage of the term established elsewhere. Wallace (1994:2), for example, includes the problem appreciation and formulation stages in the 'modeling' process, as does Little (1994:168) who focuses on the broader context within which models are constructed, rather than on the more narrowly defined activity itself.

From this observer's perspective, modeling is what management scientists 'do', and it is what management science 'is about'. The other key elements – the problem situation, the organisational context and the process of implementing improvement strategies are important, but somewhat less pivotal.

Now since the focus in *MS* is on the process of modeling, which is an activity carried out by someone, and since, fundamentally, *AT* is a theory about human beings as

biological and observing systems, juxtaposing the former with the latter places the spotlight of attention firmly on the management scientist. From an *AT* perspective, the distinctive image is that of someone 'doing' *MS* in an always-already background as a biological living system structurally-coupled to a medium, and as an observer making distinctions and explaining experiences.

In order to fully capture the distinctive contribution of this perspective it helps to contrast it with others. For this reason, the next section considers conventional images of the management scientist that are conveyed through popular discourse and through the literature.

## 4.4 Conventional images of Management Science and the Management Scientist

In pre-modern times people sought truth through religious belief or through phenomenological experience (Lawson, 1989; Brown, 1992; Magee, 1987). During the 17th Century however, this source of knowledge came under serious attack from a Descartes-inspired 'normal' science that asserts the possibility of there being more objective accounts of reality. Since then, the so-called 'scientific method' has come to be regarded as providing the means through which true and valid theories about the world may be obtained.

Underpinning objectivist notions of truth is an ontological thesis that there is a knowable world independent of all human activity. Knowledge is not regarded as cultural phenomena or the product of a single theory, rather it is 'out there' waiting to be discovered, (Lawson, 1985:xv). Wittgenstein's (1953) *'picture'* theory of language

captures the key assumption that objectivist science makes about the relationship between language and reality. Wittgenstein claims that there is a correspondence between words or thought-signs and external things and sets of things: the key feature of language is reference: 'bits' of language are used to refer to 'bits' of the world (see Gregory, 1989; Tomlinson, 1989).

The upshot of this *correspondence* or *representational* epistemology upon which science and much everyday thinking is based, is that there is an intimate interconnectedness between what we appear to know and reality. What we come to know is there before we know it. Our mental constructs of the world represent the 'objects' and the 'facts' that are 'out there'.

While, on this view, the purpose of language is to name things that exist, the job of science is come up with theories - akin to 'maps' of reality - which mirror an objectively ordered world independent of all human activity. The so-called scientific method, so the argument goes, makes all of this possible. Adherence to its strict principles allows subjective concerns to be weeded out leaving only objective results. Thus the proponents of objectivist science claim that the knowledge that they discover is not tainted and therefore may be trusted.

If, to all intents and purposes, the scientist <u>is</u> primarily a conduit through which flows ready-made information and knowledge about the world, what can be said of the <u>management</u> scientist? There are a number of ways of dealing with this question.

One is to focus on the people concerned. We scientists, Maturana remarks, ". . . *like to explain the praxis of living, and the passion for explaining is the fundamental emotion that supports what we do as such*" (Maturana 1988b:34). We might then

combine the scientist's passion for explaining with the management scientist's passion for helping deal with organisational problems and 'improving things'.

Perhaps a better way of crystallising the difference is to focus on the context in which the majority of *MS* takes place. If there is model building in pure inquiry-driven science, then generally it is subject only to the public's desire to know about some aspect of reality. In contrast, *MS* modeling - like applied science more generally - is usually embedded in a wider internal or external consulting process. Whereas 'pure' scientists are in the business of expanding the frontiers of knowledge, management scientists are in the business of producing knowledge, as Argyris (1982) puts it, *'in the service of action'*.

This applies whether the management scientist is acting as a member of an in-house group, as an academic consultant, as a specialist provider, or as an in-house or external dispersed consultant. Like the pure scientist, the knowledge that these management scientists produce may be descriptive of the world as it is, or as it could be, but its validity – as we shall see later - hinges more on its usefulness than in terms of its candor. As Zimmerman puts it:

> *"If . . . one uses industrial companies or departments as primary customers, then almost always technology is asked for and not science . . . the major criteria for success is the customer functionality which determines the value of the service offered."* (Zimmerman, 1998:414)

The point then, is that whereas the end of science is publicly available knowledge, and the ethic of science is truth, *MS* is committed to practical impact on the functioning of organisations. Consequently, it is best considered to be more of a technology than a science (see Keys, 1989, 1991, 1998; Mingers, 2001; White and Taket, 1994).

Practical *MS* is almost never pure inquiry. It is oriented toward some improvement process. There is always, by implication, some organisational pathology.

Something is not right and the management scientist is called upon to fix it or improve it. In that sense *MS* is also what Schein (2000) calls a *'clinical'* process.

Potentially, the various contractual and other obligations of the management scientist to the client, can lead to some tension between *MS* and the traditional epistemology of science. For instance, as Burgoyne (1989) points out, notions of best practice remind practitioners that they must understand the problem as the client sees it, be sensitive to the self esteem of the client, and put forward solutions that are not only technically sound, but also politically acceptable and administratively feasible. Burgoyne further remarks that clients will only come to trust and value consultants if they prove trustworthy, reliable, helpful and sensitive.

All of which implies that 'acceptable truths' may be to *MS* what 'basic truths' are to science. Whereas the scientist's knowledge is measured against the yardstick of a transcendental reality, in *MS* the agent must disclaim complete objectivity in favour of relevance and/or usefulness as the key validity criterion.

Another possible tension between *MS* and the objectivist epistemology of science occurs when the management scientist is directly involved in implementation. In these situations it is imperative that he/she comes up with something that will work. And, as Bardmann (1996:212) points out, factual analyses often fail when confronted with organisational biases and prejudices. Very few scientific approaches, Bardmann suggests, pass the quality test of practical applicability and usefulness.

In spite of this, and in spite of the gradual intrusion on the dominant *MS* paradigms of methodologies based on non-objectivist epistemologies and modeling techniques that are geared more to learning than representation, it would be hard to claim

that there has been a complete erosion of traditional scientific values in the discipline. If the literature is anything to go by, most management scientists are encouraged to operate within the same epistemological tradition as their basic research colleagues, and notions of best practice are suffused with scientific values and imagery.

The pre-eminence given to objectivity is manifest most clearly in the historically significant guidelines that were produced by the (American) Operational Research Society in 1971 (see Berger et al. 1971). Here, practitioners are urged to ensure that their work is carried out in a manner that is *"open, explicit and objective"* (emphasis added). The guidelines further claim, that OR work must be *"open to scrutiny* - (that there) *should be complete exposition of assumptions, to permit replication of the procedures by objective observers*" (emphasis added). Finally, practitioners are reminded that OR should be carried out in the *"scientific spirit"*, such that " . . . *an operations analyst should . . . take a broad and disinterested view, free of parochialism, inflexibility, or prior prejudice . . .*" (Berger et al. 1971:1141)

Elsewhere, speaking of OR's commitment to objectivity, Tomlinson remarks that:

> *" . . . the management of OR . . . implied that it was the function of OR to discover the truth, to present that truth to the decision-maker, and to leave it to him or her to accept the truth and take appropriate action. This view maintained that the OR analyst had to remain scientific, objective and separate."* (Tomlinson, 1998:404)

Although this objectivist undertaking is clearly evident in OR/*MS*, its influence may also be detected in systems. It is implicit, for example, in Checkland's (1981) description of the class of problems as falling within the remit of hard systems thinking:

> *"there is a desired state, S1, and a present state S0, and alternative ways of getting from S0 to S1. 'Problem solving', according to this view, consists of defining S1 and S0 and selecting the best means of reducing the difference between them".* (Checkland, 1981:138)

As Flood (1993:73) points out, the 'states' in question are presumed to exist independently of any particular observer. Flood further remarks about how problem solving, of the hard systems genre, has traditionally been entrapped by the unquestioned assumption that problems that are real, separable, identifiable things that can easily yield to the typical four or five stage problem solving technique. The primary tasks are seen as the establishment of essential facts of the situation, how to identify the key problems, and then how to objectively apply and evaluate the range of possible solutions. In this regard Rosenhead (1989:5) claims that hard systems provides what is perhaps the exemplar of a rational planning process in which the task of the analyst is to recognise the problem 'out there' and then turn the handle on the analytic 'sausage machine'.

The OR guidelines bear testimony to the historical bond between *MS* and objectivist epistemology. More recently many writers seem to accept that while a *MS* model is only a representation, there is a reality – an original so to speak – underpinning it.

> *" . . . a model may be evaluated by reference to how well <u>it represents a concrete reality</u>. . . "* (Allison, et. al. 1994:13, emphasis added)

> *" . . . we are all aware that models are not reality <u>but only approximations or representations of reality</u>"* (Walker, 1994:232, emphasis added)

> *"Despite the diversity of models, they have aspect in common. They are all idealized and <u>simplified representations of reality</u>."* (Eppen, Gould and Schmidt, 1988:3. emphasis added)

> *"It would be naïve for people sitting inside the closed walls of* (a) *. . . cave to interprete the shadows as the real world . . . while <u>we strive to move closer to reality</u> with our models we must always be conscious of the fact that the knowledge we produce is also based on the images or reflections we see on the walls of our own caves."* (Barraba, 1994:146)

In similar vein, Pidd (1996:20) comments that while the modeling of reality is not as straightforward in *MS* as it is in science, *"there is nevertheless a commitment to the ontological claim that reality exists in a form external to the observer."* Mason too sees

important differences between *MS* and science. Nonetheless he remarks that *"a management scientist is a scientist first . . .* (and) *. . . models are built with scientific precision"* (Mason, 1994:193). Similarly Tomlinson (1998:404) admits that: *"deep in our scientific training . . .* (there is) *. . . a belief that we should be concerned with discovering the answer or achieving the best result."*

Finding it too restrictive, some management scientists such as Ackoff, Churchman, Checkland have rejected objectivist science and have broadened their outlook to embrace more subjective epistemologies. Most, however, have been socialised in the values of objectivist science imbibing the ethos of objectivity and representation (Taket 1994). Thus, speaking of OR, Mulvey (1994:71) claims that: " . . . (it) *is generally practiced by . . . disciples of the scientific method and applied mathematics"*. Mulvey (1994:64) further claims that: *"if pressed most OR professionals fall back on the scientific method as the key to picking the correct model."* (emphasis added).

More generally, Gass (1994:209) remarks that *"analysts conduct their work under the basic experiential, empirical and objective procedures that guide all scientific inquiry"*. And, in what sounds like a contemporary reaffirmation of the ORS principles, Walker (1994:226) acknowledges that while modeling is a craft not science, " . . . *we use the scientific method in performing our work"*. He takes this to mean that there are clear principles of good practice that should be followed. These include the suggestion that: *"the work is open and explicit . . . the work is objective . . . (and) subjective judgments should be used as little as possible; when used they should be noted explicitly"*. Further, that the modeler should provide the decision maker: " . . . *with the most accurate; complete, and unbiased information possible . . . it requires a continual search for truth."*

(Walker 1994:226, emphases added). Similarly, Tomlinson (1998:406) complains that "*some still feel that it should be possible to undertake an OR study whose sole objective is to 'reveal truth'.*" Finally Lawrence and Pasternack offer the suggestion that:

> "*If you were to read 50 different texts and papers in the field, you would most likely find 50 different definitions for the term 'management science'. All however, sound a common theme. Management science is a discipline that adapts the scientific approach for problem solving to decision making.*" (Lawrence and Pasternack, 1998:2, emphasis added)

In summary, it is clear that the consulting focus of practical *MS* has wider ramifications that create some tension between it and the traditional philosophy of science. Despite this there is still sufficient evidence to suggest that, in the dominant paradigm at least, *MS* remains wedded to an epistemology which supports the basic notions of truth and objectivity in relation to the material world that it seeks to 'improve'. If the literature is any guide, *MS* still relies very heavily upon guiding norms such as empiricism and realism, and scientific imagery and ideals continue to crop up in *MS* discourse.

None of which should come as a surprise. Management scientists are no less immune to the cultural authority of science in the West, than is the rest of the population. Science, as Woolgar remarks,

> "(is only) *the tip of the iceberg of . . . the public face of the ideology of representation. It provides, so to speak, the official party line on an attitude which pervades practices well beyond the confines of professional natural science*". (Woolgar, 1989:133).

Professional science then, does not have a monopoly on the ideologies of objectivism and representation. Science is more than that which takes place in laboratories and research institutions. It at the central core of the Enlightenment project of development for progress and human emancipation, where it was centred upon the belief

in linear progress, absolute truths and rational planning. Science, as Hames (1996:4) puts it, may not entirely be *". . . the oxygen that sustains our civilisation"*. Neither, in recent years has it necessarily *"swept all rivals aside"* (Tomlinson 1989:43). Yet to the extent that it remains a founding principle of the modern perspective, we can surmise that for some, its principles will continue hold sway. In such circumstances questioning the ideologies of objectivity and representationalism will almost certainly seem to be irrational.

I shall conclude this section by attempting to firm up an image of *MS* practice that, in the next section, is the basis of an *AT*-based critique. The image is that of an agent who is inextricably involved in the process of managing organisations in support of some purpose, whilst simultaneously seeks to maintain a sense of independence and objectivity. Tomlinson's description of the 'critical dilemma' of *MS* conveys a useful sense of what I mean. He remarks:

> *"To be involved with management – indeed to be an extension of management – in the actual solution of its problems, and at the same time to be able to withdraw from the fray and dispassionately study what is being done. It is the very nature of the subject."* (Tomlinson 1998:406)

## 4.5 Interrogating objectivist Management Science – an autopoietic theory perspective on the modeling process

Let us begin, at a high level of abstraction, by reframing the role of the agent in *MS*. Thus far I have suggested that while the process of consulting in support of managerial objectives introduces a dimension to *MS* that is lacking in pure scientific inquiry, fundamentally the objectivist underpinning of dominant paradigm *MS* remains largely intact.

From an *AT* perspective, the starting point for reframing *MS* practice is that which casts the agent in the role of living being structurally-coupled to a medium, and as an observer. Whereas the various perspectives just described take living/observing to be unanalysed givens, here they are brought fully in the spotlight and their epistemological and other ramifications worked through.

On this view, *MS* is something that is tied to both living and to observing. When an agent goes about his/her daily business they do so 'always-already' as biological entities structurally-coupled to a medium, and as observers operating in a particular relational context.

Let us unpack this further. Reference to 'living' means that everything a human being does is done in his/her *praxis of living* i.e. as a system with certain constitutive biological characteristics. It further means that when we do things we do them according to our structures in the present. And, because structures at any point in time mirror historical interactions, it means that our personal histories have a generative presence. From this it follows that when a management scientist undertakes an assignment, he/she does so with a particular set of biological and structural predispositions to do things in a particular way. He/she does not arrive at the intervention with a 'clean sheet'. Heidegger (1927) would say that there is a certain *throwmness*; Gadamer (1970) would say that there is a *prejudice of tradition.*

Reference to observing extends the picture. It says that although the agent enters the intervention with a set of biologically and historically determined capabilities and prior inclinations, the 'doing' of *MS* depends upon the nature of the relational dynamics as these emerge over time. Strictly speaking, *AT* goes further than this. It suggests that, as

a phenomenon, practical *MS* belongs to the relational circumstances in which it is conducted and it is not reducible to the people who do it.

### 4.5.1 The biological domain

As we saw in the previous chapter, the main epistemological implication of bringing biology into consideration involves appreciating that observers cannot access an independent reality. This is a logical consequence of the organisational closure of the human nervous system and its structure-determinism. Hence whereas objectivist management science asserts that its models yield increasingly accurate and sophisticated understandings of the world as it exists, *AT* suggests otherwise.

Now critics might justifiably claim that while it is technically correct to assert the biological grounding of knowledge this has little practical import since the world that we live is that of our experiences and explaining experiences is primarily what counts. I shall respond to this question later. For now the key point is that 'giving up on objectivity' inevitably means discovering or rediscovering agency, and it is the topic of agency that I now wish to consider.

*Giving up on objectivity and the role of agency*

In disciplines such as *MS* where the ideology of representation is well established, the role of the agent is often regarded as being largely irrelevant. Mingers (1997:427) for example, speaks about how - in the systems discipline - the assumption of a universalistic, ahistorical, acultural, disembodied male subject holds sway. By and large the same can be said of *MS*. There is a strong sense that the agent facilitates

representation, but does not affect the 'objects' of inquiry. 'Facts' (about 'objects'), it seems, are neutral awaiting discovery by anyone who has the requisite skills and knowledge.

Agency of course, is implicit in considerations of *MS* since there has to be someone doing it. Often it is an implied functional presence. Depending upon which paradigm in which the agent is operating, he/she is someone who solves problems or designs systems in support of particular objectives ('hard systems'), helps people to find their own solutions ('soft systems'), or assists in critical reflection on system boundaries ('critical systems').

*MS* textbooks are almost exclusively pre-occupied with developing analytical and technical competences. Neither, where they exist, do the majority of accounts of interventions say much about the person who does *MS*. There is often a remarkable silence about the specific role played by the various people who were involved. The implication is that agents are basically interchangeable.

In the dominant paradigm, one could be excused for thinking that there has been something of a conspiracy of silence about the role of the agent in management science. Elsewhere however, there is an awakening of interest in the topic and various authors have opened up their own conceptual space in which to broach the topic of agency.

In claiming that in real world problem solving "*. . . it is essential always to include . . . an account of the observer . . .*", Checkland (1981:118) flags an issue that later was to become a more central aspect of the reformulated version of SSM that appeared on the scene in 1990. In the latter, Checkland and Scholes invite users of SSM

to acknowledge that they are always part of the problem situation, a view that is further reinforced by other SSM researchers (see, for example, Davies and Ledington 1991).

Mingers (1997) incorporates considerations of agency into his *'Critical Pluralism'* framework; Taket (1994) raises the topic in relation to what she considers to be a dearth of useful ideas about ethics in the operational research literature; and both Romm (1996) and Wilby (1996, 1997) examine agency in the light of their belief that practitioners ought to be fully acquainted with the inherent limitations and biases that are built into the intervention process in general and into key systems methodologies in particular.

Under *AT*, the theoretical space for opening up the question of agency has, as we have just said, to do with the biological impossibility of objectivity, structure-determinism, and, more generally, its focus on the observer. At a more philosophical level, since the 'objects' that feature in *MS* modeling can – on this view - only exist if they are actively 'brought forth' by someone, then agency arises as a logical consideration. The combined effect of this theoretical/philosophical logic is to lay down a serious challenge to the 'independent' 'neutral' 'interchangeable' researcher that Tomlinson speaks about.

There are two major aspects of this perspective on the agent that require further discussion. The first revolves around the historically-conditioned predispositions that the agent brings to interventions; the second has to do with how we conceptualise the 'knowledge' and 'expertise' that an agent brings to bear upon on a problem situation.

*The agent: structural predispositions*

If the dominant paradigm literature is anything to go by, when an agent presents 'knowledge' in the form of a model, it is assumed that the model represents something 'out there' that has been discovered. It may be about the past, about the present or about the future. The agent *qua* expert can do this because he/she has the technical knowledge and insight to make visible that which others, lacking the necessary expertise, cannot. To all intents and purposes, it is a case of the 'agent-technique-methodology' system operating as a conduit through which information and knowledge about the outside world flows.

According to this line of thought, the agent enables or facilitates representation, but is not thought to be capable of affecting the 'objects' that they study. He/she is not held responsible for the character of the designated object but can be 'right' or 'wrong' about what is truthful about objects.

Contrary to this, *AT* recasts the agent from the role of 'knowledge finder' to that of 'knowledge builder'. Whereas the dominant perspective implies that the agent can take a disinterested external perspective on a problem situation i.e. look at problem situations afresh and 'from the outside', *AT* rejects the traditional subject/object dichotomy as well as the idea that phenomena are external to the observer. Management scientists are an integral part of the systems that they study. On this view, even so-called 'hard' data – statistics for example, which is the stock-in-trade of classical *MS* - is a construction of reality; it is not a representation of it.

In systems, Churchman (1971) was arguably the first to challenge the view that boundaries exist in the natural order of physical, biological and social things, arguing that

they are mental constructs. These he claims determine what is *in view* and to be taken account of, and what is *out of view* and therefore overlooked. Since then, other scholars - Ulrich (1982) most notably - have drawn attention to the possibility that power is a major force in boundary setting, and that this can exclude people.

Let us assume a situation in which a client gives an agent some latitude in terms of how he/she approaches and deals with a problem. *AT* suggests that the agent, the 'knowledge-builder', is predisposed to construct situations using favoured distinctions and emotional preferences that have become conserved in his/her structure. According to *AT*, everything observers do is accompanied by some bodily predisposition. Indeed all behaviours involve such 'directionality'. It follows that if we look beyond agents' tendency to see themselves as objective scientists, engineers, or mechanics, we see that emotional predispositions are at the heart of their professional practice.

In theory the impact of these structural predispositions applies across the full spectrum of choices that the agent might make in deciding on how to approach the intervention. Typically choices are made about how to define the problem, about which methodology(ies) to use, about which variables to include in models, and about where physical and/or conceptual boundaries are to be drawn. Choices such as these are not made from some independent objective standpoint; someone - with a particular history - makes them.

This presents a serious challenge to the belief that there are 'correct' and 'incorrect' ways of making such choices (see, for example, Carrier and Wallace 1994:40). Structural predispositions constrain *MS* practice and predispose action to follow certain paths (see Brocklesby 1993, 1994, Romm 1996, Wilby 1997). Contrary to

the idea that agents are basically interchangeable, it means that in terms of the substance and shape of the intervention, it matters a great deal who the agent is.

Recognising that it is an agent's history of structural coupling (and not some foundational basis) that predisposes management scientists to approach interventions in distinctive ways, enables questions to be asked about how the accumulated histories of management scientists influence the shape and texture of interventions. As I suggested earlier in this chapter it also raises questions about the arbitrariness and utility of traditional ways of carving up the *MS* community. Although we are accustomed to operate with high level descriptions such as the conventional hard/soft/critical rendition of *MS*, no two management scientists will have a common history of structural coupling which implies that there is a tremendous heterogeneity and plurality of practice at the operational level. Management scientists become structurally-coupled to paradigms, to distinctive subcultures and communities, to various methodologies and techniques, and to various clients and industry groups. All of this predisposes an agent to favour certain actions and disfavour others, to do some things and not others. Across the discipline as a whole it creates an incredibly rich web of practices.

*The basis of management science expertise*

The second major consequence of bringing the biological domain into a consideration of *MS* arises out of the challenge that *AT* presents to conventional accounts of human cognition. This precipitates a rethink of what *MS* expertise is, how it arises, and – no less importantly - what role it plays in the dynamics of the organisational situations in which management scientists operate.

Derived from classical reductionist thinking, the traditional approach to human functioning identifies three separate subsystems: cognition (thinking), affect (feeling) and behaviour (action) (see, for example, Leavitt 1978, Staw 1991). As we have seen, *AT* rejects these divisions claiming that thinking is a specialised subset of a broader process of cognition that itself is conceptualised as a form of action that is associated with structural coupling. Cognition, on this view, is an integral part of our normal everyday activity. It is not, as Mingers (1994:104) puts it, *"detached cogitation, but situated practical action"*.

Maturana's idea that as biological systems (if not as observers) human beings primarily relate to the world through the pre-conscious process of structural coupling resonates strongly with the philosophy of Maurice Merleau-Ponty (1961). And it is Merleau-Ponty's ball that Maturana's colleague, Francisco Varela, picks up and runs with. In their 1991 work, Varela, Thompson, and Rosch identify two types of knowledge: *'propositional'* and *'commonsense'*. They cite the case of someone learning how to drive a car, to illustrate the difference between the two.

When someone first learns how to drive they have to learn how to start the car, change gears, use the steering wheel, use the brakes. They also have to know about and abide by the various rules and regulations that are designed to prevent accidents. For someone new to driving there is a great deal of such propositional knowledge to be learnt, although with practice most people pick up the rules of driving very quickly. Because it is relatively easy to specify all possible states in these task domains, even robots can be designed to drive a car and follow the road code.

The driving world, however, does not end at the point where one has mastered the various actions that are required to turn on the ignition, steer correctly, and know that it is wise to stop at red traffic lights. Unlike games such as chess or snooker (where there are fewer possible states) driving is a task domain that *"has the structure of ever-receding levels of detail that blend into a non-specific background"* (Varela et al. 1991:147). This background includes a complex array of variables such as weather conditions, road surface, local driving customs, pedestrians, the mood of the driver and passengers, and so on. Knowing *how* to drive a car properly - *effective action* in this cognitive domain - *"depends upon acquired motor skills and* <u>*the continuous use of commonsense or background know-how*</u>*"* (Varela et al. 1991:147, emphasis added).

As is the case of driving a motor vehicle, few of our 'lived worlds' in *MS* have pre-defined bounded environments, although from my vantage point, some are more bounded than others. Hard *MS* - linear programming or producing a spreadsheet, for example - has a highly sophisticated and explicit set of rules that can be transmitted. Although the rules still have limited jurisdiction, once understood, producing outputs is relatively straightforward.

Soft *MS*, in contrast, is manifestly different. The propositional knowledge that is required to create rich pictures, produce root definitions, and construct cognitive maps, can be acquired from textbooks; but <u>effective</u> soft *MS*, as I shall be arguing later, involves working directly with people, and responding, often in real-time, to the exigencies of the situation as it develops. Unfortunately these relationship-managing skills are difficult to capture in a propositional format. Soft Systems Methodology for example, is a much-vaunted methodology, but one could argue that the quality of the coupling between the

agent and the people in the problem situation might, in certain circumstances, be a greater critical success factor than is the methodology itself.

While the propositional content of various *MS* methodologies is undoubtedly important, people still have to rely upon their experiences and creativity in defining whatever it is that they have to deal with. Most *MS* problems yield to a combination of technical knowledge, experience and advanced common sense. Gene Woolsey, a leader in *MS* consulting, places the propositional technical knowledge of *MS*, in this wider experiential context. He defines *MS* as: *"The use of logic and mathematics in such a way as to not interfere with common sense."* (Hesse and Woolsey, 1980:17, emphasis added). Similarly, in identifying critical success factors in OR, Daellenbach and Read (1998:433) remark that practitioners need *'large doses of common sense'*.

It goes without saying that these commonsense understandings have to be learnt somehow. Yet because *"the unmanageable ambiguity of background common sense is left largely at the periphery of the inquiry"* (Varela et al. 1991:148), they rarely have chapters assigned to them in textbooks, neither are they regularly discussed at conferences. One could argue that there is a distressingly wide gap between what management scientists write about and what they do.

Maturana might even claim that because highly complex actions can be executed without the generation of an observer to appreciate and evaluate them, 'giving voice' to all aspects of the *MS* 'experience' is unattainable. As (self) observers we become aware of some of our action patterns, but because only highly abstracted and selected aspects of our operations are ever languaged, much of what we do remains unnoticed in the background. The basics may be simply so obvious that we do not see them. Even if we do

notice, we are subject to our structural determinism, so whereas the actual experience of doing something is spontaneous, thinking about, and then articulating it, is a reformulated activity that occurs post-event within this constraint.

The same difficulty applies when we ask experienced practitioners what worked for them. They don't know; they only know the story of what worked for them. Moreover, because, as Varela (1981:66) puts it, *"in finding the world as we do, we forget all of what we did to find it as such entangled in the strange loop of our actions through our body"*, thinking and describing is only possible against a background of taken for granted and largely inaccessible skills and practices that are embodied in our structures.

While the aforementioned example of soft *MS* provides a good illustration of the limitations of prepositional knowledge, the same logic does apply elsewhere. Acquiring propositional knowledge is only the first stage in becoming effective. Dreyfus (1996) – who, like Varela, also pays intellectual homage to Merleau-Ponty - agrees that 'instructor provided' knowledge is sufficient only to get someone started in a task domain. That which is learnt from teachers, textbooks and journals, mainly concentrates on detached uninvolved rule-following behaviour leading to reasoned responses. Typically it lays out ways of decomposing task environments, differentiating commonly occurring situational features, and it produces rules for dealing with these. This is very useful for the novice, but it capitulates in the face of the potentially vast number of situational factors that must be faced, especially when these differ in subtle, nuanced ways. Neither can it provide perspectives for prioritising these, i.e. in deciding what is important and what is not. Competency or proficiency in such circumstances requires active involvement, practice and experience of a large number of cases. Becoming expert, where reasoning gives way

to intuition, is even more demanding. This requires a gradual accumulation of a vast number of situational discriminations, associated responses, and acquired feedback on the success or failure of these. Experts, with this sort of experience, know intuitively what needs to be done in a situation and how to do it. They have, in Eisner's (1997) terms, an immediate grasp of *'field forces'*. They resort to rule-following behaviour and reasoned responses only when things do not turn out as expected.

The concept of *'enaction'* or *'embodied cognition'* articulated by Varela and his colleagues, and developed in the domain of information systems by Mingers (1996), adds another piece to this jigsaw. Enaction further extends the idea that knowledge is constituted in our actions. Knowledge, on this line of thinking:

> *" . . . is contained in the ability to perform special tasks. A dancer has knowledge in her limbs, an experimentalist in hands and eyes, a singer in the tongue, the throat, the diaphragm . . . "* Feyerabend (1987:106)

According to Varela et al, as an individual confronts new situations various experiences are gained through thinking, sensing, and *moving*. This means that the way we experience (and 'bring forth') the world, is very much an *active* construction involving the whole *body*. Effective action depends upon having a body with various sensorimotor and orienting capacities that allow an agent to act, perceive, and sense in distinctive ways. If the agent's body has not learned how to orient itself in such a way that the relevant cues are picked up then they run the risk of 'missing' that which others might pick up.

Again, I suspect that this is a major critical success factor in much *MS* where the agent has to respond expeditiously to the demands of the situation paying due regard to the needs of those involved. The problem is that these sorts of orienting credentials are

not easily taught. They grow out of accumulated lived experience of certain kinds of activity. They are entrenched in the day to day experience of acting in the world, and they become entangled in various ways in expert practice.

The main point of this section is that while the newcomer has to acquire relevant propositional knowledge, this is only the very first step in becoming a proficient practitioner. Really 'knowing' *MS* - acting effectively in it - makes more substantial demands, and these can only be satisfied through active bodily involvement, experience and practice. Any claim, then, that an agent is effective must speak about their whole 'being', about how they enact their daily existence in the fullest possible sense, not just about 'what they might know' in a conventional sense. What counts is not what someone says they know or what they say they can do, it is what they actually do in relation to a given set of criteria.

In addition to challenging conventional understandings about the nature of *MS* expertise, repositioning mentalistic knowledge within a much broader perspective on cognition has wider ramifications that bear upon the role played by *MS* and management scientists in the organisational decision making process. Since *MS* tends to be conceptualised largely as an intellectual and technical activity, it could be said that its prominence as an adjunct to managerial decision making rests upon a continued positioning of intellect and technical knowledge ahead of other forms of 'knowing'.

In this regard, the conventional separation of thinking and doing assumes some relevance. A further development of this separation is the notion that decisions (actions) ought logically to follow analysis (thinking). In theory this approach suits *MS* since much of its technical weaponry is explicitly designed to sustain the analytical stages of the

organisational decision-making process. Moreover, modernism's faith in conscious reflection and intellectualisation as perhaps the most sophisticated and reliable form of knowing, and the superiority of technical expertise over 'common sense' would seem to be equally favourable to *MS*. Both sets of conditions give legitimacy to both *MS* and the management scientist.

To the extent that *AT* integrates cognition and action, and to the extent that it brings embodied knowledge, intuition, and emotion into account, the aforementioned logic is turned on its head. The question then becomes whether this is tantamount to questioning the legitimacy and value of the discipline.

In theory, should such a theoretical shift occur, it could pose something of a threat to *MS*. It is unlikely however that this would be life threatening. Repositioning intellectual and technical knowledge within a broader model of cognition does not mean that the processes through which these are developed are any less important. It is unlikely for example, that anyone could intuitively formulate improvement actions in areas such as inventory control or flight crew tracking to surpass those developed using sophisticated software packages.

Yet this broader perspective on cognition does imply, in high level strategic decision-making for example, that the predictions delivered by sophisticated and finely-tuned models ought not necessarily to carry the day through the decision making process, if indeed this has ever been the case. It further suggests that management scientists ought not to be perplexed, if their best efforts do not hold sway in decision making terms. In that sense, *AT* provides theoretical support for organisational events that most management scientists are already very familiar with.

To my mind the analytical/technical capabilities for which management scientists are best known are unlikely to be seriously threatened under the circumstance just described. Ultimately if the technical knowledge produced by the management scientist helps people to act effectively in the world of their experiences, which is to say, to act so that they achieve a goal that they have chosen, then we can surmise that it will continue to play a very important role in organisational decision making.

From this it follows that the neither the management scientist, nor the model/methodology used, gain their legitimacy through being able to analyse and mirror the way the world is, and/or predict how it will or might be. Rather their legitimacy hinges on the extent to which they are useful. This being the case, the goal of *MS* is not to demystify the universe, instead it is to construct models that provide decision-makers with a means of dealing more efficiently and effectively with the problems created by their experience. In most cases the model can assist the people concerned in structuring their consideration of relevant issues. However since a model is not a neutral device for dealing with the world, and since there may be alternative forms of 'knowing' that are worthy of consideration, then the model ought not to dictate how the process unfolds.

This section began with the basic biological proposition that an agent comes to an intervention lacking direct access to an independently existing real world. The past, present and future 'real worlds' with which he/she deals are an <u>active</u> construction, albeit not one that is completely unfettered. It follows that contrary to the image of *MS* that is often portrayed through the literature, the agent is inextricably part of the system that he/she is dealing with.

As a biological entity, the agent approaches the intervention with what is, in effect, a hard-wired capacity to do things in particular ways, and – to the extent that there is an 'embodied' as well as intellectual capacity to do things skillfully - in ways that are not immediately obvious to the agent himself/herself. The same can be said of the client, the problem owner, the decision-maker, and other parties to the intervention. None of these actors enter the intervention with a 'clean slate'. Each one participates against the backdrop created by a set of historically conditioned predispositions.

However, while the structural predispositions of the various parties to the intervention are influential in shaping its character, they do not determine it. The substantive character of the intervention depends on the dynamics of the relational circumstances as these arise moment by moment through the intervention.

### 4.5.2 The relational domain

One of the key insights of *AT* is that although the historically acquired structural predispositions of the actors who are involved are important in shaping the nature of *MS* interventions, the activity of *MS* itself takes place in the relational space between the various actors and between the actors and the problematic situation. On this view, *MS* is not something that anyone actually does, it is something that happens as a result of relational dynamics. To this extent, creating this relational space in the first place is a key requirement for practical *MS* to occur. Certainly it would be wrong to automatically assume that the necessary structural couplings are in place that provide the basis for *MS* that meets the expectations of the key stakeholders.

Take, for example, the relationship between agent and client. Often (see, for example O'Hair, Friedrich, and Shaver 1998, Bennet and Olney 1986) this relationship is construed in terms of information transfer. Biologically, however, we know that this is impossible. Since both agent and client are structurally determined, anything that either party says or does is a potential source of perturbation to the other. However it is a trigger only if so recognised by the other party.

Even if there is structural coupling what one party hears when the other speaks is structurally determined. The notion that a client can transfer a mental image of their requirements in a project from their own mind to that of the consultant, who then goes away and does it, does not correspond with how structure-determined systems work. It should therefore come as no surprise when consultants are charged with 'misunderstanding the client's needs', of 'failing to read the situation correctly', of 'making inappropriate recommendations' and so on.

*AT*'s focus on the linkage between practical *MS* and the relational space in which it occurs, provides theoretical support for those who might claim to have identified a gap in the *MS* literature. Recently, a number of authors (see Miser, 1998; Ormerod, 1998; Fildes and Ranyard 1998) have suggested that while management scientists themselves acknowledge for example the importance of developing people, organisational, and relationship-managing skills, there is little by way of adequate theory on which to build good practice. On this basis, we can surmise that if they are not looking beyond their own literature, inexperienced management scientists simply do not have the conceptual tools that allow them to think seriously about these issues and to develop appropriate skills.

Having flagged this issue, I shall now put it aside until the next chapter where it can be discussed in more detail alongside other key gaps that I believe *AT* has the potential to fill. For now, I shall assume that the necessary structural couplings are in place, and return to the main point which is to consider how the nature of these couplings impact on the shape and texture of a particular *MS* intervention.

Assuming that there is a structural coupling between the various people involved (see Mingers 1997, for a more precise elaboration of the nature of these couplings), what happens in the relational space between them is critical in determining key choices that must be made as the intervention proceeds. It is generally understood that *MS* is about making choices: about how the problem should be framed, about what methodology or tools will be used, about how they will be used, about what variables should be included in the model and so on. Basically the process involves making decisions about what are, in effect, just specialised sets of distinctions. To that extent it is a process of observing.

This choice-making aspect of *MS* has not gone unnoticed in the literature. In particular, following Ulrich (1982), the notion that circumstances dictate choices such as boundary judgments has come under strong attack from the critical systems wing of *MS* that was mentioned earlier. A common theme across this diverse literature is that power relations intrude on the aforementioned choice-making process although there is no unanimity in relation to the question of how it operates.

In this context, I submit that *AT* may have some explanatory potential. Maturana himself does not have much to say about power, and critics - notably Berman 1989, and Mingers 1995 - have roundly criticized what he has said. That aside, *AT* can accommodate considerations of power, not least because it provides a very clear

description of the dynamic that operates 'inside' networks of structural coupling, and which, from moment to moment, determines decision outcomes.

In *AT* terms, the relational dynamics that underpin the choices and boundary judgements that define a particular intervention is the outcome of a co-drift between the key actors who are involved in making the relevant choices. This co-drift between two or more structurally-coupled (but not necessarily equally powerful) systems provides the setting in which these features of the intervention are 'brought forth'. And it is the latter idea: of boundaries etc. being actively brought forth through an interpersonal dynamic, that contrasts so sharply with the belief that the shape of an intervention is dictated to by prevailing circumstances. I shall now examine this claim in more detail through reference to two key aspects of practical *MS*: firstly, the notion of an 'organisational problem', and secondly, the process of modeling.

*Autopoietic theory on 'organisational problems'*

When *MS* is not being put to the task of designing and implementing new operations or procedures or evaluating existing ones, it is usually applied in determining and recommending corrective action for operations and procedures that are producing unsatisfactory results. In other words someone has decreed that a situation is problematic and *MS* analysis is the result of a request for assistance.

In fairness, much of the *MS* literature acknowledges that problems are subject to varying interpretation. Indeed it is now commonplace to hear management scientists refer to problems as social constructs. And, in many cases writers advocate a 'sweeping in' of

different points of view (see Daellenbach, 1994; Maani and Cavana, 2000; Pidd, 1996; Wilby 1997).

Much of this literature is based on the philosophy that there is a pre-existing 'real' situation albeit one that is subject to different interpretations. For example:

> *"MS is about real world problem-solving"* (Eppen, Gould and Schmidt, 1988:2, emphasis added)

> *" . . . in management science, problems, puzzles and messes are social constructs . . . different people interpret the same issues in different ways . . . there may be several valid views of what is happening and of what might be done about it."* (Pidd, 1996:72, emphasis added)

> *"Those performing a management science analysis should make every effort to observe the problem from various points of view within the organisation . . . "* (Lawrence and Pasternack, 1998:15, emphasis added)

While incorporating subjectivity into problem definition may be intuitively attractive, it is fundamentally different to what Maturana has in mind. For Maturana it is not simply a case of observers perceiving a pre-existing situation in different ways. Instead it is a case of the observer constructing the situation on the basis of the specific distinctions that he/she might make. On this view, problems are actively brought forth or they do not exist.

Of course critics might immediately respond by claiming that there is a certain class of problem whose existence does not rely upon being brought forth in the manner just described. A company heading inexorably into bankruptcy for example. Surely, one could reasonably claim, problems of this nature extend beyond people's perceptions? The answer to this question is partly yes and partly no. Yes, in the sense that the financial problems of the company might be manifestly clear to informed outsiders. No, to the extent that the company executives are unaware of the difficulties. Until something tangible happens – let us say a line of credit is withdrawn which prompts the executives

into becoming aware of the situation and trying to do something about it – for them the problem literally does not exist.

Earlier I noted the *AT* maxim *'everything said is said by an observer'*. From this, since a problem is something that is said (or at least distinguished), it follows that problems are observer dependent. Hence Efren and Lukens' (1985:28) claim that all problems are in language and until languaged they effectively do not exist. Thus, no one can take it upon themselves to identify objective deficiencies that must be sought out, diagnosed and resolved.

Hoffman (1988:116) agrees that problems are the system formed by conversations about a problem. In other words, it is the problem that creates a 'system', not vice versa. Unless observers are having conversations (with others and/or themselves) about a problem, unless they are experiencing *'alarmed concern or objection'* (Goolishian and Winderman 1988), then the problem does not exist. It follows that the organisational problems that are the stock-in-trade of *MS* are not ontological entities. Rather they arise out of particular ways of constructing. No 'solution' then, can ever be unequivocally correct in an ontological sense.

Another important logical consequence of what has just been said is that problems have an emotional as well as rational basis. Since distinctions are a feature of consensual domains that arise contingent on the flow of conversations, and since conversations involve a flow of emotions, it follows that problems have as much to do with emotions as naïve realists might claim they have to do with circumstances.

In the company-going-bankrupt example just mentioned, I said that the failure of company executives to notice (i.e. distinguish) and articulate 'the problem', means that

(in that particular moment for the executives concerned) it does not exist. But there is another sense in which a situation that might otherwise unanimously be regarded as being problematic, is not. For example, to the extent that bankruptcy makes a company's financial plight unequivocally clear and therefore removes elements of stress-inducing uncertainty, it may be welcomed by company executives - if not by creditors - as a blessed relief.

Given its overwhelmingly technical orientation, it is hardly surprising that the *MS* literature has chosen to largely ignore the extent to which emotion intrudes on practice. Partly I suspect that this can be put down to the belief that emotions have no place in rational problem solving. Relatedly it may be a logical consequence of the traditional separation of cognition (thinking), affect (feeling) and behaviour (acting). With *MS*'s strong 'think, analyse, act' focus, there is little room for emotion.

Beyond underpinning notions of what is, and what is not 'problematic', emotions intrude upon the full range of activities that go on through the modeling process. For *AT*, emotion is the bodily support that all classes of action require. It is a primary force that initiates the intervention in the first place, and it underpins the various choices that are made as the modeling process unfolds. I shall say more about this in the next section and in the next chapter.

### *Autopoietic theory on models and modeling*

I believe that the relational perspective on practical *MS* that is being developed here sheds further light on two key aspects of the modeling process. Firstly the issue of

modeling as a process of choice-making; secondly, the question of the source of a model's validity and (relatedly) agent's expertise.

In the same way that organisational problems are constituted by observers according to their preferences and the dynamics of the prevailing relational circumstances, the same can be said of the various activities that are involved in the modeling process. Thus, as I have already said, the various people involved, including the agent, approach the intervention with historically conditioned preferences that predispose it to follow certain paths. In effect, certain choices may have already been made.

But these choices are not fixed. According to *AT*, as observers, we human beings always act according to our understanding in the moment, even when – as is often the case in *MS* modeling – the task is to explain the past and/or predict the future. Hence the whole modeling process is constructed in the present according to the coherences of the present as these are reflected upon and negotiated by the various parties involved according to the prevailing relational circumstances.

As the process unfolds, as individual and collective conversations take place about what the situation 'is' and what needs to be done about it, assessments emerge contingent upon the flow of emotions and distinctions, as these reflect, and as they bear upon people's experiences. *AT* would suggest that the process through which a particular action or choice comes to be regarded as 'legitimate' is part of a broader process of formulating a coherent explanatory domain that provides an adequate account of the problem situation as it continues to be experienced. The various choices involved in modeling are not determined by external facts, nor only by people's pre-existing

preferences, they are negotiated by the various people who are involved in the relevant conversations.

This raises an interesting issue in relation to models and/or data that become enshrined in the fabric of an organisation's decision making process, and, as a result, which drive decision-making. In such circumstances, it is not uncommon to hear people claim that 'the model' (or, more commonly perhaps, 'the computer') dictates that a particular course of action be taken. It is as if the model's grasp of the prevailing circumstances is flawless.

In the light of what has just been said, claims such as these need to be treated with a good deal of skepticism. The insight that is obtained from any model is tied to a prior decision to employ a particular modeling approach and/or methodology. In effect *MS* methodologies, techniques and tools are just highly sophisticated patterns of linguistic distinctions and behavioural coordinations; each one having its own distinctive terminology, and its own 'rules' for application. These distinctions provide the mechanism through which the agent constructs the reality that he/she is dealing with. And, if he/she is involved in the problem formulation stage of the intervention then even the nature of the problem reflects these distinctions. For example someone who is accustomed to using the viable systems model might logically tend to couch a perceived organisational dysfunction largely in structural terms, whereas a practitioner of SSM might frame the situation more in terms of the existence of competing worldviews. To a large extent, the 'reality' under investigation reflects these distinctions rather than vice versa.

The same logic applies in the case of *how* a methodology or set of techniques is used. As we have seen, according to Maturana's epistemology, objects are constituted in the domain of the observer, they are not in-themselves. This means that although methodologies might have a 'domain purpose' (i.e. a purpose and conventions-for-use as articulated by proponents), that which is 'brought forth' by a particular agent in the context of a particular intervention depends on the specific distinctions made at the time. Since how a methodology is used has a major effect on outcomes, again it is a case of the 'realities' under investigation being highly dependent on the people involved.

Another reason why we ought to be skeptical of 'the model says' syndrome has to do with the proposition that the key choice decisions that lead to a particular modeling outcome are not fixed, but often emerge as the intervention proceeds (see Munro and Mingers, 2000:15, for some recent empirical support for this proposition). The 'emergent realities' of the intervention therefore are contingent upon and are a product of the flow of the emotions and distinctions that occur in historically, spatially and temporally constituted consensual domains. In this sense, models and modeling are very much a function of the context of their construction.

The danger then, is that models can become de-contextualised. Once produced, they can take on a life of their own; they appear to exist in their own right explaining and helping people to deal with a reality that exists independently. Under such a circumstance the crucial linkage between those who, directly and indirectly, produced the model and the time and place context of its production is lost.

Let us now turn to the matter of the validity of models and the source of the agent's expertise. While *MS* models are geared more towards production than explaining,

they nevertheless arise in the explanation of experiences. Since we use our experiences to explain experiences models are experiential by definition. They are not proxies for reality or some aspect of reality. As we have seen, much of the *MS* literature hangs on the idea that models are only 'unreal' in the sense that they can only ever approximate reality. The argument here is that fundamental uncertainty arises out of the impossibility of separating experiences and people's explanations of these from the relational and conversational circumstances of their production. This, of course, is in addition to the biological constraints on knowledge production discussed earlier.

Again therefore this supports the contention that the validity of models is not a direct function of the extent to which it corresponds to an independent reality. It depends rather more upon whether the model satisfies people's experiences and preferences.

Thus speaking of technology, Maturana remarks:

> *" . . . we use different technologies as different domains of operational coherences according to what we want to obtain with our doings, that is, we use different technologies according to our preferences or desires. Thus, it is our emotions that guide our technological living not technology itself, even though we speak as if technology did determine our doings regardless of our desires."* (Maturana, 1997:5)

To the extent that a model is put to use in support of some purpose, it is, as I suggested earlier, technology of a sort. Hence, its validity has little to do with 'truth' per se. Instead therefore of asking if a certain model is true it is better to ask what it does in relation to what consequences using it would have, and – more importantly - if these consequences are desirable or not.

Take a well-known example. An inventory model that does not offer the promise of lower investment in stock has little 'validity'. The same could be said about decision analysis, optimisation techniques, maintenance planning, simulation, data envelopment

analysis, or any other *MS* tool. If these models do not respectively reduce risk, reduce operating costs, decrease down time, improve strategic decision-making, and improve efficiency then, on this theoretical view, they lack validity.

The same can be said in making retrospective judgments about the validity of models. The issue is whether it was 'true' at the time according to the experiences, emotions and preferences of the people who constructed and/or applied it.

Finally, this perspective on modeling allows us to comment further on the source of the management scientist's 'knowledge'. It reiterates the view that knowledge is not something that a management scientist 'has'. Recall that cognition, according to *AT* is effective action as defined by an observer according to some criteria that they apply in their observing. Thus people are not knowledgeable in-themselves. What counts is not the agent's accumulated mentalistic knowledge or even experience of doing things, it is what he/she can 'do' in relation to some purposes that prevail in a particular place at a particular time.

'Knowledge' then, is conversation specific. In this context it is important, as cumulative models of knowledge development (see von Krogh and Roos 1995:137, Wagner 1972:610, Burrell and Morgan 1979:5) suggest, that through experience management scientists continue to hone their technical skills and keep up with the literature. However these activities 'count' only to the extent that doing such things assists in helping people to successfully do things in particular relational circumstances when measured against defined criteria that the latter apply in their observing.

From this it follows that since the technical competence of a management scientist in applying set procedures is only one aspect of the skill set required (unless of course the

client is dealing with a very clearly defined problem), there is a need for the client or service purchaser to be very careful about who they engage. On the view being expressed here, agents are very definitely not interchangeable. Indeed couching the agent as an entity that is structurally-coupled to a medium suggests that the essence of effective practice has to do with the 'fit' of various structural couplings, the adequacy of which in many cases can only be tested empirically through reference to specific outcomes.

## 4.6 Summary

Through *AT*'s conceptualisation of the agent as a biological entity structurally-coupled to a medium and an observer, this chapter has sought to deconstruct the notion that management scientists seek to maintain a sense of impartiality and objectivity while simultaneously acting as an adjunct to managerial decision-making.

*MS* is about making choices. Thus I have argued that the manner in which problem situations are formulated and decisions made about how to best deal with the problem are not determined by facts or by 'the reality of the situation'. Instead the process involves a sequence of acts and choices carried out, individually and collectively, by the people involved. The various decisions that are made as the process unfolds are the outcome of each individual operating according to constraints imposed by his/her own particular history, and, no less importantly, according to the flow of the distinctive relational circumstances as these unfold through the modeling and decision process. In effect, choices are the outcome of a conversational/negotiation process. This has major implications for the validity of the 'knowledge' that is 'produced' by models and for assessments about the source and extent of the agent's expertise.

# 5. DEVELOPING MANAGEMENT SCIENCE

## 5.1 Introduction

There is no precise point at which a critical perspective on reframing becomes a 'that's fine, but what comes next?' developmental perspective. However I shall now attempt to shift the emphasis from the former to the latter. The first section of this chapter considers what 'putting the epistemology of *AT* into practice' would mean, firstly for the individual agent-practitioner, and second for the agent-practitioner-writer who seeks to contribute to the *MS* literature. The second section of the chapter employs *AT* to develop *MS* in a rather different way, i.e. by identifying and then seeking to plug specific gaps. Two main gaps are examined. The first has to do with the relationships between the agent and relevant organisational stakeholders that determines whether the agent's expertise will be called on in the first place and subsequently used. The second has to do with the issue of implementation.

## 5.2 Putting the epistemology of Autopoietic Theory into practice

### 5.2.1 Epistemological implications : for the individual agent-practitioner

Maturana (1988b, 1993b) claims that one of reasons why explanations are so important is because they can effect how people live their lives. Replacing one explanation with another is, he claims, not a frivolous matter. Such is the case here. Switching from a conventional objectivist epistemology to one based on *AT*'s <u>explanation</u> of observing has major implications.

In this context, 'putting *AT* in to practice' involves the agent doing things in a certain awareness. In general, it involves the agent acting in the full awareness of the epistemological consequences that go hand in hand with the image of the agent (i.e. a biological entity structurally-coupled to a medium, and as an observer operating in language) that was used as the basis for discussion in the previous chapter.

Caricaturing the position, a naïve realist, believes that the various choices that must be made before and during an intervention are largely determined by external circumstances. As we have seen, the 'science' in 'management science' continues to convey an attachment to notions of objectivity and neutrality. And because of the cultural supremacy of science in the West, this gives authority to the agent as well as to any decision-maker who acts on the basis of the agent's expert knowledge. Decisions made 'in the name of science' can then proceed without any further scrutiny or without personal and/or collective accountability. As Mendez et al. put it:

> *". . . the belief that the possession of objective knowledge grants to the speaker an absolute transconsensual authority for demanding obedience from listeners and to take action in the name of truth."* (Mendez, Coddou, Maturana 1988:151)

This separation of science and ethics puts both the agent and the decision-maker is relatively straightforward position. Neither need be responsible because the justification for what they do is a world that exists external to them.

Operating according to the *objectivity-in-parentheses* explanatory path is quite different. Here, the agent understands that he/she is inextricably involved in the process of knowledge production and that any such knowledge, even in situations of widespread agreement among stakeholders, does not capture the 'way things are'. The agent further understands that there are many choices available. These are enacted implicitly and/or

explicitly as a result of both the structural predispositions of the various people involved and of the dynamics of the modeling process as these unfold through the course of the intervention.

At a personal level, such understanding implies a self-reflective form of practice in which the agent continually contemplates his/her own involvement as well as that of others. This echoes claims made by others in and around *MS*. Churchman (1971, 1979) for example, regards self-reflection as a necessary and vital aspect of 'the systems approach'. Elsewhere, the nature of the 'self-conversational' task facing the agent has been described in a variety of different ways:

> *"There is a need to be critically reflective about the historically conditioned biases and values of the researchers involved as well as the theoretical, philosophical and technical 'biases' of the method(s) used."* (Wilby, 1997:415)
>
> *" The way forward lies in gaining an increased understanding of theory and of its relationship to practice. We need to be more aware of our theoretical preconceptions and the way these effect attempts to change the 'real world'."* (Jackson, 1982:17)
>
> " (methodological) *principles constrain practice and these constraints have to be surfaced and addressed."* (Davies, 1992:625)

Operating in accordance with the *objectivity-in-parentheses* explanatory path involves acting in the awareness that all aspects of the modeling and intervention process bear the mark of the people involved, and that mechanisms of understanding affect outcomes. As I indicated in the previous chapter, all three of the problem, the model and the solution, are inextricably linked to and reflect the process of their construction. This implies that the agent ought to be aware that there are always alternative possibilities for understanding and dealing with situations.

Because there is an acknowledgement that those who are involved in the process produce rather than discover the 'knowledge' that nourishes the decision process, there is

a corresponding requirement that they be held responsible. Indeed this is one of the more distinctive features of the objectivity-in-parentheses explanatory path. As Maturana puts it :

> *" If we do not reflect on the conditions of validation of what we do then we do not take responsibility. There needs to be a sense of 'I have brought forth something and I take responsibility for it'."* (Maturana 1993b:tape1)

In practical terms, this involves acknowledging that when a choice is made then it happens in the knowledge that a choice has been exercised in the face of other possibilities. Onus is then placed on the agent and the people involved to account for why one course of action and not another is being taken. Management prerogative aside, decisions then must be defended in the light of making explicit the premises that underpin a particular course of action (e.g. problem definition, methodology/technique use) and having given serious consideration to alternative possibilities.

Because all actions and all knowledge are bound to the distinctions made by those who make them as they make them, it is ethics, not objectivity that is the foundation of change. Correspondingly there is a need to take responsibility in the light of understanding the arbitrariness, the directedness and precariousness nature of whatever knowledge is produced, and in the light of there being other legitimate possibilities. As Roy Jacques puts it, this is:

> *" . . . a world of possibility and of responsibility. It is a world in which we refuse to use 'objectivity' as a reason for avoiding personal involvement in our knowledge productions"* (Jacques, 1989:708)

Yet clearly there are limits as to how far an individual agent can, and/or ought to take responsibility for his/her knowledge production. Yes it is the case that the problem formulation and/or analytical work done by the agent helps to construct what passes for

knowledge. In so doing, he/she shapes power relations in organisations that, in turn, can significantly impact upon the lives of other people. This is an issue that agent ought to grapple with.

On the other hand knowing exactly where the boundaries of the agent's responsibility lie is not always clear. What happens if the problem is formulated in advance of the agent becoming involved? Can he/she legitimately become involved in the project and then disclaim responsibility on the grounds that he/she was not involved in that part of the process? And if the agent acknowledges responsibility for the 'knowledge' that he/she has produced ought there to be a commensurate degree of responsibility for the uses to which such knowledge is put?

Then there is the matter of whether an agent whose livelihood depends upon him/her acquiring loyal clients providing a steady stream of high quality work becomes involved in projects that are not congruent with his/her own personal value system. This is less critical for the academic consultant who has security of tenure and a guaranteed income. He/she can be fastidious in terms of the work that he/she decides to take on. It is much more difficult however for the independent practitioner, or for those management scientists who are embedded in operational units of companies, or are employed by a consulting group. The fact is that management scientists have to make a living, and they face increasing competition in their respective markets. As Zimmerman puts it, management scientists:

> *" . . . are not dentists who are always consulted when somebody has toothache. Customers who look for solutions to their problems hardly ever look for either methods or for OR."* Zimmerman (1998:415)

Many management scientists then, have to constantly demonstrate and re-demonstrate their usefulness. And they can only work on projects that they are asked to do. Being fastidious in their choice of work can be defended in philosophical or even scientific terms, but the practical reality is that not all management scientists have the luxury of being able to make these kinds of choices.

Perhaps the best that can be said is that there are no easy answers to these kinds of questions. Even Maturana (1993b:tape3) equivocates on the matter of whether the agent ought to take responsibility for the uses to which his/her expert knowledge is put. Consistent with his own proposition that objects and entities are constituted through the acts of distinction made by the observer, Maturana claims that technology is not an entity 'in-itself' that has some intrinsic value. Rather it is the use to which technology is put by human beings which lead to the consequences that we live. This seems to imply that the agent is responsible for his/her knowledge but can disclaim responsibility for what other people do with it.

Nonetheless, operating in accordance with the *objectivity-in-parentheses* explanatory path does lay bare the essential ethical basis of *MS*. In that respect it makes the agent's job much more difficult than would be the case when operating under an objectivist epistemology. On the other hand there are some tangible benefits. Without a metaphysical attachment to having to produce models that provide an ever-increasing grasp on reality the agent can get on with the business of doing as good a job as he/she can, and – importantly – as quickly as he she can.

When there is an expectation that the agent must get as close as possible to the 'true nature' of things, i.e. when there is a search for absolutes, there is no way of ever

knowing whether a model is adequate. This can delay action as much if not more than any equivocation that reflects an ethical unease.

In this regard operating according to the *objectivity-in-parentheses* explanatory path is more straightforward than seeking to produce certain knowledge. Providing the agent can account for relevant experiences, then there is an element of closure that is impossible under conventional epistemologies. In any event in practical *MS*, there are times when, as von Glaserfeld (1996:280) puts it, "*. . . there are kinds of knowledge that simply have to trusted*".

Another practical advantage of operating according to the objectivity-in-parentheses explanatory path has to do with the observation that when people operate on the assumption that there are absolute truths, problems can arise when there are disagreements about what is *'the true state of affairs'* pertaining to a situation. Mutual negation and conflict can then occur because each party operates from the conviction that they have access to the truth; each knows how 'things really are'.

If the various parties can be persuaded that such knowledge is impossible then this transforms the idea of demanding obedience in the name of truth into an *invitation* to construct a situation in a particular way according to criteria which are fully disclosed. Thus, the interpersonal dynamics operate: " *. . . as invitations to enter in the same domain of reality as the speaker . . . to create a new domain of reality where the two parties can coexist*" (Maturana 1988a:62, emphasis added). Admittedly this is not easy, but if such a conversational climate can be developed then there is the possibility that some of the emotional heat can be taken out of the situation. I shall return to this point in Chapter 8.

In finishing this section, it is appropriate to comment further on the suggestion that *MS* models exist primarily to help people cope with the problems presented by their experiences not with the problems presented by reality. Philosophical niceties aside some might claim that, in practical terms, this is an erroneous distinction.

In other words does it really matter whether the measure of a model is its ability to predict a transcendental reality or someone's experiences of it? Relatedly, is there any practical difference between viewing the world as ontologically real and viewing the world in terms of experiential reality based on instrumentally useful distinctions? No doubt there are many management scientists who would argue that we perhaps should just get on with collecting and analyzing data rather than worrying unnecessarily about epistemological and ontological issues.

Philosophy aside, to some extent the answer to these questions depends on what sort of management science one has in mind. Certainly it could be argued that there are particular classes of situation where, to all intents and purposes, it matters little whether we are speaking of 'real' or 'experienced' situations/problems/solutions. Those areas, for example, that involves the application of what Ormerod (1998) refers to as *'smart bits'* expertise, provide a case in point. When the agent is seeking to optimise the size of inventory in a factory or the amount of fuel used on delivery schedules, or is involved in designing a system for rostering aircrews and managing transport fleets, the distinction of whether the models employed mirror an independent reality or people's experiences, is not – in practical terms - critically important. This is not to say that these situations are ontologically real. They are just as much a construction as anything else. I am merely

saying that in practical terms, for problems such as these, the distinction between 'real' and 'constructed' is not a critical one.

Elsewhere, the distinction is very important. As we have seen, the raw material that sustains processes of problem definition and problem formulation are very clearly experiential, so to the extent that an agent is involved in this process then the distinction is relevant. Relatedly, as we have seen, management scientists are not just technical fixers of minor problems. Their 'helping systems to operate more effectively' role often carries with it a much more substantial involvement in determining the shape and direction taken by organisations and/or the wider society. Here the agent's research helps to construct what passes for knowledge and, in so doing, it shapes power relations in organisations. In this more strategic-level environment, people's experiences of the effectiveness of organisational structures and systems are rarely as uniform as in the crew rostering or inventory control examples cited above. Neither are measures of organisational and organisational system/processes performance always as unambiguous. In cases such as these, people can construct very different realities and apply vastly different performance criteria. Forming judgments, for example, on what needs to be done to make the education or health system more effective, or on what needs to be done to enhance the performance of a business unit, are not comparable to research that is geared towards optimising the highly technical systems just mentioned.

In the corporate strategic environment, management scientists are often involved in designing, assessing and interpreting structural innovations and in examining multiple possible explanations of an organisation's present and of its future. To the extent that these explanations are not objectively determined, then the reality-experiences distinction

is very important. It follows that here the agent ought to take some responsibility for their knowledge production. This is even more the case if the agent's work is used to inform and support the strategy of one party against that of others.

Let us now switch the focus of attention from the practitioner of MS, to the writer about MS.

### 5.2.2 Epistemological implications: for the agent-writer

Despite the heavy technical orientation of much of the *MS* literature, there is now a substantial body of work that addresses the process of *MS* (see, for example: Boothroyd 1978, Tomlinson and Kiss 1984, Rosenhead 1989). There are also a number of texts that have a technical focus, but which are also sensitive to the broad social and organisational context within which *MS* interventions occur (see for example, Daellenbach 1994, Maani and Cavana 2000; Pidd 1996). In addition, there are now many case studies of specific interventions published either in texts or in scholarly journals.

Publications of this sort go some way towards demonstrating that the discipline of *MS* is not entirely defined by its models, nor its standing measured in terms of the sophistication of its technical weaponry. Undoubtedly there is, as Ormerod suggests, a need to *"coax practitioners with a good story to tell into the open"* (1998:428). However, operating in accordance with the *objectivity-in-parentheses* explanatory path suggests that the practitioner/writer ought to do more than simply speak about the process of *MS* and report on his/her practical interventions.

The same could be said from a 'plugging gaps' perspective. Although the literature is responsible for the technical and intellectual development of the discipline, it

also fulfills an important role clarification/socialisation function. By this I mean that it ought to convey a sense of some of the key challenges and obstacles that practitioners can expect to face in carrying out real world interventions. Measured against this criterion, literature that has an exclusively technical orientation fails to strike the right chord since technology is only useful if it appropriately harnessed in obtaining tangible results. As we shall see later, it is only a limited class of problems that management scientists deal with that do not have wider social and/or organisational ramifications that must be dealt with.

What then of existing accounts of *MS* practice? One observation is that the pivotal relational aspects of interventions, if they are considered at all, are frequently relegated to a secondary and/or supporting role in relation to the writer's primary objective which is to 'bring to live' the application of some technique and/or methodology. Indeed the cases (Brocklesby 1995, Brocklesby and Cummings 1996) that are the subject of discussion in Chapters 10 and 11 are deficient in that respect.

Cases such as these have an important role to play. Yet they fall a long way short in terms of providing the sort of content that satisfies the theoretical position outlined in the previous chapter, and, at the same time, that helps to sensitise management scientists to the broader personal, social, and organisational contexts within which their work is carried out.

Given the predominantly engineering/mathematical/scientific background of most management scientists (see for example, Munro and Mingers, 2000), it is hardly surprising that they tend to report their findings by adopting the accepted scientific convention of an observer-independent reality. Perhaps issues relating to the observing subject are regarded as being either irrelevant and/or potentially distracting for the reader.

This inattention to agency is manifest in many accounts of interventions that appear in the literature. Even if the author is not silent about his own role, it is rare to find any substantive discussion on the relational dynamics of the intervention as these evolve through the process.

How then, given the epistemology of AT, might MS interventions be reported? In simple terms there needs to be more literature written by management scientists in which they recount their experience of actually producing knowledge in a decision making/action context. The focus ought to be on how knowledge was constructed by a specified individual or individuals for the practical purpose of fulfilling a particular research brief. The focus then ought to be on how the people concerned lived the process of doing the research. In other words, there needs to be more literature that is written by management scientists telling of their diverse experiences of actually producing so-called 'knowledge'.

For the most part, MS is unremittingly practical. It is about down-to-earth, everyday and often mundane activities and actions and it is these aspects of the research process that AT implies that we need to know more about. To understand what the agent is about when they are plying their craft we have to understand something about them as individuals: they influence the 'findings' they produce and the reader has to be able to take that influence into account. A useful way of putting it is to say that there is a need for a reflexive style of writing which *'lets the audience see the puppet's strings as they watch the puppet show'* (Watson 1994:78). From the writer's perspective there is a need for an internal process of self-observation of what is going on with me, the agent, and me

as a member of a group of stakeholders who are involved in the inquiry process, at the same time as there is a focus on the substantive inquiry itself.

## 5.3 Using Autopoietic Theory to identify and plug specific gaps in the Management Science literature

> *"The test is whether OR expertise is called on by decision makers to help them address important issues . . .* (and) *. . . when called upon does OR provide solutions to the issues raised . . .* (and) *. . . are our clients prepared to attest to our contribution?"* (Ormerod 1998:426 emphases added)

This statement, about the role of the OR professional in the broader organisational context, identifies what I would argue is a 'stark reality' facing those who work in *MS* and related fields. This is that if management scientists are going to be useful, and hence used in organisations, then they have to be able to demonstrate to problem owners and decision makers that they can make a difference. This simple statement highlights two key areas that I submit are worthy of attention. Being able to demonstrate usefulness to clients draws attention to the critically important relationship between the agent and the client. Making a difference draws attention to the importance of implementing actions and obtaining positive outcomes. In simple terms, in order to provide solutions to issues raised the agent must ensure that he/she pays careful attention to the nature of his/her relationship with the key actors involved and, at the same time, ensure that there is effective implementation as well as technically competent and sound modeling.

In this next section I shall be arguing that some of the conceptual distinctions of *AT* can be employed in helping us to better understand and reflect upon these two non-technical but nevertheless important aspects of *MS*.

### 5.3.1 The relationship between the agent and other key stakeholders

In *MS* there are times when the relationship between the agent and client/problem-owner/decision maker ('client' hereafter) is relatively uncomplicated. If the client has clarity on the kind of expertise required, and if there is a service-provider who can demonstrate a relevant track record, then the relationships side of the intervention process may be quite straightforward. Problem situations that yield to the sort of *'smart bits'* expertise about which Ormerod speaks, fall into this category. In these regions of *MS*, the trick is to determine requirements and then to find someone who can fix the problem. It is not a case of the client needing someone to help in the formulation of the problem, or to assist and work with others in fixing it.

As we have seen however, the boundaries of *MS* extend beyond the relatively straightforward application of technical expertise to predefined problems. The 'usefulness' of management science and management scientists extends to helping managers deal with complex problems, in dealing with 'messes', and in operating as an adjunct to the decision making process at the strategic as well as at the operational level in organisations. It is in these areas of application that I submit there is a gap in *MS*'s existing knowledge base that *AT* can help to plug.

The gap in question was alluded to in Chapter 1. In that chapter I made reference to debates that have drawn attention to the underemployment of management scientists and the dwindling influence of the discipline on organisational action. Much of the blame for this problem has been put down to the existence of what might be referred to as a significant 'intellectual' gap between management scientists and managers. By some

accounts the size of this gap is rendering the discipline in a parlous state. At times commentators have even spoken of the discipline as being in a state of crisis.

Essentially the intellectual gap is between methodologies and situations. Managers, critics claim, look to management scientists for help in dealing with complex and messy situations. Yet much management science - so the argument goes – concentrates on developing and refining various techniques, often quantitative, which deal with a very limited number of specific and recurring problem situations. Many of these situations are unlikely to be replicated in real situations. Management science's commitment to a scientific approach to inquiry that is ill suited to non-laboratory situations where human and social factors are often paramount may compound the difficulty.

Within the discipline itself, the main response has been through innovations in methodology and technique. Hence over the last thirty years there have been major initiatives in methodology development and application in areas such as soft OR, soft Systems, and problem structuring methods. The result is that in theory, management science today is better placed to deal with complex problem situations and to fulfil a strategic role in organisations than ever before.

Paradoxically, by identifying other areas in which the management scientist might be useful to decision-makers, and by extending the discipline's area of jurisdiction, new task requirements and new knowledge gaps in the existing literature become evident. In the case of the soft paradigm for example, process and relationship management becomes critically important. Yet these kinds of skills are not unique to soft *MS*. In a recent survey, OR managers ranked good project management and managing clients

relationships skills as a critical success factors (Fildes and Ranyard 1998). In the same survey, OR managers apparently acknowledge the importance of developing people and organisational skills, yet admit that there is little training provided for developing these skills. More importantly – in the context of this discussion – the *MS* literature does not provide much by way of the conceptual tools that allow management scientists to think about these key issues.

The suggestion then, is that while technical proficiency is important, there is a corresponding need, beyond that which is acquired experientially, for management scientists to understand process, and to know how to facilitate effective relationships.

The extent to which a client will judge *MS* to be useful depends partly on his/her past interactions with, and experiences of, *MS* in general, and with the particular agent and methodologies/techniques being used in particular. Satisfying past experiences become conserved in the structure of the client with the result that he/she is likely to become predisposed to adopt a favourable view. However, since a typical *MS* intervention is a process and not a discrete event (see Bennett, 1990; Bennett and Cropper, 1990; Boothroyd, 1978; Lane and Oliva, 1994; Ormerod, 1996a; Mingers and Brocklesby, 1997), utility judgements are likely to be made as the intervention process unfolds. Not all clients will reserve judgement until tangible assessments of formal actions are available.

Maturana's description of what happens when there are recurrent interactions between two structure-determined systems is conceptually useful in furthering our understanding of these important interactions between the agent and client. Equally it is again worth recalling the maxim *"everthing said is said by an observer"*. Since a client's

utility judgement is something that 'is said' (if only to him/herself) the dynamics of the observing process, including the intrusion of emotion into this process, becomes highly relevant.

In the case of two structurally-coupled human beings it is in the relational space that exists between them that the all-important consensual domain is formed and where 'conversations' in particular, and observing more generally, occurs. Maturana and Varela describe periodic interactions among structure-determined systems as: "*...a history of recurrent interactions leading to the structural congruence between two (or more) systems*" (1987:75). In this context, structural congruence connotes the idea of an ongoing mutual co-adaptation without allusion to a transfer of some ephemeral force or information across the boundaries of the engaged systems. "*If the two plastic systems are organisms, the result of the ontogenic structural coupling is a consensual domain.*" (Maturana, 1975:326).

Let us assume that a consensual domain has not already been established on the basis of a pre-existing track record. The question is then raised as to what an agent can do to maximise the chances that the 'knowledge' that he/she produces will be given due attention through the decision making process such that the people concerned conclude that he/she 'makes a difference'.

If we assume that the agent has been called upon to assist in dealing with the situation in question then we can assume that there is already some element of coupling between the agent and the client. Thereafter a key issue concerns the dimensionality and quality of this coupling.

Obviously the dimensionality of the coupling and the number of interactions between the agent and the client will vary, partly depending upon past experiences and upon the nature of the project. If the client knows about *MS* and about particular techniques, and if there is a past record of successful application then we can assume that the number of interactions that are required will be less than would be the case otherwise. This will be even more the case in situations where the task is clearly defined and the agent has a good track record. However, here I am reminded of Pidd's (1988) remark that some element of novelty is present in almost all practical *MS*, for few studies are entirely routine.

At the other extreme, if the client is unfamiliar with *MS*; if there is no record of previous successes; and, if the agent is seeking to assist in dealing with a complex problem situation, then we might expect that a greater number and higher quality of interactions will be necessary in establishing the necessary coupling. This presents a strong theoretical case against agents taking a written brief and then seeking to deal with the situation using a written diagnostic statement and producing a set of recommendations. The minimal dimensionality of the coupling combined with infrequent interactions leaves the agent vulnerable to the client making unfavourable judgements depending upon his/her own conversational flow as these unfold through the duration of the intervention. Moreover there is a danger that the agent may fail to comprehend the client's emergent needs.

The logic of structural coupling is that every interaction counts, not just key ones at the beginning and end of the intervention process. Indeed it may be critically important to ensure that the client feels that he/she is being helped moment by moment. It is

difficult to see how the agent can ensure that this happens where there are only spasmodic interaction and/or where the quality of the structural coupling is poor.

This theoretical position probably also explains why, in recent years, the consulting industry has tended to emphasise the need for long standing relationships. This has become a important issue in *MS* as it has become less of an in-house activity and more something that is outsourced. Theoretically where longstanding relationships do occur, and where there is an ongoing sequence of quality interactions, we can surmise that there is a gradual process in which the two parties become structurally-coupled with one another; each becoming more and more aware of the other's requirements. Theoretically, structural coupling connotes a shift of emphasis from 'information transfer', which of course is impossible for a structure-determined system, to a process of simultaneous adjustment.

Although recurrent interactions are the basis for developing a strong structural coupling between the agent and client, there are other things that the former can do to improve his/her chances of being seen to be useful. One strategy is to become what Dell (1982, 1985) and others have referred to as a *'scholar of structure-determinism'*. Roughly translated, this means that the agent must have some notion of how, in the relevant context – the relevant people think and what their priorities are.

This is both an individual issue and a social issue. Individuals construct their worlds in distinctive ways depending upon their backgrounds, and a similar process is at work with organisations and their various subunits. Organisational systems, like families and other social units, also have their own distinctive languages and cultures, and in that respect are just as much 'closed' and subject to their own structure-determinism as

individuals. For any external agent who is seeking to have impact penetrating these linguistic and cultural barriers can create serious difficulties.

In practical terms the agent must try to infer, from what they can observe and from what they hear, what the clients' concepts are and how they operate with them. Listening to the language these people use and observing their behaviour is important because it reveals their structures and the various worlds that they bring forth. Only on the basis of some such hypothesis can the agent then devise ways and means of ensuring that his/her contribution towards dealing with the problem is appropriately packaged (see discussion below and in Chapters 9 and 11, on *'orthogonal interactions'*) and actually used. We can surmise that this involves a number of things: sensing needs – even those needs which the client cannot verbalise; communicating the benefits which accrue from the use of *MS* techniques, establishing relevance; and not least, convincing the client that benefits can be delivered in timely fashion. All of which is much more feasible if the agent can combine high quality interactions with a good knowledge of what are the client's favoured distinctions and emotional predispositions.

The distinctions that both parties use are particularly important. Since the principle of structure-determinism disavows the possibility of information transfer from the agent to the client it is important, where possible, that the agent employ the terminology of the client. If the client is involved through the process then the effectiveness of the conversation between the agent and the client depends upon a common set of distinctions being used. On this theoretical view it is important that the agent give thought to how he/she presents the content of specialist methodologies and techniques to clients through the intervention. The client may need to be as structurally-

coupled to the proposed methodology/technique as he/she is to the agent, and to *MS* in general. Again this depends on the client's previous experiences. However since methods and techniques are basically specialised patterns of distinctions and coordinations of behaviour it is important that the client is comfortable with these and can see their value in the context of the intervention. In many cases the agent needs to be seen as a normal human being not as separate expert with special tools.

Beyond this, as I suggested earlier, the agent needs to acknowledge that utility judgements can occur through the whole intervention process. *AT* suggests that to the extent that 'problems' are not fixed but emerge in the domain of the observer, it would be unwise for the agent to automatically assume that a problem has been 'nailed down' and that he/she can then simply then go away and get on with it. Obviously seeking clarity on the nature of a problem at the beginning of the process is important, but so too is retaining an open-mindedness about this. Again if the agent can establish the sort of multidimensional/multi-interaction coupling that I am suggesting then there is a gradual 'coming together' of the various parties. Each one gradually learns to understand the other, and expectations and requirements become clearer.

To the extent that this happens, it avoids an agent projecting onto the client unrealistic and/or inappropriate ideas about what is and is not possible and about outcomes. If client expectations are 'realistic' (in these terms), then it is less likely that there will be disappointment when results fail to meet expectations. If the agent and the client are only very loosely coupled or if there is only a minimal level of 'dimensionality', and/or infrequent interactions, then there is a strong possibility that the client may develop expectations that the agent cannot realistically satisfy. Alternatively,

there is a possibility that the agent commits what some would claim to be the cardinal sin of *MS*; namely finding the best possible way of doing the wrong thing.

Finally, the agent needs to understand that it is highly likely that each of the parties who are involved in the intervention may be participating in any number of conversations about the intervention, and in other conversations that impact on it through the intersection of these conversations through their bodyhood. This means that at no time ought the agent be complacent in relation to his/her understanding of the position of the client.

In summary, there are numerous possible reasons why a potential client might decide to not call on the services of a management scientist in dealing with a problem in situations where *MS* has the potential to assist. Equally there are many reasons why a decision maker might choose to not act on the recommendations of a management scientist, or ignore the predictions made by a model or the recommendations of a report. While many of these reasons are beyond the control of the individual agent, to some extent the agent can take action to improve the possibility that the client will make positive utility judgements. In this context the value of *AT* lies in its ability to provide theoretically relevant concepts that, beyond skills acquired experientially, allow the agent to reflect upon the importance of these key process and relationship management issues.

### 5.3.2 Successful implementation: taking action and achieving positive organisational outcomes.

To the uninitiated, a cursory glance at some of the dominant paradigm literature might indicate that the *MS* discipline is defined purely by its modeling techniques and

methodologies. And to some extent it is. As I have already argued, the activity of modeling does convey a sense of what is distinctive about *MS*. At the same time it would be wrong to lose sight of the reason for doing *MS* in the first place. *MS* is, and always has been, about facilitating taking action in support of organisational outcomes. Thus, speaking of British OR, Ormerod (1998:420) comments that practice has always been considered to be the *"touchstone of the profession . . . "*. And Tomlinson reflecting on many years of practice, remarks that

> *" . . . unless change actually took place, the whole OR exercise – so far as the decision maker was concerned – was pointless. Like it or not, the objective of the OR practitioner was to be a change agent or catalyst within the organisation."* (Tomlinson, 1998:404)

Although *MS* is fundamentally a practical activity, the issues that are involved through the process of implementing changes that result from *MS* analysis do not always get the attention they deserve in the literature. Pidd (1988:115), for example, chastises management scientist writers for not giving as much attention to the implementation of their work as they do to the technical aspects that underlie their recommendations.

In recent years there has been increasing recognition that social, organisational and political skills are critical to the success of *MS* practice (Jackson, Cropper, Keys 1989), and ideas have been mooted about how to facilitate implementation (see, for example, Checkland, 1981:238-240); Checkland and Scholes, 1990:290).

Yet relative to its practical importance, it would be hard to claim that the *MS* literature itself contains an adequate theoretical account of the implementation process and of the difficulties that implementation can present. From an educational/training angle the *MS* literature itself has relatively little to say on the difficulties that an agent can expect to have to deal with (and how these might be overcome) that might complement

the knowledge and skills that are otherwise acquired through direct practical experience or by studying other disciplines such as organisational behaviour and human resource management.

In understanding the nature and magnitude of the difficulties involved in implementing a decision to act in a particular way and achieving the desired results, it helps to think about what are the different targets of interventions that take place in the name of *MS*.

To this end, there are a number of useful classification schemes. Checkland (1981:180), for example, argues that changes of three kinds are possible: changes to *structures*, to *procedures*, and to *attitudes*. His *natural*, *designed*, and *human activity* classification of system 'types' is also useful. Elsewhere, borrowing an analytic framework from Habermas that delineates three conceptually different 'worlds' – the *'material'*, the *'personal'*, and the *'social'*, Mingers and Brocklesby 1997, and Mingers 1997, employ this as one way of looking at the relative strengths of different *MS* methodologies. Under this schema interventions in the material world include designing new physical systems or modifying existing ones. Interventions in the personal world aim to alter people's individual thoughts and feelings. Interventions in the social world target shared perceptions, systems of meaning and work practices.

Using specialised methodologies or combinations of methodologies the management scientist can target any of these worlds. However, Mingers and Brocklesby also point out that the problem situations that management scientists are called upon to deal with are often, if not always, multidimensional. This, for example, means that the introduction of a new material system will have ramifications beyond the immediate

context of its application. If positive outcomes are to be forthcoming, commensurate changes may need to take place in the social and personal worlds of the people who are required to interact with the new system. Thus there may be difficulties if only a limited view of the problem situation is taken, for example by focusing only on variables that are easily measured or quantified. This has not gone unrecognised by management scientists such as Checkland who notes that, since structural and procedural change always takes place in a culture, such interventions are likely to succeed only when they are perceived as meaningful within that culture and within its worldview. The main point is that while it is possible to conceptually separate material, personal and social aspects of interventions, in practical terms these dimensions are inextricably intertwined.

Much of the problem revolves around the observation that while *MS* necessarily deals with different kinds of systems, they nevertheless, as Kirby and Capey (1998:324) remark, in general all 'contain' human beings. And it is the potential for clashes and mismatches between the 'solutions' offered by the agent and the prevailing culture(s) and structure of the organisation that creates difficulties. It is in this context that the question arises as to whether *MS* has adequate models/theories/language for dealing with the problem, and if they have not, then where might management scientists look. What does seem clear however is that at a practical level a client is unlikely to see *MS* as making a difference if the innovative solutions that it offers clash with the prevailing culture or if the culture prevents these from spreading more widely.

Bringing about change then is not merely a matter of deciding to do that. There needs to be a recognition of how difficult it is to change patterns of behaviour and thought, particularly those that have worked in the past. At the discipline level this

means, as Daellenbach and Read (1998:433) have recently pointed out, that *MS* needs to come to grips with, and interact constructively with issues of organisational structure and managerial culture

Given what has just been said, it is important that both management scientists and decision makers have an adequate theory of change/implementation that applies to social and personal as well as to material change. While it is important that the agent carries exemplary technical analyses of problems, more often than not clients are looking to achieve tangible results. From their point of view, analysis for its own sake is unlikely to be very appealing; there is a need for results. If these are not forthcoming, if *MS* is not seen to be 'making a difference', then the faith that these key actors have in *MS* may be seriously compromised.

Before we come to what might be extracted out of *AT* in relation to the matter of intervening in human and social systems it is important to issue an important caveat. Since the whole *AT* edifice is built on the idea that all realities in the multiverse of realities are equally legitimate, the 'passion' to change others, as Birch (1991:371) notes, can only be misconceived.

On this view it is hard to imagine how *AT* could possibly endorse, let alone facilitate interventions that occur in the name of a particular reality. Since such interventions are the stock-in-trade of *MS*, this presents major difficulties for anyone who might raise the possibility that *AT* can provide practical assistance to management scientists. It is even more problematic for anyone who might offer the suggestion, tentatively examined in this thesis, that *AT* might provide a philosophical underpinning for the conduct of *MS*. This is flagged as an issue that requires much more detailed

consideration than is possible here. For now I shall skirt the ethical issues involved and simply concentrate on examining how *AT* might assist a management scientist in bringing about the tangible changes that the decision maker is seeking to achieve.

When looking at what implementation-related knowledge might be extracted out of Maturana's work, the balance tends to favour saying more about what change agents cannot do and what is not possible, than about what they can do and what is possible. (See, for example, Dell 1982:40). Yet this is not overly problematic since knowing what not to do is better than having no guidelines at all.

At the outset, the degree of difficulty in orchestrating personal and social change will depend upon the size and magnitude of the gap between the existing situation and that which is required to accommodate and/or sustain the desired system or behaviours. That having been said, the main lesson – which has a number of different aspects - is that the change agents need to fully appreciate the difficulties that are involved in orchestrating personal and social change. In *MS*, as Beer (1980:72) points out, this often goes unrecognised. According to Beer, architects of change often recognise that the system at their own level of recursion is autonomous but insist on treating the systems their system contains in functional terms. To the extent that such systems are populated by autonomous human beings, this is clearly an ill-advised strategy.

Given the 'multiple legitimate viewpoints' position that we have come to associate with soft *MS* and the 'sweeping in of different views' philosophy that is characteristic of much critical systems thinking, it is highly unlikely that practitioners who align themselves with these paradigms would fall into the trap that Beer describes. Doubtless the same can be said of experienced management scientists from the dominant

paradigm who have a history of implementation successes. To that extent the commentary below is targeted at the rest of the *MS* community.

From an *AT* perspective, the crux of the change/implementation problem begins with the idea of structure-determinism and its disavowal of the possibility of linear cause-effect explanations of change. To argue that, without hindrance, a decision-maker can bring about a particular outcome in another complex system, i.e. as an external perturbation can determine their own effect inside a system, violates the principle of structure-determinism. Such thinking, according to Maturana, perpetuates what he refers to as the myth of *instructive interaction* (see Maturana 1988b:9, Goolishian and Winderman 1988:132).

In further unpacking this idea, it is again helpful to restate that in foreshadowing possible implementation difficulties, the agent is dealing with not one but two phenomenal domains. Consideration of the biological domain reminds us that the proposed change must be one that can be accommodated by the structures of the people concerned, as these have been historically conditioned by past interactions. In simple terms this confirms what many management scientists already know, namely that there is a need for machinery, equipment, software, and systems more generally, to be designed with the physiological and cognitive capabilities of the user/operator in mind. It further reflects the idea that people's spontaneous and/or sub-conscious responses to change initiatives ultimately have to do with their prevailing structure. I shall return to this point shortly.

While it is important that consideration be given to the biology-related response of people to new systems, consideration of the relational domain is no less important. Indeed it may be more so.

Recall that *AT* reminds us that the distinctiveness of human beings has rather more to do with the way in which they interact with one another and with how they go about explaining their experiences and acting on these, than it does with structural characteristics. As systemic entities, human beings exist in a relational space under continuous structural change. Here meanings and understandings evolve as people go about their daily business doing things and interacting with others. These are sustained by on-going conversations and concrete day-to-day routines (coordinations of coordinations of behaviour) that are what they are because they 'work' in the context in which they arise. It logically follows that change agents and implementation teams must grapple with the flow of meanings that emerge out of 'grass roots' daily interactions. The flow of meanings is not from those who are 'managing' and attempting to control the process.

All of this has some important implications. Firstly it says something about intervention models that prepare the ground for substantive change by first seeking to persuade the targets of change that their existing understandings are inappropriate under current or foreshadowed circumstances (see Gibb-Dyer, 1985; Tunstall, 1985; Wilkins and Patterson 1985). The aim here is to appeal to people's sensibilities by convincing them that acquiring new understandings is an important prerequisite for ongoing organisational viability.

Of course this is much easier said than done. Getting members of a target audience to reflect critically on their existing understandings let alone getting them to

voluntarily adopt new ones is beset with difficulties. Maturana reminds us that for much of the time we human beings – operating in accordance with the objectivity-without-parentheses explanatory path - believe that we see the world 'as it is'. Further, we grow into the language of our consensual domains gradually so it is often difficult for us to see how our rationalities reflect anything other than 'the way things are'. Observing naively; that is, separating the influence of idiosyncratic language from that which is seen, is very difficult once language, words and symbols become an integral part of our experience. Thus, *"we do not see what we do not see, and what we do not see does not* (for us at least) *exist."* (Maturana and Varela 1987:242).

When people's understandings are sustained by entrenched interpersonal conversations and day-to-day actions the likelihood that they will seriously question these may be somewhat remote. Like the 'sandboxes' in which children play their games (Efran et al. 1990:45), each domain is operationally closed. Each one has its own boundaries, its vocabularies, and its grammars of interaction, and our normal mode of bonding with it is tacit and unreflective. Thus, says Maturana, we cannot ". . . *observe a cognitive domain by operating in it"* (1988a:61). So even when we are inclined to reflect on our own habitual understandings it is hard to stop our interpretations being grounded in, and constrained by, the very rationality which is being reflected upon.

Secondly Maturana's assertion that emotion is <u>the</u> central aspect in a culture raises difficulties when it comes to putting in place an implementation strategy and bringing about substantive change. If the aim is to bring about personal and/or social change it needs to be borne in mind that people's rationalities". . . (are) *grounded on basic premises adopted through our emotioning. . . that we live . . . as manners of existence . . .*

*as all-embracing manners of being . .* " (Maturana 1988a:62). Rational domains then are emotionally grounded.

This means that if the aim is to engineer a switch from one rationality to another then there is a need to acknowledge that an emotional conversion is required. Further it means that the degree of legitimacy that the people concerned attach to the proposed change depends upon the size and strength of the 'emotional gap' between the existing situation and that which is proposed. In simple terms the point is that people's emotions colour how they respond. Their rational arguments in favour of or against a proposed change are secondary to their emotions; they are not primary.

Under such circumstances the implementation team may believe in the cogency of a particular problem-solution set and argue the case for change rationally. They may then be perplexed as to why others are less enthusiastic. But such a response ought not to be surprising. Problems have to do with emotions not circumstances. For this reason it would be ill advised for the team to assume that the target audience will even acknowledge that there is a problem, let alone – at an emotional level - buy into its solution. Unless, because of their emotional predispositions - members of the target audience are having conversations about a problem then to all intents and purposes it does not exist.

Even more fundamentally, how the targets of change respond to rational argumentation has to do with <u>their</u> experiences, not those of the decision maker or change agent. The perceived legitimacy of any action is relative to the explanatory domain that is brought forth in the domain of descriptions as the various parties explain their experiences. What counts in forming judgements about project proposals and/or plans,

are the experiences and the deduced experiences of the observer. If through his/her actual and/or deduced experiences the individual concerned considers that the proposal lacks credibility, is unrealizable, or in some way threatening, it is unlikely that he/she will voluntarily commit to it.

Moreover, as we have seen, in organisations these kinds of assessments are inherently social. People explain their experiences, they make deductions and develop ideas about what is rational and what is a legitimate action collectively i.e. in networks of structural coupling. This places responsibility on the change agent to assess not only the strength of people's emotional attachments to pre-existing rationalities, but also the strength and durability of the conversations that nourish and sustain these. And, since *AT* reminds us that the operational mechanism through which people 'understand' their worlds, i.e. language, is grounded in concrete day-to-day coordinations of behaviours, routines and 'doings', it is equally important to assess the extent to which daily routines, practices and organisational structures that reflect dominant conversations will support or frustrate the envisioned change.

> *'Change in any particular social or non-social human community takes place as conversational change; that is, as a change in the configuration of the network of coordinations of actions and emotions that constitutes it and defines its class identity . . ."* (Maturana, 1988a:72)

These daily practices arise naturally in social settings where they are an integral part of the coherences of the situation. In such a context they are what they are because they work; they are viable. It follows that any proposal that involves changing (directly or indirectly) people's understandings and behaviours must be assessed in relation to the coherences of the situation in which it is proposed that they be introduced. When

people's understandings are embedded in organisational practices they cannot simply step outside of these by going along with some new preferred viewpoint or belief system.

Finally, the inextricable inter-linking of people's rationalities with spontaneously emerging day to day practices reinforces the point made earlier that change agents cannot be presumptuous about where they draw the boundaries around the situation under consideration. There are sections of the literature – SSM most notably - that do advocate attempting to foreshadow likely cultural difficulties in advance of taking action. Here there is a sense that there is 'a situation' that can be characterised using culture-relevant concepts such as roles, norms and values. Aside from the question of whether these conceptual tools are adequate beyond their application 'on the hoof' (Checkland and Scholes 1990:49) during an intervention, there is a sense that there is a situation that has its own set of roles, norms and values that can, to useful effect, be analysed by the change team.

To some extent going through this process is prudent. The problem is that cultural boundaries do not reveal themselves that easily. Certainly we are not speaking of concrete organisational boundaries. People are members of a social system not because of their presence within physical boundaries, but because they operate under the emotion of mutual acceptance in relation to the network of coordinations of behaviours that realises it (Maturana 1998:64). Cultural boundaries then are emotional not physical. In other words they are circumscribed by the conversations in which these emotional predispositions are conserved not by a physical boundary.

In short, persuasion and direct instructions are always going to be highly capricious ways of promoting desired personal and social change. Attempting to

deliberately change people's understandings is difficult enough when they are directly involved in and are structurally-coupled to the process of debate that leads to recommendations for action. When they are not - and clearly there are limits as to how many people can be directly involved – it presents an even bigger challenge. In daily life we know that people do not always change their opinions when teachers, parents, managers, politicians and the like present them with sound logical reasons why they should. So where members of the target system are sufficiently autonomous that if it does not suit they can reject advice, guidance, or new 'knowledge', or if they can simply ignore or circumvent any material changes that are being introduced, the difficulty is exacerbated.

All of this is, as I intimated earlier, is in the fashion of pointing out what ought not to be attempted, and what is generally not possible. But there is another side to all of this. In bringing the nature of the difficulties of implementing change into clearer focus, *AT* does open up the possibility that the agent and/or the implementation team – providing that they can deal with the ethical issues involved - can take steps to maximise the chances that there will be positive outcomes.

In relation to the matter of implementing chosen designs in autonomous systems, Maturana (1997:2 part 3) claims that " . . . *if the design respects the structural coherences of the domain in which it takes place . . . the way a system responds to an environmental perturbation can be highly predictable*". Again we are back with the idea of the agent exploiting the idea of 'being a scholar of structure-determinism'. In theory this allows the agent to develop a communications strategy that is based on an

understanding of what messages will act as triggers and which have a higher probability of leading to favoured outcomes than would be the case otherwise.

If, as is frequently the case, it is necessary to present a rational argument in support of some preferred outcome, then such knowledge allows the agent to frame messages in terms of what the target audience is trying to conserve. In other words, the change can be appropriately 'packaged' taking into account the emotional basis of people's rationalities.

In this regard, agents who appreciate the culpability of language are in a much better position than those who do not, since language is a key instrument in eliciting structural change. If the objective is to lead people in the direction of a new understanding, or if the intention is to change behaviour directly, change agents must have some notion of how the target audience thinks. As I said earlier in relation to the agent optimally positioning himself/herself in relation to the client, it is only on the basis of some such hypothesis, in this case of the target audience's structure, that the change team can devise ways and means of bringing about the desired change. It goes without saying however, that since change agents are subject to their own structure-determinism, models of the target audience's structure can only ever be hypothetical.

Despite this, it is difficult to imagine how a change agent who does not construct a provisional and tentative model of the target audience's structure, and does not frame messages accordingly can expect to bring about the desired understandings. Ideally the model is constructed on the basis of direct interactions with the people concerned. In this regard starting out with preconceived, and untested models of the target audience's structure, may be worse than having no model at all. This is the scenario that Beer

complains of, namely operating on the assumption that a target audience will simply go along with what is proposed because it is merely a functional component of the higher level system in which the agent and the implementation team is operating.

While the concept of structure-determinism suggests that it makes good sense to attempt to foreshadow how people might respond to change initiatives, structural coupling suggests a different but complementary strategy. Efren et al. highlight the difficulty of relying only on the former.

> *". . . . crude guesses can be made about how people will respond to a particular situation. These are just hunches, and in particular cases they will prove to be way off base. However one has to begin somewhere. Structure-determinism reminds us to remain experimentalists. We must be "two-headed" – we plan and predict but are not surprised when predictions go awry and plans need to be updated . . ."* (Efran et al. 1990:196)

In being open to the possibility that how target systems respond to an external perturbation is never going to be entirely predictable, the key lesson of structural coupling is that the adequacy of the 'fit' between the proposed change and the target audience needs to be tested behaviourally/operationally as well as assessed intellectually.

This notion provides theoretical support for the so-called *'spiral'* theory of change that is well known in the area of software design. Typically the spiral model is contrasted with the *'waterfall'* model which typically follows the 'prescription, determine requirements, determine alternatives, determine the tradeoffs, design, test to make sure it meets the design requirements . . . etc.'.

The spiral model is fundamentally different in that it advocates what one could refer to as a process of ongoing 'reality testing' of the proposed design through the intervention. Essentially this involves building a little, let users work with it a little, build some more, work with it some more, etc. The same logic, although not argued from a

structural coupling perspective, is replicated in Checkland's SSM. By conceptualising the process of intervention as an in principle never ending process of reflection (learning), taking action, further reflection, further action etc. the 'intervention' process is extended to encompass the ongoing management of the situation under investigation. This is clearly compatible with that aspect of structure-determinism that suggests that how a system responds to an external perturbation is not always going to be predictable. The cycle of action-reflection-further action mirrors the process through which there is a gradual structural coupling between the system and those who are require to operate or work with it.

Explained slightly differently, it could be said that much of the unpredictability of analytical/intellectual assessments of the nature of another system's structure is because these assessments take place in the domain of descriptions of the observer, i.e. they can only be educated guesswork. A clearer picture is possible by assessing the structure operationally, that is by interacting with it. In this sense the notion of structural coupling provides theoretical support for what virtually ever management scientist will already know, namely that direct experience is a very good guide as to what will and what will not work. At the same time this is a strong argument against the idea that analysis ought to be complete first before the intervention takes place.

Another aspect of this stage-by-stage approach to intervening is that when a target audience does not respond positively to a particular proposal, it does not automatically follow that they will act similarly in the future. Even a hostile reaction demonstrates that the message has been heard. Should a similar message be delivered at a later date, it will

act on a different structure. It is wrong therefore to assume that events that fail to trigger an expected response at a particular point in time will not ultimately be successful.

Let us now return to the problem of external agents having to confront and find ways of circumventing the effect of 'naturally occurring' forces and trends. Again the position is not quite as hopeless as the earlier discussion might have intimated. A useful analogy for what can be done is that which happens when a gardener prunes trees. Trees are naturally growing living systems that operate according to their own structure-determinism in various networks of structural coupling with the soil, the wind, rain, sun, atmospheric conditions and so on. Yet with intelligent feeding, pruning, and watering, trees can be shaped to suit the preference of the gardener. Translated into organisational terms this means that instead of trying to change people directly, the emphasis may need to be on designing an environment where, as Efran and Lukens (1985:23) put it, people *"thrive, respond, and change themselves"* .

Citing Disney World as an example, Efren and Lukens suggest that people need to be provided with opportunities to have new experiences which fit their structure yet which can still, at least to some extent, satisfy the objectives being pursued by the change agents. Disney World, these authors claim, realise that people want new experiences, that they want to be surprised and confronted, but only in certain ways; they want to be 'safe' and they will only respond to what fits with their structure.

Schein (2000:37) puts it slightly differently. He claims that there is a need to reduce learning anxiety by creating what he refers to as a *'psychological safety'* for the learner. In similar vein, Kenny and Gardner (1988:5) remark that the key problem for the agent is to prevent the target audience becoming so disconfirmed that they seem to lose

membership of the *'consensuality club'*. Often this can be achieved by, where possible, remaining firm on the goal, but giving plenty of participatory power to the learner on the means.

For the change agent/implementation team, the message would appear to be that changes (i.e. 'legitimate' actions) arising out of explanations that are substantially different to those that currently operate for the target audience, are unlikely to meet with the approval of the people concerned. Because, " . . . *the observer constitutively cannot make distinctions outside the domain of operational coherences of his or her praxis of living*" (Maturana 1988b:59), people cannot just step outside of their existence and commit themselves to some new understanding that is alien to their daily life existence. Hence:

> *"Disney opens the gates of his creation to the members of the public but does not drag people in nor make them like what they see. - Disney put together some attractions he thought people might like, but it is the consumer who either buys or decides to shop elsewhere."* (Efran and Lukens, 1985:72)

If the Disney experience has wider applicability it suggests that while changes can be packaged as an attraction that may appeal to people, ultimately they make up their own minds about what they will and will not accept. If it is a case of the latter the implementation team then must consider the benefits and costs – economic, organisational, and ethical – of imposing change on a recalcitrant target audience.

Finally, on the matter of the operational closure of cognitive domains and people's natural disinclination to voluntarily contemplate alternatives, again all is not lost. Providing the target audience's structure allows it and providing information is presented as an <u>invitation</u> to enter a new domain and not as a demand for <u>obedience</u>, then people can be gently moved in a new direction. Maturana makes clear (1988b:33) that

people do have the capacity to attend to the context in which their experience is embedded and that they can expand or change their cognitive domains. Indeed he points out that this happens naturally in daily life when someone is suddenly relocated to a different cultural environment (Maturana and Varela 1987:242), or when he/she faces a predicament of some sort.

Change agents can precipitate the latter through what Maturana describes as a *'significant orthogonal interaction'*. This happens when the agent, as speaker, chooses a statement or interaction/intervention that is outside the domain of conversations that defines the existing system, but which takes place in the domain of existence of the recipient (see Chapters 9 and 11 for further discussion on this). Again the issue of emotion and preferences arises here. Because " . . . *the emotional dynamics of co-existence . . . goes through seduction, not through obedience*" (1988b:77), ways have to be found that will involve people wanting to make the change or wanting to accommodate it, the emotional shift being as important if not more so than the rational shift.

Summarising the previous section, we can say that the terminology and logic of *AT* provides useful information that suggests how a change/implementation team can act to maximise the chances that there will positive outcomes. While in recent years increasing recognition has been given to the importance of the tangible and less tangible aspects of planning an implementation strategy, it is a moot point as to whether the existing *MS* literature is capable of providing the language and conceptual tools that are necessary. I submit that *AT* has much to offer in this regard.

What *AT* shows however, is that in the final analysis implementation outcomes will always be uncertain. Even if, for example, members of the target audience can be persuaded that there is logic and value to what is proposed, there is the question of the extent to which the target audience is able to make the necessary changes. Convincing people that a new understanding or behaviour makes sense is one thing but there is more to it than that. New rules have to be inculcated, and new skills, new daily practices, and new emotions often have to be learnt. Because the coherences of any rational domain are woven so completely and unobtrusively into the fabric of everyday life, and because the conversations sustaining the old understandings may still exist, leaving one and moving into another can be notoriously difficult even if the will to do so is there.

And ultimately what happens to people in any moment depends on their structure. Activities that take place in language such as requests and agreements to change, are only one among an infinitely large number of external and internal perturbations. Thus Maturana claims that the trajectory of life is basically a purposeless drift in which people constantly change according to their structural determinism in response to various internal and external perturbations as they drift naturally through a medium.

Even if a sincere commitment to adopt a new desired understanding is made, and this leads to structural change, the outcome is still uncertain. The structural change involved in replacing one set of understandings with another is but one moment in an on-going history of structural couplings, hence it would be rather naive to believe that a new coupling completely supersedes that which preceded it. There will always be doubts about whether someone who has been thoroughly socialised in the understandings that pertain to one domain can prevent their unwelcome intrusion into another.

Finally, this line of thinking suggests that the change team should always retain a sense of scepticism about the relevance and appropriateness of its plans. Recall that the plans of the implementation team arise out of reformulated explanations of experiences that, to a large extent, pertain to the specific conversation in which they arise. And, as Maturana and Bunnell, have recently put it:

> *" . . . people's plans are always embedded in a domain that is much, much larger than the domain in which they have their understanding. For this reason they must be open for failure. And when we fail it is only a failure with respect to the desires of the people involved in the exercise, not a failure in the target system."* (Maturana and Bunnell: 1998:30)

On this view, the idea of implementation failure does not come into it; the target system continues to operate 'properly' according to its structure-determinism and according to its own coherences. Neither, in the event of 'failure', would it make sense to speak of resistance because as Dell (1982:40) remarks, " . . . *people do not resist one another. They simply continue to be who they are . . . to claim that* (there is resistance) . . . *is a very egocentric point of view*". While this is an interesting theoretical point it is unlikely to provide much comfort to an implementation team that, having invested heavily in a change programme, see their efforts nullified by prevailing circumstances.

## 5.4 Summary

In seeking to develop *MS* in a manner that is consistent with the epistemology of AT, this chapter began by looking at what *'putting AT into practice'* would mean for the individual agent-practitioner. Focusing on the proposition that *MS* is essentially a process of choice-making, I argued that this implies a form of critically reflective practice that takes account of how the structural predispositions of the people concerned, and the

nature and flow of the relational circumstances, impact upon key outcomes. Focusing on how discarding notions of (transcendental) objectivity brings into focus the essentially ethical basis of *MS*, the chapter then examined aspects of practice that are made more difficult and aspects that are simplified as a result of this epistemological shift.

The second aspect of putting the epistemology of *AT* into practice focused on the implied switch from *MS* as a knowledge discovery activity to *MS* as a knowledge production activity and what this might mean for the reporting of practice through the literature. Specifically I argued that it implies a style of reporting that concentrates as much on the process through which choices and decisions are made as it does on the substantive intervention itself.

Moving beyond the purely epistemological aspects of *AT*, and in relation to the need for *MS* to have impact or 'make a difference' in organisational settings, the second part of the chapter has focused specifically on two key gaps in the literature. The first of these draws attention to the pivotal nature of the relationship between the agent and other key stakeholders, the second to the problem of implementation and the need to obtain tangible outcomes. In both cases the concepts and language of *AT* have been used to show what contribution it can make in relation to a better understanding of these process, and, particularly in the case of bringing about substantive organisational change, to highlight the various obstacles that change agents are up against.

# PART THREE – VIABLE SYSTEMS

Having employed *AT* as a lens to examine *MS* as a general class of activity, the focus now shifts down a level. Parts 3 and 4 (Chapters 6-9) focus on two of the better-known *MS* methodologies: Stafford Beer's *Viable Systems Diagnosis* (VSD) approach, and Peter Checkland's *Soft Systems Methodology*. These next two chapters examine the first of these.

## 6. VIABLE SYSTEMS: A CRITICAL PERSPECTIVE

### 6.1 Introduction

Initially, following its positioning in Jackson and Keys' (1984) *'System of Systems Methodologies'*, the Viable Systems approach was chosen as an example of the 'hard' systems approach to *MS*, although now, knowing more about the Viable Systems Model (VSM) and about how people use it, I am less sure about where it is best located. To some extent there are aspects of viable systems interventions that embody key aspects of hard systems thinking (see Checkland, 1981:xiii). For example, as we shall see it has a strong engineering focus. The distinctive competence of hard systems methods is in helping decision-makers in designing a system that will meet stated objectives. In other words there is a heavy emphasis on answering the sort of 'how' question to which engineers turn their attention. This focus comes through very strongly in a number of descriptions of the VSM and accounts of its application (see, for example, Cummings and Brocklesby 1993, Brocklesby and Cummings 1996, Flood and Jackson 1991, Flood and Zambuni 1990).

Having said that, there are a number of prominent VSM researchers who tend to use the model in support of what is arguably a much 'softer' position. Here, used in a 'conversational' format as much emphasis might be placed on debating what should be done as on debating the how question. This approach reflects a more general shift in cybernetics away from biological and mechanistic analogies, and – more importantly – a shift away from the application of methodologies in a supposedly neutral objective domain, towards approaches that acknowledge the subject-dependency of the domains under study. This shift, partly promoted by Beer himself, has been strongly articulated by second order cybernetics scholars such as Clemson (1984), Espejo (1987, 1989a,b), and Espejo and Harnden (1989a).

The main focus of this chapter is on the VSM. I leave comment on the methodological aspects of the VSM until the next. In what follows, I begin with a brief introduction to the theory. This is followed by a commentary on the use of the term autopoiesis in the theory of viable systems. Here I take issue with the suggestion that the terms autopoiesis and viable system are virtually interchangeable. Despite this, in the final section, I examine the possibility that it might be possible to exploit complementarities between the two approaches when used in an intervention mode.

## 6.2 Stafford Beer and the Theory of Viable Systems

Stafford Beer's project was to discover the laws of system viability by which he means the ability of a system to maintain an independent existence (1979:113). Through studying the way in which the nervous system organizes and controls the human body – a *known to be viable system* – Beer arrives at a set of laws that led him to claim govern the functioning of all viable systems. His main account draws mainly on the example of the human being. However in the second edition of *'Brain of the Firm'* he claims that he could have reached the same conclusions had he chosen to employ as a model other

viable systems including single biological cells such as the amoeba, whole animal species, or complex social systems such as enterprises and cultures. The results are the same, he says, " . . . *as they must be if viability as such has its own laws and enshrines its own principles* . . . " (1981:75).

Among his major works *'Decision and Control'* (1966) outlines the methodological procedures used by Beer in fulfilling his project. *'Brain of the Firm'* (1972, 1981) sets out a neurocybernetic model of the workings of the human body and nervous system, and *'The Heart of Enterprise'* (1979), builds the model of systems viability from cybernetic first principles. Finally, *'Diagnosing the System for Organizations'* (1985) presents these ideas in the form of a diagnostic/methodological tool that is designed to assist people involved in organisational analysis and design in understanding the logical coherences of the model (see Jackson 1989b for a detailed synthesis of this literature, and see Flood and Jackson 1991 for a potted description of the VSD methodology).

Looking at the structure of the VSM, Beer found that all organisms displaying viability shared five basic properties. They all exhibited, in some shape or form: autonomous *'operational elements'* that directly interface with the external environment, that enact the identity of the system; *'co-ordination'* functions, that ensure that the operational elements work harmoniously; *'control'* activities, that maintain and allocate resources to the operational elements; *'intelligence'* functions, that consider the system as a whole - its strategic opportunities, threats, and future direction; and, finally, an *'identity'* function, that conceives of the purpose or raison d'être of the system, its 'soul', and place in-the-world.

As a viable system the human body exhibits an efficient interplay between, operational elements - such as skin, limbs and vital organs; co-ordination functions like the nervous system; control, through the pons medulla at the base of the brain allocating

resources, like blood; intelligence, characterised by the brain's use of the senses and thought; and identity in what might be described as an individual's sense of self, or soul. A viable human being, like any organism, requires all five functions to be in place and working in unison.

Beer's main thesis is that the conditions outlined above: the five systemic elements and the various communication channels running between them, and between them and the environment, must be present in all viable systems. Starting from two different positions: a model of the human nervous system, and cybernetic first principles, Beer ends up with a macro level picture of what structural and communication arrangements are necessary in order to facilitate ongoing organisational viability.

## 6.3 Autopoietic Theory and the Viable System Model: theoretical insights

### 6.31 Beer's use of the term autopoiesis in Viable Systems Theory

At the outset it is important to acknowledge that there is already a close connection between viable systems theory and ideas associated with autopoiesis, and there are some fairly obvious surface similarities. Since Beer's model for his viable system is perhaps the living system that we know most about, this should not come as a surprise.

Beer's choice of terminology also speaks of overlapping concerns. The term *viability* itself has biological connotations, and the vernacular use of the term 'survival' has to do with maintaining life. The central characteristic of viability – *maintaining an independent existence* - is similar to Maturana's insistence that living systems are separate from and exhibit autonomy from the medium in which they exist. Both speak of their respective systems as being 'subject to their own laws' (Beer, 1981:103; Maturana and Varela, 1987:48).

It is also important to acknowledge that the idea of autopoiesis is already employed in the context of viable systems theory. Beer cites the concept as a useful set of ideas for the VSM, and for practical applications such as the well-known Cybersyn project; he endorses Maturana and Varela's work in the preface to their *'Autopoiesis and Cognition'* (1980), and further claims that Maturana provides: " . . . *the quintessence of this* (i.e. viable systems) *cybernetic thinking . . .*" (Beer, 1979:405).

In addition, the term autopoiesis itself crops up from time to time in descriptions of the viable systems model (Beer, 1979:405-408; Flood and Jackson, 1991:96). Finally, as we shall see later, VSM researchers such as Espejo and Harnden have clearly been influenced by aspects of *AT*. Raul Espejo, a compatriot of Maturana's who worked with Beer on the Cybersyn project continues to link *AT* and viable systems, albeit in a quiet unobtrusive sort of way.

In extending the basic resonances between his thinking and that of Maturana, Beer remarks:

> *"Naturally I had very closely compared the conditions for life as expounded by the theory of autopoiesis with the conditions for a viable system . . . to me they were complementary and mutually enriching . . ."* (Beer, 1981:338)

Cementing the link between the two theories further, he says that we may argue that *"a viable system is autopoietic (and) the autopoietic faculty . . . is embodied in the totality and in its systems one . . .* " (1981:338).

In the light then of Beer's conviction that a viable system is an autopoietic system juxtaposing the two sets of theory involves assessing the grounds on which Beer makes this claim. Inevitably, since he claims that complex systems such as enterprises and social systems are also viable systems, this leads us into territory that has been well-traversed by others (Zeleny and Hufford 1992, Hejl 1984, Mingers 1992a). However I do not intend rehearsing those arguments here.

It is possible to use Maturana's distinction between the *'specification'* of a system, the basic form (or *'organisation'*) that the specification implies, and the specific systemic forms (*'structure'*) through which the basic form might be realised, to show why – in a very basic sense – autopoiesis and viable systems are certainly similar but fundamentally different entities. Recall that objects are brought into existence through the 'doing' of the observer. On this basis when an observer specifies some real world entity as a 'viable system', he/she is carrying out an operation of distinction that implies a basic form in which Systems 1-5 and the various flows of information around the viable system facilitate the on going maintenance of an independent existence.

In contrast, specifying the entity 'living system' implies an organisation constituted through a specific dynamic or *'manner of relating'* (Maturana 1993b) of molecules; namely one in which the molecules produced produce networks that produce more molecules. Through such means, the class identity autopoiesis specifies the entity living system, the canonical example of which is the single biological cell. In this sense autopoietic and viable systems are fundamentally different.

In fairness, it is easy to see where the confusion has arisen. In explaining this it helps to begin by drawing attention to Varela's (1981:103) claim that potentially there are many examples of systems that are *'capable of specifying their own laws'*, and therefore can be regarded as exhibiting autonomy. Under such a conception, autopoiesis and viable systems do indeed arise as autonomous systems i.e. as specialised subsets of a more general class entity. Yet, as I shall now aim to demonstrate, autopoietic and viable systems are clearly not autonomous in the same way.

Maturana's interest in the autonomy of living systems developed out of the experimental work that he and others carried out in the 1960's. Subsequently he sought an explanation to support a conviction that the purpose of a living system is internal to it, and to counter an idea – apparently prevalent at the time - that human beings are

relatively more independent from their environments than are other living systems. In this context, autopoiesis – components producing networks of components which produce more components – arises as the central mechanism that provides all living systems with a degree of autonomy and independence.

This, of course, is not to say that the environment is unimportant. The canonical example of the individually autonomous living system - the single biological cell - is not completely autonomous because it is embedded in a multi-cellular organism which in turn exists in, and is affected by, an environment. The cell's autonomy has to do with its bounded self-defined nature, not its complete insulation from the systems in which it is embedded.

In contrast, as one might expect for someone with a cybernetics background, when Beer speaks of autonomy - either in the body or in social systems - he means that the system is responsible for its own regulation. In complex systems, this means maintaining a stable internal environment – *'homeostasis'* – in the face of environmental complexity, or more precisely, using Ashby's (1952) terms, as a means of absorbing *'environmental variety'*. In complex environments it is necessary to have autonomous subsystems to have sufficient amplification to survive. Thus, the human body must have an autonomous nervous system because control mechanisms cannot be controlled consciously by the brain. In enterprises, the task facing management is to find the optimum level of operational autonomy that is necessary in order to survive but which still permits overall system cohesiveness. At higher levels of recursion the total system is autonomous from the system of which it is a part for the same reason.

It seems to me that Beer presents autonomy in functional terms, i.e. as something that is necessary for survival or task accomplishment. Viable systems are autonomous because their environments are complex, and the amount of autonomy ceded to subsystems varies in relation to complexity in the environment. The VSM can then be

seen as a complex working out, in organisational or structural terms, of how – depending on circumstances – varying degrees of autonomy might be managed so as to ensure viability.

For Maturana it is not a case of autonomy being necessary. Rather autonomy is a constituent condition; it is more fundamental. Whereas Beer comes up with a list of characteristics of viable systems, of which autonomy is one, for Maturana autonomy is the fundamental condition that constitutes a living being. Living systems are autonomous because they are autopoietic, i.e. because the components are the result of a self-producing dynamic.

On this view, autonomy is a more fundamental condition. And it is autonomy that provides living systems with a particular identity that can be maintained in the face of significant and enduring structural change. In *AT*, autonomy is not a variable; living systems are living because they are autonomous in the manner in which their molecules relate. If molecules stop producing networks to produce more molecules then the system dies.

This is not to say that Maturana discounts the importance that Beer attaches to the need for systems to respond to environmental circumstances. It is just that on Maturana's view, autonomy is not the mechanism through which this occurs. For Beer, autonomy is a mechanism for adaptation; for Maturana, the mechanism is structural coupling. The point is that while autopoietic and viable systems are both autonomous, the nature of the autonomy in the two cases is quite different.

However, the situation becomes more confused since Maturana (1991) does acknowledge that autopoiesis can exist in the conceptual as well as in the molecular/physical domains. Thus arises the possibility that the VSM might be a conceptual autopoietic system, if not a physical/molecular one.

This widening of the applicability of autopoiesis beyond the physical/molecular domain is an inevitable consequence of how the term is defined. Maturana, for example, acknowledges that because autopoiesis describes a *relational* phenomenon that exists in the *dynamic* space, clearly it does not depend on any particular set of components, and is thus potentially applicable to other non-physical/molecular systems. He remarks:

> *"When I first spoke about living systems as autopoietic systems, I was speaking of molecular systems. Later . . . I realised that it was necessary to make the molecularity of living systems explicit in order to avoid confusion. A computer model of an autopoietic system does not take place in a molecular space . . . and this is why we did not claim to have a living system in the computer."* (Maturana, 1991:376. Also cited in Mingers, 1995:45)

Thus in making molecularity explicit Maturana is incorporating 'molecularity' as a specific aspect of the class identity 'living' while retaining it as a structural feature that is manifest in some but not in all examples of the more general class entity.

While there are those such as Mingers (1995:45), who embrace this wider applicability of the term, and therefore opt for a definition of living that includes <u>all</u> autopoietic systems, Maturana now incorporates molecularity into the organisation of the entity living system. In other words it is molecularity <u>and</u> autopoiesis that specifies living, i.e. " . . . *a living system is an autopoietic system in physical space* . . . " (Maturana 1981:22-23). Or more precisely: " . . . *any such autopoietic system <u>realised in physical space</u> is a living system"* (Whitaker, 1996:6).

A viable system then, is not a living system. But is it a conceptual autopoietic system? If my interpretation of what Maturana and Varela mean by the term self production is correct, then the answer to this question is no.

In Beer's preface to Maturana and Varela (1980), he latches on to the terms organisation and structure. In true cybernetic style, he focuses on Maturana and Varela's remark that an autopoietic system is a homeostat, i.e. a device for holding a critical

systemic variable constant within certain limits. Beer then goes on to say that Maturana and Varela's *'definitive point'* is that, in the case of autopoietic homeostasis,

> *" . . . the critical variable is the system's own organisation. It does not matter, it seems whether every measurable property of that organizational structure changes utterly in the system's process of continuing adaptation. It survives".* (Beer 1980:70).

In the same piece, he then goes on to say: *"their* (i.e. Maturana and Varela's*) 'it'* (i.e. autopoiesis*) is notified precisely by its survival in a real world."*

Elsewhere, Beer (1979:405) remarks,

> *"The enterprise(s) . . . staff may come and go, its departments may be closed down or opened up . . . and still it has and retains its identity. In cybernetic terminology, this enterprise is called autopoietic."*

In trying to make sense of this, the meaning that Maturana and Varela attach to the term organisation is pivotal. Recall that this term describes the <u>manner in which a system's components relate</u> that defines its class identity; it is the general that underpins the particular.

Now while I would agree with Beer that this distinction between organisation and structure is very important, in tying it to autopoiesis, I submit that he misrepresents Maturana and Varela. He interprets them as saying that autopoiesis is the continuation and preservation of a system's identity in the face of change. Thus,

> *"any cohesive social institution is an autopoietic system because it survives . . . and because it may well change its entire appearance and its apparent purpose in the process"* (Beer, cited in Mingers 1995:119).

Moreover, Beer remarks:

> *"Look at any great institution: a hospital, a university, a multinational company, a social service, a country. All these things change, in so far as their elements are replaced; all these things change, insofar as some features disappear while others are invented. But Guys Hospital, Oxford University, the steel industry, education, and Britain itself, are recognisably themselves. There is, as in our bodies, every kind of change. But there is no alteration . . . . In the concept of autopoiesis we have the final testimonial to viability. The viable system is directed towards its own production".* (Beer, 1979:405)

All of this suggests that in latching on to Maturana and Varela's distinction between organisation and structure, Beer has substantially modified the meaning of the term autopoiesis. Autopoiesis is not, as he would have us believe, the continuation and preservation of identity in the face of change; it is a very specific type of organisation that constitutes the class identity 'living'. Molecular systems such as motor vehicles and items of footwear and clothing change constantly through use, and they maintain a specific identity; there is structural change with conservation of organisation.

But this does not make them autopoietic systems. The same is true of hospitals, universities (to use Beer's examples) and other human social systems. Beer is right in saying that these change constantly but retain a sense of identity. But autopoiesis defines its own organisation; it is not open to observer-dependent assessments of what it is that is conserved. Autopoietic systems define their own identity; they do not require an observer to do this for them.

In Beer's examples it is a particular observer-defined organisation (Maturana's term) of the system that is conserved. When Beer says that Guys hospital and Oxford University have existed for hundreds of years, he is saying that something has been conserved. It could be the way people interact, it could be the manner of production, or it may just the name. The point is that something has been conserved, and it this that gives the system its identity to some observer.

That all of this is the case is not, as Beer seems to think, autopoiesis. All systems exist as long as there is conservation of whatever it is that defines them. All systems arise this way. But we cannot say, as Beer does, that because systems exist or are viable over time, then they are autopoietic. It is only when a particular organisation is conserved – when there is a conservation of a specific manner if relating of the components, that we can speak of autopoiesis.

Specifically there is nothing in the theory of viable systems that implies self-production of components. In specifying the entity 'viable system' an observer does not have to specify that the components of System 1 must be produced by the system itself. All that is specified is that they directly produce whatever it is that an observer decides the system produces. Such is the case with viable systems such as enterprises. If an observer were to attribute the purpose 'knowledge production' to a university then the various knowledge producers within the university and its component organisational units would fulfill System 1 functions. But there is nothing to say that such researchers must have been produced within the system, even though some may have been, nor that they are engaged in producing systems to produce more knowledge producers.

Again it is possible to see what might have led Beer to make the claims that he does. He is correct when he says that people can enter and leave an organisation and that departments may be closed down or opened up - yet it still has and retains its identity. Ostensibly this resonates with Maturana's claim that molecules enter into a living system, participate in the autopoietic dynamic and then leave. In both cases there is a flow of matter through the system. But it is only when the molecules participate in a specific dynamic – producing networks that produce more molecules – that we can speak of autopoiesis. Indeed it is only when the molecules participate in this dynamic that we can legitimately regard them as components. Participating in the production of any other specific identity is something different entirely. Autopoiesis is not just the conservation of any identity, it is the conservation of a particular identity – the class identity of living systems – self production of components.

In debating the distinction between viable systems and autopoiesis, the teleological nature of the former is significant. Whereas notions of purpose, function, and goals are pivotal to the theory of viable systems, these concepts are irrelevant to autopoiesis. No matter how you look at viable systems it is difficult to get away from the

idea of purpose. Observer-dependent judgements on the identity of the system will often have recourse to notions about 'the basic purpose of the system'. In turn these rely on assessments being made about the relationship between the viable system and the wider environment: for example: "the purpose of a university is to prepare students for the world of work". This, of course is possible for an observer who can observe both the system and the environment and explain the former in relation to the latter.

Internally the various components of the VSM are defined functionally (or, *'allopoietically'*) in terms of the purposive role that they play in supporting the overall purpose of the system. For example the purpose of System 1 is to do what the system does, the purpose of System 2 is to coordinate the activities of operational units; the purpose of System 5 is to formulate policy, and so on.

Inevitably then when the VSM is applied to some sort of social system, it is brought forth in functional terms. In other words it is constituted as something that fulfills some external purpose and its internal structure is defined in functional terms.

All of which is very different to autopoiesis. Maturana acknowledges that in our culture we often have a need to speak about the function or purpose of systems. However he argues that these distinctions are misleading, because they obscure how the system operates, which is as a result of how they are made or built, their components and relations. It is not because of a purpose. The distinction purpose belongs in the domain of the observer; it is a tool that people use to explain their experiences, and it arises as an explanation when the observer can observe the interaction between the system and its environment.

Purposes are not to be found in nature. The basic function of a living system is to produce itself and no more (see also Capra, 1996:106). On this view the notion that internal components have purposes in relation to the system of which they a part is

equally misleading. The components of biological systems have no conception of an 'outside'

Having said all of this, it would be wrong to suggest that Beer believes that the purposes of organisations are pre-given. Attributing one or more purposes to a viable system is one of the first tasks that an agent must carry out in a VSM intervention. And for Beer, the purpose of a viable system is what it does, as this is defined by an observer. Thus, " . . . *purpose is a mental construct imported by the observer to explain what is really an equilibriul phenomenon of polystable stable systems*" (Beer, in Maturana and Varela, 1980:67).

So to the extent that Beer does give credence to the role of the observer, he and Maturana do share common ground. However this does not suggest that there is some fundamental point of similarity between a viable system and autopoiesis. Among the other points of contrast that I have discussed, the teleological nature of the viable system, and the non-teleological nature of autopoiesis identifies them as fundamentally different kinds of entity. This lead me to the conclude that while there can be abstract autopoietic entities, Beer's viable system is clearly not one of these.

### 6.32 Extending the idea of autopoiesis beyond the single biological cell

Since Maturana does concede that autopoietic systems can, in principle, exist in non-molecular as well as in molecular domains, this opens up the possibility of extending the idea of autopoiesis to systems such as work organisations that are the most common target of VSM interventions. Indeed this debate about the wider applicability of autopoiesis has gone on independently of what Maturana may or may not think. There is now an extensive thread through literature that has reflected on this question (see for example the special issue of the International Journal of General Systems, 1992; see also Beer 1980; Hejl 1984, Mingers 1992a; Luhmann 1986).

Since the aim of this chapter is to focus specifically on what insights *AT* can shed on the theory of viable systems, it is not appropriate to delve into this wider autopoiesis-social systems debate in any great detail. Nonetheless in the context of what I am trying to do one key contribution to this wider debate – namely Luhmann's claim that social systems are *'communication autopoietic systems'* – is worth commenting on, if only briefly. According to Luhmann social systems are autopoietic systems in a communication space. The elements are communications, and there are networks of communications that give rise to more communications. Luhmann's example is interesting because, in contrast to Beer, it demonstrates that Luhmann employs the correct meaning of the idea of self-production, whereas Beer – as we have seen – does not. Luhmann then does open up one possible way in which – it seems – we can legitimately describe social systems such as organisations as autopoietic.

While Luhmann does open up a potentially useful way of thinking about organisations, there is a potential danger involved in going down this track. This revolves around the idea that in autopoiesis it is the dynamic that is central, it is not the system's components. On this view, while the various components participate in the dynamic, they are – nevertheless - relatively unimportant. The consequence of this is that Luhmann's work has a somewhat dehumanising quality about it. And while this may be acceptable at the level of abstract social theory, it is less so when it comes to practical real world interventions such as those that tend to be the focus of VSM studies. In practical and ethical terms it is hard to see why in systems such as families and human organisations, one would want to, indeed are able to, neutralise the impact of the particular set of human beings involved.

As Umpleby (1994) notes, Maturana's political experiences in Chile have influenced his strong opposition to any social theory that provides direct or indirect support to the repression of individual expression. In any event I shall attempt to show in

the next chapter that while the particular components involved in autopoietic systems are indeed irrelevant, the human beings who belong to human organisations are clearly not.

Philosophically Maturana's position on what constitutes the human being as the class of living system that it is, also has relevance in the context of the present discussion. For Maturana this has to do with the unique languaging capabilities of human beings. It is in the nature of being human, says Maturana that we live our lives fully immersed in language. This gives us the ability to reflect on our self and our circumstances and to act on the basis of these reflections.

In practical terms this means that those human beings that collectively, through their interactions, produce the identity of a social system will have opinions on the circumstances of their membership of that system. In all probability they will like aspects of it and dislike aspects of it; and they have the capacity to act on these reflections. This is far removed, for example, from the liver as a factory for producing blood. Because the liver cells do not language in the manner of human beings, they are incapable of reflecting on what is happening to them. As I said in the previous chapter, the response of target audiences to proposed change is an important matter that anyone who is involved in practical organisational interventions has to confront at some stage.

Another possible way of extending the applicability of idea of autopoiesis to social systems arises out of Maturana's willingness to concede that autopoiesis applies to multi-cellular organisms such as human beings as well as to single cell systems. To the extent that multi-cellular organisms are comprised of what Maturana and Varela (1987) refer to as *'first order'* autopoietic systems, then according to Maturana the former are autopoietic systems of the *'second order'* as well as first order autopoietic systems.

In 1987, he and Varela would not commit to this proposition. However Maturana (1993b) is now prepared to claim that biological knowledge has advanced to a point where it can be shown that organisms are indeed autopoietic.

If we then extend this first order, second order idea, it is not unreasonable to conclude that social systems are autopoietic systems of the *'third order'*. In this sense social systems are, as Beer claims, autopoietic systems (of a sort).

I venture to suggest however, that little is to be gained by thinking in these terms. Were he to consider this matter, I would expect Maturana to claim that while the idea that social systems are autopoietic systems of the third order, such a distinction is a trivial aspect of social systems in which there is a loss rather than a gain of understanding. What makes a social system a social system is not that it can be distinguished as a collection of human beings, rather it is its own organisation, namely the emotional predisposition that unites its various members through a common acceptance of each other in networks of conversations (see Maturana, 1988a:64, 66, 69). Remove this underlying organisation and you lose the dimension that makes a social system what it is.

Again this raises the question as to whether applying autopoiesis to social systems is at all desirable. When – under this distinction of autopoiesis of the third order – autopoiesis is seen as the fundamental aspect, it raises the spectre that individuality is lost. One can see that autopoiesis secures stability, and one can also see this in the VSM, but individuality is lost. Such loss of individuality may be acceptable in systems such as ant nests or bee colonies (which, incidentally, Foss 1989, describes as an exemplar of autopoiesis), but one can question its appropriateness in human social systems. It does not sit at all comfortably with the ability that human beings have to reflect on their existence and to have identities and formulate purposes of their own, and, importantly, to act on these.

The point is that when autopoiesis and/or viable systems are taken to be the central aspect of social systems such as human organisations, the individuality of human beings is lost. In the case of autopoiesis this is because it is the dynamic that is important not the individuals who participate in it. In the case of viable systems it is because the

overall purposes of the system are paramount. Irrespective of which theoretical perspective is used, the result is the same: much of what is important in the constitution of human beings is potentially destroyed and/or ignored, and this can have significant practical and ethical consequences.

In summary while it is possible to be true to the original meaning of the term and extend its applicability to incorporate social systems in the way that Luhmann does and in the way that Maturana himself does, it is difficult (although as we shall see later, not impossible) to see any great value in doing this.

More generally in the context of trying to understand where Beer has gone wrong in speaking of viable and living systems almost as though one term is synonymous for the other, and in the context of looking forward to imagining how *AT* might contribute in further developing VSM-type interventions, it is worth recalling what happens when an observer names a complex entity that specifies a system.

Here the key point is that when an observer distinguishes 'X' as a 'viable system', a 'social system', or a 'living system' then - by definition - the observer is implying a particular organisation that defines a specific identity for that general class of system to which 'X' belongs. In daily and communal life, human beings grasp the patterns of relations that underpin the organisation of these class entities. Witness for example someone who might say "this is not just a teaching institution this is a <u>university</u>". The words that people use reflects the understanding of what are the relationships among the components. When an observer's gaze falls upon someone else, and he or she distinguishes a <u>living system</u> then the other's molecularity comes to the forefront and his/her 'self' recedes into the background. Similarly when another <u>human being</u> is distinguished, the molecular aspects are less important. On this view, when we speak of viable, social and living systems we are plainly speaking of different things. And what people specify determines what they get.

From an interventionist perspective, Beer's unwillingness to distinguish more carefully between viable systems and autopoietic systems is much more than a basic lack of linguistic precision. It is important because it means that, in explaining organisational systems with a view to improving their functioning, Beer is ill-placed to gain leverage from the unique insights that autopoiesis – and, even more importantly, the wider body of ideas with which it is associated – can offer.

## 6.4 Summary

This chapter has examined how the term autopoiesis is used in viable systems theory and has explored the wider ramifications of this. It has looked at the process that is involved in distinguishing composite entities such as autopoietic and viable systems, and - on that basis - has concluded that these are very different entities.

However, for those management scientists who are able to operate with both sets of distinctions, this opens up new possibilities that potentially can extend the comprehensiveness of VSM-type interventions. To this end we can now shift the focus from critique to one that examines areas of complementarity. I shall do this by looking at specific ways in which *AT* can assist management scientists who employ the VSM as an intervention methodology. I aim to show that despite there being some fundamental theoretical differences between viable systems and *AT*, at the operational level, there are areas of complementarity that are worth exploiting. To some extent the next chapter can be taken to be an examination of Beer's claim that there is indeed complementarity between the two bodies of ideas, although clearly the nature of this complementarity is different to what he seems to have had in mind.

# 7. DEVELOPING VIABLE SYSTEMS INTERVENTIONS

## 7.1 Introduction

In examining how *AT* might develop and enhance VSM-related interventions, it is important - given the discussion of the previous chapter – to remember that we are speaking about *'autopoietic theory'*, not just *'autopoiesis'* per se. I shall be looking therefore at aspects of the complete *AT* frame outlined in Chapter 3 with a view to considering how it might render assistance to VSM practitioners.

In order to show how *AT* can be used to develop viable systems interventions it is necessary to have a clear perspective on the central question that the VSM seeks to address. There are a number of possibilities here, but I shall take this to be that of helping organisational actors understand how, in a particular context, they might go about putting in place mechanisms for ensuring that there is adequate adaptation between the organisation and its subunits and the so-called environments in which they are embedded. Against this background I shall be examining how the literal and metaphorical application of *AT* can assist. Three major areas of contribution will be examined.

Firstly I shall argue that the phenomenological aspects of *AT* can provide an epistemology for use that is lacking in the primary VSM literature. To some extent this possibility has already been foreshadowed by Espejo and Harnden (1989a,b) who have outlined a 'conversational' approach to VSM interventions that is clearly informed by Maturana's thinking. I shall examine the need for this approach and then suggest that a more detailed employment of *AT* concepts can extend the basic position outlined by Espejo and Harnden.

Secondly, within the context of this 'conversational' approach to using the VSM, I shall argue that since the model of cognition that underpins the VSM is limited by its own presuppositions, this tends to place unnecessary restrictions on the content of

discussions about the nature of the 'fit' between organisation and environment. To counter this I shall argue that although the model of cognition that underpins *AT* is theoretically incommensurable with that of cognitivism, at the operational level they are complementary. What I aim to show is that the actions that are available to the agent in terms of understanding and/or intervening in organisation/environment relations depends on the specific distinctions used. In that regard, new and/or different distinctions – in this case based on *AT* – opens up new possibilities for describing, understanding and intervening in this process.

The third area of contribution extends the second. It presents my thinking about how aspects of *AT* can be combined with the VSM to produce what I am calling a 'guided evolution' approach to managing the organisation-environment interface.

## 7.2 Providing an implementation epistemology for the VSM

On Beer's own admission (1994:350), all empirical investigations and implementation methodologies require adequate epistemological grounding. Yet it is a moot point as to whether Beer himself has provided a clear enough statement on the epistemological issues that are involved when it comes to using the VSM in an intervention mode.

Take, for example, the three main works. In both *The Heart of Enterprise* and *Brain of the Firm*, Beer's main concern is to explicate the logical coherence of the VSM. In *Diagnosing the System* (*DTS*) there is a step-by-step guide that aims to help people in understanding it. The latter is a timely piece of work since the aforementioned tomes are voluminous compositions, and not especially user-friendly. But while understanding the technical aspects of the model is an important prelude to putting it into practice, the two are not the same thing. Relative to the needs of management scientists and other users,

there is still something of a vacuum in relation to the epistemological status of the 'knowledge' that the VSM can generate and how one should go about generating it.

The main problem is that the philosophical underpinnings of the VSM are ambiguous and this makes implementation unclear. In particular it makes it difficult for the neophyte practitioner to easily perceive a concrete research strategy. In much of Beer's early work he tends to use positivist language which suggests that the VSM is primarily a tool for mirroring systems that have real ontological status. This implies that an expert can use the VSM to identify real cybernetic deficiencies in the functioning of organisational systems.

Not long after this there is evidence to suggest that Beer would not accept a naive objectivist interpretation of the VSM. He insists, for example, that models are neither right nor wrong, but more or less useful. Moreover he acknowledges the subjective nature of systems. The VSM, says Beer, *"is an arbitrary designation . . . selected from the infinite recursion, to be called a 'system'."* (1979:405). Elsewhere, speaking of the scientific tradition to which cybernetics and the VSM belong, he states " . . . *scientific accounts* (of reality) *do not pretend to be reality itself"* (Beer 1994:377). Finally, in his preface to Maturana and Varela's *'Autopoiesis and Cognition'*, Beer (1980:67) states that the notion of purpose *"is a mental construct imported by the observer."*. Later in the same piece he says, *"what I am now sure about . . . is that nature is not about codes: we observer's invent the codes in order to codify what nature is all about"* (1980:69).

This final comment seems to nail Beer's ontological colours well and truly to the mast: viable systems do not exist in nature. It implies that he has gradually shifted away from a naïve objectivist underpinning for the VSM and now takes what might best be described as a 'softer' line. It comes as some surprise then, that aside from a relatively insignificant comment that readers could easily miss, to the effect that the using the VSM does not determine absolute facts, but establishes a set of conventions (Beer, 1985:2),

most of the content and style of presentation of *DTS* implies that viable systems are unproblematic primary entities that exist in a real world.

To my way of thinking then, there is some ambiguity. On the one hand, much of Beer's work gives the impression that viable systems are primary and the observer secondary. On the other hand, some of his statements seem to reaffirm Maturana's position, which I would take to be that a viable system is merely a distinction made in the phenomenological domain of the observer. Users and potential users of the VSM might be forgiven for being somewhat confused about exactly what is the philosophical basis on which Beer advocates use of the VSM. Compounding the difficulty, none of Beer's major works have outlined in any detail what 'putting the VSM into practice' really means. The emphasis has tended to be on developing the logical structure and coherences of the model. It has been left to others, notably Espejo (1993); Espejo and Harnden (1989a,b); and Flood and Jackson (1991), to generate a methodology for the VSM.

All of which opens up the possibility that Maturana's epistemology might provide the unambiguous philosophical underpinning that that is lacking in Beer's own writings, and, just as importantly, provide a strategy for use. The problem " . . . *is not so much that a more formal development of the model is necessary, but that there is a need for a sharper focus upon methodology.*" (Espejo and Harnden 1989b:453)

In acknowledging this gap in the VSM literature, Espejo and Harnden seek to develop and articulate an epistemology for the VSM. In the process, they aim to clarify what is entailed in attempting to utilize a 'cybernetic approach' in *MS*. Their concern is similar to Beer's: effective implementation, they claim: " . . . *involves 'bedding' methodology within the context of a particular epistemology.*" (Espejo and Harnden 1989b:441).

The distinction between models that aim to describe a pre-existing reality and models that, "*generate a reality . . . among a community of observers*", is at the core of

Espejo and Harnden's epistemology for the VSM. In simple terms, these authors advocate employing the VSM to provide a context, a frame, or a tool for conversation among a community of observers such that the conversation follows a convergent rather than divergent path. In other words the VSM becomes an instrument to focus debates about the functioning of organisations.

This conversational approach to using the VSM resonates strongly with what I said in Chapter 5 about what an *AT*-based approach to modelling might involve. Indeed, given the relatively few direct references that Espejo and Harnden make to Maturana, his ideas echo much more strongly through this work than might be immediately obvious. In that sense however, these authors do provide a very useful indication as to what, in the context of VSM interventions, putting *AT* into practice might involve. Having said that, there are several points about their general approach that I now wish to comment on.

Firstly there is the matter of what Beer 'really meant' in relation to the ontological and epistemological underpinnings and how the VSM ought to be used. Reading Espejo and Harnden leaves one with the impression that anyone who takes the VSM to be a literal representation of real world systems and uses it accordingly (i.e. to identify and remedy objective cybernetic deficiencies) would be doing serious violence to Beer's intention. Yet, as I have just said, some of Beer's language does seem to create that impression. More importantly, I venture to suggest that given the content and style of presentation of *DTS*, there is a much better 'fit' between Beer's intent and the 'methodology' that Flood and Jackson (1991) articulate for the VSM, than between Beer's intent and the process outlined by Espejo and Harnden.

Flood and Jackson advocate using the VSM in situations *"in which there is a general and easily attainable agreement about the goals to be pursued"* (1991:88). It is plausible to imagine how Beer's detailed exposition of the logical structure and coherences

of the model could be employed were a situation to be interpreted in this way. Indeed *DTS* provides exactly the sort of information and clarity that such a situation would demand.

In contrast, if, as Espejo and Harnden (1989b:453) suggest, the cybernetic approach involves " . . . *hav*(ing) *the participation of* <u>*multiple viewpoints*</u> *in order for the complexity of a social enterprise to be at least partially expressed.*", it is difficult imaging what role there would be for the detailed content of *DTS*. Aside from the fact that Beer's descriptions of the VSM make no explicit mention of negotiation forums or participation to take account of individual viewpoints (see Flood and Jackson, 1991:111), the practical difficulties involved in operating across multiple viewpoints, <u>and</u> at the level of detail contained in the book would be formidable. Espejo and Harnden (1989b:454) claim that the methodology needs to " . . . *generate a forum that enables protagonists in a conversation to* <u>*enhance*</u> *their degrees of freedom for action in a given organisational context*" and I agree with this. Realistically however, I cannot imagine how the process outlined in *DTS* could sustain this objective. I submit that the level of detailed analysis that *DTS* requires in support of a <u>particular</u> statement of purposes is more likely to detract from people's freedom than enhance it.

In order then to avoid a potential alienation effect among the various participants to the dialogue, I submit that it is necessary to work with significantly less detail than *DTS* implies. This would bring the model closer to its portrayal in Espejo's computer-based 'VIPLAN' method. Here it is much easier to imagine how the VSM – used in a multiple-perspective mode - could be used to promote convergent dialogue.

Admittedly Espejo and Harnden say they are only *"<u>suggesting</u> the way ahead"* (1989b:444), which raises the question of whether they go far enough. Now I shall not pursue the point in detail here since it is addressed directly in the context of discussing SSM, which has the same 'convergent dialogue' objectives that Espejo, and Harnden speak about. Indeed from an *AT* perspective Chapters 8, 9, and 11, examine in some

detail what 'managing' conversations involves. At this point then, I shall simply comment briefly on two aspects of Espejo and Harnden's general approach that are worth elaborating on beyond what has already been said.

The first area that needs spelling out in more detail is what these authors take *'generating realities in a community of observers'* to mean. Only when this has been done can a proper assessment be made of the extent to which we can envisage the VSM fitting into this process. And only then will we be in a position to specify what difficulties this might raise for those who seek to employ the VSM in this fashion.

I fully concur with Espejo and Harnden's claim that it is possible to employ the VSM to "*generate one or more realities as part of a conversation among a group of observers*" (1989b:451). In this vein, it is relatively straightforward to imagine how the VSM might be used within the context of soft systems methods that operate using a conversational/learning format. For example an agent might employ the VSM during SSM's 'stage 4'. This would involve using the VSM to operationalise a particular *Weltanschauung* (in this case a defined statement of organisational purpose) and then employing it as a template for learning about a 'real world' organisational equivalent.

At the same time, it is important that we do not underestimate the complexity of this process of learning, particularly where divergent *Weltanschauungen* are involved. Indeed I submit that it is critical that whoever is facilitating this process has a grasp of the various individual and interpersonal dynamics that are involved when a group of people get together in these kinds of situations. It is only through an understanding of these dynamics that it is possible to have a sound appreciation of the various feasibility issues that are involved and of the demands that conversational methods place on facilitators.

Again, since this substantive topic is covered in more detail later, I shall not delve into it here. Suffice to say that if the agent is to take a lead from Espejo and Harnden, he/she needs to know how the 'reality generating' process occurs spontaneously in daily

life and how this might apply in more contrived settings such as those envisaged in major organisational interventions.

As Espejo et al. (1996:93) put it, *"conversations . . . rely on the participants' ways of thinking and explaining the world."* Indeed they do, which makes me wonder why – since he has clearly been heavily influenced by Maturana's thinking - Espejo does not take the next logical step in that book which is to spell out exactly how the phenomenological aspects of *AT* might help. Perhaps this is because the book is multi-authored. Inevitably this imposes constraints on the direction that the work takes.

Ostensibly there are grounds for being optimistic about the extent to which the VSM can dovetail within the spontaneous processes through which human beings explain their experiences. As we saw in Chapter 3, explanations comprise a generative process that fits with the listener's experiences and which satisfies some other validity criterion that applies in his/her listening. In a sense, this makes the VSM just another – albeit highly specialised - explanatory domain. It has its own operational coherences and, like any other explanatory domain it is subject to its own rationality and specifies its domain of legitimate actions. It is possible therefore to reframe the anatomy and physiology of the VSM (the 5 subsystems and the communication channels that link them) in Maturana's format of a generative mechanism that for a particular observer might account for his/her experience of 'effective' or 'ineffective' organisations. This would be something along the lines of: *"If operational units have sufficient delegated authority to deal with external variety, if there are mechanisms for coordinating the disparate activities of these units, if. . .and if. . and .if. . . then the result will be an organisational system that has the capacity to adapt itself to known and unknown environmental circumstances."*

While all of this is relatively unproblematic in theory, in practice it is much less so since we cannot assume that the generative process adequately explains the

experiences and deduced experiences of the various participants. Just as important, we cannot assume that participants will give as much credence to the preference criteria that underpin the VSM as one might assume is the case with a committed user. The agent may buy into VSM logic as this is manifest in principles such as the Law of Requisite Variety, 'double-loop' learning, the importance attached to 'satisfying' the needs of the environment, the desirability of ceding control to operational units and opening up channels of participation, and so on. However, it would be erroneous to assume that all parties to the conversation will be so favourably predisposed to these principles. In that regard it may take a good deal of effort on the part of the agent to explore these possibilities in advance of the substantive intervention.

All of this is saying that while there are grounds for believing that there is a potential 'fit' between the logic of the VSM and what people do in daily life in explaining their experiences and communicating these across communities of interest, we cannot presume that everyone who might be expected to use the VSM in the conversational format outlined by Espejo and Harnden will be equally predisposed to it. Some of those involved may not be able to imagine its generative mechanism leading to particular performance outcomes as these are, have been or could reasonably be expected to be experienced. Others may simply not like or agree with the various theoretical and moral imperatives on which it is based.

The second aspect of Espejo and Harnden's epistemology for the VSM that I believe could benefit through a more direct and explicit linkage being made with Maturana's theory of the observer, has to do with the social context within which tthey propose that the VSM be used. As I have just said, using the VSM as a conversational device in a community of observers for the purpose of coming up with a shared explanation of how an organisational situation is now, and what might be done to improve it, is consistent with Maturana's description of the immediate context in which

explanatory domains emerge spontaneously i.e. explanations arise out of conversations. However in daily life people decide themselves who they are going to converse with and quite often conversations take unexpected turns and are more or less effective and/or satisfying to the people concerned.

My experience is that formal intervention-related conversations are no less precarious in this regard than those in daily life. All of which means that once again the agent is placed in a position where his/her theoretical knowledge and practical experience can be pivotal in terms of determining the chances of the intervention being successful.

Against this background, it is interesting to note that Espejo et al. (1996) agree that effective implementation of the VSM depends on the quality of the conversations in which the model is used. Yet neither they nor Espejo and Harnden, provide much by way of detail about what a conversation, beyond the vernacular meaning of the term, involves. Espejo and his co-authors hint at the sort of things that are required when, for example, they speak about how managing 'moods' is vital for creating effective interaction (1996:72). That this is not developed in any great detail makes it difficult for those who are charged with the responsibility for effective implementation of the VSM to know what is required. Discussion of these points is now put on hold until the next two chapters.

## 7.3 Synergising two models of cognition - broadening the basis of VSM conversations

At this point in the discussion there is a major shift of focus. Thus far I have been looking at how *AT* might provide the sort of unambiguous epistemology for using the VSM that I have argued is lacking in the primary VSM literature. Although this is a form in which *AT* can 'help' the VSM, the balance is very much loaded in favour of the former; *AT* is 'calling the shots' so to speak.

In the next two sections, the balance swings back towards the VSM. I now wish to look at how *AT* might help in relation to what one could argue is the central question that the VSM - when used in an intervention mode – addresses. As I said earlier, I am taking this to be dealing with the question of how organisational actors might go about putting in place adequate mechanisms for ensuring organisation-environment adaptation.

In the VSM, mechanisms for adaptation – theoretically grounded in the Law of Requisite Variety - are largely structural. And it is this structural orientation of the VSM, which stands in stark contrast to autopoiesis. For Maturana, living is something that either happens or it does not. As long as there is molecular self-production, and as long as autopoiesis and adaptation is conserved, then living continues. Maturana says little in specific terms about the structural forms through which autopoiesis might be realised. Maturana does not say how living systems are autopoietic. He identifies the basic dynamic that constitutes the organisation of the living, he does not spell out in detail how it works.

Beer, on the other hand, sought to understand in some detail how systems are viable. To maintain an independent existence systems must do certain things. It does appear, remarks Beer, *"that the VSM has sufficient generality to justify its origin as an attempt to discover how systems are viable"*, and, used in an intervention mode, it " . . . *generates considerable power to describe and predict, diagnose and prescribe."* (Beer 1985:152 emphasis added). Beer answers the how question by spelling out in some detail the 'anatomy' and 'physiology' of the viable system (see Beer 1985:xii). Indeed he actually uses these terms, employing the term anatomy to describe the basic structure of the system, and physiology to describe the manner in which the various functional elements work in combination to produce whatever it is that the observer decrees that the system produces.

Having said that there is this difference in emphasis in the two theories it is possible to identify the kernel of a structural theory in autopoiesis. And interestingly there is a basic resonance with the VSM. For example, Maturana and Varela (1980:90-93) state that the basic dynamic of self-production involves three types of relations.

The first: *'constitution'* outlines the physical topography of the system, i.e. its three dimensional geometry. It creates a structural environment within which self-production takes place. This is very similar to the overall configuration of the VSM and the role of those aspects of the VSM including Systems 2-5 and the channels of communication that facilitate the autonomous operation of the various Systems 1.

The second set of relations – *'specification'* – ensures that the components produced by the networks are the ones necessary for autopoiesis. These determine the identity of the components – synthesising the chemical constitution of proteins and enzymes – and are quite similar to Systems 3 and 3* in the VSM. These systems provide basic guidelines for the Systems 1 so as to ensure that they are producing what is required to sustain the identity of the system as a whole.

The third set of relations – *'order'* – is concerned with the dynamics of the process. For example it is important that the correct amounts of molecules are produced at the correct rate and the correct time. Chemicals and feedback loops are required to ensure that autopoiesis continues. Again this is quite similar to System 3 in the VSM which manages across the Systems 1 and monitors their performance and takes corrective action, and System 2 which has an 'anti-oscillatory' function.

Despite these similarities, even if the idea of autopoiesis could be unproblematically transferred from the biological to the social domain, which it cannot, it does not match the organisational diagnostic qualities of the VSM. If *AT* is to be of any value to those who seek to intervene in the functioning and performance of organisational systems then it must do so by drawing upon ideas that extend beyond autopoiesis per se.

In this vein, I shall now argue that although the VSM and *AT* are based on fundamentally different sets of theoretical propositions they are not necessarily incompatible at the level of practice. Indeed I shall be arguing that combining the two theories opens up the possibility of more comprehensive insights and better-informed interventions.

### 7.3.1 The underlying theory of cognition in the VSM

In order to appreciate how the VSM and *AT* might be combined at the operational level, I believe that it is first necessary to show how they differ at the theoretical level. This requires that we make some rudimentary distinctions in relation to what the VSM assumes about cognition. Cognition is pivotal in this discussion since it is the process through which systems 'come to know' the environment or medium in which they operate. In the organisational context this point has been acknowledged by a number of scholars who are seeking to explain organisation – environment adaptation in terms of basic cognitive processes (von Krogh and Roos, 1995, 1996, 1998; Sparrow, 1998)

The best-known account of the human nervous system considers it to be an instrument through which the human being obtains information from the environment (see Varela et al. 1991). By manipulating symbols that represent features of the world the nervous system constructs a model of the latter that is used to compute behaviour that is adequate to ensure survival. Here the viability of the organism depends upon there being an 'accurate' or 'realistic' symbolic imprinting of the environment within the nervous system, and the organism behaving in accordance with these representations by initiating an appropriate response. Overall the process is a bit like using a road map in planning a route. There is a destination, there is a map that accurately represents the terrain, and there is action that is governed by intent; a relatively straightforward process it seems.

Initially, as Capra (1996:259) notes, the computer model of cognition asserted that representations are localised in the brain and that symbol manipulation was based on sequential rules applied one at a time. However cognitivism now accepts that representations are global and highly distributed patterns of brain activity (Morgan 1986).

Since the VSM and cognitivism share the same cybernetic origins it is not surprising that the former has many features in common with the latter. As we have seen, the VSM operates on the premise that organisational systems are embedded in a pre-existing environment, that: " . . . *is full of challenges and opportunities . . . (and) the VSM must respond to this environment.*" (Beer 1979:277).

How then do viable systems respond? According to VSM logic, autonomy is ceded to the system's operational units so that they can take responsibility for understanding the needs of their own local environments, and have the capacity to act on such knowledge without undue constraints or wider interference from the system as a whole.

Beyond this, the VSM's logic for adaptation maps almost precisely on to the cognitivist logic just described. At multiple recursion levels System 4 collects information of the environment. It then processes this information and builds models of the environment, of the viable system itself, and of the various System 4 components. Typically, claims Beer, the latter are dispersed through the organisation; only rarely are they properly integrated. Thereafter it is a case of System 4 communicating the results of its deliberations through the rest of the system such that action may be taken to sustain adaptability and survival, and to promote advancement.

Presented in such bald terms the manifestation of cognitivist logic in the VSM is clear. However in order to reinforce the point further, witness the following: First, the Cartesian split between external object and internal knowing subject is evident in Beer's

shorthand reference to System 4 as having responsibility for *'outside and then'* in contrast to the *'inside and now'* focus of System 3.

Secondly, designating System 4 as the *'response system of the enterprise'* (1985:113) reflects cognitivism's pre-occupation with the idea that change largely originates in the environment and that cognitive systems must respond and adapt to external opportunities and threats. While it would be wrong to claim that Beer does not acknowledge the systemic importance of innovation and 'inventing the future' because he does. However much emphasis in the VSM is placed on responding to pre-existing circumstances.

In this context System 4 assumes significance as *"a logical necessity"* (1979:277) that Beer (1979:235) claims *" . . . houses the viable system's whole apparatus for adaptation"*. System 4, he says, is the *"response system"* (Beer 1985:113) of the enterprise. He further points out that there are two key adaptive mechanisms: spontaneous and conscious, System 4 being responsible for the latter. In the same manner in which spontaneous homeostatic processes maintain water levels, balance, and decrease or increase respiration in animals, it is through spontaneous processes that, *" . . . the organisation keeps its day-to-day enterprise going."* (Beer 1985:109). However he rightly points out that the environment for human beings and organisations is more than the various microenvironments that are picked up by sensory surfaces that then invoke autonomic responses. In organisations such environmental challenges and opportunities are manifest in activities such as research and development, corporate planning, and market research, their responsibilities – according to Beer – being *" . . . to monitor what is actually happening in the big wide world, and correctly assess the trends."* (1985:113 emphases added).

This view is consistent with the perspective provided on organisation-environment relations in much of the traditional strategic management literature. Here

there is a strong sense that the environment can be defined through reference to system purpose-related characteristics including the needs, preferences and perceptions of clients; the behaviour of competing firms. On this traditional view corporate survival and advancement hinges on strategists being able to identify characteristics of the environment, plan a response strategy and implement it. Sequentially it is a case of analyse, diagnose, plan, implement and assess the results.

Thirdly, System 4 mirrors the model/representation-building feature that is central to cognitivism. Says Beer:

> *"Every regulator must container a model of that which is regulated . . . unless the enterprise as a whole contains an adequate model of its total environment how shall it be viable? And of course the notion of environment must itself contain a regulatory model of the range of possible futures. This is precisely an explanation of the necessity for System Four"* (Beer, 1979:234)

Fourthly, cognitivism's claim that the system functions adequately when symbols accurately represent some aspect of the world is inherent in the VSM. As Beer puts it,

> *" . . . any would-be regulator must contain a model of that which needs to be regulated, and that if this model has a given level of variety, it will handle only that equivalent variety in the outside world. So many of our would-be regulators in society have low-variety models of what has to be regulated. As a result . . . as soon as the system to be regulated adopts a possible state that is not allowed for in the model . . . regulation fails."* (Beer 1979:235)

Fifthly cognitivism's assumption that representations are not localised but globally dispersed through the brain is manifest in Beer's (1979:231) claim that System 4 activities are *"disparate and disseminated,* (and) *are active all over the organisation"*.

Finally, cognitivism's supposition that data processing is governed by rules is replicated in the VSM principle that data collection and model-building activities are subject to relevance criteria embodied in the ethos of the organisation established, in consultation with the rest of the system, by System 5.

In summary, according to the VSM, organisational evolution involves a gradual process of organisational change as the firm responds to (and sometimes precipitates)

processes of flux and transformation in the environment. Viability, on this view, is a reflection of the degree to which the firm is adapted to those prevailing external circumstances that bear upon its purposes.

- The system maintains viability in two ways. Firstly by ceding autonomy to operational units and providing a structure through which this autonomy can be managed for the benefit of the system as a whole. Secondly through a continual process of monitoring what is happening in the environment and ensuring that this information is communicated effectively and efficiency through the system. From an interventionist perspective these are the key areas that are singled out for attention.

### 7.3.2 The *AT* perspective: insights from an empirical case study

So the question arises, what can *AT* add to this? What new distinctions, based on a very different model of cognition, arise as being potentially useful in terms of broadening the debates about how organisations ought to facilitate adaptation with their environments? What alternative possibilities for action might there be? And what feasibility issues might *AT* suggest as having a bearing on the proposals for action that might emerge out of a conventional VSM study?

In order to illustrate key theoretical points, over the next few pages I shall draw upon material extracted from a recent empirical research project that has proceeded in parallel with the present study. This project is part of a larger multidisciplinary research enterprise (locally known as *'The Exemplar Firms Study'*, see Campbell-Hunt, Brocklesby, Chetty, Corbett and Walsh, 2001). The main aim of the research is to investigate the origins of competitive capability of a number of globally successful New Zealand exporting companies. I have been looking at these companies from a systems-based (mainly VSM) perspective, while colleagues have taken strategic management, marketing, operations and human resource perspectives respectively. Data from my

contribution to the project (Brocklesby 2001) is used here for illustration purposes only; I am not using it to test the validity of the theoretical propositions that I shall be making.

When examined from a VSM perspective the exemplar companies, almost without exception, exhibited many of the key structural requirements of viability. These are spelled out in detail in my contribution to the main publication. Given their continued survival in the face of major economic dislocations (removal of tariff protection, removal of subsidies, intensification of competition) this was unsurprising. At the operational level senior executives and company owners have put in place mechanisms through which relatively autonomous operational units were able to first understand and then deliver on customer requirements. And, in the wider system, mechanisms – often informal – were in place to ensure that there is adequate coordination, control, intelligence, and that there is an overall sense of identity and guiding purpose. In simple terms these companies bear witness to the observation that in the same way that it is possible to use a map to plan a route, it is possible to design an organisational system with specific purposes in mind.

The VSM was very useful in identifying key aspects of the functioning of these organisations. However it very quickly became clear to me that it was limited as a source of insight into some adaptive processes that seemed to be important in explaining the distinctive niches that, over time, these companies had carved out for themselves. In particular through discussions with senior executives and company owners it quickly became apparent that an evolutionary dimension was necessary in order to adequately account for the competitive capability of these companies.

Now there is nothing to prevent a user of the VSM employing it to examine developments over time. Equally there is nothing in the VSM that demands that an evolutionary perspective be taken. Witness, for example, the process of inquiry that is spelt out for the VSM in *DTS*, and in Flood and Jackson (1991). Neither give much credence to historical processes. Yet without an historical perspective it is hard to see how there can be

an adequate explanation of how the organisational system had come into being. And from an interventionist perspective historically acquired competencies and rigidities are important when it comes to making extrapolations about its future, particularly in relation to what new developments may and may not be possible.

Relatedly, the cognitivist road-map model of corporate evolution does not sit comfortably with what for some of these companies seem to have been a somewhat haphazard and indeterminate evolutionary path. It could not account for example as to why one company developed a capability in security systems when it started out in mechanical pumps. Neither could it account for how another was able to convert a capability in resin production into one of waste management, and why the company acquired a manufacturing facility in Vietnam instead of in Korea or Malaysia. None of these developments were the result of a System 4-type strategic planning or existing operational units responding to new environmental opportunities.

I found myself asking similar questions about all of the companies. I came to the conclusion that whereas the cognitivist/strategic intent model is helpful when it comes to explaining the functional (purpose-related) effectiveness of an organisation at a particular point in time, it is silent in explaining other key developments. And when the key people involved suggest that these developments are pivotal in explaining why their company is performing so well, then it seemed to me that there is a need for a theoretical perspective that is sensitive to historical and evolutionary processes.

Another issue that arose revolved around the real people who have been involved in these companies. Others, notably Jackson (1989), and Flood and Jackson (1991) have commented on how the VSM neglects qualities brought by the human actors who make up organisations and on how it has little to say about the social processes that go on in organisations. Evidence from the exemplar companies supports this viewpoint. I found it

very difficult separating the 'fit' between these companies and their environments from the preferences, emotional commitments of the key actors.

Again I am not suggesting that the VSM cannot accommodate these aspects of organisational behaviour. With creative and intelligent application I believe that it can. What I am arguing is that, from a VSM perspective, with its structural and 'here and now' focus, they do not immediately arise as being of critical importance. Indeed it is quite likely that inexperienced users of the VSM may not pick up on these points. Since I am arguing that the VSM and *AT* may be usefully combined to create more comprehensive insight into processes of organisation-environment adaptation, it now needs to be shown how these and other relevant considerations logically arise out of *AT*.

Since *AT*'s rudimentary cognitive distinctions were introduced in Chapter 3, I shall not rehearse them again here. At this point, it is enough to say that *AT*'s depiction of the closed and circular nature of the nervous system contrasts sharply with cognitivism's 'open' perspective. Yet the key point and, in this context, the central source of insight, is not the closure and circularity of the nervous system. Instead it is the mechanisms of structural change that, in spite of this closure, allow the living system to adapt itself to the environment.

Recall that there are three of these: the flow of molecules through the system, internal system dynamics, and structural coupling. Since the latter - in the context of this discussion – is the most important, I shall comment only briefly on the first two.

*Flow of molecules*

According to *AT* a key source of change in living systems is the flow of molecules through the system. To some extent the structure of the living system reflects the characteristics of the molecules that flow through it.

Used metaphorically, the obvious organisational parallel to the flow of molecules through the living system is the people who enter the company, participate in it, and then sooner or later leave. The basic identity (organisation) of the company - as defined by an observer - might remain constant over a significant period of time. However how this identity is realised (structure), including the specific 'fit' between the system and the environment, depends to no small degree on the predilections and qualities of those who participate in it.

Of course it is a bit banal to say that people really count. Yet this is a point that much of the management and organisational literature with its mechanistic bias often miss. And while the personal qualities of individuals can be incorporated within the VSM framework there is nothing in the model that says that this is an important consideration.

In any event I am not just saying here that real people count; I am drawing attention to how much they count. In Brocklesby (2001) I provide a number of examples of how, in the exemplar companies, the personalities, personal qualities, preferences, emotions and values of the key actors are absolutely pivotal in understanding the direction that these companies take, as well as the specific forms of adaptation that emerge. Moreover, from an interventionist perspective, the predilections of these individuals are critically important in considerations about what 'improvement' actions are and are not possible.

*Internal dynamics*

Another source of change in closed systems is that which arises as a result of the system's own internal dynamics. These can be incremental, as in aging for example, or they can be discontinuous as in stage-related change. Theories of the latter have become very popular in the organisational and strategic management literature (see Cameron and Whetton 1983, Gioia and Chittipedi 1991, Stubbart and Smalley 1999). Most however

take a strongly deterministic line on corporate evolution that is quite different to the multidimensional perspective being developed here. When applied metaphorically to organisations, *AT* does not support the idea that change is pre-programmed and determined by an inner logic. While internal dynamics are clearly important, they are not the only factor involved.

One thing however is clear. In reaching positions of strong competitive capability the exemplar companies have moved through a number of stages as they have aged and grown. Some of these have involved major transitions that are broadly comparable with those that occur in biological and evolutionary development. Examples include those that we (the researchers) have come to refer to as *'going global'* (i.e. internationalisation), and *'coping with gushers'* (i.e. dealing with unprecedented levels of product demand). These periods were difficult and painful, and required a major expenditure of energy plus careful management. Senior executives spoke of 'agonising' over these steps.

In all cases coping with these steps necessitated a major and speedy reconfiguration of the firm's structural coherence. If biological systems survive under new environmental conditions eventually its structure will cohere. By contrast, in organisations it is desirable – if not vital - that when a structural change that is necessary for adaptation disturbs existing coherences, these are quickly re-established.

Again this is something that can be easily overlooked in the VSM. The VSM is concerned with the coherent functioning of the system as a whole. Yet because it does not have a strong evolutionary/historical dimension it does not draw attention to these major transition stages, nor the need for those who are involved in improvement projects to think very carefully about where various action possibilities stand in relation to the system's developmental dynamics.

*Structural coupling*

To the extent that the characteristics and predispositions of the people who belong to the organisation and the organisation's internal dynamics impact upon its structure, then these factors partly determine what does and what can happen to it. This suggests that the structural modifications that might arise out of a VSM study ought to be crafted with these considerations in mind, or – as minimum – be taken into account during that part of the conversation that assesses the feasibility of structural options. Allusions to this thinking may be detected in the aforementioned work of Espejo and his colleagues, but there is little sensitivity to it in the primary literature itself.

Important as these factors are, they do not tell the whole story when it comes to understanding the nature of the 'fit' between organisation and environment. Indeed, I submit that from the perspective of a VSM practitioner (or manager more generally) who is seeking to obtain a better understanding of the nature of the 'fit' between organisation and environment, *structural coupling* (the primary adaptive mechanism) has the most to offer. Partly this has to do with the intrinsic value of the concept itself; partly it has to do with its literal rather than metaphorical applicability. On Maturana's view, the applicability of the two key concepts: structure-determinism and structural coupling, is not limited to biological systems. In principle they may be applied to any (observer-dependent) complex entity, organisations included.

Structural coupling, as ought now to be quite clear, is a co-evolutionary process through which two recurrently-interacting systems change each one according to its own structure-determinism.

While the mechanisms for adaptation in the VSM and structural coupling are superficially similar, there are some important differences. In the VSM, the notion of *'variety engineering'* is important because it demonstrates the importance of the system being able to respond to environmental circumstances that bear upon its key purposes. As

we have seen, according to VSM logic, in high variety environments this involves ceding autonomy to operational units. The VSM then comes into its own in demonstrating how organisationally such autonomy should be managed in order that the integrity of the system as a whole and its overall adaptability is not compromised. On this view adaptation is something that must continually be worked on analytically, managerially, and structurally.

Since purposes are such an established aspect of organisational dialogue it would be inadvisable to deny the importance of purpose-related mechanisms of adaptation. Yet adaptive mechanisms that ensure the survival of organisations extend beyond those that have to do with satisfying some on-going purpose. To the extent that purposes show up in peoples' explanations of their experiences of organisational behaviour, they are clearly important. But this does not make purposes a fundamental aspect. Semantic descriptions, according to *AT*, should not be confused with system operation.

Irrespective of this, it is clear that the idea of structural coupling hints at a multi-dimensional form of adaptation that extends beyond those that are grounded on semantic and linguistic descriptions that have recourse to the notion of purpose.

Another point of comparison is that although the VSM does not ignore the two-way relationship between an organisational system and the environment, the model does - as we have seen - tend to emphasise the extent to which the system <u>responds</u> to the environment. Since structural coupling delineates a <u>co</u>-evolutionary process it highlights the complex interdependent relationship between system and environment. Moreover, unless users are very careful it is easy to slip into a cause-effect mode of thinking with the VSM. It is possible to interpret the law of requisite variety as implying that a particular external variety condition or state pre-determines its own effect <u>inside</u> the regulating system. If the appropriate internal state is not there then viability is threatened.

In this context, structural coupling is helpful because it reminds us that the organisation is a structure-determined system that can only do what its structure allows. Variety engineering, on its own, does not convey much sense of the real difficulties that organisational systems can have in coming up with what may be perceived to be the appropriate response to an external state.

Biologically, what is interesting about structural coupling is that it provides an explanation of how systems of common origin become differentiated as a result of the interplay between the historical conditions under which they live, and their intrinsic structural characteristics. In the case of biological differentiation, Maturana and Varela (1987:108-110) use the analogy of someone flicking down drops of water on the sharp peak of a hill. Repeating this experiment many times reveals that some drops - the larger and heavier ones for example - form a straight-line channel down the hill. Others meet obstacles that they elude by moving off to the left or right in different ways as a result of their small structural differences in weight, size, speed of travel and so forth. Slight changes in the wind will also move some of the droplets away from the initial direction.

Eventually, it will be possible to observe many channels of drift down the hill. These channels result from the interaction between the structural characteristics of the droplets and the irregularities of the terrain and the wind. Looking from above, that is in a two dimensional (i.e. flat) perspective, it should be possible to observe the starting point in the centre and a number of crooked lines extending away from the centre in all directions. Some of these lines are curtailed because the emergent structural form of the water droplet is not compatible with the environmental feature that it encounters. In this case adaptation ceases. Other lines continue moving on, branch-like, away from the original starting point to all points of the compass. The location and positioning of these lines, relative to each other, and at a particular moment in time, represent a complex pattern of system differentiation.

What relevance might this analogy, based on the idea of structural coupling, have for organisational evolution, and more importantly, for those who are seeking to intervene to improve the fit between organisation and environment?

Clearly there are some aspects of the analogy that do apply to organisations. Just as water droplets cannot avoid the effects of gravity, in organisations it is very difficult to reverse the effects of history. Also structure-determinism applies irrespective of whether we are speaking of water droplets or companies; whether and how a company responds to an external pertubation depends on it. And in principle, where the company ends up in an evolutionary sense is no different to the point where one of Maturana's streams of water reaches sea level having tumbled down the sides of the mountain. The differentiated position of the company depends on its historical interactions. In addition, its competitive position can depend upon the competencies acquired through the same process.

Other aspects of the analogy are less easily transferable to the organisational context. In general, mountains are much more stable than corporate environments which makes planning in the latter a much more tricky business. On the other hand, whereas droplets of water act according to their structure at any moment in time, in organisations human beings are part of that structure and, to some extent at least, are able to exert an element of control over direction. Whereas evolution in biological systems is not a directed process, i.e. it has no goal in mind, organisational interventions are clearly different. All of which raises the question of how much control can be exerted, and, from an interventionist perspective, what can be done to maximise the chances that a particular intervention will lead to the desired outcomes.

I believe that the following adaptation of Maturana and Varela's analogy is useful in helping us to answer this question. Imagine being able to grasp in two hands a plastic model of the hill used in the aforementioned analogy. The goal is to guide a droplet of water towards a particular target by moving the model to the left or the right, and by

tipping it forwards or backwards. In this exercise the water droplet represents the company, the target represents a position of viability in a particular market. The model of the hill – its contours and varying textures - represents the business environment. The person holding the model and attempting to control the movement of the droplet is anyone who has the responsibility for taking actions to enhance the functioning of the organisation in relation to a particular set of purposes.

Now in guiding the movement of the droplet it helps to pay attention to how its structure is involving. Is it getting bigger and heavier or smaller and lighter for example? Equally it is necessary to look carefully at the terrain over which the water droplet is travelling. In organisational terms this translates into an intervention strategy that is based on the idea that VSM conversations need to take place against a background in which there is an appreciation of the nature and origins of the system's structure and an appreciation of the external environment. In this next section I aim to flesh out in much more detail what this would mean in practice, including – particularly in relation to the second point – some of the difficulties that are involved.

## 7.4 A 'guided evolution' approach to VSM interventions

### 7.41 Understanding the structure of the system

*AT* suggests that there are a number of aspects to this problem. Firstly the proposition that external events only trigger changes that are a pre-existing feature of the organisation means that external events do not determine the substance, direction and/or the timing of organisational change. From an interventionist perspective it means that attempting to 'work against' the system can be counterproductive. Certainly it is possible to observe developments that are taking place in organisations and conclude that they have been 'caused' by some external event. However from a structure-determinism angle

external events only trigger possibilities that are already embodied in the structure of the system. In the aforementioned exemplar firms study, the internationalisation process provides a good example of this. When the introduction of export incentives and deregulation of the domestic NZ economy precipitated internationalisation in these firms, it was a case of the former acting on pre-existing capabilities; it was not a case of creating new ones.

The idea of structure-determinism is relevant in an intervention context because it highlights the need for the people involved to understand the structural characteristics of their firms. It suggests that they should to resist any temptation to force a particular change on the organisation even when some external circumstance might appear to 'demand' such a response, or when there is every indication that the action in question appears to have worked well elsewhere.

In the exemplar companies there is plenty of evidence of key people having such understandings. Although there has often been awareness that change is necessary, these managers have generally not allowed external circumstances to dictate its pace and substance. Their proclivity to speak about *'not forcing it'*, and the need for changes to gradually *'drop into place'* (see Brocklesby 2001:186-188) bears this out.

A related point has to do with the coherence of the system. As long as a biological system survives, its structure consists of a coherent set of interrelated characteristics. In business enterprises however, coherence is more likely to be a source of competitive capability than a basic requirement for survival. In competitive environments companies with limited operational coherence can survive, but it is difficult to imagine how they can prosper beyond the short term. Moreover this becomes an issue as a company goes through major transitions. Here competitive capability is dependent upon re-establishing coherence quickly.

Secondly, application of the idea of structural coupling implies a need for change agents to be sensitive to historical/temporal processes. To illustrate this point we can return to the water-mountain analogy. Let us imagine that a company has evolved to a position that is roughly equivalent to the six o'clock position when looking at the mountain from a position directly above it. Let us further imagine that the optimum in terms of maintaining or enhancing viability is ten o'clock. How easy is it to get from six to ten? Obviously this will depend on the particular competitive action or behaviour that the company is attempting to mimic. However, what we can say is that the current position of the company is not arbitrary. It is where it is because of the interplay between its historical interactions and its structural characteristics. This means that nullifying the effect of these may be difficult if not impossible. Further it suggests that seeking to mimic actions that may appear to have worked well elsewhere may be ill-advised.

In the case of the exemplar companies we were able to trace their strong competitive positions back to key events in their histories: tax breaks, export incentives and tariff protection, involvement with particular collaborative partners, the involvement of key individuals during critical developmental periods, and various chance events. From this I conclude that intervention debates ought, as a matter of routine, to consider whether the historical evolution of an organisation's structure will facilitate or frustrate a desired action.

Having said that, it is important to acknowledge that according to the logic of structural coupling <u>all</u> historical interactions count. There is therefore no way of knowing how the evolution of an organisation occurs in all its aspects. Thinking back to Maturana's analogy of the droplets of water flowing down a mountain, a relatively insignificant and unnoticed interaction, such as a sudden gust of wind, might – in the fullness of time – have momentous consequences. It could be instrumental in setting the droplet off on a completely different trajectory than would have otherwise been the case.

So far in introducing a dimension to the debate that would not necessarily emerge out of a conventional VSM study, it has been a case of ensuring that the people concerned look backwards, to history, in order to know what is and what is not desirable in the present. It is equally important however to look forward, and to do so in a way that goes beyond the future 'environmental scanning' activity that we associate with System 4 in the VSM.

As two structurally-coupled systems co-evolve through their interactions, each one will inevitably trigger changes in the other. In the exemplar firms study the relationship between a company and a collaborative partner provides a good illustration of this. As a result of these interactions there are wider evolutionary ramifications for both parties, which is one reason why the firms have gone to great lengths in making sure that they establish the 'right' connections. The various cases show that collaborative partners are important in a variety amplification sense because they assist both in developing knowledge of a particular market and in delivering a product to that market. At the same time partner choices are important because, for better or worse, they alter the firm's structure and hence its evolutionary trajectory. The cases provide a number of examples of companies going on to bigger and better things through leveraging off a collaborative arrangement. Conversely there are other examples where managers have terminated a relationship with a collaborator because they have foreseen the possibility of longer-term undesirable effects.

### 7.42 Knowing the terrain

The main thrust of the last section has been on the need to frame consideration of ways of responding to opportunities and threats in relation to an appreciation of where the organisation has come from, where - given different scenarios - it might be going, and of what – given its structure – is and is not possible.

However, going back to the water droplet analogy another kind of appreciation - knowledge of the terrain – is also of critical importance. It is only when there is an appreciation of how the structure of the water droplet interacts with the terrain over which it passes that it is possible to maintain even a semblance of control.

At this point the issue is not that the VSM fails to emphasise the importance of having adequate environmental knowledge because it does. What is at issue is how relatively unproblematic it considers this process to be. Yet Beer does hint at possible difficulties. For example, he claims that innovators:

> *"are impelled to use the existing arrangements for addressing the Outside and Then, because those channels and transducers are available. But if they are to 'hear' the answers, they cannot rely on the existing arrangements --because they are designed to filter out exactly that novelty which may inhere in the replies"* (Beer 1979:239).

In order to deal with this problem the VSM contains provision that allows innovators to devise new *attenuating filters* and new *transducers*, *"in order to understand the novelties which (by definition) they are not aware of in advance"* (Beer 1979:239).

Here I believe that Beer is hinting at a problem that is much more substantial than he admits. It is a problem that *AT* brings into sharp focus.

Ostensibly, dealing with *'the problem of the environment'* is quite straightforward. Surely, it might be claimed, this is exactly why the viable system has a System 4? Scanning and modeling the environment is exactly what System 4 does. Surely it is also straightforward in the case of the person who is holding the model of the mountain and attempting to maintain control over the movement of the water droplet. Notwithstanding the occasional surprise, most of the dominant features of the terrain can be easily spotted and appropriate adjustments made. But is it as simple as that? I shall be arguing that it is not. Paradoxically, since the focus has now shifted from the structure of

the system to the environment, I shall be arguing that, to some extent at least, understanding the latter is conditional upon understanding the former.

Earlier, I said that because of its pre-occupation with structure, the VSM is not ideally suited to grappling with some of the human and social dynamics that, in real organisations, intersect with structures but operate independently of them. The issue that I shall now discuss is, I believe, a case in point. It has to do with what we should take to be the outcome of the process through which those involved in System 4 activities gain an appreciation of the environmental dynamics that bear upon the performance of the system in relation to defined goals and purposes.

In the VSM, the outcome of this activity is a model or a representation of whatever is perceived to be happening (or likely to happen) 'outside' in relation to some purpose. Theoretically there are two possible objections that might be made to this thinking. The first has to do with *AT*'s proposition that the structure of a system defines what external events it admits as a source of perturbation. It also determines the nature of the response. It thus follows that the external 'realities' identified by System 4 - 'opportunities and threats' for example - do not exist as independent phenomena 'out there'. Rather what is seen reflects the structure of the observing system.

The second possible objection has more to do with autopoiesis itself than with structure-determinism. Here, applied in a Luhmaniann sense where meanings replace cells as the fundamental unit of autopoiesis, the problem has to do with the self-producing context within which the 'looking outside' takes place. Some (see, for example, Mingers, 1995; Whitaker, 1996) consider the use of autopoiesis in this context idiosyncratic because it has not been framed in terms of the canonical elements of the concept. However, others (see Capra, 1996:207; von Krogh and Roos, 1995:86) support Luhmann's contention that communicative meanings can be subject to the same dynamic that occurs in the exemplar case.

Thus, drawing upon Luhmann, Capra outlines the nature of the problem:

> *"A family system, for example, can be defined as a network of conversations exhibiting inherent circularities. The results of conversations give rise to further conversations, so that self-amplifying feedback loops are formed. The closure of the network results in a shared system of beliefs, explanations and values – a context of meaning – which is continually sustained by further conversations."* (Capra 1996:207)

What is seen to 'exist out there' thus depends entirely on what those involved do, as they go about making distinctions and enacting various institutionalised 'ways of seeing'. On this view, the threats and opportunities identified by System 4 reflect the distinctions used, which, in turn, reflect the preferred identity that the organisation, or part of it, seeks to maintain.

From a VSM perspective, the problem is to ensure that there is sufficient internal variety to match any emergent external variety. Difficulties might occur when old language/old models/old representations simply do not have the variety to deal with a new circumstance. In this event the VSM would predict loss of viability through the system being in violation of Ashby's Law. The solution is to develop new, extended, or simply better models, i.e. a more active and effective System 4. In simple terms the issue is one of knowing how to build more variety into the system so that new emergent situations and trends can be recognised, modeled, and dealt with.

For *AT* a failure to employ the relevant distinctions is only part of the problem. Recall that Maturana takes cognition to be an integral part of our normal everyday activity. So the problem has to do with practices. When practices are repeated day after day people become more adept at them and more likely to continue using them. But procedures that were once selected for a particular context can become irrelevant or dysfunctional under changed circumstances; people carry on doing what they do best rather than looking for more effective options.

Extending this, recall how Varela et al.'s concept of *enaction* further develops the idea that knowledge is constituted through actions. The way people experience the world,

is very much an active construction involving the whole body. If the body has not learned how to orient itself in such a way that the relevant cues are picked up then the individual risks failing to notice that which others, as a matter of routine, pick up. The difficulty is that these sorts of orienting credentials are not easily acquired. They grow out of accumulated lived experience of certain kinds of activity. They are entrenched in the day-to-day experience of acting in the world, and they become entangled in various ways in expert practice.

Having said that there is this pre-linguistic capacity or incapacity to 'see', linguistic distinctions are manifestly important. Formulated and sustained through on-going conversations, these are rooted in day-to-day practices and behaviours. And it is in this sense that it may be claimed that the 'external world' reflects the 'internal world'. It further means that while, from a VSM perspective, external knowledge is important in sustaining on-going viability, since the knowledge production process has a self-referential quality, it can also impede viability. On this view, while a finely-tuned outward looking System 4 is important it may not be enough.

In very simple terms it is a case of finding ways in which, in formulating 'new knowledge' existing distinctions can be challenged and called into question. There is a need to understand the extent to which company structures, favoured distinctions, and institutionalised patterns of conversation, and - more generally – ways of doing things, are empowering or restricting people's ability to identify opportunities and threats. In a very real sense it is a case of appreciating the 'inside' in order to appreciate the 'outside'.

Extending the idea of observing beyond the relatively vague notion of making distinctions is helpful because it provides some clues as to what is required in increasing the number of realities that are available to organisational members and, as a consequence, the action possibilities that flow from these.

Unpacking this further, we can say that there is a need to firstly increase the range of experiences (of what is going on 'out there') that are available to the organisation; secondly, there is a need to increase the generative processes that might account for any one of these experiences; and thirdly, there is a need to broaden the range of criteria that are used in assessing the legitimacy of these generative processes.

To do this it is necessary to disturb existing conversation/observing patterns. This can be achieved by bringing in new people, by using outsiders and by re-jigging existing staff. Since knowledge structures arise out of the day-to-day interactions among people they are rarely organisation-wide. In many cases there will be many knowledge structures each one of which is based in specific patterns of interaction and conversations through the organisation. A source of creativity therefore is to have people from one part of the organisation to participate in conversations elsewhere, i.e. to 'flow' molecule-like through a system in which they do not usually participate. In such circumstances traditional conversation patterns are disrupted because there is a change in internal couplings.

More generally, looking at the situation from a Luhmann-based perspective, the organisation knowledge self-production process is a dynamic that exists apart from the particular people who might be involved in its development and maintenance. This confirms anecdotal evidence that in most organisations there are some people who participate in the 'official' knowledge production dynamic, and therefore are subject to its self-referentiality, and that there are those who despite being otherwise active and influential members of organisation, resist it. This makes such people an important source of creative and novel distinctions. Extrapolating from this it suggests that while much of the organisational behaviour, human resource management and strategic management literature takes the view that steps should be taken to maximise the involvement of people in strategic conversations and to create an homogenous culture based on shared values

(as, to some extent, does the VSM), this is an argument in favour of retaining heterogeneity and a degree of non-involvement of some personnel.

Ostensibly the exemplar firms deal with this problem in a related but slightly different way. Being relatively small, although these companies have been active in terms of developing their own strategic processes they have been compelled to incorporate knowledge generated externally into their strategic thinking. Because they have lacked skills in key areas they have come to rely very heavily on external collaborators as a source of industry and customer-relevant knowledge. This provides a good balance between 'diving in' i.e. developing your own knowledge capabilities, and engaging in 'stepping out' processes that break institutionalised frames and traditional ways of thinking.

This may partly explain why, to this point in time, the companies have avoided getting stuck in present identities and evaluating environmental opportunities and threats in such terms. Effectively this has allowed several of the companies to reinvent themselves a number of times within the bounds of their structural capabilities.

### 7.4. 3 Beyond seeking to control the process of organisation – environment 'fit'

The previous two sections have focused on the two key elements that structural coupling suggests are required in order to maintain an element of control as the organisational system, as Maturana might put it, 'slides through', and 'changes congruently' with its environment. This is accomplished by initiating actions that are crafted out of an appreciation of the dynamic relationship between the structure of the organisational system and the environment.

At the same time we have seen that structural coupling is, in itself, a form of 'knowing': a form of knowing that is associated not just with the brain or, in organisational terms with strategic action, but with the whole process of individual and

organisational living. On this view, the mental and strategic processes that take place in human beings and in human organisations are a subset of much wider and more fundamental cognitive processes. This perspective on cognition acknowledges the importance of intuition and what we often refer to as 'gut feel' in organisational decision-making. Moreover it raises the possibility that spontaneous adaptive micro-processes that are dispersed throughout the whole organisation might be as important, in viability terms, than rational-analytical activities that are the outcome of interventions and strategic management.

One possible interpretation of this theoretical line is that managers can pursue a non-interventionist 'let things happen' strategy on the grounds that 'if the organisation survives', then, by implication, 'it must know". Unfortunately, the name of the game in most organisations is not merely survival but advancement.

However there is a variation on this theme that I have been able to identify in some if not in all the exemplar companies. Basically it involves allowing developments to occur naturally but acting, where necessary to remove elements that are clearly non-adapted. A good example, again discussed in Brocklesby (2001), is what we have labelled a *'sow and reap'* approach to internationalisation. Lacking the resources of international competitors, New Zealand companies cannot adopt a highly analytical/strategic approach when it comes to deciding which international markets to target. Instead they have very successfully adopted a strategy in which the 'seeds' of development are sown in many markets, and then subsequently entered those markets where the seed has 'taken root'.

This is not to say that a 'non-strategic' approach to strategic development is easy. With the benefit of hindsight it is possible to identify a number of situations where the viability of the exemplar companies has come under threat because owners, and/or managers, have waited for too long in removing non-adapted elements. For example

several have waited too long in downsizing or in withdrawing products that fail to capture the imagination of the market.

Clearly this is a very different intervention strategy to that which is based on seeking to first understand the dynamic relationship between the structure of the system and the environment as a means of engineering a 'better' system. To some extent this strategy is hinted at in the VSM where system one units are given sufficient autonomy in order that they are able to respond 'naturally' to external developments. Yet the structural coupling perspective takes the idea to another level. Much more emphasis is placed on how spontaneous and often involuntary actions embody a form of 'knowing' that is just as 'intelligent' as more abstract and cerebral processes. Certainly it suggests that organisational 'intelligence' extends beyond the activities of System 4 at various recursive levels; it is to a large degree embodied in day-to-day operational activities.

Finally I wish to touch upon a point that was raised earlier. No matter how much those who are interested in improving the fit between organisation and environment engage in conversations that are designed to better understand the structure of the system and to appreciate the environment in which it operates, there will – as Beer himself acknowledges – always be 'surprises'. In the language of structural coupling there will always be 'chance encounters'.

In terms of the analogy that has been used through the chapter the person controlling the droplet of water may have an acute understanding of its structure and that of the terrain, but his/her control will always be precarious. It may not always be possible to avoid all the obstacles on the way down the hill. Neither may it always be possible to steer the droplet into channels that can direct or increase its momentum towards a desired endpoint. Best efforts aside, it is clear that there will be a good deal of unpredictability in terms of how the droplet interfaces with and responds to the terrain. There may be a sudden gust of wind, the flow may come face to face with a previously hidden obstacle,

or suddenly the flow is being swept into a channel. Whatever the situation, the direction of movement of the water down the hill is altered.

In the exemplar companies we observed many examples of these kinds of events. Although the outcomes, in most cases, were compatible with the broad strategic direction of the company, it would be stretching things to say that these developments were a product of strategic direction. It seems to me that an intervention strategy that is informed by the logic of *AT*'s structural coupling is one that is open enough and flexible enough to accept these kinds of developments, and where possible, gain leverage out of them.

## 7.5 Summary

In summary this chapter has argued that *AT* has the potential to assist those who are interested in taking steps to improve the nature of the 'fit' between an organisational system and the environment in which it is embedded. It can do this by providing a clear epistemology for using the VSM in a distinctive 'conversational' way, and – within this - by incorporating issues that the VSM, because of its cognitivist underpinning, does not necessarily draw attention to. The chapter has focused on how structural coupling in particular is useful in drawing attention to the historical processes through which organisations constantly change as they co-evolve with their environments, and in highlighting what actors can and cannot do in directing a process that extends beyond the environmental scanning/strategic intent and variety matching focus of the VSM.

In the VSM there is a strong sense of having first established what objectives are being pursued, the key to viability is to design a structure that can successfully navigate the various opportunities and threats that exist in the wider environment. *AT* leads to the conclusion that knowledge of the external world is <u>not</u> transmitted from a pre-existing environment, rather it reflects processes at work within the organisation. It suggests that prior knowledge – embodied in organisational conversations and daily practices - can have

a major impact upon the process of learning that culminates in the discovery of 'new' knowledge. This self-referentiality of knowledge production has the potential to seriously impair the ongoing viability of the organisation over time since it leaves the organisation highly vulnerable to 'surprises'. *AT* points to what steps can be taken to minimise the most harmful effects of this.

In more general terms *AT* allows us to contemplate the possibility that attempting to look from the 'outside of the organisation inwards' is as important, if not more so, than looking from the 'inside of the organisation outwards'. Paradoxically, it further suggests that a comprehensive understanding of the environment is conditional upon an understanding of the organisation.

# PART FOUR – SOFT SYSTEMS

Notwithstanding the caveats that were issued in the last chapter about the somewhat ambiguous philosophical underpinnings of the VSM, there are many who would place it firmly in the hard systems paradigm of *MS*. I now wish to examine a methodology that has often been described as providing the first significant 'break' in *MS* from this orthodoxy. Indeed some regard the methodology in question – Peter Checkland's Soft Systems Methodology (SSM) – as the exemplar of the 'soft' *MS* paradigm (see, Galliers et al. 1997).

## 8. SOFT SYSTEMS METHODOLOGY: A CRITICAL PERSPECTIVE

### 8.1 Introduction

This chapter begins by outlining the main features of SSM. Next I employ *AT* to cast a critical eye over the various propositions that, retrospectively as it turns out, Checkland has looked towards in providing theoretical and philosophical support for SSM. This includes a discussion on the work of Sir Geoffrey Vickers, which in some respects, is very similar to Maturana's phenomenology.

Having done this, using *AT* as a lens through which to examine the theoretical and methodological aspects of SSM, I then focus on a number of key theoretical areas that bear upon its purpose. These include the nature of the cognitive structure that underpins people's descriptions of problem situations, the origins of people's understandings, and

relatedly the dynamic through which emerges accommodation among diverse perspectives. Finally I reflect critically on some other issues that *AT* raises about SSM. These include the matter of the meaning and sustainability of emergent understandings, the issue of bringing about substantive change, and finally the question of ethics.

## 8.2: Peter Checkland and Soft Systems Methodology

SSM first appeared on the *MS* scene in the 1970's and 80's, Checkland having discovered that traditional hard systems approaches were of limited applicability in highly complex and ambiguous situations. The distinctive competence of traditional hard *MS* methods is in helping decision-makers in designing systems that will meet stated objectives; hence much of the emphasis is placed on answering the how question. Here, since in soft systems thinking " . . . *ends, goals, purposes are themselves problematic* . . ." (Checkland, 1981:316) - both the what and the how are at issue. Correspondingly, the distinctive intervention image is of problem owners and other stakeholders generating an adequate formulation of what the problem situation is ('problem setting'), and doing this interactively; it is not an image of a solitary thinker or 'expert' choosing from a range of possible actions in solving a pre-existing problem.

SSM has developed considerably since Checkland's original formulation in 1981, and there is now an extensive literature that describes various approaches to it, and that reports on a diverse set of applications. Although SSM has been subject to a number of revisions and modifications during the period between Checkland's two major contributions (1981 and 1990), the original 'seven stage' model is the best known, and will be the main object of interest here.

In its original form, SSM is a seven stage iterative process. Stages 1 and 2 are 'finding out' stages, the outcome of which is captured in a *'rich picture'* of the problem situation. A rich picture depicts a range of diverse viewpoints about a problem situation

identifying various issues, conflicts, and difficulties. It draws attention to cultural and political aspects of the problem situation, and it attempts to provide a complete picture of 'what is going on' rather than reducing problems to their component parts.

Once the various aspects of the problem situation have been identified, Stage 3 involves reducing these to a number of notional *'human activity'* systems each one of which is underpinned by a particular viewpoint, or *Weltanschauung,* that gives it meaning and justifies its existence. Having been captured in a *'root definition'*, Stage 4 involves converting these systems into logical models, or *'holons'*, of what the defined system needs to do, to be what it is. Each model, based on the logic of a 'purposeful whole' delineates a sequence of activities that are linked together denoting major dependencies. These activities - expressed using verbs - describe what one can logically deduce has to happen for the system to perform as it is described in the root definition. Once the various participants are satisfied that a model is complete, meaning that it is coherent, internally consistent and meaningful in relation to a particular viewpoint, Stage 5 involves using the model as a template to structure further investigation into the situation depicted in the rich picture, with a view to identifying desirable changes. Stage 6 involves evaluating the political and cultural feasibility of the changes identified in Stage 5; and finally, Stage 7 develops an agenda for dealing with the situation.

Checkland (1990:44) considers that the aforementioned activities act as a catalyst through which participants gradually learn their way towards an accommodation or to a shared appreciation of the problem situation, thereby allowing action to be taken to improve it.

## 8.3 The theoretical underpinnings of Soft Systems Methodology

For the most part the architects of intervention methodologies aim to advance a particular set of theoretical propositions. Thus Stafford Beer made some observations

about the functioning of the human nervous system, worked through the ramifications of key principles of cybernetics, and brought the two sets of ideas together in the form of the Viable Systems Model.

It is interesting to note therefore (see Checkland 1981:260), that SSM was pressed into service with virtually no theoretical backing. Only later, having examined SSM to ascertain what theory was implied by it, does Checkland align the methodology with various theoretical positions. It transpires that most of these belong to the so-called interpretive paradigm and prominent among the scholars to whom Checkland refers is Sir Geoffrey Vickers.

It was discovered, says Checkland, that the process of SSM mapped to a remarkable degree the ideas Vickers had been developing, hence, that SSM " . . . *makes practical use of Vickers concepts*" (Checkland, 1995:5).

From Checkland's vantage-point, the starting point for Vickers is the '*Lebenswelt*': an interacting flux of events and ideas unfolding through time. '*Appreciation*' of the *Lebenswelt* depends upon the '*appreciative setting*', which Vickers takes to be a state of readiness to see and value things in one way rather than another. Decomposing this state of readiness reveals three sets of judgments, all of which are based more on relationship managing than they are on goal seeking. The first has to do with the nature of the reality that pertains to the issue in question (*'reality judgments'*), the second has to do with the desirability of such reality (*'value judgments'*), and the third has to do with views about how the situation might be.

The source of these judgments is not the situation itself because there is no absolute set of standards that can govern this. Rather it is the outcome of previous sets of judgments. Thus previous reality judgments create categories by which people classify objects of experience which means that when a new reality judgment is made, they carve something out of a field of attention and assimilate it into categories developed in earlier

appreciative cycles. In passing it is interesting to note that Vickers claims that in categorising objects the key is not the object as an entity in-itself, rather it has to do with how we relate to the object, although the relation is sometimes taken for granted. This is remarkably similar to Maturana's suggestion that objects arise as tokens for coordinations of behaviour, and once having arisen in this fashion, the object then 'obscures' the coordinations

Returning now to Vickers, previous value judgments also create *'standards of values'* (i.e. appropriate norms). These explain whether a situation will be judged good or bad, desirable or undesirable. This process involves comparing events and ideas in the *Lebenswelt* with pre-existing standards. Once made these various judgments contribute to the ideas stream of the *Lebenswelt*. They also provide the standards that carry forward to further appreciative judgments. Most importantly however, they are the basis for action or regulatory behaviour that is designed to improve the situation in terms of maintaining or changing relationships. Finally action taken contributes to the events stream. These processes describe the basic form of Vickers' *'appreciative system'*. While the basic form remains constant over time, its concrete manifestation – i.e. the *'appreciative setting'* - changes over time.

The next question concerns the basis on which Checkland (1995:8) might reasonably claim that *"SSM offers a way of discovering what Vickers refers to as the appreciative setting"*. There would appear to be two key aspects to this. Firstly since Checkland's notion of *'Weltanschauung'* – which we shall come to shortly – plays a mediating role between the so-called external world and an observer's interpretation of it, it is broadly equivalent to Vickers' *'appreciative setting'* and the reality and value standards that provide the criteria for judgment.

Secondly the process envisaged in the idea of the appreciative system of comparing judgments of fact and value with envisaged alternative relationships and

comparing reality judgments with standards, i.e. comparing the 'actual' with the 'norm', mirrors one of the key defining features of SSM. This is to compare conceptual models based on declared *Weltanshauungen* with each other and with their so-called real world equivalents.

To this extent, Checkland can justifiably claim that SSM operationalises particular aspects of Vickers' epistemology. However, whether Vickers provides an adequate level of theoretical support to sustain SSM's broader purposes is another matter

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### 8.3.1 Some basic resonances

Philosophically Checkland and Maturana have much in common. Checkland arrives at his epistemology via the phenomenology of Husserl and Schutz, via the hermeneutics of Dilthey, and, as we have just seen, via Vickers' idea of the appreciative system. Maturana arrives at his epistemology via the biology of cognition. Despite these differences in origin, there is a basic compatibility and some surface similarities. Fundamentally, both Maturana and Checkland are interested in, and theorise about, the conditions and limitations of observing systems. Checkland comments that in contrast to a 'positivist stance' where the emphasis is on the *"given world as known through experimental evidence"* (Checkland, 1981:316), there is a need, for a 'phenomenological stance' that involves a *"readiness to concede primacy to the mental processes of observers rather than to the external world."* (Checkland 1981:315).

The process of observation then has primacy for both Checkland and Maturana. Neither are interested in seeking to understand the nature of an external world and/or explaining behaviour through reference to 'external' causes. They both acknowledge therefore that multiple versions of social reality are always available. Indeed since SSM advocates learning by using a number of models - each one reflecting a particular

viewpoint - the idea that there are equally legitimate perspectives that people can bring to bear on problem situations is a major defining feature of SSM.

Maturana, Checkland and Vickers acknowledge that people's 'realities' develop intersubjectively through social processes, and, within this social context, they believe that knowledge is the result of actions carried out by an active, not passive, subject. Indeed Vickers' frequent reference to the Piagetian idea that if the observer does not have the relevant schema then they have nothing to refer to (and to all intents and purposes 'it' does not exist) is remarkably similar to Maturana's proposition that objects are brought into existence through acts of distinction.

In addition to these basic epistemological resonances, Checkland and Maturana share an ambivalence towards goal seeking models of human behaviour. While they both agree that goals and goal seeking shows up in people's descriptions and explanations and is thus not unimportant in shaping behaviour, goal-seeking models do not convey anything like a complete picture of how human beings function. Referring once again to Vickers, Checkland claims that the key issue for human beings is not goal seeking, rather it is to maintain or improve relationships with objects and events. As we have seen, for Maturana goal-seeking and other mentalistic processes are a subset of a much more fundamental process of cognition that is bound up with the process of structural coupling.

Finally, Checkland makes frequent reference to Vickers' claim that the world is one of flux and transformation, i.e. it is a constantly changing pattern of events and ideas. For Checkland the main implication of this is that, in principle, the intervention process is an on-going cycle of action and reflection; there are no permanent solutions This resonates very strongly with Maturana's claim that human beings learn to distinguish objects through their interactions with others. Because interactions are continually changing, and because we 'bring forth' our worlds through the distinctions that we make,

this means that we live in a constantly changing world. The world is transformed each time we participate in recurrent interactions.

## 8.4 Critique

### 8.4.1 The cognitive boundaries that underpin people's understandings of problem situations

Unless someone is simply inventing a story as they go along, the concoction of words, concepts and sentences that people use in describing a problem situation and in contemplating what to do about it, do not come out of nowhere; they are part of a greater whole. Thus for Checkland (1981:220) " . . . *every statement about a problem situation must be a statement about the system plus a particular Weltanschauung associated with it*". Thus the notion of *Weltanschauung* ('*W*' hereafter) arises as a central and distinctive feature of SSM. Indeed Checkland (1981:18) goes so far as to claim that " . . . *this concept is the most important one in the methodology*".

Given its centrality, the *W* concept is surprisingly underdeveloped. Checkland variously describes it as *'a view of the world'* (1981:214); *'an in-built image of the world'* (1981:217); *'a set of assumptions about a problem situation taken as given in communication between members of social groups'* (1981:283); and *'an unquestioned world-image by means of which we perceive the world'* (1981:285).

This raises an immediate difficulty. At this level of generality it is hard to see how these descriptions meet the need of a methodology that, according to its proponents serves to: " . . . *change as well as to explore . . . people's viewpoints* . . . " (Checkland and Davies 1986:109, emphases added).

Recognizing the vagueness of the *W* concept, Fairtlough (1982) identifies a number of different senses in which Checkland uses the term in SSM. Checkland and

Davies (1986:109) respond by remarking that the term is deliberately used in a broad sense. They elect not to further elaborate on its structure and content claiming that little would be gained by pursuing debates about what they regard as 'personality assessment'. This is a curious response since we are told that the whole point of SSM is to explore and change *Weltanschauungen*. Surely Fairtlough (1982:132) is right; if the aim is to change something, then we need to know what it is that we are changing.

In fairness to Checkland, it must be said that in more recent writings he does speculate more on the basis on how human beings come to experience and make sense of their worlds. Thus Checkland and Scholes (1990:300) claim that initially there is an act of selection of some subject of discourse. This creates a *'figure-ground'* relationship that can be *'predicated'*, i.e. something can be 'said about' the figure. Characteristic of human beings, Checkland and Scholes then say, is their ability to formulate many alternative predicates. Discourse then involves comparing one predicate with another or with perceived reality. These comparisons create arguments in relation to evidence and underpin decisions to act in different ways.

It is relatively easy to comprehend the link between these general statements about what human beings do, and the process envisioned in SSM. And it is this observation that allows Checkland to make the further observation that SSM:

> *" . . . is a more organised and formal version of what we do anyway when we think purposefully . . . you cannot help but use the form of SSM whenever you do serious organised thinking . . .* (which is why) *. . . using it seems so natural."* (Checkland and Scholes 1990:300)

Questions remain however about whether Checkland delves deeply enough into the process through which human beings attribute meaning to situations in which they have an interest. To say that we make a set of distinctions that we then say something about and that the various 'somethings' can be compared, is not a particularly useful insight. In a sense, Vickers' notion of the appreciative setting is more useful since it

provides a much clearer picture of the basic form of that which a user of SSM seeks to reveal and explore. The specific focus is on the various judgments and the standards through which they are arrived at. But questions still remain as to whether these concepts are adequate or whether there might be something better. It is with this question in mind that we can now examine what *AT* has to say on the subject.

In very general terms, Maturana would concur with Checkland's proposition about the figure-ground relationship (see Maturana and Varela 1987:40). For Maturana, relating mentally to any being, object, thing or unity involves making an act of distinction that defines 'it' as being separate from the background in which it is located. The sole condition for existence is the cleaving of a unity from an ambience by the act of distinction made by an observer. Thus whenever we refer to 'it' we are implying an operation that constitutes 'it' or 'brings it forth' i.e. brings it into existence. Having enacted distinctions in our descriptions of experienced phenomena, Maturana claims that we can then do a number of things. We may decide to do nothing – i.e. we experience a situation in a particular way but do nothing with it; or, we can do as Checkland says, which is to predicate or say something about that which we have distinguished.

Now whereas Checkland tends to speak about people interpreting and/or appreciating situations, Maturana, as we have seen, says that people have a predisposition to explain. Subsequently much of his epistemology is geared towards articulating the basic form of an explanation and the various social contexts in which these arise. The result, as we saw in sections 3.2.1 and 3.2.4 is a quite sophisticated working out of the structure and content of the 'greater whole' from which emerge statements, actions or simply the meanings that observers attach to situations.

The point being made here is that whereas Checkland stops at the point where an observer predicates or 'says something about' a figure ground relationship that is based

on distinctions embodied in a particular *W*, Maturana is much more precise in identifying the basic form that this 'saying something about' takes.

In this regard I submit that *AT* provides a more useful working description of that which SSM, and other problem structuring/reframing methods, purport to explore and change. In drawing attention to the generative process, to the 'informal validity criterion' (or 'preference'), and to the coherences of the resultant explanatory domain, *AT* identifies with some precision, exactly what a user of a problem structuring methodology such as SSM ought to be exploring and revealing. To the extent that an accommodation necessitates change in peoples' viewpoints it identifies what, of the context from which a particular viewpoint emerges, needs to be changed.

Turning briefly to Vickers, Maturana would almost certainly agree with the proposition that people make reality and value judgments. But whereas Vickers epistemological framework stops at that point, Maturana traces the process right back to the point at which someone constructs an explanation to account for their experiences. Thus when an observer makes a reality judgment it arises out of a broader context that is made up of a domain of reality whose coherences emanate from the structure of the generative process and other criteria that are applied in accounting for the original experience. The same happens when an observer makes a value judgment.

This suggests that in making sense of these judgments – in exploring and possibly changing them - the key focus ought not to be on the judgments alone since they are artifacts of a more fundamental cognitive structure. Ultimately what is important is the observer's experience of the problem situation, the generative process that, in his/her experience, accounts for it, the basic preferences that give it credence, and the nature of the coherences of the various domains that are thus created.

### 8.4.2 The origins of people's understandings

The main purpose of SSM is to enable a group of people with diverse and possibly conflicting views about a problem situation to negotiate a course of action that will lead to a sense of overall improvement. Now while much has been said and written about the role played by SSM's distinctive technology in this process, the overall mechanism through which idiosyncratic views are converted into a shared commitment to act is not entirely clear. This is covered in more detail in the next section. At this point my contention is that some of this uncertainty stems from a lack of theoretical clarity in SSM and its supporting literature on what sort of phenomena people's understandings are, where – as phenomena - they are located, and importantly, how they originate.

One thing that Checkland, Vickers and Maturana would all agree on is that people's readinesses to interpret, judge, and explain situations in particular ways is the result of historical and social conditioning. Thus for Vickers the nature of the appreciative setting that applies in any moment is partly determined by reality and value standards that emerge out of previous cycles of judgment. The social dimension is implicit in Vickers' claim that the basic idea of the appreciative setting applies at the social and societal as well as at the individual level.

For his part, Checkland acknowledges that the *Weltanshauungen* that participants bring to bear on a problem situation depend upon their backgrounds and their historical interactions (Checkland and Davies 1986:109-111). In doing so he implies that people's understandings are communally intersubjective (i.e. not individually subjective) phenomena, and that there are social constraints on individual action (1981:216, 218; 1986:111; 1990:192).

Although he acknowledges the existence of these constraints, Checkland stops short of exploring them in any detail. Instead, he places great store by the ability of SSM to provide the catalyst for individual learning, and, "*given the irreducible freedom . . .*

*(of people) . . . to select from a range of possible meanings"* (1981:218), on creativity and change. Others have noted that Vickers leans in the same direction.

> *" . . . Vickers in his pioneering analyses underscored the subjective nature of appreciative systems, this paying attention to the role of individual creativity and experimentation and to the concomitant changeability of appreciative systems."* (Tsoukas 1996:855)

Generally in SSM the aim is to generate and explore a diversity of individual views about a problem situation and free people of the conceptual frameworks that constrain their thinking. It is mainly concerned with individual subjectivities and as such operates almost exclusively at the personal level (see Mingers, 1984). It is particularly strong for analysing and assessing why people experience situations the way they do and exploring ways in which the people involved could view it differently. All of this reveals Checkland's seeming preference for a voluntarist approach in understanding human action and an idealist stance that emphasises the connection between people's thinking and their action.

Having acknowledged the intersubjective and historical rooted-ness of meaning, it is not entirely contradictory that Checkland should then choose to focus almost exclusively on the ways in which SSM can creatively initiate new meanings. Perhaps he is right in thinking that getting on with doing what is possible is better than fretting over what is not. Yet if the aim is to be creative, then surely formulating a strategy for this ought to be based on a sound understanding of the nature and source of the relevant constraints that act upon this process. In any event, while the provision of an unambiguous theoretical statement on the extent to which social factors and previous interactions do and do not constrain the adoption of new meanings may not be absolutely essential for SSM practitioners, surely it is desirable that they know about this.

There are a number of aspects of *AT* that bear upon this issue. The first, which I shall skirt over briefly, takes us back to Maturana's theory of languaging. Recall that

language is the mechanism through which observers make distinctions. Further recall that language is not a means of garnering information about an independent reality that is then processed in the brain. Rather it is an inherently social phenomenon, one that is based on co-ordinations of behaviour that arise in specific networks of structural coupling. As Maturana and Bunnell put it:

> *"Language, as a phenomenon, is . . . a manner of flowing in living together in a path of coordination of the coordinations of coordinations of behaviour. Language happens as a consensual behaviour, it arises in the living as a feature of living together in the particular life that one is living . . . (and) . . . language is not abstract. Language has the concreteness of doings, it is the coordination of doings . . . different words belong to different histories of coordinations, they are not innocent . . . language does not pertain to an abstract domain . . . it pertains to the concrete domain of doings."* (Maturana and Bunnell, 1998:9, emphases added).

This suggests that the changes that SSM seeks to bring about do not stop at the level of people's verbal descriptions. Moreover it suggests that there will always be limits on what change is possible. If the people concerned are being asked to contemplate radical changes in their understandings it is not simply a case of using new words or thinking of new ways of describing something, which is how SSM sometimes comes across. Instead it is akin to asking people to rewrite their own individual and social history. Barriers to individual learning reside not only 'in the head' of the individual, but also in the daily practices and behaviours that comprise the very fabric of the organisational and social contexts in which they live.

Another aspect of this problem is that it is not only a question of ultimately having to deal with the social and behavioural roots of people's descriptions and understandings, but account also must be taken of the closed and conservative nature of the conversations that both reflect and nourish these (see Figure 9). In this context, depending upon the nature of the change envisaged, the task facing any external agent who is seeking to bring about a change in an individual's thinking, whether this be the person facilitating the SSM process, the other participants who are involved, or the client

who commissioned the exercise in the first place, may be very difficult. I have spoken about this before and will pick up on it later.

Looking at the situation from a related, but slightly different angle, Maturana's account of the relationship between the two phenomenal domains that are involved in observing is also helpful. As we saw in Chapter 3, Maturana claims that if we do not look to the origins of observing (the conditions that generate it) we do not see that there are two domains involved – the domain of physiology and anatomy, and the domain of relations with a medium. Moreover, we do not appreciate that each domain has a very specific role to play. We are inclined, he says, to regard observing as an endowment or as a property of the human condition. As a result, we collapse the two domains into one. This, claims Maturana, is a serious logical error, for while the two domains interact they do not superimpose, and importantly, because phenomena belonging to one are not reducible to the other. Simple acts of human behaviour such as walking arise out of the interaction between the body and the medium. Thus the anatomical movements that are involved in walking result in very different observable behaviours depending on the nature of the medium. On this view, all behaviours are relational phenomena that arise when we witness a system interacting with a medium. They are, as Maturana and Varela put it, " . . . *an outside view of the dance of internal relations*" (1987:166, emphasis added).

The same logic applies when people think about, describe, and explain problem situations, as they are required to do in SSM. As Kay (1997) has recognized, these phenomena - including the *W* concept itself - arise in the relational domain and must be explained through what goes on there.

Against this background, there is more than just a hint that Checkland commits the logical 'collapsing-two-domains-into-one' error about which Maturana speaks. Witness Checkland's writings that suggest that there is some direct *W*-related

physiological or anatomical structure - responsible for information processing - hard-wired into the brain. For example, " . . . *we attribute meaning to the observed activity by relating it to a larger image we supply from our minds*" (1981:215 emphasis added), and, "(We have) . . . *in our heads stocks of ideas by means of which we interpret the world outside ourselves*" (1990:19, emphasis added), and " . . . *Weltanshauungen are the stocks of images in our heads . . . which we use to make sense of the world . . .* " (1989:81 emphasis added).

If, contrary to this, each *W* does not have its own localised engram inside the nervous system, then to what extent can such cognitive structures be said to be linked to the individual? On Maturana's view the only such thing that is carried around is a nervous system whose structure reflects past interactions. This structure makes observing possible and it circumscribes what understandings are possible as well as those that are not. Thereafter the specific understanding that emerges is a reformulation of the speaker's experience that takes place in language in the relational domain, in the moment in which it occurs. In a sense then it is a case of meanings residing in relationships. It could be said that meanings lie between people; they do not lie in their heads.

Another important ramification of couching the *W* concept in terms that are more appropriate to the domain of anatomy and biology has important consequences for what is taken to be the key dynamic involved in an SSM debate. Because Checkland regards people's thoughts and descriptions as reflecting the operation of some filtering and information processing structure in the brain, he is forced to explain changes in people's understandings through reference to changes that are occurring to them. It comes as no surprise therefore, to hear SSM being referred to as a 'learning system'. SSM, says Checkland,

> " . . . *embodies a paradigm of learning . . .*" (1981:287); it is " . . . *a learning system*" (1989:78), where " . . .*the outcome . . . is . . . a learning which leads to a decision to take certain actions . . .*" (1981:213).

Obviously SSM can promote learning. Indeed Maturana claims that learning takes place constantly as there is a constant structural transformation of the system in line with transformations in the medium. Because there is a reciprocal relationship between the two domains, what happens in the relational domain can trigger structural changes in the nervous system. Essentially this is what learning is (Maturana, 1988a:74; Winograd and Flores, 1987:44-47).

On the other hand, it would be erroneous to automatically associate shifts in people's thoughts and descriptions with learning. Again based on the theoretical distinction just described an equally plausible explanation is that they merely reflect the specific circumstances of the relational domain in which they occur. Because Checkland does not make the necessary distinction he cannot account for such changes other than to assume that there has been learning.

Summarising this section we can say that Maturana's precise clarification of the two domains that are involved in observing provides a much more robust theoretical basis for understanding some of the nuances and complexities of the constraints and enabling factors that bear upon people's 'understandings' of a problem situation. On this view, observing phenomena such as *Weltanschauungen* are at least explainable through reference to relational phenomena than they are to anatomical phenomena. While people's statements and understandings about a problem situation that might emerge through an interactive process such as SSM are realised through their historically conditioned anatomical structures, essentially they are social or relational phenomena that in any instant will, to a large extent, reflect the circumstances pertaining to this domain. Having made this point we can now examine what these circumstances might be.

### 8.4.3 The diversity to accommodation dynamic

We are told that SSM leads to a shared appreciation of the problem situation or, more commonly (according to Checkland and Davies 1986), to some form of accommodation, although it is not entirely clear how this is supposed to happen.

This is not to say that Checkland is unaware of the complexities involved. He admits, for example, that *"the methodology can orchestrate conflict as well as promote consensus"* (Checkland 1981:17). The aim is shared perceptions, which, Checkland argues, are essential for corporate action. Yet, he further admits that these *" . . . will have to be established, negotiated, argued, tested, in a complex social process"* (Checkland 1989:76, emphasis added). Moreover, Checkland acknowledges that *" . . . getting to (the) . . . accommodation, and to the motivation to action which is an equal concern requires cultural knowledge."* (Checkland and Scholes 1989:44)

According to Vickers, regulative action taken to improve a situation involves comparing notions of 'what is' (measured according to relationships) with notions of 'what could be'. Similarly for Checkland it is a case of comparing one set of predicates in relation to the 'figure – ground' distinction with another set, and comparing these with what is perceived to be the case. In SSM, comparing conceptual models with the so-called 'real world equivalent' mirrors this apparently 'normal' process through which human beings relate mentally to the world.

Thus Checkland and Scholes claim that SSM is just a more formalised version of what we do anyway. Yet if we accept the point made in the previous section that the 'learning' involved in SSM has more to do with group practice and collective action than with what goes on inside peoples heads then – surface similarities notwithstanding - we are speaking about a fundamentally different kind of process. This entails an entirely different set of demands being placed on the people involved, especially the facilitator

The question then, is whether the rather simple dynamic of which Vickers and Checkland speak provides a rich enough conceptual basis for explaining what is involved when people come together for the purpose of debating and negotiating what to do about a complex situation of concern. Checkland (1989:82) speaks about how we have to "*learn our way collaboratively to the most relevant perceptions in a particular situation in order to take action to improve the situation*". But while learning is regarded as being of central importance there is no compelling theory of learning in the SSM literature. Communication and interaction are clearly pivotal to the SSM process, but there are no theories about these. Again it seems to be a case of there being an absence of good theory on which to build good practice in relation to these issues.

Overall there is little in the SSM literature that one might regard as a sound theoretical explanation of how the social process envisioned in SSM leads to the expected outcome. Reaching accommodation is left as some form of trick that the agent must somehow conjure up as best he or she can, and/or as something that for some unexplained reason, magically occurs as a result of learning.

Again this raises the question of how *AT* might contribute. The emphasis in this chapter is mainly on critique so for now I shall confine myself to making a single theoretical point. Considerations of its wider ramifications and, in particular, how it might be employed more constructively, can wait until the next chpter.

In answering the question of how the SSM social process must be managed in order to produce the desired outcome, *AT*'s concept of *conversation* is pivotal. And conceptualizing the SSM process as one such process has wide ramifications.

Reading Checkland one gets the impression that when someone participates in *the* SSM debate, his/her contributions reflect a specific *W* - hard-wired in the brain - that arises out of previous experiences and group memberships. When there is a change in understanding then it means that there has been learning and that a new *W* has taken over.

In contrast *AT* suggests that when people enter the debate they do so having a predisposition to provide a number of different descriptions. The one that emerges in the moment depends upon what happens as the conversation proceeds, i.e. through its flow of languaging and emotioning. Logically, because people can easily oscillate from one pre-existing understanding to another depending on the state of the conversation, it would be unwise to assume that a description provided at one stage in the debate automatically replaces or nullifies earlier descriptions.

Moreover, the description provided depends upon the specific conversation in which the participant is involved. This might be with the other members of the group, with the facilitator, or it may simply be a self-conversation. In this regard it would be wrong to assume that all of those present are participating in the same conservation, or if they are, that the 'strength' or 'modulating effect' of the conversation is the same for them all. Further, it would be wrong to assume that participants do not switch from one conversation to another as the debate proceeds, or that they cannot be involved in a number of different conversations concurrently.

Nonetheless, what happens in the debate can trigger structural change in the participants, and it is the nature of this that largely determines whether or not accommodation, sustainable or otherwise, is possible. In the worst-case scenario there will be no accommodation; if there is an accommodation it may be a somewhat transient and fleeting experience, or it may have greater durability. All of these options are possible. Again I shall put on hold the question of what a knowledge and understanding of *AT* might do to assist in managing this process until the next chapter.

### 8.4.4 The meaning of emergent understandings

The previous section hints at the difficulties that might be involved in arriving at the point where the various participants can agree on a common problem-solution set or

reach an accommodation. These difficulties do not often surface in the various published case studies of SSM. It is reassuring therefore to hear Checkland remark that if his experience is anything to go by, most of those who participate in SSM debates are willing to acknowledge that there has been learning, and the success of the methodology is: " . . . *measured by the readiness of actors to acknowledge that learning had occurred . . .*" (Checkland, 1981:253).

Even so, one still wonders how much credence can/should be given to such apparent shows of faith in the methodology. This is an important matter because claimed insight without observable behavioural change that extends beyond the physical and temporal boundaries of the debate may be greeted with some skepticism. Sponsors in particular may need more convincing especially if they have been asked to commit significant time and resources to a process that might be based on unsubstantiated premises and may conclude without any discernable change or tangible benefits having been attained. It is relatively easy for a participant to say that learning has occurred and/or that his or her views have altered as a result of being involved in the exercise. It is much more difficult to state categorically that there has been change and learning. As Nicholls (1987:239) puts it: "*It is a mistake to think that the right reframing/problem structuring statement transforms experience – not for long it doesn't*".

*AT* suggests that we can only read so much into participants' statements about how much and what they have learned, how they now regard the problem situation, and what they intend doing about it. As we have seen - relative to Maturana - Checkland operates with a very narrow conception of human cognition: one that privileges mentalistic and linguistic aspects over others. The value of the implicit mentalistic conception of learning is also questionable. SSM, it seems, is premised on the notion that learning more about a problem situation is necessary. It circumscribes a process in which participants acquire new knowledge, where there is an internalization of new ideas.

Maturana rejects this model of learning claiming instead that learning is something that goes on all the time, that it is an everyday social practice.

Maturana would not deny that in daily life human beings engage in the sort of cognitive activities such as perceiving phenomena, predicating, comparing, and deciding, that Checkland (1981:214) claims are mirrored in SSM. But these thinking processes tell only part of the story. Behind the scenes there is a great deal more cognitive activity going on. As we have seen Maturana reasons that cognition involves all actions that have to do with maintaining both autopoiesis and adaptation between the organism and a medium. Moreover, on his view learning is structural change that arises out of the mutual triggering of changes in the organism arising out of its recurrent interactions with the medium. As such, learning occurs all the time, not just in formal learning contexts such as SSM. Indeed most learning is done in everyday settings, in networks of structural coupling or in 'communities of practice' (see, for example, Fox, 1997:730). In this regard, SSM is a contrived, and possibly relatively uninfluential learning environment.

*AT* claims that learning is a flow of recurrent interactions in which a person's structure changes. In contrast, Checkland seems content for participants to merely acknowledge that learning has taken place. Unfortunately, this does not necessarily imply that there has been enduring structural change. Even when there is significant structural change, participants may not be in a position to accurately describe it. *AT* suggests that we live mostly unconsciously so many of the interactions that occur in SSM will occur beyond what is accessible to the participant through language. If there is structural coupling then all such interactions will participate in structural change. But when a speaker rehearses 'what happened' during the SSM debate, and what he or she has learnt, the listener is hearing a reformulated story of what happened. The listener is not hearing an account of what actually happened because the speaker does not have access to this.

### 8.4.5 The wider sustainability of emergent understandings

Let us now turn to a matter that was flagged earlier. If, through the exercise, the various participants were structurally-coupled, then inevitably they will have changed. Logically they could not now act exactly the same as before. But this is not tantamount to saying that they will necessarily act in the agreed manner. Because the intervention-related conversation is only one of many (recall that the bodyhood of the observer exists at a node of intersecting conversations), contradictions between domains of action inevitably arise. This can involve the observer privileging one and dismissing the other. He/she for example might decide to 'write off' the SSM conversation as something of an aberration. Alternatively the contradictions between the two domains might create *'emotional conflict'* (see Maturana 1988a:53-4). What seemed like a good solution one day can create severe personal distress the next. What seemed like a credible explanation and a noble intention in the SSM workshop may be discounted outside it.

The point is that agreement through the SSM exercise is not the same thing as its realisation through coordinated action later. The successful implementation of agreed outcomes is subject to the on-going commitment of the participants to their shared domain. It follows that in moving beyond agreement to commitment there is a need for some sort of emotional contract. Clearly this is something that the facilitator and/or client must be worked on.

A related problem is that in its transient existence, an SSM intervention cannot serve the purposes of the wider organisational system even if the people involved intend that it do so. It exists in the moment as a situated and bounded set of conversations.

Presumably since he refers to the *Lebenswelt* Checkland does acknowledge the contextual and temporal nature of understandings. But it is not clear that he fully acknowledges the nature of the problem. Certainly he acknowledges that the debate moves on and things change but does he acknowledge the multiplicity of conversations

that are involved? I am less sure about this. The possibility that at some point in time the SSM conversation is the 'active' one for a participant (see Figure 10) does not, in itself, grant it any necessary significance. Indeed there might have been unlimited creative thinking going on, but if the conversation in question lacks the modulating power of others, then its capacity to bring about enduring change is likely to be severely curtailed.

After the debate, the former participants engage in different conversations in different networks of structural coupling. They serve a wider purpose as they resume roles as members of the organisation. We can take from this that as a minimum if agreements are to persist then ways need to be found to ensure that the SSM 'conversation' somehow continues beyond the physical and temporal boundaries of the SSM debate itself.

### 8.4.6 Bringing about tangible change in a target audience

I have been suggesting that people often see things differently as a result of their participation in SSM-type interventions because the conversational flow has moved on, not necessarily because there has been fundamental change i.e. learning. However, if successfully carried out, there will be learning and this will extend from the individual to the group.

The next problem is that SSM has little to say about how such learning should be transmitted outside the immediate group of people involved and transformed into collective action on the part of the target audience. Again Checkland acknowledges the problem. It is extremely important, he says, *"to consciously to think out and articulate the process by which the new structures and processes will be broadcast, explained, argued, internalized, implemented, made to work"* (1990:266). Beyond that SSM has little to say about how individual learning is transferred into an organisational context.

One potential problem is that if there is consensus (either about what to do, and/or about how others are likely to respond to possible actions) there is a tendency to assume that the group has 'got it right'. Yet consensus is never static, neither does it imply objectivity. As someone once said, 'consensus is apt to shift when voting rights are extended to different groups of people'.

In referring back to Vickers' idea of an appreciative system Checkland (1981:181) does admit that 'attitudinal' change, in particular, can be difficult. Hence, as a general principle, he reasons that where possible participants ought to carry out the feasibility debate not in isolation, but with the people concerned. Thereafter: *'once changes have been agreed implementation of them may be relatively straightforward'* (Checkland 1981:182). Indeed it may. But clearly it will not always be possible to involve all of those who are the target of a change initiative, which means that there will be occasions when it is necessary to attempt to directly change people in the situation. Equally there will be occasions when changes are agreed to by a particular group of stakeholders – senior managers perhaps – who agree on a course of action that involves other people not involved in the debate. In these circumstances the absence of an adequate theory of change/implementation becomes a matter of concern.

### 8.4.7 Ethics

Finally there is the important matter of the ethical basis of the intervention. Surprisingly since Checkland clearly appreciates the contended nature of a term such as 'improvement', SSM does not appear to have a sound ethical foundation. Indeed, Atkinson (1989) refers to ethics as a 'lost dimension' in SSM studies. As we have seen, SSM is held to be an approach that can assist people in determining what to do. Ultimately though, this is an ethical question. Conflicts over ends or values cannot be resolved through learning more about a problem situation.

Whereas hard systems methods, and a functionalist reading of the VSM, can side step this issue on account of the agent's supposedly objectivity, SSM cannot. Because it accepts the legitimacy of different perspectives, SSM must take a position in relation to the possibility that actions arising out of a particular *W* may have serious implications, and possibly adverse consequences for others. In that sense, it could be said that SSM needs a critical dimension.

Indeed Checkland and Davies (1989:19) claim that such a dimension can be accommodated in SSM. They do not elaborate, however where ethics fits into this picture. The closest that Checkland gets to answering this question is in his description of the reformulated version of SSM. Here, he and Scholes invite potential users to consider assessing the *ethicality* of conceptual models " . . . *if it seems appropriate in a particular field.*" (1990:42 emphasis added).

So ethics it seems is optional. *AT*, in contrast, brings ethics fully into the spotlight. Because all knowledge is bound to distinctions made in the cognitive domain of observers, it is ethics, not objectivity that is the foundation of change. Anyone who intervenes in the lives of others has to weigh their actions alongside the claim that *"all manners of living are operationally legitimate"* (Maturana 1991b:89). By this Maturana means that when objectivity is bracketed, all rationalities are indeed operationally legitimate in the biological domain, i.e. any set of distinctions made by a person's nervous system is legitimate, but in the human domain of coexistence, which is a domain of values, it becomes a matter of desirability and preferences.

On this view, no one can claim any transcendental objectivity as a foundation for influencing others. Instead, people have to take personal responsibility for their preferences, for what they espouse, and for their actions. Improvement arises not as universal betterment but only to those who would define it as such. And people will only

define it that way because it makes sense to *them*, given *their* history of interactions, according to the conversations that are taking place within *their* praxis of living.

## 8.5 Summary

In summary, SSM's remit is to transform a diverse set of perspectives into some form of agreement, or into an accommodation. This takes the user and participants into the domain of human descriptions, meanings, explanations and worldviews, as well as into the social processes that sustain these.

Against this background, I have argued that there is lack of clarity in the SSM literature, in relation to the following. Firstly, it does not have enough to say about the precise nature of the cognitive structures that underpin people's descriptions. This makes it difficult for a user to know exactly what it is that he/she should be seeking to reveal, to explore, and possibly change. Secondly, the literature is not sufficiently clear about the relative balance between individualistic creativity, which SSM strongly emphasises, and the social and historical processes that underpin people's descriptions which Checkland acknowledges. Emphasising the former while acknowledging the latter would appear to create something of a tension, if not an outright contradiction. Thirdly, I have argued that in SSM the underlying social dynamic through which diverse perspectives are apparently transformed into an agreement or into an accommodation is by no means clear. In SSM this is supposed to happen through a collaborative process, but there is no detailed theory of interaction on which it is based.

In two out of these three cases I have shown how in principle *AT* can assist in providing the degree of theoretical clarity that is required. Discussion on the third is deferred until the next chapter.

Beyond this the chapter has raised other concerns about SSM. These include: the meaning and significance that ought to be attached to any emergent understandings, in particular to people's willingness to acknowledge that learning has occurred; secondly,

there is the matter of the sustainability of emergent understandings beyond the boundaries of the debate, in particular the issue of translating individual learning into wider organisational learning; and finally, there is the issue of Checkland's failure to develop an adequate ethical theory which I have suggested is necessary given SSM's philosophical acceptance of the notion that in any situation there are multiple legitimate viewpoints.

# 9. DEVELOPING SOFT SYSTEMS INTERVENTIONS

## 9.1 Introduction

Whereas the previous chapter began by speaking about some important resonances between Checkland and Maturana, this one begins by focusing on a key difference. It revolves around what they are and what they do. First and foremost, Maturana is a scientist whose "passion", as he describes it, is to explain phenomena. Checkland's primary interest has more to do with doing. His affinity is for intervening in situations in order to bring about an improvement. He then reflects on these interventions and writes about them.

That there are these similarities and differences between Maturana and Checkland raises the possibility that a complementary application of their respective ideas might be possible. Philosophically they have enough in common to counter any suggestion that such comparison would be akin to comparing apples with oranges, or that such comparison would be a futile cross-paradigm dialogue. Equally, their distinctive focus - one mainly (but not entirely) a thinker, the other mainly (but not entirely) a doer – provides sufficient contrast to hold out the promise that dialogue between them might be beneficial.

In this vein, the following chapter considers three main areas in which *AT* can be used to assist and/or further develop SSM. Firstly, it can help in providing a possible way out of a philosophical contradiction that some critics claim to have identified in it. Secondly, *AT* can provide support for some key principles of SSM that Checkland admits have mainly been developed experientially and which lack theoretical backing. Thirdly, and most importantly, *AT* can provide assistance on areas of SSM that, relative to its main purpose, I shall be arguing are currently under-theorised.

## 9.2 Clarifying the philosophical basis of SSM

Borrowing heavily from Vickers' notion of an appreciative system, Checkland claims that in daily life individuals act purposefully having considered alternative hypothetical forms of relevant relationships. This process is manifest in that part of SSM where participants are required to compare nominal models of 'human activity systems' with corresponding aspects of the problem situation. We are told (Checkland and Scholes 1990:300), that to all intents and purposes, this is how human beings think purposefully in daily life.

A number of critics (see Davies 1992, Fricke 1997, Rennie 1989), have attacked this aspect of SSM on the grounds that it reflects philosophical contradictions and tensions in the methodology. The essence of this critique revolves around the claim of Checkland and others (see, for example, Galliers, Mingers and Jackson 1997), that SSM is 'the most sharply expressed' of the interpretive phenomenology-inspired systems approaches. Critics dispute this, pointing to what they regard as an unacceptable dualism in which elements of classical objective reality sit side by side with elements of phenomenologically created reality. Critics claim that this 'reality confusion' is philosophically unsound in that it breaches the so-called paradigm incommensurability thesis (Burrell and Morgan 1979), and is inappropriate in a methodology that claims allegiance to hermeneutics and the interpretive paradigm.

McLachlan (1995) voices a related, but more pragmatic concern. He provides empirical evidence to support the notion that because the thinking that is used in successive SSM sessions may derive from incommensurable philosophies of science, the process can be highly confusing for participants.

Davies (1992) questions whether there is any fundamental difference between nominal models in SSM that are explicitly observer-dependent, and the so-called 'real worlds' with which they are compared. While Checkland reminds users not to confuse

the two activities, Davies claims that such advice is based on a false dichotomy because ordinary language, she suggests: "*creates discourses of reality which are as relevant as the discourses created for abstract conceptualisation*" (1992:603). This echoes Berkeley's famous dictum that human beings can compare ideas only with other ideas but never with what they are supposed to represent (see, for example, Magee 1987:139-143).

To the extent that Maturana believes that people's 'real worlds' are constituted through acts of distinction brought forth through language, then he would concur with Davies' suggestion that fundamentally there is no difference between conceptual models and that with which they are compared. At the same time, he argues that while some of our realities are lived as if they exist independently of us, others are 'bracketed', and that we can and do oscillate between the two explanatory paths: *objectivity-in* and *objectivity-without-parentheses.* Ostensibly, this provides theoretical backing for the comparing and contrasting activities that are such a distinctive feature of SSM.

There are however, some important differences between Checkland and Maturana on this point. While Checkland accepts that it is not always easy, he clearly believes that people can intellectualise the difference between the nominal and the real, and they can move between one mode of thinking and the other. For Maturana, shifting from one explanatory path to the other is not an intellectual activity. Rather it occurs commensurate with shifts in the flow of emotions as people participate in conversations with others. This reinforces the point that having the emotional predisposition to bracket reality and maintaining this through the conversation is likely to be a critical success factor in SSM. I shall return to this issue shortly.

Philosophically, it seems unlikely that Maturana would side with anyone who might attack SSM on the basis of the paradigm incommensurability thesis. Instead of regarding objective and subjective conceptions of reality to be fundamentally opposed to one another, I interpret Maturana as saying that these are both part of an objectivist

worldview. Indeed, I interpret him as saying that it does not make sense to speak of subjectivity at all unless one accepts the basic premise that there is an underlying objective reality. On this view, subjective realities are a personal assessment that is made with reference to a supposedly objective reality (see Maturana 1988a, Fell 1993). In this sense, subjectivity belongs with an objective explanatory (i.e. not bracketed) path. It is an essential aspect of objectivity and is not, as the incommensurability thesis would have it, opposed to it.

This raises doubt about whether SSM does indeed stand as an exemplar of the interpretive paradigm in systems thinking. Certainly since SSM privileges the direct experiences of participants, it is consistent with one of interpretivism's main tenets. But this is not necessarily inconsistent with objectivist thinking. Indeed Checkland's lengthy discussion on different 'classes of systems' (see Checkland 1981:110-121) contains many similarities with Bhasker's (1989) *'critical realist'* philosophy. This philosophical position distinguishes that which exists independently of observer, i.e. *'the real'*; that which in principle is observable, i.e. *'the actual'*; and that which is observed, i.e. *'the empirical'*. The same discussion of Checkland's also demonstrates some resonances with Habermas's philosophical position that is based on the distinction between the so-called *'material'*, *'social'*, and *'personal'* worlds (Habermas 1984, 1993).

In passing, it is interesting to note that there are also similarities between Vickers and Bhasker. For example Vickers' *'lifeworld'* (the 'underlying flux of events and ideas') broadly corresponds with Bhasker's *'actual'*, and Vickers' *'perceived facts of the situation'* broadly corresponds with Bhasker's *'empirical'*. To the extent then that there are these similarities between Bhasker and Vickers, and to the extent that Checkland acknowledges his philosophical debt to Vickers, this lends weight to the suggestion that feasibly one could consider Checkland and Bhasker to be on the same philosophical wavelength.

Returning to Maturana, it seems that his 'bringing together' of objectivity and subjectivity offers Checkland a way of responding to critics who submit that objectivist and subjectivist thinking should not be combined in a single approach. Taking a lead from Bhasker and Habermas, and in accordance with Maturana's *objectivity-without-parentheses* explanatory path, the way out is for Checkland to take the line that the social world has the same independently existing status as the material world, the latter which he accepts already. According to this philosophy both worlds exist independently and both (especially the social world) can be perceived subjectively depending on the Weltanschauung of the observer. In other words, Checkland's own philosophy, and that which is manifest in the 'comparing and contrasting' stage of SSM could be presented as being in accordance with the *objectivity-without-parentheses* explanatory path, and/or critical realism.

Although there would still be some resonances between them, this would put Checkland in a different philosophical camp to Maturana. For Checkland *Weltanschauungen* would become a filter through which the observer interprets an independently existing outside world. For Maturana, these are explanatory domains that constitute the world that the observer lives. Whereas the former believes that we humans live in a physical space that we experience in different ways, the latter believes that the physical space exists through us, i.e. things do not exist *as things* unless we bring them forth through the socially-mediated distinctions that we make in language.

In the light of the comments made in this section it is interesting to note that Checkland himself has recently questioned the philosophical and practical value of distinguishing between the 'real world' and the 'systems world' (see Tsouvalis and Checkland 1996).

## 9.3 Providing theoretical support for basic SSM principles

This section aims to show how *AT* can provide useful theoretical backing for aspects of the SSM approach to interventions that have been arrived at not as a result of some theoretical imperative, but as a result of practical application. As Checkland puts it:

> *"The work on the development of the soft systems methodology was, not, during its course, based on any explicit theory. (Basically we) . . . took hard systems engineering methodology as given, applied it to unsuitably soft problems and introduced modifications dictated by the problem situations themselves"* Checkland (1981:260)

Only later, says Checkland, was SSM examined to ascertain what theory was implied by it. During its important formative years then, the development of SSM was guided largely by the exigencies of problem situations and the systematic reflection by Checkland and his colleagues in the light of their experiences in using it.

A good deal of the theoretical support that *AT* can provide, has to with the twin concepts of structure-determinism and structural coupling. Firstly however, it is worth noting that *AT* can provide theoretical support for what is perhaps one of the more defining features of SSM and other similar methodologies. This is that if the intent is to have people reflect critically on their understandings with a view to expanding or changing these, then it is important to temporarily 'remove' such persons from the location where they carry out their daily business

### 9.3.1 Changing the conversational setting

All of what has been said to this point about the conservativism of conversations and cognitive domains suggests that people tend not to learn from their experiences when they are completely immersed in the situation that generates them. Human beings are conservative, keeping to their usual patterns unless their experiential and conceptual pot is stirred (Efran et al. 1990: 184-197)

Temporary excursions beyond the boundaries of a particular cultural setting such as those envisaged in SSM offer no guarantee that there will be substantial and sustainable change. However one can see how by bringing a group of people from diverse backgrounds into a SSM workshop setting it is possible these persons to generate options they would not generate themselves, or in their own cultural setting. Whether the result of this is a breaking of usual patterns of thinking remains to be seen, but it does generate some room for participants to think independently and consider alternatives that perhaps would not have arisen otherwise.

### 9.3.2 Developing understandings through a process of mutual adjustment

In hard systems thinking it is frequently a case of analysing a situation in some detail as a means of arriving at a rational solution. This process mirrors cognitivist logic which holds actions to be the outcome of a process of manipulating representations of an external world.

As we have seen, *AT* operates according to a different set of assumptions about human cognition. It operates on the principle that living systems gradually move towards a position of adaptability to a medium not by working out in advance what should and should not be done, but through the gradual process of mutual adjustment between two or more structurally-coupled systems or between a system and a medium.

To a large extent, this process of mutual adjustment is mirrored in human conversations/interactions generally, and in SSM, in particular. In SSM it is manifest in a 'back and forth' process of interactions/perturbations that continue until some form of accommodation or new understanding is reached; or, until the parties decide that they are not making progress, and decide to go their separate ways. It is a case of the people concerned collectively pursuing questions, trying out plans, and devising new methods. By doing this in an iterative format they gradually discover what works for them. It is not

a case of analysing a situation in great depth and hoping that, through the addition of more and more information, the way forward will suddenly appear.

### 9.3.3 The behavioural and social basis of consensuality

Maturana claims that people's explanations emerge spontaneously as they go about coordinating their behaviours and formulating distinctions with others. Shared understandings are developed and negotiated over time in the course of conversation. The clear message is that there is an underlying action/behavioural and social basis to the process of explaining.

This seems to be what is intended in SSM. Aside from its distinctive philosophical underpinning, the identity of SSM is inextricably tied up with the distinctive set of tools – rich pictures, root definitions, conceptual models – that participants use for doing things collectively. In using these tools, various objects are created as things that can be shown, discussed, examined, and refined. In doing these things, the people involved learn to appreciate other's perspectives on the problem situation and learn about the behavioural coordinations that underpin people's descriptions. This reflects the key theoretical principle of *AT* that 'doing things together' and sharing is the basis for developing shared explanations.

Relatedly, participating in the technical activities just mentioned is a form of structural coupling. It gives the people involved something in common with which they can couple; it allows them to form a novel coupling that opens up the possibility for carving out new distinctions that can point the way forward. In the process, they create new alternatives that had no previous existence or they can revisit previously existing options in the light of new experiences. This is consistent with Maturana's claim that new or recast distinctions expand the possibilities of living. In this sense, SSM does indeed

provide a social context in which it is possible to devise and test new or better ways of doing things.

This raises the possibility that while *AT* can provide theoretical backing for the social process and mode of interaction that is envisaged in SSM, it does not necessarily speak to the importance of the specific technology used. Indeed it suggests that in moving towards accommodation among stakeholders, what is critical is the establishment of a structural coupling and a consensual domain among the participants; the sophisticated technology of SSM is only a catalyst through which this happens. Indeed it may be a relatively unimportant one.

### 9.3.4 Dissolving problems through conversation

The next issue has to do with the question of exactly what it is about SSM that transforms substantive variations in perceptions of a problem situation among a group of stakeholders into a commitment to an agreed course of action. For Checkland this has to do with learning. The distinctive activities that are involved in SSM provide a catalyst through which the persons involved learn their way to a new understanding that they are comfortable with. Yet, as I have already said, there is no explicit theory of learning in SSM.

*AT* suggests that accommodation and a willingness on the part of participants to move forward constructively can be the result of learning. Equally it might be the result of something else. In Chapter 3, I showed that while explanatory domains reflect the structure of the observer, they also reflect the moment by moment flow of the conversation(s) in which he/she is participating. Now the extent to which there is latitude for changes in explanations, depends upon the structure of the observer. Within these limits, explanations change commensurate with the flow of conversations. And since they

are defined in relation to explanations, the same can be said of the "problems" that are the subject of SSM debate and inquiry.

This being the case, establishing an effective conversation opens up the serious possibility that 'problems', to all intents and purposes, might simply disappear of their own accord. Again it raises the theoretical possibility that the distinctive activities and technology of SSM is important, not so much to promote learning, but as a catalyst to facilitate the development of a conversation among the various stakeholders/participants. In other words, it may be the case that when SSM "works", it does so simply because there are people who are engaging with each other and flowing in conversations. In this sense, we might say that SSM creates a conversational flow that allows people's viewpoints to unfold and be observed. Subsequently, depending on the precise nature of the flow, explanations change and previously existing problems can simply disappear.

Relatedly, *AT* provides theoretical support for the practice in SSM of speaking of 'problem situations' rather than 'problems', and its general approach of keeping things open and suspending initial judgements. Where there is a pre-existing focus on 'problems', i.e. when the 'problem' is taken as a point of departure, naturally the inclination is to then work very hard on solving it. If, on the other hand, it is possible to instigate some sort of conversational flow, the problem can either disappear naturally, or its significance reduced.

### 9.3.5 The theoretical basis of SSM's approach to change/implementation

The importance that SSM attaches to making sound political and cultural feasibility judgments in relation to possible actions, acknowledges that external agents do not always have it their own way when it comes to orchestrating changes to human and organisational systems.

Aside from formulating actions against a background of knowing what may and what may not be possible, SSM's approach to change – in theory, if not always in practice - involves a cycle of intervention, assessment of the results, reflection, debate, further interventions etc. Again, this basic principle is highly compatible with the 'enhance understanding through mutual adjustment' process that is implicit in the structural coupling model of cognition. It is also consistent with the idea that the best way of understanding a system's structure is not through detailed analysis but by interacting operationally with it.

The development of SSM as an intervention approach provides a good illustration of this point. While it would be churlish to claim that Checkland has been remiss in working through the theoretical and philosophical underpinnings of SSM, theory has clearly not been the main driving force in the development of SSM. From the outset, Checkland modified the systems engineering used in 'hard' problem-solving where it became apparent that these did not suit 'soft' situations (see 1981:260, 1989:74). Subsequently, the development of SSM has been through modification of the approach in the light of the experience gained. This process is highly consistent with the theoretical principles just mentioned.

### 9.3.6 Life as a continually unfolding present

For Maturana human beings live their lives always-already in an ever-changing present. We formulate our explanations in, and according to, the coherences of the present, and this applies even when our explanations include reference to the past and the future. Checkland does not theorise about this in any great detail, however the modus operandi of SSM reflects the same idea. In speaking about different modes of using SSM, and in reference to Vickers work more generally, Checkland does speak about the on-

going flux of events and ideas from which our judgments arise and within which SSM interventions occur. This is reflected in the way that SSM work and ends:

> *" ... ending a systems study which uses SSM is an arbitrary act: the flux of events and ideas moves on, there are no permanent solutions, and systems thinking has to be envisaged as a process which is in principle never ending."* (Checkland 1989:99).

Again, although the language is very different, there is a basic resonance between the theory of Maturana and Checkland's theorising about the practice of interventions using SSM.

## 9.4 Plugging gaps on under-theorised aspects of Soft Systems Methodology.

In his main works, Checkland establishes and makes explicit the theoretical links between SSM and interpretive social theory. For that reason, it would be grossly unfair to claim that SSM per se is under-theorised. Yet the question still remains as to whether – relative to its main purpose – users and potential users of SSM have access to sufficiently well-developed theory that can provide the basis of successful application. In this next section, I shall be arguing that there is such a gap in the SSM literature, and that *AT* can assist in filling it.

In trying to take some of the complexity out of the process it is necessary to work with a relatively simple image of the key transformation that SSM seeks to bring about. For this purpose we can say that SSM involves a process through which new descriptions of a problem situation emerge that are the basis for new actions. We can surmise that these are underpinned by new or redrawn boundaries to the cognitive domain that fixes both the way the problem situation is viewed and the outcomes deemed to be acceptable as solutions; in other words a new problem – solution set.

Now because, on Maturana's view, descriptions arise in conversations, i.e. in flows of languaging and emotioning between structurally-coupled observers, then it seems reasonable to regard SSM as an exercise in which these boundaries will emerge as a result of conversational negotiation. But conversations do not necessarily just happen. And when they do happen they are not always effective or satisfying to the people involved. Making them happen, and making them effective, requires a good deal of effort and thought on the part of whoever is managing the process, and arguably, on the part of the participants as well.

Given the complex task that SSM is designed to carry out, users need more than a bag of tools and an understanding of the basic philosophy behind SSM. They need to have an acute understanding of what human beings are, how they operate and how they interact with one another. In this context, the value of *AT* rests on its ability to produce insight into the conditions that are necessary to generate an accommodation or shared understanding of the problem situation in the first place, by providing insight on the likelihood that such a understanding will emerge, and more generally by providing insight into how a facilitator might be proactive in dealing with the various demands that obtaining accommodation places on the various people involves. In the SSM literature, reaching accommodation is left as some form of trick that the agent must somehow conjure up as best he or she can, and/or as something that for some unexplained reason magically occurs as a result of learning. I shall be arguing that it is not that straightforward.

### 9.4.1 Getting people to participate and to 'bracket' objectivity

The most basic challenge facing the agent is getting people to participate in the debate in the first place. Maturana's idea that we are always flowing in emotions and it is emotions that allow us to enter one kind or other relational domain explains something

that all experienced SSM users will know, but inexperienced users may not. This is that we cannot assume that the people who one might wish to participate will be inclined to do so. Davies (1990) speaks eloquently about the seductive power of systems approaches, and she is right. It is very easy for someone to become seduced into thinking that an approach that is intuitively attractive to a user or has worked for him/her in the past, and/or in a different setting, will be viewed similarly by others. A good deal of effort may need to be put into demonstrating to the persons concerned that participating in the debate will be satisfying and rewarding.

Assuming people are prepared to be involved, the next challenge facing the facilitator is to ensure that they are willing to suspend prior judgements and be open to the possibility of generating new meanings about the situation under discussion. Because, on Maturana's view, an accommodation can only arise as a result of conversational negotiation, and because conversations depend upon people participating under the emotional state of *'mutual acceptance'*, it follows that the attitudes, demeanor and epistemological predilections that the participants bring to the SSM debate can drastically effect the outcome.

According to *AT*, not only does emotion allow us to enter one kind or other relational domain, but it also gives 'character' to what we do. Thus in practical terms, unless people are inclined to bracket objectivity then the recurrent interactions that are necessary for a conversation to take place will simply not occur.

If they are operating in the *objectivity-without-parentheses* explanatory path, creating movement in terms of how people view the world is difficult, if not impossible,. There may be a 'conversation' of sorts, but if people participate under an emotional state that assumes that they have access to the truth, or that they 'know best', then it may be difficult to reach an accommodation, let alone to arrive at a shared appreciation.

People must be open to new possibilities, and be willing to learn from each other.

If the aim is to create a domain of consensual coordinations of actions then, as Maturana puts it,

> *"All that is needed . . . is that the participant living systems should already have at their first encounter the necessary structural disposition for their recurrent interactions to take place, structural plasticity in the domain of their interactions, and the initial structure that allows them to conserve organisation and adaptation while their structures change under recurrent interactions."* (Maturana,1988a:46)

Reflecting on some of the critical success factors involved in using SSM, McLachlan (1995:174), notes that some participants 'get' Checkland's epistemology, while others cannot simply cannot move beyond the idea that there is a real world to which they have access. Indeed, in speaking of the difficulty that he has had with conveying basic ontological and epistemological distinctions to students arriving at his classes from the Open University, Checkland (1992) acknowledges the problem himself. Given his seeming success in using the methodology, one can only assume that Checkland has worked out ways of ensuring that his workshop participants arrive with, or somehow acquire, the appropriate epistemological and ontological understandings.

In practical terms, a user of SSM must find ways of ensuring that participants 'bracket objectivity' such that any disagreements about the situation of concern are couched more in terms of personal experiences and preferences than in terms of what may or may not be 'really going on'. Because people who enter the debate thinking that they 'know the facts' close off other possibilities, ways must be found of avoiding the mutual negation and conflict that can occur when people operate from the conviction that they have access to the truth.

Quite how an agent might best achieve this outcome is still something of a mystery. It is always likely to be something of a hit or miss affair. Yet there are clues to be extracted from Maturana's work. One such clue revolves around the proposition that although for much of the time in daily life we live our understandings as though they

mirror the world 'as it really is', we routinely switch from an explanatory path that assumes an independent reality to one that does not, in accordance with changes in the flow of our emotions. Basically we move from one to the other, according to how we relate to the other person. If the relationship is important to us then we are more inclined to bracket objectivity than if it is not. Subsequently we participate in the relationship in full awareness that we are neither objective, nor that we own the truth. We may not agree with what the other has to say but we are willing to listen, and we are willing to give the grounds for the validity of what we claim. If the relationship is the important thing, then the conversation can continue, even in situations where there are fundamental disagreements.

> *" . . . the reality we live depends upon the explanatory path we adopt, and this in turn depends on the emotional domain in which we enter at the moment of explaining. Thus if we are in an assertive mood, and we want to impose our views on the other without reflection, de facto negating him or her, or if we are directly in an emotion that negates him or her, we find ourselves operating in the explanatory path of objectivity without-parenthesis"* (Maturana 1988a:41)

It seems then, that to a large extent, the task is one of fostering good relationships. The message is that the SSM user ought to give serious attention to how he/she might instill a sense of maintaining and enhancing the relationships between the various participants. Specifically the agent needs to assess the extent to which there is already a positive relationship among the key participants and the extent to which there is a desire that they remain so, or can be improved. In the absence of these conditions, ways must be found to ensure that a feeling of goodwill can be developed, if only temporarily.

The main point, is that for a conversation to take place the participants must be prepared to bracket objectivity, at least for the duration of the SSM exercise. Daily life experience suggests that people can and will do this if they value the relevant relationships. In the absence of such goodwill, the interactions that are necessary for a

conversation to take place at all (and which are necessary for a shared understanding to emerge), will simply not happen.

### 9.4.2 Creating and managing effective conversations

Assuming that the various participants enter the intervention not under duress, and assuming that they are willing to bracket objectivity, then the next challenge facing the facilitator – arguably the most difficult - is to create a conversation and manage it effectively. I contend that there are three related aspects to this and I shall deal with them individually.

The first aspect arises specifically out of the proposition that conversations take place in networks of structural coupling. This means that the participants must be structurally-coupled both with each other and with the broader debate and the various technical activities that sustain it. According to Checkland and Davies:

> *" . . . the view of social reality implicit in SSM is that it is a process . . . in which participants continually negotiate and renegotiate with others their perceptions and interpretations of the world outside themselves. This model of social reality . . . clearly depends upon processes of communication . . . between members of social groupings"* (Checkland and Davies 1986:111, emphasis added)

None of which is likely to happen unless the various participants are structurally-coupled with one another. Indeed, since, on Maturana's view (1988:193), the constitution of social 'groupings' (Checkland and Davies' term) entails the reciprocal structural coupling of the components; it does not make sense to speak of SSM as a social system unless this condition is met.

In practical terms for one person to have any influence at all on another, the two must be structurally-coupled.

> *"No doubt we would like that the other hears what we say, but that does not happen unless we have been interacting recursively with each other sufficiently long to have become structurally congruent in a way that results in that we become capable of coherent behaviour through talking with each other."* (Maturana 1997:1)

In other words, the structure of each participant must be such that the other participants and the various technical activities involved can trigger structural change. 'Participating' in the debate is not enough.

Since none of this can be expected to happen automatically, it means that resources permitting, the facilitator ought to aim at understanding the structure of the various participants in order to promote effective interaction. All that was said previously about being a 'scholar of structure determinism' applies here. There is a need to know what triggers people, to recast questions and statements in such a way that these are capable of triggering a response. There is a need to develop a clear understanding of the various participants, to understand their meanings, their values and priorities, and promote a style of communication that takes place on their terms.

The second aspect, closely related to the first, is manifest in the following quotation:

> *"Managing conversational moods – meanings and emotions – is vital for creating the space for effective interaction. Awareness of the mood of a conversation is achieved by actively listening to the other person, assessing the motivations revealed by their statements, working out our own expectations and being aware of the cultural context in which the conversation is taking place. These are all conversational skills necessary for effective interaction."* (Espejo et al.1996:72)

In this regard, the role of the facilitator is akin to that of a highly skilled conversationalist who, in a participant-manager role, has to create a space for and facilitate what Goolishian and Winderman (1988) refer to as a 'dialogical conversation'. This involves creating a group climate in which all of the participants feel that they have a significant role to play, and that their views of the problem situation will be treated as valid by others. Once this has been accomplished he/she has to coordinate however many conversations there are, as the SSM process develops.

Now we come to the third, and arguably most distinctive *AT*-related aspect of creating and managing an effective conversation. This relates to the essential purpose of SSM.

As I have said, if it is possible to establish and maintain a conversation then the basic conditions exist for an accommodation to emerge spontaneously. In social systems generally however, this is not something that happens overnight, and it is prudent to assume that in typical SSM interventions, which often only extend over a few days and/or two or three workshops, there may not be enough interaction/time to allow this to happen.

This opens up the possibility that a facilitator might take a more proactive role in moving the participants towards an accommodation. In SSM, since Checkland (1981:17) admits that the approach " . . . *can orchestrate conflict as well as promote consensus*" it becomes doubly important. Presumably in most of the situations where SSM is used, exacerbating existing conflicts is not a desirable outcome, and it would therefore be helpful to know how it might be avoided.

In the context of this more proactive conception of the facilitators role, application of *AT* ideas in the domain of family therapy (Dell, 1982; Hoffman, 1988; Efran and Lukens, 1990; Mingers, 1997; Bilson, 1997) assume some relevance.

The most basic point of comparison between family therapy and SSM is that in both cases there is disquiet about a situation, there is a sense that an improvement is possible, and that the people concerned need help in finding it. In family therapy, the object of the exercise is to work with existing family members to bring about a sense of improvement. This involves intervening to bring about a change in the conversations that, on this view, define the organisation of the existing family, or constitute it.

Broadly speaking, the role of the SSM facilitator mirrors that of the family therapist. But there is an important proviso. Effectively the task facing the therapist is to 'replace' one family with another. Contrast this with SSM where the aim is to take members of not one 'family' (i.e. 'constituents') but a number of these, and to create a new one.

In looking at what can be done, it is appropriate to return to Maturana. He proposes that a change in a network of conversations might occur in two ways:

> " . . . a) through the encounter with other human beings in a network of conversations that do not confirm it, or through the experience of situations that do not belong to it; and b) through interactions that trigger in us reflections upon our circumstances of co-existence with other human beings." (Maturana 1988a:70)

Frequently (as I implied in section 9.3.1), the first of these two cases is implicit in the very act of 'doing' SSM. Broadly it is the equivalent of what happens: "*when we encounter . . . foreigners, either when on a trip abroad or when visitors come to us, or when we move beyond the normal ranges of our community*" (Maturana 1988a:70). However, in the second case, direct intervention will often be necessary. And it is in this context that the family therapy experience is worth taking a look at.

By definition, reaching an accommodation will often involve having to confront and modify the very understandings that made the intervention necessary in the first place. However, as I have now said on many occasions, the various social groups to which the participants 'belong' (the various 'families') have internal processes that are conservative and closed

> *". . . as a consequence of their manner of constitution, cognitive domains are closed operational domains: an observer cannot get out of a cognitive domain by operating in it. Similarly an observer cannot observe a cognitive domain by operating in it"* (Maturana 1988a:61).

Although Maturana would not see it as an *autopoietic* process per se, the above proposition does resonate with Luhmann's use of the term autopoiesis that I spoke about in Chapter 7. Recall that Luhmann develops the basic idea of autopoiesis to account for the possibility of conversations exhibiting inherent circularities. The results of conversations give rise to further conversations, so that self-amplifying feedback loops are formed. The closure of the network results in a shared system of beliefs, explanations

and values – a context of meaning – which is continually sustained by further conversations, and which is very difficult to break out of (see also Capra 1996:207).

Not only are these conversations difficult to break out of but, more importantly (in this context) they are highly resistant to direct change from the outside. Yet an external agent/facilitator can interfere with these self-generating processes.

Accordingly, this is done by acting *'orthogonally'*. An orthogonal interaction occurs when an external agent seeks to bring about change by interacting with a subject outside the closed domain of the social system to which he/she belongs, but within the domain of his/her wider experiences. Since the aim is to trigger new conversations it is important that through his/her interactions the therapist does not confirm existing understandings. On the other hand, if interactions are to promote accommodation and have a positive triggering effect they must be within the subject's wider domain of experiences. If the subject perceives interactions such as: "how about we do 'X' or 'Y'?" to lack credibility, to be impractical, unrealizable, undesirable or threatening, then the odds are that they will not respond positively.

Applied in relation to SSM, knowing where the boundaries of what this 'zone of orthogonality' are, presents a serious challenge to the facilitator. It puts a great deal of onus on him/her to become familiar with the patterns of actions that define existing conversations and to appreciate the participants' wider domains of experiences. Having achieved some familiarity with these it then becomes necessary to know how, when and what form orthogonal interactions will take place. Inevitably many decisions of this sort will need to be made spontaneously, often in real time, and in a group setting. However this does raise the possibility that this form of interaction might be more appropriately carried out working with the various participants on a one-on-one basis, and for that reason more consideration may need to be given to finding the optimum balance between individual and group-based SSM activities.

If effective orthogonal interactions can be achieved, it provides the basis for something of a 'manufactured' accommodation between the various participants. More ambitiously, it may produce a conversation whose modulating effect on the structure of the participant is sufficiently strong that he/she exits the intervention wanting to participate differently in the 'family' conversations that were the source of the original difficulty. Under this circumstance we can speak of learning having taken place. I shall return to this point shortly.

Summing up the previous discussion leads me to question the wisdom of Checkland's declaration that there is no great mystery in using SSM. He comments that while someone who is familiar with the various technical activities such as naming human activity systems and building models, " . . .*can greatly facilitate a study*", the most important aim of such a person is to " . . . *give away the approach, to hand it over to people in the problem situation . . .*" (1990:10)

In the context of the argument developed to this point, I have serious misgivings about the wisdom of this advice. Elsewhere, Ormerod (1998:428) has questioned the ability of management scientists and operational researchers to take on the role of facilitator, and it is hard to imagine that members of organisations more generally, are any better equipped to take on this role. It is one thing to claim that the form of thinking embodied in SSM is what human beings do anyway when they are involved in 'serious organised thinking' (Checkland and Scholes 1990:300). It is another thing entirely to leave the complex social process envisioned in SSM in the hands of a non-expert. In systems terms, a group is very much more than the sum of the parts. The addition of a social dimension brings much more complexity into the situation than if we are merely speaking about someone using SSM as a tool to impose some order and structure on their own thinking about some situation or other.

The main point of this section is to draw attention to what I believe is a significant gap in the SSM literature. While both Checkland and Vickers acknowledge that the shared perceptions which are essential for action, are established, negotiated, argued and tested in a complex social process (see Checkland 1989:77, Checkland and Davies 1986:111), the SSM literature itself contains very little that an inexperienced user of the methodology might draw upon to assist in facilitating and/or managing this process. The aim is to create greater understanding by expanding the domain of experience in which a preferred explanation is embedded or in which a new explanation can emerge. However, expanding these experiences depends on the structure of the observer; participants must be open to new experiences; it also depends crucially upon the quality of the conversations and on how these are managed.

Checkland places a good deal of emphasis on the participative nature of the methodology, but the rich theoretical perspective that is necessary in order to fully comprehend the issues involved is largely absent. In this context I submit that Maturana's theoretical insights can usefully complement the necessary conversational, facilitation, and interpersonal skills that are usually developed through appropriate experience. Theoretical understanding is important, if only because it forewarns the person concerned about the kinds of issues that he/she may have to confront. And it gives them the opportunity of thinking in advance about how best to proceed.

### 9.4.3 Arriving at a workable plan of action/making feasibility judgments

The previous section has highlighted the need for a theoretical language that is sufficiently rich to allows users of SSM to gain insight into the key conversational processes that underpin the development of shared understandings and accommodations. This theoretical need arises as a result of the importance that is attached to carefully managing the process through which these arise. We now come to the question of

whether SSM is adequately theorised in relation to what is arguably the main purpose of all intervention methodologies, i.e. taking action in the world. The question is whether SSM is adequately theorised in relation to maximising the chances that the people involved will come up with an action plan that is capable of producing positive outcomes.

To some extent, Checkland downplays the necessity of having to demonstrate that the methodology 'works' in the more traditional sense of bringing about substantive organisational change. As we saw in the last chapter, for Checkland, the key criteria of SSM's practical success depends upon the willingness of actors to acknowledge that learning has taken place. This is consistent with survey data which suggests that SSM is mainly used to enhance people's understandings, not necessarily to bring about change per se (Mingers and Taylor 1992).

Be that as it may, clients who are being asked to invest large sums of money and resources in an open-ended process, might reasonably view insight without observable behavioural change with a certain amount of cynicism. And, as Checkland and Scholes (1990:299) themselves point out, when people hear about SSM (or any other methodology for that matter), they naturally ask: is it any good? does it work? Moreover, in mentioning scientists and engineers who *"tend to overemphasise the importance of logic, and fail to notice cultural aspects which in fact determine whether or not change will occur"* (Checkland, 1989:97, emphasis added), Checkland shows that he is not blind to the importance of achieving substantial change as well as learning.

To this end, the original 'seven-stage' SSM requires that there be a systematic assessment of the political/cultural feasibility of 'systemically desirable changes'. In later versions of SSM, some rudimentary tools are provided for this purpose (see below).

If we assume that a successful intervention partly depends upon the quality of the feasibility judgments that the participants arrive at, then the question arises as to whether SSM participants have access to the theoretical and conceptual knowledge that is required

to adequately perform this task. Indeed, I fully concur with Checkland and Scholes judgement that if we are going to intervene in human affairs and grapple with their full complexity " . . . *we had better have available some ways of enquiring into the 'systems of myths and meanings' which constitute what we mean by a culture.*" (Checkland and Scholes, 1990:45)

In order to assist those involved Checkland and Scholes suggest that they should analyse the problem situation in relation to three key interacting elements: *'roles'*, *'norms'* and *'values'*. Now while there is little doubt that these conceptual tools will almost certainly provide useful descriptive information about the problem situation, it is not altogether clear whether they are sufficiently rich to enable the necessary feasibility judgements to be made, or whether these concepts are indeed the important ones.

According to Checkland (1981:180), changes of three kinds are possible: changes to structures, to procedures, and to 'attitudes'. Since the third of these involves changing people directly, it presents the greatest challenges. However since structural and procedural change always takes place in a culture, attitudes are an important consideration in relation to the other two change targets. Thus Checkland claims that changes will be implemented only if they are perceived as meaningful within that culture, within its worldview. Put simply, the feasibility of change options need to be assessed in relation to how the change will be interpreted by the people involved.

Consideration of *AT* leads me to conclude that the key feasibility judgment to be made is the extent to which the target audience regards the proposed change to be a legitimate action. If it does, then we can surmise that the change is relatively feasible; if it does not, then it may be necessary to consider intervening in the processes that lead the target audience to this judgment. Quite how much effort is put into this will depend upon the resources available to the participants. This is an important consideration, because as we have seen, perceptions of legitimacy are inextricably intertwined not only with

people's explanations, but also with the conversations that sustain these and the behaviours that underpin the conversations.

In this context, Maturana's claim that emotion is the central aspect in a culture is pivotal. If the aim is to change people's understandings it needs to be borne in mind that rationalities are *". . . grounded on basic premises adopted through our emotioning. . . that we live . . . as manners of existence . . . as all-embracing manners of being . . . "* (Maturana 1988a:62). Rational domains are emotionally grounded; we therefore move from one rational domain to another emotionally not rationally. This means that the degree of legitimacy that the people concerned attach to the proposed change depends upon the size and strength of the 'emotional gap' between the existing situation and that which is proposed.

The same can be said of the target audience as was said about the SSM participants: commitment to the realization of the outcome of the debate is likely to occur only when the people involved can relate the ideas for change to their world of day-to-day experience.

Feasibility judgments then, are the result of an assessment of people's emotional attachments to pre-existing rationalities; to the strength and durability of the conversations that nourish and sustain these; and to the extent to which daily routines, practices and organisational structures that reflect dominant conversations will support or frustrate the envisioned change. This final point arises out of Maturana's theory of languaging that reminds us that the operational mechanism through which people 'understand' their worlds – language - is grounded in concrete day-to-day coordinations of behaviours, routines and 'doings'. These arise naturally in social settings where they are an integral part of the coherences of the situation. In such a context they are what they are because they work; they are viable. It follows that any proposal that involves changing (directly or indirectly) people's understandings and behaviours must be

assessed in relation to the coherences of the situation in which it is proposed that they be introduced. When people's understandings are embedded in organisational practices they cannot simply step outside of these by going along with some new preferred viewpoint or belief system. It further means that the circumstances in which the understandings have developed and the material practices in which they have become inscribed need to be a central consideration in making the necessary feasibility judgements.

### 9.4.4 Beyond the debate: putting the plan into action and delivering tangible outcomes

In section 9.4.2, I argued that it is possible for people to learn their way towards an accommodation and to agree on actions in the protected environment of the SSM debate. Ultimately though it is what happens in the target problem situation that counts. By making political and cultural feasibility judgments it is possible to foreshadow likely consequences, and I have suggested that *AT* can assist here is highlighting what factors need to be borne in mind in forming these judgements.

It goes without saying however, that there can never be any certainty in the conclusions that are reached. How others will react and what the likely outcome will be are difficult to predict. This is why the iterative aspect of SSM is so important; it allows for a gradual understanding of the target system to emerge not so much through intellectual means but through direct operational interactions.

According to SSM logic, having assessed the feasibility of various systemically desirable changes, the participants then must agree on a course of action. At this point implementation becomes the primary concern. Clearly, beyond debating and defining changes the aim of SSM is to do something about a situation (Checkland and Scholes 1989:52), and as I have said in previous chapters, beyond modeling the implementation process using SSM itself, there is little guidance provided on dealing with this matter.

Much of what, in a practical sense, *AT* has to say about this has already been said in Section 5.3.2 where I traversed the matter of implementation in *MS* more generally in some detail. Here I shall simply extend the line of argumentation developed earlier in this chapter in relation to the possibility of external agents intervening in the process through which meaning is constructed. The point is that all of what was said about a facilitator acting orthogonally in relation to the SSM participants applies equally to the various target audiences. It follows that an intervention strategy ought to be crafted with this in mind.

More to the point perhaps, is what the participants themselves might do. Here the argument is that since neither facilitators nor change agents can be a member of the target organisational system and cannot structurally couple with it in the same way as its members do, the task is to <u>indirectly</u> interfere with these self-generating processes by changing those members who they can interact with: in this case the SSM participants. If the coupling between the individual concerned and the SSM process is sufficiently strong and if there has been sufficient orthogonality in the interactions, then the individual concerned " . . . *stops being conservative of the social system . . . and becomes heretic in it*" (Maturana 1988a:70). This is a major source of change in target systems but it does not come easily. Obtaining the necessary degree of structural change in the participants places a very high premium on the facilitator successfully creating and managing the conversational process according to the principles that have just been discussed

## 9.5 Summary

In this chapter I have argued that *AT* can develop SSM in three major ways. Firstly, I have suggested that it can assist in resolving a possible philosophical conflict that a number of critics have claimed exists in SSM. It does this by linking objectivity and subjectivity together according to the logic of Maturana's objectivity-without-parentheses explanatory path in manner that dissolves the objective – subjectivity duality on which rests the validity of the so-called paradigm incommensurability thesis.

Secondly, *AT* can assist by providing a theoretical explanation for a number of aspects of SSM's distinctive approach to interventions that have arisen through practical application rather than as a result of some theoretical imperative. To some extent it could be said that *AT* provides the sort of theoretical explanation that explains why SSM works when it does, and why, at times, it does not work. These explanations have to do with notions about how shared understandings develop as a result of the mutual adjustment of participants, about the behavioural basis of consensuality, about how - if an effective conversation can be established - problems can 'disappear' of their own accord, and about how a gradual understanding of what 'works' in a target system arises as a result of 'operational interaction' with it.

Thirdly the chapter has argued that *AT* can assist by providing guidance on aspects of SSM practice that, relative to its purpose, I submit are currently under-theorised. Checkland places much emphasis on the <u>participative</u> nature of the methodology, yet the rich theoretical perspective that is necessary in order to adequately understand and manage the process effectively is hard to detect. Based on an explanation of how human beings construct their realities, how they learn and how they change, and how they interact with their environments including each other, *AT* demonstrates the importance of a user of SSM thinking carefully about a number of aspects of SSM that

extend way beyond the boundaries of the specific task requirements facing him/her. These include thinking about what needs to be done to get people to the debate with a 'positive' attitude, thinking about how to create and manage an effective conversation, thinking about what is involved in making 'intelligent' feasibility judgements, and thinking how the change team ought to operate after the SSM exercise, including consideration of what needs to be done to put plans into action. The main thrust of this section has been to argue that technical proficiency and an understanding of the basic philosophy behind SSM is only a starting point for successful application; users need to have an acute understanding of human beings, what they are, how they operate and how they interact with one another. Successful application of SSM may be contingent upon there being understanding of how human beings function in relation to the objectives that are pursued in SSM. This, I am suggesting ought not to be left as some form of conjuring trick, or as a skill that can only be picked up through experience.

# PART FIVE: REVISITING SYSTEMS INTERVENTIONS

In Chapters 7 and 9, I sought to tease out some ways in which *AT* might assist users of the VSM and SSM respectively. I did this by examining theoretical and practical contributions. I now wish to extend further the practical application of this thinking.

In the spirit of Checkland and Scholes' (1990) suggestion that: "*. . . every study . . . .can be mined repeatedly for insights relevant to new concerns in an ongoing programme of research*" the next two chapters revisit VSM and SSM interventions that previously I instigated, designed, carried out, and subsequently had published.

Before I come to the actual studies, it is important to issue a couple of caveats. The first is that the content of the next two chapters does not constitute a form of empirical 'testing' of the ideas discussed so far. The aim is merely to illustrate, and to better capture of flavour of what bringing some of these ideas into the world of practice might involve.

Secondly the descriptions are not comprehensive. Since each study was designed with particular purposes in mind, it is not possible to cover all of the points that have been made about the two methodologies in question. Basically I shall concentrate on aspects of each study that I believe are worth focusing on, and, about which I can speak with some authority given the passage of time and the limited the scope of the interventions.

# 10. REVISITING A VIABLE SYSTEMS INTERVENTION

## 10.1 Introduction

In Chapters 6 and 7, I opened up space for a complementary application of *AT* and the VSM and attempted to show what this might involve in theoretical and practical terms. It is now appropriate to flesh out the practical side of this in more detail. The following chapter aims to do this by re-examining a VSM intervention that I carried out some years ago (see Brocklesby 1995, Brocklesby and Cummings 1996). The main objective of the chapter is broadly consistent with the approach taken in earlier chapters. Here I employ *AT* as a frame to reflect critically on the intervention as it was originally conceived and carried out. This then becomes the basis for saying how things might have been done differently, had – from the outset - *AT* been adopted as a guiding framework.

The chapter begins with a discussion on the background to the case and briefly rehearses the process, findings and outcomes as they emerged at the time. It then assesses these in the light of the new knowledge provided by the emergent *AT* perspective on the VSM developed earlier.

## 10.2 Background on the intervention

This intervention took place during 1992 at Telecom (NZ) Ltd. during a period of extensive re-organisation and downsizing. During the 1980's the New Zealand Labour Government instituted a massive program of economic and commercial reforms that included the corporatisation (and subsequent privatisation) of all of its state trading organisations. In 1987, as part of this process, Telecom (NZ) was formed to operate the telecommunications business of what was the New Zealand Post Office (NZPO hereafter), a Government Department. Whereas the NZPO had traditionally fulfilled a number of social and political objectives, Telecom was required to operate as a

commercial concern, and its vision statement became: *"To satisfy the needs of our customers, shareholders and employees with exceptional business efficiency and superior service."* Yet at the time of the change, the company was thought to be ill-equipped to compete in the new competitive environment into which a number of network providers and equipment suppliers had already entered. Telecom's network (mainly analogue) technology was out of date, its management structure highly centralised and bureaucratic, its operations were inefficient, and customers perceived that it offered poor quality service.

Over the next few years, the company made a number of key strategic responses to this situation. These changes were embodied in a complete re-organisation of the company's structure, and it is this aspect of the change that was the focus of the intervention.

The main structural change at Telecom involved the formation of four autonomous regional operating companies ('ROCs' hereafter) to operate the main business of the Telecom network. Under this structure the four ROCs were made responsible for business in their geographical area, a fifth provided common network and international services. All other businesses were established as new venture companies. Thus, many functions previously carried out at head office were decentralized into the various ROCs. Over a period of 12 months, the corporate office was reduced in size from 1800 to 400 staff. The company had moved with breath-taking speed from a highly centralized bureaucracy to a decentralized collection of very autonomous business units.

Although Telecom management was keen to devolve decision making responsibility in an attempt to get closer to the customer, it did recognise that decentralization could well lead to dis-economies of scale and inconsistent policy development and implementation across the company. In response, the company sought to 'cross' the ROC structure with a number of bodies known as 'Functional Councils'

('FC's' hereafter), to create a matrix-type organisation. Each FC had an area of jurisdiction such as human resource management, information systems, finance, commercial, and business planning. Their purpose was to bring people with relevant expertise from across the company to coordinate and develop consistency in these areas.

Before its demise, the NZPO's structure had evolved to meet the requirements of politicians and Treasury as well as customers. Decision-making was the prerogative of head office managers, who answered to a government minister, and who were far removed from the customer. Much of the time of these managers was spent making submissions and seeking approval from ministers and cabinet committees before implementing decisions.

The prime decision making body within Telecom now became the Management Board. This was comprised of the CEO, the heads of the six autonomous business units, and some key corporate office staff such as the Corporate Financial Officer. The Management board was empowered to make decisions of a strategic nature. They were answerable to a board of directors, the Main Board, who represented shareholders concerns. The Main Board generally approved Management Board decisions, but had the power to veto such decisions if it saw fit.

The structure outlined above had been up and running for a little over a year, when I was approached by Rob Wilkinson (Board Member and Head of Corporate Strategy) to undertake a review. I was approached having become quite well known to Wilkinson's assistant Nick Holdgate. Holdgate had been a student of mine on the Victoria University MBA programme. Following a relatively brief conversation, I was asked by Wilkinson to undertake an assessment of the suitability of the existing Telecom structure given the increasingly complex and turbulent environment in which the company was currently operating. I enlisted the support of a colleague and employed a

research assistant to help. Later I shall say more about the nature of the contract between the researchers/consultants and Telecom and how it came about.

## 10.3 Epistemology-related aspects of the intervention

Chapter 7 concluded that the primary VSM literature is lacking both philosophically and in methodological terms. Specifically it was argued that although there is a strong positivist flavour running through much of the primary VSM literature, Beer's epistemology for the VSM is somewhat ambiguous. Echoing sentiments expressed by (Espejo and Harnden 1989b), I argued that this philosophical uncertainty can create difficulties when it comes to applying the model as an intervention tool, not least because it puts the onus on users to work out themselves exactly what the VSM is supposed to represent. Is the model a tool for mirroring the real world, or is it something else? The answer to this question is not entirely clear. Luckily, as we saw, there is a secondary literature that has sought to develop its own philosophical and methodological groundings for the VSM.

According to Checkland (1981:38), hard systems interventions refer to that general class of situation in which there is a desired state, where there is a present state, where there are alternative ways of getting from the former to the latter, and where problem solving interventions consist of defining the two situations in question and selecting the best means of reducing the difference between them. On that basis, the Telecom intervention, as we formulated and conducted it, was fundamentally an exercise in hard systems thinking and practice.

From the outset, the intervention was framed in the context of significant changes that were taking place in the local and international telecommunications environment. Deregulation, the intensification of competition, and the introduction of new technologies were the most significant of these. For the research team, the brief was to assess the

capacity of Telecom's existing corporate structure to deal with these complex forces, to identify deficiencies and to recommend changes. Hence the intervention had a strong 'engineering' feel about it. Since we were required to analyse the existing system and suggest how this might be modified to allow it to do certain things 'better'; the 'what' was taken care of. Our brief was to focus largely on the 'how' question. Epistemologically, there was a very strong sense among the team that it ought to be possible to employ the VSM to identify independently existing deficiencies in the Telecom structure and come up with a blueprint for a 'better' organisation.

Looking back, our general approach was very strongly influenced by the process outlined in Beer's *'Diagnosing The System'* (1985), and in Flood and Jackson's reading of the VSM 'method' (to the extent that there is one) in *'Creative Problem Solving'* (1991). Thus, following an initial 'system description' stage, which confirmed that the study would focus primarily at the corporate level, the intervention took on a solid analytical flavour. Our contract made provision for us to carry out detailed interviews with board members, with managing directors from the various ROCs, and with managers occupying more specialised roles. Following a period of intensive data collection, we planned on analysing the data that would form the basis of a report containing recommendations that we would then present to the Corporate Strategy Group. From there, Wilkinson – the Head of Corporate Strategy and Board member – would make recommendations to the two boards.

Having carried out an initial round of interviews, and having digested the content of a large number of documents, the great bulk of our time was spent in discussing what Telecom 'looked like' in VSM terms; in identifying weaknesses in its functioning; and, in debating the logic of various structural solutions. Much attention was given to ensuring that we came up with 'correct' diagnosis of difficulties, and being able to represent these through carefully crafted and highly elaborate VSM diagrams. It was not long before the

office in which we were working began to resemble the design unit of an engineering or manufacturing company. We consumed copious quantities of flip charts and pieces of A3 paper as we gradually worked our way towards a clearer appreciation of the problems facing Telecom, and what might be done about these. We were conscious of the fact that exemplary diagrammatic presentation of our thinking would be required, especially in the first major feedback session to the Corporate Strategy Group.

Once an initial picture of the Telecom system-in-focus had been sketched, we then attempted many other drawings, focussing on more detailed representations of variety matching between various subsystems, and exploring in more detail aspects of S1's at lower levels including the ROCs and new venture companies. All in all, the approach to the intervention bore many of the hallmarks of a hard systems intervention. In that sense it was relatively uncomplicated.

The most glaringly obvious aspect of the intervention that allows me to now characterise this as a hard systems intervention, is that no serious consideration was given to the possibility that there might be alternative viewpoints regarding Telecom's place in the world, and in relation to the nature of the problems that it was facing. From our perspective, it was simply a case of operating with the statement of corporate purposes that we extracted from background literature provided by the Corporate Strategy Group; we did not consider other options.

In retrospect, I cannot be sure how much credence we gave to the possibility that there <u>might</u> be alternative and/or more useful descriptions of the 'purpose' of the Telecom system. What I can say with certainty is that at no point in the proceedings did we give any thought to the possibility of convening the sort of 'negotiation forum' about which Espejo and Harnden (1989b:245) speak, and which would be central to any VSM intervention that even pretends to be informed by the epistemology of *AT*. There was no mechanism for exploring different views and achieving agreement on action. As it turned

out, the team took the application of the VSM and the objectives of the Telecom system as a given. We ignored the strong possibility (given its size, its diversity, and - given a somewhat precarious economic position – its volatile employee relations environment) that there would be a range of different views on what was the fundamental purpose of Telecom.

I am equally certain that the three university-based researchers and Rob Wilkinson – the instigator of the intervention - had a great deal of conviction that under the circumstances the VSM was the most appropriate tool. We were unanimous in our belief that the VSM would provide exactly the kind of information and guidance required by the Management Board to support its strategic thinking and its decision-making.

Earlier I spoke of Lynda Davies' admonition to systems practitioners that they must avoid being seduced into believing that their elaborate models capture the way the world is. Indeed Davies warns of the danger of: " . . . *holding a stick diagram of a tree against awesome, terrible, beautiful landscapes and worshipping the stick diagram as our salvation.*" (Davies 1990:386). Looking back on the intervention now, it is somewhat distressing to think that reasonably experienced University academics and consultants (two with backgrounds in the social sciences and humanities who should have known better) could unwittingly fall into the trap that Davies describes. Yet on reflection, and looking back through the various documentation that was collected and produced through the study, I cannot find any evidence that suggests that serious consideration was given to the possibility that the VSM may not provide a comprehensive mirror image of the key features of the company that we were dealing with. Neither can I recall us contemplating the possible use of alternative tools or methodologies.

The formal mission statement of Telecom at the time was '*employing information technology to enable people to communicate more immediately across distance*'. Despite its blandness we used this as the basis of our modeling and analytical work. Little did we

contemplate the possibility that this mission statement was open to multiple possible interpretations, and/or that it might not adequately frame the strategic arena that the company saw itself in or give any guidance as to its future strategic direction. This decision on our part was to create difficulties later, as different interpretations became the basis for substantive opposition to some of our key recommendations.

In Chapter 4, I suggested that one of the problems when people operate on the assumption that there are absolute truths, is that problems can arise when there are disagreements about what is 'the true state of affairs' pertaining to a situation. Mutual negation and conflict can then occur because each party operates from the conviction that they have access to the truth; each knows how 'things really are'. The stark reality of this did not occur to us until we began feeding ideas back to members of the Corporate Strategy Group, to individual managers, and in the final presentation. It led to rather animated disputes over what Telecom's predicament was, and what needed to be done about it.

Once the study got underway, it gradually began to dawn on us that not everyone with whom we spoke agreed with the basic statement of mission that was governing our analysis. Substantive opposition was later expressed from a number of middle and senior ranked managers studying on the Victoria MBA programme who had been in the organisation for a long time and who had progressed through the old NZPO hierarchy. Despite all the corporatisation changes that were occurring around them, there was still a lingering sentiment among these executives that the company had wider purposes; specifically to be a good corporate citizen which included mopping up the country's unemployment.

This diversity of opinion became manifest in the response of senior managers to some of our recommendations. For example one of the 'deficiencies' revealed by our analysis had to do with the manner in which the Functional Councils – because there had

been no direction 'from above' and because of the predilections of individual convenors - had begun to take on a wide range of functions leading to inconsistencies across them. We had been led to believe that the FC's had been set up to provide a coordinating mechanism across the ROC's, and had analysed their operation in these terms. However on feeding this information back to the senior managers concerned it became a source of major contention firstly as to whether the FC's were indeed set up for this purpose, and even if they were, the extent to which they were performing adequately.

Although these substantive and very basic differences of opinion about Telecom's role in the world and the role and performance of key units within it, came as something of a surprise, I cannot say that this seriously deflected us from our mission. We simply took the most obvious technical objectives as laid out in the corporate mission statement as a given, and, in line with the terms of the contract, away we went to apply our collective expertise and thinking to the problem.

Although I had been teaching a rudimentary approach to the VSM for some time, neither my colleagues nor I had any substantial experience in using it in a 'live' situation. Despite this, in the context in which we were operating, we were very much 'the experts'. In our eyes, and in those of Wilkinson/Holdgate, our knowledge of the VSM provided us with a highly specialised way – denied to the other actors - of understanding the 'reality' of the organisation. The strength of this self-image stands as a timely reminder of Maturana's claim that when observers operate in accordance with the *objectivity-without-parentheses* explanatory path, their realities are frequently lived as the reality. Indeed, I believe that this was the case here. Moreover, our 'expert' self-image reinforced our conviction that providing our analysis was technically sound, there was every chance that the Board would act on our recommendations.

In Section 5.2.1, I argued that this *'X-ray vision'* or *'smart bits'* (Ormerod 1998), approach can work well in operational situations (if not in more strategic environments)

where the problem is relatively unambiguous. In the Telecom case, we were soon to discover that this was not what we were facing.

Let me explain this by recounting the nature of the conversation between myself and Rob Wilkinson that led to the commissioning of the intervention in the first place. Essentially it arose as a result of a discussion during which I suggested that the VSM might be an appropriate tool to use in shedding light on the organisation-environment adaptation issue that was occupying the attention of the Board and the Corporate Strategy Group at the time. The focus was on the likelihood that an 'external' perspective, looking at the organisation 'afresh' might be worthwhile. I did not exactly say that it would be possible for us to 'objectively' analyse the situation facing Telecom, but I have a feeling that this was being implied.

Since the VSM was virtually unknown in NZ at the time, I had expected Wilkinson's response to be either ambivalent, or at best lukewarm and non-committal. It therefore came as a great surprise when he readily agreed to the proposal. Later Wilkinson was to reveal that he had previously attended an executive training course in Australia where he had come across the VSM. He remarked that at that time he did not fully understand the approach, although he could see that it had potential application in his work. It could therefore be said that there was some degree of prior structural coupling between Wilkinson and the VSM.

From that point on, the die was cast; the whole intervention was framed around the use of the VSM. Indeed the title of the project that was to become embodied in the consulting/research contract became: 'A Viable Systems Model Study of Telecom New Zealand.'

Looking back, Wilkinson's enthusiasm for the intervention in general, and the VSM in particular, was a double-edged sword. On the one hand, it provided the researchers with an ideal opportunity to apply the VSM in an organisation that was one of

New Zealand's largest and arguably most prestigious. It provided us with enough funding to employ a research assistant. And there was a guarantee that we would have access to whatever resources we needed, including direct access to the senior management team and board members. From a management academic's point of view, it could not have been better.

On the other hand, having signed the contract, subsequent events were to show that some of the people who we were later to deal with, including wider members of the Corporate Strategy Group (Lewis and Chester), were clearly unsure about the desirability of framing the 'problem' in VSM terms. The same disquiet was evident during our interviews with the senior management team. It raised its head again during the first feedback session with the corporate strategy group. During that session it became manifestly clear that we had seriously overestimated the extent to which Lewis and Chester saw this as a useful study and one that would make a difference in the sense that it being likely that its recommendations would be taken seriously at Board level.

We entered the first feedback session thinking that it would simply be a case of explaining our analysis to the assembled audience. In the event, more time was spent in discussing the appropriateness of the VSM for dealing with the issues at hand and in defending our assessment of how, in VSM terms, Telecom was performing. Reading between the lines, it seemed clear to me that Lewis and Chester and some of the senior managers were not entirely sure that the study was necessary in the first place, although no-one said so at the time. Several months later Chester and Holdgate admitted as much, as has Tone Borren (one of the ROC managing directors) very recently.

Looking at this in *AT* terms, I would now say that we paid insufficient attention to the need to establish a consensual domain among the key actors in relation to the problem definition and how it ought to be tackled. I would also say that we failed to adequately monitor and manage the quality of the conversations as the intervention proceeded.

Wilkinson and Holdgate aside, the conversations between the researchers and the key Telecom actors were seriously lacking in terms of the 'convergence' that is necessary if an intervention is to have wider impact. The structural coupling between the researchers and the wider group of actors whose support would be necessary to make the sort of impact that we had hoped for was conspicuously absent.

In Chapter 4, I suggested that the substantive content and 'shape' of *MS* interventions is a product of the historically generated predilections of the people involved and the nature of the relational circumstances as these evolve through the intervention. Given the latter, and with the benefit of hindsight, it is clear to me that we did not spend anywhere near enough time interacting with this wider group of executives with a view to instigating the 'back and forth' process of mutual adjustment that, according to the logic of structural coupling, results in two parties gradually becoming aware of each other's needs and capabilities. Our inclination to deal almost exclusively with Holdgate and Wilkinson, and to press on resolutely according to the terms of the research contract, in the false promise that the wider group of actors would, through some magical process, appreciate the need for what we were doing, accept the validity of our analytical work, and concur with our assessments of what needed to be done to improve the functioning of the organisation, was not only theoretically inept but also professionally naïve.

Adding to the difficulty, it soon became clear that our use of VSM terms such as 'requisite variety', 'amplification', and 'second order negative feedback' was creating something of an alienation effect on the part of Telecom executives who were largely unfamiliar with the VSM. I believe that Lewis was particularly irritated by it.

At one point, it was virtually a 'stonewall' situation. We were pressing on according to the preparatory work that we had done. Meanwhile, despite a superficial politeness, others sought to question or dispute much of what was being said. Partly this

stemmed from the fact that our adversaries were coming to the VSM 'cold'; there having been no serious attempt on our part to obtain 'buy in' to the methodology at an earlier stage; partly it was because we simply did not countenance the possibility that the convoluted technical language that we were using and the elaborate diagrams and drawings were simply not meaningful to the people concerned. We were blind to the principle that understanding the structure of people who you are seeking to influence is the basis for effective communication and interactions. Under no circumstances could we reasonably have been described as 'scholars of structure-determinism'. Neither did we attend much to the quality of the various conversations including our own. We had been almost totally preoccupied with the need to produce an exemplary analysis of the situation and, in *AT* terms, we had failed completely when it comes to establishing the necessary coupling that would allow serious progress to be made. If ever there was an example of a divergent rather than a convergent conversational path, then this was it.

Arguably, it was the strength of the initial coupling between Wilkinson/Holdgate, and myself that led us to assume that this would extend to the other key actors. Perhaps I had over-estimated Wilkinson's clout in the organisation; perhaps I simply assumed that he was speaking on behalf of his colleagues. In any event, the fact is that we erroneously assumed that this wider group of actors would accept the model and the premises on which it is based.

Certainly beyond my initial conversation with Wilkinson, there was no attempt to obtain their 'buy in'. This being the case, and given the inextricable linkage between domains of explanation and domains of legitimate actions, it is unsurprising that the managers involved came to doubt the wisdom of some of our suggestions about what ought to happen. It may even be the case that the recommendations that did receive a favourable hearing were supported for reasons that were quite different to those that were governing our analysis.

Given these difficulties, it is not difficult now to understand why several of the key actors did not agree with the actions that we were proposing. In carrying out our analysis and in preparing for the various feedback sessions, our whole approach had been to carry out an exemplary analysis with a view to 'getting at the truth', and our preparation consisted of ensuring that we were able to defend our recommendations through reference to the VSM template. The possibility that key actors might not unquestioningly buy into this template and therefore fail to see the wisdom of what we were suggesting did not really occur to us.

Moreover, we had not reckoned on the intervention conversation being just one of many that was occupying the attention of Telecom executives at the time. We had blindly assumed that the issues under discussion would be as important to these people as they were to us. That this was shown to not be the case came as a bit of a shock to me, since I had 'lived' the intervention for several months and had devoted a large proportion of my working life to it.

That we were operating according to the *objectivity-without-parentheses* explanatory path is further borne out when I now reflect on our incredulity to senior Telecom managers' outright rejection of our main substantive recommendation for structural change. We were fairly sure that we would know what an 'improvement' over existing arrangements would look like. And from our vantage point, the most glaring weakness of the Telecom structure at the time was the organisation of the company along geographical lines. To us it seemed patently obvious that the local environments of the four ROC's did not exhibit variety states that were sufficiently different to justify their operation as separate business units. This was leading to diseconomies of scale.

In looking for an appropriate measure of environmental diversity, it seemed very obvious to us that the two key 'markets' that Telecom dealt with were 'business' and 'residential' and it was along these lines that we argued very strongly that Telecom be

restructured. To us it was obvious: if you are looking to devolve decision making responsibilities down to the point where it makes a difference, then the distinctive market segments have more to do with the distinctive needs of particular groups of customers than it has with where customers are located geographically.

Given the apparent logic of this proposition, it came as a major shock to the researchers that this proposition was ruled out almost from the outset. In fact this recommendation did not even make it to the final report because Wilkinson made it very clear to us that Peter Troughton – Telecom's chief executive – would simply not countenance the idea of a business/residential split.

Why? Looking back, the researchers and Wilkinson and Holdgate shared the same VSM-related 'storyline' about how and why the Telecom situation had come to be what it was. This being the case, it was very easy for this group to appreciate the systemic logic of the situation and agree on what needed to be done. Recall that according to *AT*, the perceived legitimacy of an action is relative to the generative 'storyline' that an observer accepts as a plausible explanation of the experience to be obtained, and some underlying preference for that explanation over others. And as I said in Chapter 7, the VSM's generative process that accounts for organisational viability has to do with ceding autonomy to operational units and putting in place an appropriate meta-system to manage it for the good of the system as a whole.

It is now quite clear that Troughton, Lewis and Chester, and the ROC managing directors were operating with very different realities to ours. For a start the ROC managing directors had much to gain by continuing with the present arrangements. Troughton had ceded them significant autonomy; they had been able to construct their own empires, or 'fiefdoms', as one head office manager described it. Had the ROCs been dismantled, they had much to lose. And Troughton stood to lose the support of the

American owners of Ameritech and Bell Atlantic if he were to commit to a new structure that belied the new company's North American influence.

Politics and vested interests aside, the possibility also arises that these actors were simply operating with very different explanations about Telecom's current and likely future predicament. Obviously I can only speculate on what these might have been.Yet from the fragments of information that I have been able to assemble, I now reject the possibility that it was simply a case of Troughton failing to appreciate the limitations and inadequacies of the existing arrangements as these were presented through our feedback and in the final report. On the contrary, I now believe that it was Troughton's placing of the argumentation within a broader context – a broader explanatory domain in Maturana's terms – that led him to reject the substantive recommendation; it was not the recommendation per se.

Essentially I believe that Troughton was prepared to carry short term costs and short term inefficiencies in order to enhance the performance of the organisation in the long run. It has recently been suggested to me that his 'generative process' might have foreshadowed the importance of a period of intense competition between the ROCs as being critical in generating a longer-term efficiency. In other words he was operating according to a logic that suggests that the disadvantages of duplicating services and excessive competition across the ROC's were, in the longer term, outweighed by the advantages.

A second example that bears testimony to how the various actors were operating with different realities, again draws attention to the structural focus of the VSM. Now it would be grossly incorrect to say that the VSM inevitably sets people off down a track that leads to proposed structural modifications, because this is not necessarily the case. Having said that, my experience is that a perceived structural deficiency, perhaps accentuated by less than optimum organisational performance, does provide a primary

reason why VSM interventions often occur in the first place. In such circumstances, structural adjustments and other concrete changes are likely to be recommended, and this was very definitely the case in the intervention under discussion here. From the very beginning, Wilkinson and I agreed that we would come up with a diagnosis of the effectiveness of the corporate structure as a whole and of the main structural units within it. Indeed a listing of these key structural units was written into the contract.

Once again, not all members of the wider client group saw it that way. Although we continued to frame the problem largely in structural terms, others pressed the point that there were wider issues involved, and that even if the outcome was to be structural modification of some sort, there would be no guarantee that this would lead to improvement. Frequent references were made to NZ Post, which was another of three trading arms of the NZPO prior to corporatisation. In the first three years of its existence as a state owned enterprise, NZ Post had apparently undergone four major structural transformations, none of which, in the opinion of some Telecom managers, had brought about substantive change in the functioning and performance of the organisation.

The point that these managers were pressing on us is that real organisations are not inanimate, malleable objects that may be easily shaped in the image of the analyst or key organisational actors. Indeed we ought to have known this since, during the investigation, we unearthed a good deal of evidence that pointed to the lack of congruence between Telecom's formal structures on the one hand, and informal spontaneous actions and behaviours in the company on the other hand.

A good example of this tension between the formal and the informal was borne out in the results of a survey, carried out during 1992 that had sought to assess the suffusion through the organisation of the then 'new' statement of strategic position. This survey showed that upwards of 90% of staff were unaware of the so-called 'strategic

excellent positions' that had been expensively formulated and promulgated through the organisation. Even fewer staff were able to say what the so-called 'S.E.P.'s' involved.

Summing up, I believe that the main reason why this intervention did not have the sort of impact that we, the researchers, envisaged, can be put down to a combination of inexperience on the part of the researchers involved and the epistemological grounding of the study.

In retrospect our mistake was to enter into discussions with the very people who would arbitrate on the usefulness of our work believing that our elaborate VSM analysis and diagrams captured the reality of the situation facing Telecom at the time. Not only was the VSM driving the substance of the investigation in a 'preference' and a process sense, but its language also came to dominate the key conversations.

We did not consider our involvement. To the research team, the intervention was being driven by the nature of the circumstances facing Telecom at the time and the debates that were taking place among the senior management group. Looking back, I now believe that it was not the situation that was driving the intervention at all. The intervention was being driven as a result of a strong pre-existing intellectual and emotional commitment to the VSM on our part, which was reinforced through our interactions with Holdgate and Wilkinson. Contrary to the original intent we were not taking a disinterested external perspective, nor were we looking at the situation 'afresh'. In some respects the intervention and our approach to it was a classic case of the 'model says' syndrome that I spoke about in Chapter 4.

Finally, in looking at the Telecom intervention through the lens of *AT* it is important to consider the ethical stance taken by the researchers. As it turned out, the unwillingness, for whatever reason, of the Management Board to act on our recommendations for structural change, may have been a blessing in disguise. The fact is that had Troughton and the Board decided, on account of our recommendations, to

dismantle the ROC structure, the repercussions of this would have echoed through the entire organisation. Indeed – give Telecom's size and standing in New Zealand – it would have affected the whole country. Certainly the vast majority of Telecom employees would have been affected in some manner or another, many adversely.

Since our analysis was identifying many examples of inefficiencies and the duplication of provision within the company and since it was seeking to remove these, it is reasonable to assume that switching from a geographical to a customer-based structure would have necessitated a substantial reduction in the number of people employed by the company. Inevitably there would have been compulsory redundancies.

Whether, in the light of such reflection, the researchers would have acted any differently, I cannot say. What I can say is that the ethical basis of the intervention was not the subject of discussion at the time.

## 10.4 Reframing the intervention

Let me now try and speculate on how, if the intervention were to take place now, informed by the logic of *AT*, things would be done differently.

First and foremost, I am assuming that nothing would change in relation to the motivation for carrying out the intervention in the first place. Recall that in addition to seeking a site for the application of the VSM, the primary motivation of the researchers was their desire to have a substantive impact on the decision making process in the organisation. This is not to say that we expected that the Management Board would unquestioningly go along with our recommendations, rather that they would give them a fair hearing in the light of other possibilities, and in the light of the organisational, political and cultural constraints operating on them, and in the organisation, at the time. Very definitely, the aim of the exercise was not to merely satisfy the conditions of our

contract with Wilkinson in order to obtain financial gain. In simple terms, we wanted to 'make a difference' in the wider organisation.

Given this, and given the inherent ambiguity in a project of this sort, perhaps the most obvious difference between what actually happened and what I would now want to happen, is that there would be a major shift of emphasis away from conducting exemplary analysis towards – in the first instance at least - establishing good relationships and promoting effective communication between the researchers and the wider client group. I would not now simply take Wilkinson's brief as a given and jump headlong into detailed and elaborate analytical work. I would want to first convince Wilkinson of the need to suspend detailed analytical work pending the establishment of sufficient coupling between the researchers and the immediate and wider group of senior managers who we were seeking to influence. In the language of *AT*, the primary need would be to establish a network of structural coupling that is capable of providing the basis for effective conversations and the establishment of a consensual domain that covers the nature of the issues to be dealt with and how they ought to be dealt with. On the face of it, this is not too dissimilar to what might be involved if the VSM were to be embedded within a broader process such as Checkland's SSM or Flood and Jackson's Total Systems Intervention. However its focus is quite distinct.

Certainly there would be much less emphasis in the early stages of the study on detailed analytical work. Indeed I would not now want to presume that the VSM would be the primary analytical tool, although I might be prepared to admit my preference for using it. Instead, I would want to place more emphasis on having a sufficient number of high quality interactions with the relevant actors to provide for the necessary structural coupling. The aim would be to instigate a back and forth process of mutual adjustment, that allows the various parties to appreciate each other's needs and their respective contributions. This would involve conceptualising the intervention as a communication

process that concentrates on the needs of the clients and the benefits that the researchers and the tools that they bring with them can deliver. As part of this process, onus is placed on the researchers to sense the manifest and latent needs of the client group, including those needs that the client cannot verbalise. Overall the task is to develop close working relationships based on a genuine mutual interest.

Unlike Wilkinson and Holdgate, Chester, Lewis and the other senior managers attended our feedback sessions in a state of complete or partial ignorance not only about the VSM, what it is and what it does, but also about the researchers and their skills, experience and qualifications. It is now clear that these things had a bearing upon the credence that the wider client group attached to our analysis, and this, in turn, affected their evaluation of our recommendations. The structural coupling between the researchers and Peter Troughton, the most influential actor in the decision making process, was even more precarious since there was no direct contact at all between him and us.

In applying *AT*, it is axiomatic that there is an acceptance of the existence of multiple realities. Roughly translated this means that an important part of the 'back and forth' process of mutual adjustment involves considering alternative possible constructions of the problem situation and alternative means of addressing it. Given this, I would not now assume that the VSM is necessarily the only or best way of tackling the problem. Implicitly, if not explicitly, the agent is always likely to be predisposed towards channeling key decisions that determine the shape of an intervention away from avenues of inquiry that he/she cannot deliver on. Unless the agent is prepared to withdraw from the intervention, it is most unlikely that he/she will commit to using a tool or methodology that he/she has no prior experience of successful application, or with which he/she is uncomfortable. If the VSM is to be used, then there is a need to first obtain 'buy in' from the various participants. This needs to happen at a basic emotional level; the wider client group needs to appreciate the implicit and explicit preferences and generative

process on which the VSM is based (see Mingers 1997a:420-422 for an interesting general discussion on this point)

In the Telecom case, most of the key actors were coming to the VSM 'cold'. I would now want to expand the immediate client group to include more members of the senior management team. I would plan on spending more time attempting to convey a broad understanding of what the VSM is, what it does, and what benefits will be delivered. I would envisage spending much more time communicating and demonstrating the benefits that can be derived from its application.

Beyond developing an adequate degree of coupling between the VSM and the wider client group, I would now take steps to maximise the chances that the group sees the relevance of the service being provided to their particular needs. In the original intervention, the 'Telecom case' was the predominant conversation that was occupying the minds of the various researchers for several months. Our mistake was to assume that the same applied in the case of the others. We did not take sufficient account of the fact that for most of the Telecom staff who were involved in the project, this was but one conversation among many; and, for some, not a very important conversation at that. The point is that without establishing centrality and relevance it is most unlikely that key actors will be able to testify to the usefulness of the intervention in the context in which it takes place.

Part of this 'establishing relevance' involves exploring how the VSM fits with the experiences and emotional preferences of the people involved. Recall earlier I suggested that although intervention tools such as the VSM are geared towards taking action, they are also specialised forms of explanations. To that extent, in the absence of being able to demonstrate 'results' in advance, perceptions of utility hinge upon the various parties accepting the VSM as an adequate explanation of, in this case, organisational viability. If Maturana is correct, we know that this involves the listener accepting the basic

preferences that underpin the model and the generative process that, in theory at least, results in organisational viability. Moreover, if the actors are to genuinely commit to the model its various propositions must 'fit' with their experiences and deduced experiences. If key actors have not experienced what the VSM would predict that they ought to experience, or if the actors cannot imagine experiencing these and further deduced experiences, then expecting such persons to commit is going to be difficult. Under the scenario being debated here, I would want discussion of the proposed methodology to be geared towards establishing the VSM as a viable explanation for the people concerned. And this debate would need to be conducted, where possible, using the language and terminology of the persons concerned.

Assuming that the wider client group that I have envisaged understand the VSM and can see its relevance in the context of contributing to the relevant strategic debate, thereafter it is a case of promoting convergent rather than divergent dialogue, and importantly, monitoring the satisfaction of the clients as the intervention proceeds.

This discussion identifies major differences of substance and emphasis between how the intervention was conducted and how I would now wish to proceed, having taken account not only of the epistemological imperatives of *AT*, but also of the researcher's desire to have a substantive impact in the organisation in question, and not merely satisfy the terms of the contract.

The crux of the difference between the two scenarios is that the actual approach taken was based on a relatively narrow conception of the problem situation and how it ought to be addressed. It involved very limited interactions between the researchers and the wider client group, and it placed a disproportionately large amount of attention on the analytical skills of the researchers that culminated in the presentation of a written diagnostic statement and recommendations by a team of so-called 'experts'.

The scenario that I now envisage places more emphasis on the need for the researchers and the immediate client group to suspend judgements about how the problem is conceptualised and about how it might be addressed. It emphasises high quality interactions between the researchers and a wider client group leading, through a process of mutual adjustment, to a greater sensitivity on the part of the researchers to the wider client group's needs and preferences, and their appreciation of what the researchers can deliver. Much more emphasis would be placed on understanding the client group and communicating with it on its terms, and there would be a periodic monitoring of the utility judgements being made by the client group as the intervention proceeds. Finally I would not now want to place quite as much emphasis on the necessity of seeing concrete structural changes taking place. Indeed I am inclined to the view that, on balance, the value of tools like the VSM hinges more on the extent to which – through their specialised patterns of distinctions – they can promote different ways of thinking about and constructing situations. To the extent that such tools do seek to instigate new (coordinations of coordinations of) behaviours, then this happens indirectly as a result of linguistic changes, than necessarily by targeting behaviours more directly from the outset.

When examined against the broad *AT*-based intervention template just described, the Telecom intervention as a whole could not possibly be regarded as an unmitigated success. However, there were aspects of it that did accord with the notion that VSM interventions can be legitimately measured against the extent to which they can promote novel conversations and new ways of thinking about situations rather than necessarily promote structural modifications. I shall briefly provide two illustrations from the Telecom case that lead me to this conclusion.

The first of these came about as a result of the request that we examine the performance of the Functional Councils. Recall that these were charged with ensuring consistency across the operating units. Over time however, each FC had grown to

become something of a 'jack-of-all-trades' - developing strategy, seeking to control resource allocation, monitoring performance, and developing broad policy or identity guide-lines. In practice, the convenors and members of the FC's were uncertain about what their role in the organisation should be.

This ambiguity had allowed the FC's to define their roles in ways that suited the predilections of their members. There were two reasons for this. First, their terms of reference were loose enough to allow them to branch into what they perceived as 'more interesting' areas. Second, the Management Board, which also found itself doing all of these functions simultaneously, felt overloaded and wished to devolve some responsibility to the FC's. Not to be outdone, the remaining corporate office departments (there still remained seven filling largely advisory roles), also issued advice, policy directives, strategy papers, and sought to monitor performance.

Taken as a whole, this amounted to a disturbingly large amount of meta-systemic confusion with a barrage of often-mixed signals being sent in the direction of the operational elements. In addition, with so many bodies carrying out so many functions, the important System 4 'future' focus was not being done particularly well by anybody.

The second example, which is related to the first, concerned the work overload of senior managers. Most of the top ten or so executives within the company found themselves carrying out all sorts of systemic functions at once, overburdened and unable at times to distinguish between their many roles in the new structure. A ROC managing director, for example, would likely be expected to head their operational unit, be a part of one or more functional councils, advise corporate office functions, and sit on the Management Board. The matrix structure had created a mass of committees and advisory boards, often duplicating each other. One senior executive related the story of how he sat down one day and figured out how much of his working week was, on average, taken up by such committees - twenty-seven percent. He promptly withdrew from the FC's he had

been assigned to. Many did the same, and morale began to suffer accordingly. The matrix structure, having been conceived with good intentions, had developed a mind of its own and grown far beyond its usefulness, as matrix structures are prone to do (see, for example Bartlett and Ghoshal, 1990).

The systemic role confusion of the various structural bodies that we examined combined with excessive workload were undoubtedly key concerns to senior managers. However the inability of managers to conceptualise their various formal roles in terms of appropriate behaviours, was equally worrying. Roles frequently became 'contaminated' by extraneous concerns, where for example, meta-systemic company-wide matters were allowed to interfere with System 1 management, and meta-systemic functions were purloined by narrow System 1 agendas. These managers found it increasingly difficult to separate the collective wood from their individual trees.

Among the senior management team there was, in particular, a strong feeling that the Management Board was not functioning properly because it was pre-occupied with detail. In response to the question of what role the Board should play, the group unanimously supported the idea that its main responsibility should be policy making and direction setting (System 5). However, research revealed that these functions were being compromised by the inclusion of agenda items relating to a range of Systems 1-3 business. The meta-system, as it stood, did not possess the variety needed to adequately manage and provide for the system as a whole.

Against this background, one of the successes of the Telecom intervention was that once the client group began to get the hang of VSM terminology there were embryonic signs of it becoming the sort of powerful conversational tool that was envisaged in Chapter 7. Indeed the VSM did provide a set of distinctions to the Telecom managers that allowed them to conceptualise their roles, and those of the various structural units in the organisation, in a novel way. It allowed them to clarify priorities

and consider how it might be possible to prevent role contamination. Prior to being exposed to this language these managers simply had to deal with the difficulties created by role ambiguity, role conflict and overload as best they could. They did not have the wherewithal to appreciate what was creating the problem in the first place, neither were they in a position to think through possible solutions.

## 10.5 Broadening the basis of the organisation-environment adaptation debate.

Revisiting the Telecom intervention in the light of what was said about the nature of organisation-environment adaptation processes developed in Chapter 7 brings into focus several interesting aspects of the organisation – not considered at the time – that have a bearing upon its predicament vis a vis its environment. I shall not rehearse the theoretical argumentation involved, suffice to say that the emergent *AT* perspective developed in Chapter 7 draws particular attention to three key sources of organisational change. These relate to the specific individuals involved in the organisation at the time, the internal dynamics of the company, and most importantly the evolutionary perspective based on structural coupling.

Given our predilection at the time to think of the VSM as primarily drawing attention to structural deficiencies, and given the precise wording of the contract that governed our analysis, it is understandable that we did not pay much attention to the purposeful role of particular individuals inside the organisation. Yet even at the time we were fully aware of the extent to which the shape and functioning of the company was reflecting the characteristics, biases and preferences of key actors.

There are many examples of this, but I shall mention only two. Firstly, the 'new' Telecom was very much Peter Troughton's 'baby'. In New Zealand at the time, Troughton had carved out a reputation as being a tough, uncompromising, chief

executive. He was very high profile in terms of media coverage, and arguably the first of what during the 1990's was to become a long line of local 'hero managers' (Jackson and Parry 2001). I would not go so far as to say that Troughton was completely free to fashion Telecom in his own image, but he was infinitely more influential than his counterparts in the other two post-NZPO companies.

For example, Troughton's commitment to fostering an entrepreneurial culture at Telecom, was legendary. During the period of our involvement in the organisation I well recall reading a comment attributable to Troughton that confirmed his blanket refusal to appoint anyone with a public sector background to a managerial position. Indeed, later he was to boast that of the 300 external appointments that were made between 1990 and 1992, none were from the public sector.

Another example of how particular individuals leave their mark on organisations was reflected in the frequent oblique references made by Telecom staff to the Bell Atlantic and Ameritech representatives on the Management and Main Boards. Since, following corporatisation, these two North American companies had acquired a financial stake in Telecom, there had been a succession of nominees from these companies. By all accounts only one of these representatives was prepared to adopt a hands-off approach in his dealings with local management. The over-involvement of the others had become manifest in a unyielding commitment to the geographical structure based on the North American Bell/*AT*&T System, and emerging conflicts between a North American-influence work culture and traditional New Zealand public sector values. Of the latter stories circulating around the organisation at the time revolved around the extent to which the new 'hiring and firing' culture instigated by the North American owners was conflicting with the traditional security of employment culture, and how the encouragement of, by New Zealand standards, extremely generous remuneration

packages - especially for young 'high flyers' – was conflicting with traditional remuneration criteria based on job evaluation and seniority.

The point is that it is impossible to separate the Telecom of the early 1990's from the key people who were involved in it at the time. Again, from an interventionist perspective, the issue is the potential incompatibility between recommendations for change that arise out of detailed analytical work and the influence of real human beings over the way in which the company operates. Any analytical work that does include within its ambit, consideration of these kinds of issues, is, I submit, unlikely to be successful.

In the context of the Telecom intervention, the other two main sources of structural change – internal dynamics and structural coupling – are related so I shall deal with them together. As we saw in Section 7.3.2 both of these forces draw attention to the evolutionary/historical dimension and show to a very large extent the structure of a system at a particular point in time reflects its own internal development as well as previous historical interactions.

Looking back, it is now a source of some incredulity on my part to think that in our analysis and in framing our recommendations we did not stop to consider where the company was at in its evolutionary development. Our approach was consistent with the guidelines provided by Beer (1985) and Flood and Jackson (1990). Both of these texts recommend what amounts to taking a snapshot of organisational systems at a particular point in time.

As it turned out, we did attempt to acquaint ourselves with the recent history of the company. However this was primarily for the purpose of understanding some of the recent structural changes that had taken place. We made no serious attempt to place Telecom's current predicament within a broader evolutionary/historical context either as

a means of explaining what changes may and may not be possible, or in understanding how existing trends might manifest themselves into the future.

Again, the company's blanket rejection of our suggestion that it ought to be structured along the lines of business and residential services bears testimony to the limitations of a 'snapshot' approach based on ahistorical analysis. When examined in isolation of the company's evolutionary development and its longer-term agenda, structuring the organisation along business/residential lines makes a good deal of sense. It is only when an historical and longer term perspective is taken that it becomes apparent that there is some the wisdom in the idea that retaining the ROC structure to allow the benefits of internal competition to be welded into the fabric of the company before then moving to a more market sensitive structure.

While *AT*'s focus on internal dynamics and structural coupling suggests the need for those involved in interventions to be sensitive to the evolutionary and historical grounded-ness of organisations, structural coupling also reminds the people involved in the intervention that sensitivity is also required in aligning recommendations for change with what is possible for that organisation.

Now in most cases I suspect that experienced VSM practitioners will formulate their recommendations in the full or partial knowledge of what is and what is not possible in the organisation under investigation. Others might take a lead from methods such as SSM where, having decided on what change is 'systemically desirable'; systematic consideration is then given to its organisational, cultural and political feasibility.

The point however, is that from what I can detect, there is nothing in the main VSM 'guides' that explicitly draws attention to this important requirement. The danger is that leaves open the possibility, clearly evident in the Telecom intervention, of a disproportionate amount of attention being focused on the imperatives of the intervention model and on analytical integrity.

## 10.6 Summary

This chapter has revisited a VSM intervention in the light of the emergent *AT*-based perspective on the VSM developed through Chapters 4 to 7. It has sought to critically reflect on the original intervention from a perspective that is grounded in the epistemology of *AT* and consider how things might have been done differently had *AT* been used as a guiding perspective from the outset.

Since there was such a heavy emphasis on both the observer-independent nature of the work and on exemplary analysis, the intervention shared some of the key characteristics that we tend to associate with hard systems thinking. Looking back this focus created difficulties since the key actors whose cooperation was necessary for the study to have a major impact inside the organisation did not share some of the key fundamental premises on which the analysis was carried; neither did these actors agree with some of the analysis and the recommendations that followed.

Reframing the Telecom intervention involves shifting the focus from modeling that is assessed according to the extent to which it accurately and objectively mirrors the system under investigation, to one whose validity is measured more in terms of the utility judgements made by the actors involved. In practical terms this would involve placing much more emphasis on the relationships side of the intervention and understanding the extent to which the historical biases of the primary researchers impacted on the study than was the case initially. Much more attention would be paid to establishing the conditions that reveal a clear understanding of the different parties needs, expectations and perspectives, that allows for the development of high quality conversations among a wider group of actors, and an ethical awareness on the part of all of those who are

involved in producing knowledge, coming up with explanations, and making decisions on action, as the intervention proceeds.

Finally the chapter has identified a number of examples in which the kind of conversations envisaged did eventuate, and, in support of the illustrations taken from the Competitive Capability Project in Chapter 7, it has commented briefly on aspects of the Telecom intervention that show up now in the light of the theoretical position described in that chapter.

# 11. REVISITING A SOFT SYSTEMS METHODOLOGY INTERVENTION

## 11.1 Introduction

This intervention took place during 1994. Like the Telecom intervention discussed in the previous chapter it came about as a result of initial contact having been made between myself and a senior manager in the organisation who had previously been one of my students at Victoria University.

Terence Broad had led a varied career initially in finance, production management and then marketing and social research before being asked to jointly head the Wellington operation of CRM Ltd. a market and social research company with offices in Auckland, Wellington and Christchurch.

Several years earlier, as part of his studies, Broad had been introduced to systems-based problem solving approaches including SSM. Now, in the light of difficulties that he was facing at CRM, he approached me with a view to carrying out an intervention using SSM.

Both Broad and I hoped to gain something worthwhile out of the intervention. For me it was a case of wanting to get back into applied work after several years of largely theoretical work and heavy teaching and administrative commitments. At the time I had plenty of experience of teaching SSM in the classroom but limited experience in using it in the heat of a real problem situation.

Broad had specific objectives of his own. He indicated that from his perspective there were two key issues that required investigation. The first of these was substantial dissatisfaction and poor morale on the part of professional research staff which had led to very high levels of staff turnover and shortages in key areas. This had frustrated the company growth strategy. For Broad, the most pressing concern was how to retain high

quality junior and middle ranking project managers. In the research industry project managers are pivotal to the success of companies since they manage the research process from diagnosis through research design to making recommendations, and it is they who develop and nurture relationships with clients and drum up new business. Without high performing research staff the company simply could not function.

Relatedly, Broad was concerned about poor communication and strained relationships among the various groups inside the company. Problems had arisen as a result of the external client-centred focus adopted by research staff, and a more production oriented view of the company held by field and other support staff. Later I was made aware of additional tensions between research staff and Broad himself. This was based on a perception that company management was excessively business and financial results orientated.

As it turned out, a key aspect of the SSM intervention did look specifically at what might be done to recruit project managers and assess their performance, and the published write-up of this intervention (Brocklesby 1995) focuses primarily on that aspect. However in the first instance I was reluctant to frame the intervention in these rather narrow terms. Rather than couch the problem in terms of merely retaining and rewarding project managers I suggested that we focus on relationships in the company and between the company and clients since there was already a strong feeling that a better understanding of relations across the various groups in the company would provide the basis for a clearer perspective on a number of more specific problems of which poor morale and turnover among project managers was only one. The question then was not to be 'how to recruit and retain' project managers; it was to be 'what is the nature of the relationships between the various staff groups at CRM, and between CRM and its clients, and how might these be improved so as to provide a better working environment and an improved service to clients.

Since Broad understood the underlying philosophy of SSM and was astute enough to appreciate that there was a range of different perspectives on these issues in the company he readily agreed to this broad-based framing of the intervention. He appreciated that the intervention might uncover potential problems as well as make progress in dealing with those that he had already identified.

In many ways the situation was very typical of the unstructured, ambiguous, multi-perspective 'mess' that SSM is best known for dealing with. This being the case, in the first instance, it made sense to proceed by adopting a fairly conventional approach in using SSM.

## 11.2 The evolution of the study

Since, unlike most published SSM case studies, the object of this exercise is not to report on the intervention per se, but to re-evaluate it using a particular theoretical lens and according to a different set of concerns, I shall keep references to the substantive detail of the intervention to a minimum. However since it is important to appreciate how the intervention unfolded a brief description of this follows.

The study began with an initial finding-out phase that involved open-ended discussions with approximately 12 staff across the four main groupings. Basically Broad came up with a list of key individuals, himself included, who he considered would have something useful to contribute and who would be willing to get involved. The list included another general manager, five research staff including one research director and four project managers, three field staff including the field supervisor, the company accountant, and the company information technology specialist. This provided representation across the four main groupings: management, field, research staff, and support services.

Over a three-week period I contacted these people as part of a general familiarisation and information collection phase. During these conversations we identified a range of issues of concern; I obtained their support to attend at least one full-day workshop with the expectation that another might be necessary; and I provided a very brief and potted description of SSM.

The issues identified in the preliminary meetings were fed back to the staff during the first full day workshop. Initially the intention was for there to be a relatively short period of comment and discussion during which the group would construct an expression of the problem situation in the usual rich picture format. Subsequently the plan was to collectively identify problem themes. However this turned out to be a difficult process. There was much more discussion on the nature of the problem situation than I had envisaged and the process was substantially delayed.

During this phase of the intervention much time was spent trying to capture the nature of existing relationships across the various groups within the company. Eventually this narrowed down to a listing of what the participants described as 'cultural tensions' within the company. The main areas of tension were between:

- **'production focus' and 'client focus'**
  the 'technical proficiency' orientation favoured by quantitatively-inclined survey researchers and technical staff on the one hand, coming into conflict with a more 'relevance'/client-focused orientation favoured by the majority of research staff and project managers.

- **'client focus' and 'business focus'**
  the client-focused approach of project managers that favoured cementing long-term relationships with clients often involving 'under-pricing' proposals clashing with a 'business'/short terms results orientation of management.

- **field and research**
  the need for field staff to balance workload with the availability of staff (often women with young children) conflicting with the needs of researchers/project managers' for flexibility and expeditious completion of requests for data collection.

- **between one project team and another**
  rigid boundaries between project teams preclude shifting assignments in order to balance workload. At critical times some project teams were under resourced, others were over resourced. Project managers acculturated in a particular way of 'doing things' were often reluctant to work on other group's projects.

Not only did the group agree that these tensions were significant problem areas in their own right, there was also a sense that they were responsible for much, if not all, of the interpersonal difficulties and communication problems that were manifest through the company.

In addition to these areas where internal accommodation was sought, the group also examined the pivotal relationship between research directors and project managers on the one hand, and clients on the other. Here the main issue revolved around the fickle nature of the relationship between project managers and research directors and their clients. Clients apparently have an 'organisational memory', but it was one-sided and only counted if work was of a poor quality. They appeared reluctant to cement long term relationships with individual project managers or the company. Because of this project managers felt that they were always on trial, continually having to demonstrate their credibility, no matter how experienced they were or how favourably their work had been received in the past.

Despite the delay in proceedings, towards the end of the workshop the group did get around to that part of the SSM process that involves generating relevant systems and building models. However it quickly became obvious that if the group was to agree on action outcomes and formulate an implementation plan, there would be a need for at least one and possibly more workshops, and a good deal of work carried out independently of these. For that reason it was agreed to reconvene at a later date.

In the event, the second workshop took place approximately six weeks later, during which discussion was structured around 3 models that related to the topic of internal relations and communication. Over the next three months or so, I had a number of meetings with various people where we looked in more detail at the issues that had been generated. And, independently of the second workshop, I and a group of three

project managers used 2 further models as a basis for examining the research-client relationship issue and the implications of this for the recruitment, retention and performance evaluation of projects managers. In the event, we ended up developing a series of competency profiles for project managers, and it is this aspect on which the published write-up of the intervention concentrates.

At this stage, having provided sufficient background information, let me now interrogate the process just described on the basis of what was said about some of the possible synergies between *AT* and SSM that were mooted in Chapter 9. Obviously since the intervention was designed with a very specific set of purposes in mind, and since my involvement did not extend to the implementation phase, I can only re-examine the case in the light of a subsection of issues that have been raised to this point.

## 11.3 Reframing the intervention

My general line of attack in designing and managing the intervention was guided by a desire to exploit the creative potential of SSM to generate new understandings among the various participants that might provide a basis for actions designed to improve communication, create more empathy among the various groups, and instigate better relationships. The workshops and the various feedback and discussion sessions that I conducted with individuals were designed with this purpose in mind. At the same time, I was fully aware that that there were many uncertainties involved, and that there was no guarantee that there would be a successful outcome.

The first thought that I have about how things might have been done differently revolves around the specific role played by the collaborative SSM workshops as a vehicle for instigating a serious consideration of the issues under investigation. It forces a re-think of the emphasis that was placed on the SSM workshops relative to other possibilities.

At the time of the intervention, I was operating on the assumption that it was necessary to get the various participants into an SSM-inquiry mode, and - importantly - into a workshop setting, as soon as possible. Since I was assuming that collaborative learning was the key to a better understanding of the problem, it seemed to me that the sooner the workshop began the better.

During the two workshops, I adopted the traditional SSM approach to exploring *Weltanschauungen*, i.e. by choosing relevant *W*-based systems, modeling them and then comparing these with their 'equivalents' in the real world. By all accounts this process worked reasonably well. Most of the participants seemed to be genuinely happy with the way the two events were conducted; they indicated to me, if not to Broad and/or each other, that they had learned something. Moreover there appeared to be a genuine willingness to follow up on at least a couple of the action plans that had been formulated and get them implemented. However, as it turns out, the results were very disappointing. Some action plans were not implemented while others did not extend beyond a few months.

Obviously I can only speculate on the reasons why the intervention could be regarded as being unsuccessful in these terms. One possibility however is that the participants did not take sufficient ownership of the proposed actions. If there is any truth in this then clearly questions need to be asked about whether the <u>process</u> that resulted in a these outcomes was actually addressing the 'real' issues.

In SSM the process of comparing and contrasting conceptual models is supposed to make transparent people's worldviews that govern the way they see things, and in the light of that, reveal workable solutions. No doubt there have been many SSM interventions where solutions have been 'realistically' grounded, where people have taken 'ownership' of actions and had them implemented.

The danger, however is that if a facilitator places too much emphasis on creativity and learning – as I suspect I did in this case - the overall inquiry process can become skewed towards generating new understandings instead of shedding light on existing understandings. In such a circumstance, confronting existing understandings directly may provide the basis for a more realistically 'grounded' accommodation (if one is possible) than through the rather indirect process that is envisaged in SSM. Even if such an inquiry reveals that an accommodation is unlikely, at least it provides the facilitator with an indication of what he/she is dealing with.

Through the period of the intervention we constructed a number of rich pictures that were designed to capture significant perceptions that the various groups had of each other. Learning-wise, there is little doubt that these were useful. Having produced the rich pictures however, there was no further direct systematic examination of existing viewpoints. Thereafter the focus was on creativity and learning, on finding something 'new' that would be the basis of an accommodation. To the extent that the group did re-examine existing perceptions it was done indirectly through the comparing and contrasting process, and as a secondary process during the 'feasibility' debate,

Even if the SSM comparing and contrasting process does reveal otherwise tacit pre-existing understandings, there remains the important matter of whether it goes far enough in terms of revealing whatever it is that might be creating difficulties. This takes us back to the question – raised in Chapter 8 - of whether in the first instance, a systematic examination of the explanatory domain concept might be worthwhile. Recall that I argued that this concept is much more fully worked-out than is the case with SSM's *'W'*.

Since I did not elaborate on what, in practical terms, this might involve, and have not pursued that line of inquiry further, I cannot develop it here. However I have in mind a process that is not dissimilar to that envisaged by Eden (1989, 1990, 1994), who uses

the technique of *cognitive mapping* in problem structuring intervention. Since cognitive mapping like *AT* also seeks to elucidate a generative process there are some similarities. However the process envisaged by Eden embodies a very different set of theoretical principles (based on Kelly's *'Theory of Personal Constructs'*) so there would be no simple one on one transfer. At this stage I merely flag the possibility that there might be some synergy that may be worth exploiting.

The main point is that although SSM may provide a more expedient basis for formulating creative possibilities for action, the wider sustainability of these is always likely to be a concern since appreciation of existing understandings proceeds only as a by-product of creative thinking and not as a primary activity in itself. My proposition is that *AT*'s concepts and terminology provides a more thorough description of existing cognitive processes that are at work and draws attention to the prevailing conversations and social and material practices that sustain them. Such understanding, if it can be obtained, provides a very clear understanding of the substantive differences between the protagonists. If this does not then suggest an accommodation, i.e. by pointing to a larger cognitive domain where the two parties can co-exist, at least it highlights what you are dealing with. Moreover, such knowledge can be extremely useful if a decision is subsequently made to try to exploit SSM's creative potential.

In retrospect then, I believe that it might have been better to delay the SSM workshops pending a more thorough inquiry into how the various protagonists view their respective situations. In my over-eagerness to exploit the creative potential of SSM to generate new understandings in support of an action strategy, I overlooked the possibility that a more durable plan of action, with a stronger sense of ownership from the participants might have arisen had I/we first taken a closer look at existing understandings and the various practices that sustain them. At this stage I am inclined to think that this is an activity that ought to take place working with the various groups

rather than across the groups although this is a matter that would need to be looked at in the light of prevailing circumstances and resource availability.

Since the object of the exercise here is to show how a knowledge of *AT* might augment SSM rather than possibly replace it, let us now look at how what was said in Chapter 9 might assist in reframing what actually happened rather than what might have happened. Essentially this boils down thinking about how I might now put in place a process through which an initial genuine accommodation between the various parties could be reached (i.e. create a consensual domain) which could then be used as a springboard for reaching more specific agreements on how the problems thus identified, might best be dealt with. The broad aim would be to try and instigate a conversational process that would make possible the mutual adjustment process that I have proposed is the basis of an accommodation. The key point from Chapter 9 is that while the SSM process does open up space for these processes to take place, I am arguing that they cannot be expected to happen automatically, and for that reason must be actively worked on.

*Bracketing objectivity*

Let us assume then that the SSM debate is to go ahead. At this point, if people are to develop a shared rational domain that is the basis for new actions, and – by implication – if they are to undergo sufficient structural change that makes an accommodation possible, it is axiomatic that the basic conditions that allow a conversation to occur are present. This being the case, first and foremost it highly desirable that the participants come to the SSM debate with both a capacity and willingness to *bracket objectivity*. If not then this must be developed during the process.

For Maturana, bracketing objectivity is essentially an emotional and relational issue. We are, he says, always flowing in emotions, and it is emotions that allow us to

enter one kind or other relational domain. Put simply, unless people with diverse views are inclined to bracket objectivity and both value and seek to preserve/enhance the relevant relationships then the recurrent interactions that are necessary for a conversation to take place will not occur.

Through the CRM study I was generally impressed by the apparent open-mindedness of most of the participants. Partly I see this as a reflection of the relatively small size of the company, and the natural interdependencies between the staff especially those who interact periodically through project teams. No doubt the apparent open-mindedness of the participants also reflected the fact that they had been counselled to be prepared to 'step outside of the box' and be non-judgemental.

There were some exceptions however. There were times when individuals found it very difficult to countenance the possibility that their opinion on a subject might symbolise anything other than 'the way things are'. In this regard the attitude and demeanour of the two field representatives presented the greatest challenge. Broad also admitted that he found bracketing objectivity difficult. He attributed this to a belief that 'open-mindedness' can easily translate into equivocality that can be make life difficult in the fast-paced business world. This corroborated an earlier classroom discussion in which I recall Broad and other MBA students remarking on the importance of decisiveness and promoting the virtues of making your mind up on an issue and sticking to it.

Looking back on the workshops there was a real intransigence on Broad's part in the way that he responded to a number of suggestions that were being made by his colleagues. For example there was a strong suggestion from project managers and research staff that the company might position itself as a boutique consulting-based operation as well as a 'mass production' provider of information. This, they argued, was desirable because it was what companies 'really needed', and because it would be more intrinsically satisfying for them as professional workers. This latter point derived from a

more general feeling on the part of research staff that there was insufficient time and resources in the company being devoted towards enhancing their own stock of human capital which they saw as being detrimental to their long term careers. So in addition to the possibility of a change in company philosophy, Broad was further urged to think about and discuss the possibility of providing researchers with more opportunities for attending conferences and participating in executive and professional development programmes.

At the outset, Broad's response was unequivocally clear. Having first indicated that he empathised with the researchers, he then proceeded to show that *'given the reality of the situation'* discussion of these points would be a pointless exercise. He argued that the idea that the company might shift the balance further in favour of securing consulting than data-providing relationships with clients was not feasible given the lower profit margins of the former and the money to be made from the latter. Further he denied the possibility that more money might be made available to support professional development activities.

At the time, the obstinacy of Broad and the field staff did not seem to seriously impede the flow of the discussion, but, for all I know, it may have done. And it is with that in mind that I now comment on what might have been done in advance of the debate to mitigate against this possibility.

In very general terms I would give much more attention to the emotional basis on which people participate. This is something that needs to assessed, and if there is an indication that the appropriate emotional state is not there in advance of the debate then it needs to be developed somehow. As I have already said, much of this comes down to the relationships between the participants. Even though existing relationships might be unsatisfactory they can still be deemed to be important, i.e. they are deemed to be worth

preserving and enhancing. I would want to emphasise and work on the latter. If the relationship is important then people are much more inclined to bracket objectivity.

Quite how a facilitator might go about developing this emotional predisposition will obviously depend very much on the situation in question. In the CRM case I would do what I have just suggested, i.e. work more on highlighting and strengthening the existing levels of awareness of the various inter-dependencies within the company. Broad for example, was aware of the researcher recruitment problem, but was he aware of how important researchers are in fulfilling the raison d'etre of the company? Judging by his response to the suggestion that it might be worthwhile discussing overall company philosophy, I think not. Equally there was a real sense that some field staff were completely oblivious to the fact that ultimately their jobs depended on project managers reaching out into the business and Government communities to drum up new business.

*The context of the conversation*

The next 'before-the-debate' consideration concerns the nature of the circumstances under which it takes place. In theoretical terms this consideration arises out of the proposition that although the conversations in which we participate are self-sustaining, they are subject to external influence, and they do change through:

> " . . . *the encounter with other human beings in a network of conversations that do not define it, or through the experience of situations that do not belong to it* . . . (or, put differently) . . . *when we encounter . . . foreigners, either when on a trip abroad or when visitors come to us, or when we move beyond the normal ranges of our community*" (Maturana 1988a:70).

To some extent, these processes are implicit in the very process of 'doing' SSM. But whether they are fully exploited in practice is another matter entirely. Certainly it is doubtful whether this was the case in the CRM study. Recall that there were two separate workshops with a gap of six weeks in between. Further recall that a significant proportion of the investigation involved me working with individuals as opportunities arose during

the course of the working day. Also significant is the fact that the workshops were held on company premises. This resulted in the inevitable temporary absence of key people and in other disruptions.

All of this being the case, it is hard to imagine that these arrangements provide an adequate context that seeks to instigate a process for calling into question otherwise highly conservative understandings. So what might be done differently? Resources permitting, I would lobby for the workshops to be held on neutral territory, and over a longer period of time. Equally I would want to revisit the balance between individual and group activities with a view to putting more emphasis on the latter and less on the former, although I acknowledge that some individually-focussed activities may be desirable.

Summarising to this point we can say that if there is to be a conversation that provides a basis for an accommodation among diverse interests, it is important to assess and, if necessary, to work on the emotional predispositions of the participants, and to think very carefully about the context within which the debate is to occur. Obviously it is necessary to do this in advance of the debate taking place. Once the debate is underway it is then necessary to first develop a 'conversation' (in *AT* terms), and then to maintain it through the process. It is to these issues that I now turn.

*Structural coupling*

Based on the logic of the argument presented in Chapter 9, there are four key requirements for maximising the possibility that the to and fro process of mutual adjustment which is the basis of an accommodation among divergent interests will eventuate. The first of these is to ensure that there are adequate structural couplings in place between the various participants, and between them and the broader environment of the debate.

As we have see, conventional SSM wisdom is that people gradually learn their way towards an accommodation, or to a solution that was not available to them previously, through the Singerian comparing and contrasting process. But this is only going to happen within a conversational process that cannot be assumed to already exist, and which may need to be worked on. In turn, meaningful conversations – those in which mutual adjustment takes place - will only occur if there is structural coupling between the participants.

Looking back on the two workshops my approach in dealing with the non-technical aspects of the SSM process was almost exclusively 'hands-off'. Certainly there was little advance preparation for this aspect of the process, neither did I give it much thought as it unfolded. Since, as we saw earlier, Checkland proposes that SSM can be 'given away' to the participants, I did not then - and I do not now - take my lack of concern about process as being in violation of any key SSM principle. At the time, I took Checkland's counsel to indicate that facilitators ought not to interfere with processes that are either relatively unimportant, or, that people are perfectly capable of managing themselves.

Through both workshops my disinclination to manage the process opened up space that others – Broad especially – were quite prepared to fill. And, with the benefit of hindsight, I can now see that this may have introduced into the proceedings a dynamic that potentially, if not actually, may have been detrimental to the whole process. Certainly it was the case that the many of the process-related decisions such as when the group ought to start, break, and conclude for the day, were instigated by Broad. For the most part, my role was to handle things from a technical point of view; beyond that I did not want to interfere.

Since the issue of poor relationships and communication difficulties between different groups of staff was instrumental in precipitating the study in the first place, this

might naturally be taken to reflect the unsatisfactory nature of pre-existing couplings within the company. So the question is how, to the extent that conversation might be problematic in these circumstances, can a facilitator act to deal with this problem? Although the emphasis here is on what might be done during the debate, since this depends on what is done before it, I shall comment on that aspect first.

During what is commonly referred to as the 'finding out' stage of SSM, I spent some time speaking individually, often via the telephone, to those who were planning on attending the workshop as well as to a couple of people who were not. Part of SSM's so-called *'cultural stream of inquiry'*, I took this to be necessary in order to familiarise myself with the situation, and to collect background information. However, I did not look at this background work from the angle of preparing myself for the facilitation-related aspects of the debate. And, for that reason I probably put rather too much emphasis on the situation, and rather too little emphasis on the particular individuals with whom I was interacting. This is a subtle distinction but an important one nevertheless.

Although getting too close to the participants can present difficulties of its own, having a broad familiarity with them is advantageous. Managing the conversational process places a premium on the facilitator knowing the people concerned, being able to 'speak their language' – if only in a rudimentary sort of way - and appreciating their concerns. Checkland (1990:242) speaks about *'remaining rooted in the world of the participants'*, but does not expand on or theorise about this in the context of managing the process side of SSM.

In the CRM study this issue was crystallised in the difficulties that arose in opening up useful dialogue between the field interviewers and research staff. More often than not, the former are women with small children who are working part-time in order to supplement their income. Generally they do not have formal qualifications, and the work that they do is highly structured. Researchers – on the other hand – usually have honours

and/or masters degree qualifications supplemented by professional training. Their work is analytically challenging and often highly ambiguous.

Despite their natural interdependencies these two groups have very little in common. Some would say that they are almost 'separate breeds'. Moreover, they do not come into direct physical contact during the course of a working day. Given the difficulties that we experienced, it is now clear that establishing sufficient coupling between these two groups (both within and outside formal debates) that might form the basis of an effective conversation is something that ought to have been worked on.

To all intents and purposes Broad was the only person who it could be said I was 'in touch with'. As one of my ex-students, I had known Broad for some years; there had been several conversations between us about his job, about the company he worked for, and about his career. Indeed I had come to know him quite well.

Now I am inclined to think that the whole SSM exercise can be jeopardised if the facilitator begins a collaborative workshop without first having got to know something about the people taking part. In the CRM study my main focus was on exploiting the creative potential of SSM to produce new understandings. And since the key mechanism for this is the comparing and contrasting process I was inclined to think that the 'real' intervention did not begin until that point was reached.

With the benefit of hindsight I would now put much more emphasis on background work that extends beyond that of gradually building up a picture of the cultural and political context within which the problem situation is located, to one that also builds up a adequate portrait of the people who are to be involved in the debate. Without such background preparation, I would not now be confident that I would be able to facilitate the sort of debate that is necessary to produce an accommodation.

This background knowledge also assists in formulating a strategy for using SSM. In the CRM study, Broad and I had agreed from the outset that this would be a 'SSM

study', and initially the intervention was referred to as such. Now I am inclined to think that whether this is a good or a bad thing will depend on a variety of factors, not least on the people who are to be involved.

In order for an effective conversation to take place not only must participants be structurally-coupled with one another, but they must also be coupled with the various tools and techniques that make SSM the distinctive methodology that it is. In this study, with the exception of the difficulties that a few people had with modeling and with 'cycling between the two worlds', people generally responded positively to what was being asked of them. However I suspect that this was more a result of luck rather than good judgement on my part. In other studies I have been less fortunate and there has been a noticeable 'alienation' effect on the part of some individuals.

In circumstances where there is ample evidence to suggest that the approach might invoke a negative reaction or 'get in the way', then it may be appropriate for SSM to remain unobtrusive to the participants, i.e. use it tacitly instead of explicitly. This is something that Checkland himself has been known to endorse (see 1990:239).

*Collaborative action as the basis for an accommodation*

Let us now consider the significance of 'collaborative doing' as providing a basis for accommodation. In SSM, the key to reaching agreement on action is the learning that arises through the comparing and contrasting process. Technically the choice of models is less important than the way in which they are used in comparison (see Davies and Ledington 1991:107). Logically however, since the nature of what is learned depends upon the various choices that are made through the modeling process the various participants will often feel that they need to select systems and construct models with great care which is why debates about the 'relevance' of notional systems are such an important part of the SSM process.

During, between, and after the two workshops, the various participants and I put a great deal of emphasis on the so-called *'below-the-line'* SSM activities. From the perspective of reaching accommodation – if not from the perspective of dealing with specific issues - I would now question the wisdom of this. If the emphasis is on reaching agreement (where the actual nature of the agreement may be relatively unimportant) the critical point is not what people do, it is that they are doing things, and – importantly - doing them together. With the benefit of hindsight, the amount of time that the group initially spent in a sometimes frustrating and prolonged debate about which systems to model and which to reject, may have been time wasted. I say this even though I accept that such debate is, in itself, a form of doing, and can therefore be part of the process of gradually moving towards accommodation and not necessarily antithetical to it.

During the first workshop, the group spent a lot of time debating and arguing the relevance of various nominal systems. Thereafter, scrupulous attention was paid to formulating root definitions and constructing models. In particular because the emphasis was on learning, and because Checkland puts great faith in the value of *issue-based* and *counter-intuitive* models for generating creative insight, this was an avenue that I sought to exploit. Unfortunately these systems seemed to generate more somewhat acrimonious debates about relevance than did the others. For that reason they seemed to highlight the differences between people rather than assist in moving forwards. I would be hard pressed to claim that such debate was surreptitiously steering the group in the direction of mutual understanding, which is what was supposed to be happening.

Looking back on the half a dozen or so SSM interventions that I have facilitated, I am struck by the fact that generally, if people can get the hang of drawing rich pictures, of constructing root definitions and modeling – if they can 'get on with the job' - so to speak, then this has resulted in there being some sort of agreement at the end of the process that I have felt to be genuine if only within the context of its construction. When

people have found these activities difficult or, for other reasons, have not become involved in the process, then the emergent climate has tended to make finding an accommodation difficult, or if an accommodation does emerge, one that hints at expediency.

In the CRM study the approach that I took might have destroyed the very process that is critical in moving towards accommodation. Because it is one of SSM's most distinctive feature and because the whole exercise had been billed as a 'SSM workshop', I was intent on getting to the modeling stage as soon as possible. Once there I was anxious to hurry the group into making decisions about relevant models as a means of getting to the supposedly critical comparison stage. In retrospect in my haste to reach this stage I ran the risk of destroying the very 'doing' process that, in *AT* terms, is pivotal in reaching an agreement.

From another angle however, there were a couple of occasions when I somewhat spontaneously resorted to 'below-the-line' activities simply because in that moment I lacked a clear perspective on which direction the group ought to be heading. Frankly I have usually taken this to reflect my own lack of experience or ability as a facilitator, which it may well be. When the tone of the conversation between Broad and the researchers, for example, became emotionally charged and potentially quite disruptive, I sidestepped the issue by redirecting the group back to modeling. The effect - if not the intention – of this was to give me some breathing space and it allowed the people concerned to calm down.

Previously when the debate has become strained or has looked like it might peter out, acts such as these have tended to be expedient and spontaneous. I now regard such action to be theoretically sound. In moving towards an agreement and in dealing with problems, the trick is to keep the conversation flowing. Whereas unstructured conversations and debate can easily 'dry up' or reach a stalemate when two or more

opposing factions confront one another, having resort to technical activities such as modelling or embellishing a rich picture, can provide a focus that can prevent this from happening. When people are interacting and doing things the conversation can continue. And, if the conversation continues, then by definition, the participants are structurally-coupled. If the participants are structurally-coupled in conversation then the mutual adjustment process that leads to an accommodation has a better chance of happening spontaneously.

Rehearsing this aspect of the intervention now, I would not be as concerned as I was about the specific content of the below-the-line activities. Since the nature of what is learned does reflect the various choices that are made through the modeling process, it may still be necessary to select systems and construct models with some care. However I am now more inclined to say that this comes after the event, i.e. in looking in more detail at particular issues, rather than being a critical aspect of generating an accommodation in the first place.

Initially the main emphasis ought to be on getting people involved and getting them working together so as to create a positive climate that will facilitate mutial adjustment. If compromises have to be made in relation to specific learning objectives or in relation to the technical precision of root definitions and/or models, then I would not now be unduly concerned about this. If the aim is to create an arena that maximises the chances that an agreement will be reached - if only a temporary one - then the primary objective is to get the various participants talking and interacting first. Interestingly this does more than just hint at the wisdom of combining SSM with more conventional teambuilding exercises, and I submit that this is a possibility that is worth exploring further.

Finally, it is worth reiterating the importance of the participants doing things collectively and collaboratively. In general if people are to develop a shared rational

domain that is the basis for actions then the more direct contact between the participants the better. During the study under discussion here, the two workshops provided an opportunity for the various participants to fully interact in relation to the main issues under consideration. Unfortunately because I was working with very busy people who were operating in a highly competitive environment, time was at a premium. As a result, much of the work that was done with the various participants was done on an individual basis, occasionally via the telephone and often during short periods in between other pressing engagements. Earlier I suggested that it might be desirable to have a more systematic examination of individual meanings in advance of the collaborative debate. Notwithstanding this, once a decision has been made to go ahead with a debate in search of an accommodation, I would want to impress on the client or sponsor the importance of having people spend as much time as possible doing this in a collaborative setting.

*Managing mood and emotions*

Managing emotion and mood is the next critical facilitation-related issue that arises out of the perspective being developed here. I have already said that emotion is important because it impacts on the likelihood of establishing a conversation in the first place, and because it largely determines the extent to which people are open to new ways of thinking. However it is just as important once a conversation is underway.

In connection with this, the main issue that arose during the CRM workshops revolves around when the facilitator introduces competing perspectives into the debate. Given SSM's iterative nature, there is nothing to say that competing perspectives must be introduced at an early stage, although in my experience this is usually the case. In the literature, users of SSM are encouraged to illustrate competing perspectives in the rich picture, and this tends to be formulated relatively early in the proceedings.

To this end, the following verbatims - taken from my field notes - capture a sense of some of the perspectives that were revealed during the preliminary interviews and which were integrated into the rich picture.

*"All the company is concerned about is cash flow and generating new business; it is not interested in our longer term professional development" (project manager)*

*"We are consultants to our clients, we are not boffins. It is important that they tell us what their problems are, not tell us what to do" (research director)*

*"I just wish that clients would show more loyalty to us. No matter how good a job we do, they always seem prepared to go elsewhere. It is quite demoralising" (project manager)*

*"They (research staff) always want things at the last minute. That is fine providing everything works first time, every time. Unfortunately we cannot guarantee that" (support staff member)*

*"Half the time we do not have a clue what she (project manager) wants" (field staff member)*

*"I have to constantly juggle (field) staff availability with meeting their (research staff) deadlines. Often I just think that they are being bloody minded. In the past I have gone through hell and high water to find staff who can do a job only to find out later that there was no deadline. They do not exactly treat us like dirt, but. . . "*

*"The problem in this job is that you just do not have enough time to think" (project manager)*

Insofar as the introduction of viewpoints such as these can introduce a strong emotional undercurrent to the debate, the process must be carefully managed. In the extreme, failure to do so can preclude establishing the very conversation that is the basis of an accommodation.

During one of the workshops, I was alarmed at the intensity of the field supervisor's attack on the research group. She recounted the story of one of project manager, not long graduated from university, who apparently *'always issued impossible deadlines'*, and *'never showed any respect to field staff, including me, and I am old enough to be her mother'*. Subsequent debate seemed to confirm the general veracity of this comment. But that is not the point. What is important is the marked impact that it had on the debate. Two of the researchers became quite defensive about it, another more apologetic. Needless to say the flow of the discussion was impeded.

Facilitators then must make a judgement call. Do competing perspectives be introduced early in the debate (on the grounds of seeking some degree of 'completeness' before moving into modeling)? Alternatively, ought the first priority be to establish a coupling between the participants and get the conversation going, even if this involves temporarily steering clear of difficult issues. I am now inclined to think that the latter is preferable, although I acknowledge that it is not always possible in advance of knowing what issues are likely to create difficulties, or delay them, if indeed they do arise prematurely, or at an otherwise inappropriate juncture.

In general terms, I cannot recall giving much attention to this aspect of the debate, although I can recall several occasions when there was a degree of emotional discomfort and tension among the participants. There were times when the process appeared to being driven primarily by Broad's agenda, other times when the concerns of the research staff were holding sway. On both occasions I detected irritation on the part of the other members of the group, although it would be stretching a point to state categorically that overall this had an adverse impact on the outcome.

Being in possession of relevant background information is important in managing this aspect of the SSM process. However beyond this I now venture to suggest that facilitators ought to consider enhancing their conversational skills through active listening, through learning how to recognise and appreciate the emotional underpinning to statements, to have greater empathy, and through being more sensitive to mood.

Previously I laboured under the illusion that the critical facilitation requirements of SSM were to fully understand its epistemological base, and have command over its technical aspects. Now, given the importance that I am attaching to establishing and maintaining the conversation, I believe that monitoring the interactions of the participants, monitoring their emotions and moods and that of the group as a whole, are equally important if not more so.

Indeed, depending upon the experience and skills of the facilitator, serious consideration might usefully be given to having two people involved; one responsible for the technical aspects, the other responsible for the human and social dynamics that I have been referring to.

*Direct intervention to facilitate accommodation*

The previous three points bear upon the question of how the SSM process might be managed – beyond its purely technical aspects - so as to maintain the necessary couplings in order to maximise the chances that the to and fro process of mutual adjustment that is the basis of an accommodation among divergent interests, will happen spontaneously. However, there is always likely to be an element of uncertainty surrounding the likelihood of this happening. Perhaps there may not be enough time for this, or perhaps not enough collaborative activity going on. More to the point, since this is something that Checkland himself acknowledges, the collaborative activity that is going on might reinforce or further precipitate competing perspectives, instead of providing ways of revolving these.

In thinking about the role that a facilitator might play to this end, once again we come face to face with the problem of the closed, conservative nature of the cognitive domains from which people draw their understandings. Confronting, and possibly changing these understandings, as we have already said earlier in this chapter, is possible through non-confirming interactions or through confirming experiences. To some extent this is implicit in the SSM process particularly when there is an emphasis on collaborative rather than individual activity, and particularly when this happens in what amounts to a different 'cultural' environment.

Beyond that, change can also come about through " . . .(significant) *interactions that trigger in us reflections upon our circumstances of co-existence with other human*

*beings"* (Maturana, 1988a70). As we saw in Chapter 9 these are referred to as orthogonal interactions. And it is in relation to this possibility that the precise nature of the actions and interactions of a more proactively minded facilitator assumes great importance.

Earlier I said that in order to promote effective interactions and structural coupling between the various participants it helps to get to know them as individuals. I also said that doing so could create a different set of difficulties, and that it is important that the facilitator is mindful of these.

By way of explanation recall my comment to the effect that the only person in the study who I felt that I knew well enough was Broad. In my discussions with the others I was focussing almost exclusively on building up a picture of the problem situation that we were facing. Even if I was aware of the possibility in advance – which I was not – I was in no position to act orthogonally with these people, even though some relevant information about their experiences and priorities had emerged through this process.

I was however in a position to act orthogonally with Broad, but since this was not a consideration at the time I failed to do so. More alarmingly, when I look back at my interactions with Broad, I now believe that I may have been reinforcing the very cognitive domains that reaching a genuine accommodation might have hinged upon him leaving.

For instance, on more than a few occasions I empathised with the concerns that he had expressed about the performance results-orientated pressures that shareholders were subjecting him to. This may well have coloured my interactions with him during the whole exercise. Moreover it may well have damaged my credibility with the research group who were arguing for a softer line on this commensurate with their desire to under price on proposals as a means of cementing longer term relations with clients.

I shall never know whether this was indeed the case. However that Broad evidently did not seriously contemplate the possibility of modifying his position may

partly have been a consequence of my failure to explore the 'zones of orthogonality' that I spoke about in Chapter 9. No doubt because of my lack of relevant knowledge about the other people involved there were occasions with them, when - through my interactions - I merely reinforced prevailing views, and/or did, or said things that were beyond their domains of experience.

Thinking now about what a 'significant' triggering orthogonal interaction might have been is very difficult, particularly since the 'significant' aspect of this is very important. In the search for an accommodation, perhaps I could have exploited issues that were going on in the company at the time by presenting these in an appropriate 'zone of orthogonality' manner to the people concerned. Interactions could have been couched in terms of the number of staff who had left in recent months as a result of dissatisfaction and frustration at how the company was operating internally, and/or in terms of the dangers that might stem from the intensification of competition that had occurred as new independent companies and international conglomerates had set up in Australia and New Zealand. Certainly there are these, and no doubt many other, possibilities.

## 11.4 Summary

This chapter has revisited a SSM intervention in the light of some of the emergent *AT*-based perspective on the approach that was developed mainly in Chapter 9. It has sought to illustrate key theoretical points by thinking about how things might have been done differently had account been taken of these in designing and conducting the intervention.

Reframing the intervention involves shifting the focus, often in quite subtle and nuanced ways, away from seeking to exploit the creative aspects of SSM by giving disproportionate amount of attention to the precise nature of modeling activities. Instead more emphasis is put on placing such activities within a broader context. This involves

thinking in more detail about how to establish the conditions that are necessary for a genuine conversation to take place among the participants. This is something that cannot be presumed to exist already but which is vital in harnessing the process of mutual adjustment, out of which an accommodation of diverse interests is born.

Specific aspects of this context that arise as important before the debate include its choice of setting and the emotional predisposition under which the participants get involved. During the debate emphasis switches towards the process of creating and managing the various conversations that take place. This places the onus on the facilitator to engineer the necessary structural couplings not only between the participants themselves (for which adequate background knowledge is required) and between them and the tasks that they are being required to fulfil; to acknowledge the crucial role played by collaborative 'doing' activities; and, no less importantly to manage individual and collective moods and emotions as these evolve through the process. Where the facilitator takes on a more proactive role in search of an accommodation, rather than merely creating the conditions under which this might be expected to occur naturally, he/she has to know where the 'zone of orthogonality' resides for any individual in order to trigger movement in otherwise highly conservative domains of understanding.

Finally, through this chapter I have identified some important dilemmas facing the facilitator, and/or judgment calls that he/she must make. These include finding an appropriate balance between individual and collaborative activities; knowing how much emphasis to put on SSM relative to other possible activities such as team-building and cognitive mapping; identifying the optimum time at which to introduce into the debate competing perspectives; and finally, having a clear perspective on whether the emphasis is on finding an accommodation, in which case the content of *'below-the-line'* activities is relatively unimportant, or on exploring the creative possibilities for dealing with specific difficulties, in which case the content of such activities may be vital.

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# PART SIX : CONCLUDING DISCUSSION

## 12. DISCUSSION AND CONCLUSIONS

I shall begin this final chapter by recalling where the discussion has been and, in general terms, provide a summary of its distinctive contribution. Then I will identify and respond to possible criticisms. Finally I shall speculate on where this strand of research might go next.

### 12.1 Summary and the distinctive contribution of this work

This investigation set out to juxtapose *Autopoietic Theory* and *Management Science* partly on the grounds that it seemed to be an interesting academic exercise, but also because there were reasons for thinking that looking at *MS* from a novel vantage point might convey some direct practical benefits. Since it is well known that disciplines can withdraw into themselves, since they need an external focus for the subject to be refreshed/regenerated, there was a sense that *AT* might be useful in plugging gaps in the existing *MS* literature and raising questions that may not otherwise be raised. *AT* was chosen for this task not because it is the only source of theoretical inspiration, or the only spur for thinking differently about *MS*, but because there were some strong a priori reasons for thinking that it may have something to offer. These reasons were outlined in Chapter 1.

Having, in the first instance, outlined the 'what', the 'why', and the 'how' of the research, the main substantive task in Part 1 of the thesis was to construct a frame – based on *AT* - that would be sufficiently robust to withstand the rigours of 'application' through the main body of the work. Given the breadth and complexity of the theory, given its interlinking of cognitive, epistemological, affective, ideological and ethical

considerations within the same frame, it became immediately obvious to me that in applying *AT* to *MS*, some aspects of the former would be more relevant than others. This is especially so in the case of application at the methodological/technical level, although it also applies at the level of the discipline as a whole.

Unfortunately, I was not then in a position to know exactly what these aspects would be. More importantly, having attended one of Maturana's public seminars - at the Open University in 1997 - and having witnessed him stressing repeatedly the essential coherence and systemicity of his work, i.e. presenting it as a 'system of ideas' and not merely a collection of disparate components, I wanted – at least in the first instance - to respect this attribute.

Early on in the piece, I further realized that although it would be necessary to develop a sound working knowledge of specific aspects of *AT*, I would also need to appreciate and have some command over the context to which these belong and which gives them their distinctive meaning. Put rather bluntly, I felt as though I needed to be able to see the wood as well as the trees. Often it seems that where the 'application' of *AT* revolves around just one or two aspects, things can easily get distorted and intended meanings can get lost in the process. As a result, much of the originality and power of *AT* is lost in the process. Witness for example, Beer's use of the term 'autopoiesis' in the Theory of Viable Systems; further witness Morgan's impoverished use of the term in his book of organizational metaphors.

While I could readily appreciate Maturana's sentiment that his work be understood as a systemic whole and not as a random collection of thoughts, to my mind the primary literature did not really help in grasping this aspect of *AT*. As a result, I found the systemic nature of *AT* very difficult to pin down. Initially I found Maturana and Varela's *The Tree of Knowledge* useful, but for my purposes this places too much emphasis on the early biological work, and not enough on Maturana's own later work

that develops the phenomenological aspects of *AT* in greater detail. Maturana's epic paper *Reality: The Search for Objectivity or the Quest for a Compelling Argument*, was much closer to what I was looking for, and for many months I poured over what is now the most dog-eared and many-times annotated of the collection of photocopied papers that occupy several bookshelves in my office. Even there however, Maturana's presentation is somewhat reductionist, the crucial linkages between the elements - to my way of thinking anyway – not being adequately explained.

To a much lesser extent the same could be said of John Mingers' text *'Self-Producing Systems'*, although that is understandable since Mingers' own brief was to open up the opaque language of autopoiesis and provide a full account of the breadth of its impact across a number of domains which the book does extremely well. Mingers' project was not to 'bring it all together'.

In any event, I rapidly came to the conclusion that understanding the parts necessitated understanding the whole, so I set out to produce my own simplified account of *AT*; a sort of 'beginners guide' so to speak. This is described in Chapter 3, and is presented in an even more simplified 'mind map' form in Figure A.

It is important to see Figure A as a mind map and not as a SSM-type rich picture. The figure is my <u>own</u> account of Maturana's system of ideas showing their interconnected-ness. It does not seek to represent what *AT* 'is', or present alternative perspectives. In the spirit of structure-determinism I am reminded of a comment that Maturana made at the Open University seminar in response to an interlocutor who had clearly misunderstood him. Maturana – with a smile on his face - remarked: *"I am fully responsible for what I say but completely <u>irresponsible</u>* (sic) *for what others hear"* (emphasis added).

So the summary of *AT* is very much a personal interpretation. Nonetheless, given the complexity of the ideas on which it is based and given the inaccessibility of much of

its literature, I would regard this as a significant contribution. Certainly it is a perspective on *AT* that evidently I have put to good effect in introducing this 'connectedness' aspect of *AT* to students in teaching.

Although the content of Figure A is relatively straightforward, the fact of the matter is that I found the subject extremely difficult to grasp. For that reason, the frame construction part of the exercise took much longer than expected. Certainly it was a much more time-consuming and convoluted process than I have found to be the case in constructing the traditional 'literature review'. *AT* ranges across a number of highly specialist fields which means that assessing the implications of any of these elements in any area of practice – *MS* included – was clearly not going to be a simple task.

Having eventually constructed the frame, I then set out to 'apply' it. In this context I have taken the term 'application' to mean looking at *MS* with both a critical and a developmental eye. Thus Part 2 of the thesis focuses on *MS* as a general class of activity, Part 3 focuses on Viable Systems, and Part 4 on Soft Systems. Finally Part 5 has sought to illustrate some, though by no means all, of the insights generated by reviewing two previously carried out interventions.

Since the juxtaposition of two ostensibly unrelated sets of ideas is a legitimate academic exercise in its own right, it is not absolutely necessary that looking at *MS* with *AT* in mind will bestow upon the former clear practical benefits. Indeed much of the discussion through the work has been unashamedly theoretical, and I can see that the implicative transitions to the world of practice are not altogether clear.

Nonetheless since a distinctive contribution of this thesis has been to outline what contribution *AT* might play in <u>developing</u> *MS*, it is incumbent on me to now spell out, in very general terms, and by way of a summary, what direct role I believe *AT* can play in this discipline. Naturally I shall be mindful of the nature of the discipline and the predilections and priorities of those who practice it.

The possibility that *AT* might provide a broad unifying paradigm for *MS* along similar lines to those sketched by scholars who have looked towards Habermas (see Jackson, 1991; Flood and Jackson, 1991), and Bhasker (see Mingers 2001), has not been addressed in this work. This being the case, I shall simply leave this question open and earmark it as an avenue for future work. Beyond that, it seems to me that there are two possibilities.

The first involves taking *AT* to be a scientifically-grounded broad-based philosophy, a set of theoretical propositions, and a distinctive set of concepts and language, that provides the basis for a distinctively different way of 'doing' *MS*. Recall that I started out with the proposition that essentially *AT* is a particular kind of explanation. It is an explanation that posits a generative process that seeks to explain the nature of the life, the emergence of basic languaging capabilities, and - in human beings – the emergence of the observer. In some cases (such as where someone undergoes a religious or political 'conversion', or where there is a new scientific discovery) an explanation has the potential to dramatically change the life of anyone who happens to accept its generative process as a valid explanation of his/her experiences whether these be actual or deduced. In less earth-shattering circumstances, accepting an explanation can affect the way the person goes about doing things; they tend to do things differently.

Now it is entirely possible that for some of its supporters *AT* - like a religion - can wreak major changes, if not havoc, in their lives. Effectively it becomes a form of 'being'. At the other extreme, changes might be more modest; at the margins, and/or so subtle that they might only be detectable by the person concerned. In between these two extremes there is a myriad other possibilities.

To some extent, these extremes are mirrored in the preceding pages. Chapter 5 for example, has outlined in some detail what it would mean should a management scientist seek to fully operationalise some of the main tenets of *AT* in his/her practice of, and

writing about, *MS*. Later, in revisiting a previously carried out SSM intervention, Chapter 11 suggests that we are speaking more of a subtle and nuanced shift of emphases – perhaps from technique to process - than of anything more substantial.

In the first of the two situations just described, operationalising *AT* as a philosophical underpinning for *MS* raises a number of feasibility questions. For many management scientists, especially those who have been trained and socialized in the norms of objectivist science, getting to grips with and putting into practice the epistemology of *AT* means relinquishing existing ways of thinking, and formulating a different view of the world. Doing this is no easy task; it requires a fair amount of thinking and effort on the part of the individual concerned. Relatedly, there is the organisational feasibility of putting *objectivity into parentheses*. By and large objectivist rationalities of one sort or another are still the dominant discourse in the kinds of organisations that most management scientists deal with.

These are important considerations that cannot be easily dismissed. However, since I have discussed them at some length elsewhere (see Brocklesby 1997), and since this feasibility issue has not been a major avenue of inquiry through this project, having flagged the issue, I shall leave it at that.

The second substantive role that *AT* might play in *MS* has more to do with there being direct practical benefits than with the kind of intrinsic commitment on the part of the management scientist that I have just been speaking about. To some extent this is what I have been getting at through that part of the discussion that has been identifying 'gaps' in the literature and looking at how *AT* might fill them.

There are many examples of such gaps in the preceding pages. The question of ethics and the issue of management scientists taking responsibility for their actions; the problem of implementing change in closed, conservative social systems; the role of emotions in formulating proposals for change and in organisational and professional life

more generally; the skewed dehumanized reporting of *MS* practice in textbooks and journals; the ambiguous epistemological grounding of viable systems interventions; the absence of good theory to help in thinking about improving the handling of process issues in *MS*, and in SSM in particular. These are just some of the gaps that I have suggested that *AT* might assist in filling.

Essentially the *AT* frame that I have employed here is all about human beings and about the social processes that circumscribe and govern their lives. This being the case, it is hard to imagine that *AT* does not have a useful role to play in *MS*. When, for instance, a management scientist spends time interacting with an individual manager there are aspects of individual counseling which is an important languaging and conversational process. When he/she goes into a meeting or facilitates a problem-structuring group or helps in formulating strategy, he/she has to draw on models and skills that have to do with how to work in a group, or in a 'network of structural coupling' as Maturana might put it. If he/she gets involved in designing or planning an IT system that is going to impact on company performance, he/she has to know how you might best go about getting it implemented which might necessitate knowing about the extent to which it is possible, and how an external agent might 'successfully' perturb closed structure-determined systems.

Currently aspects of *MS* practice such as these are not theorized about to any great extent through its own literature. Many people have drawn attention to the importance of developing people and organisational skills, but, as some of these have acknowledged, there is not much good theory on which to build good practice in relation to these issues. Neither is there much training. I am suggesting therefore that *AT* can provide some useful conceptual tools that allow management scientists to think about these kinds of issues.

Although the basic proposition is not new, it is nonetheless worth restating. Practical *MS* is something that is carried out by real people, and it is as much about

people and the organizational systems and settings in which they participate, as it is about technical systems and mathematical algorithms. This is an important consideration if only for instrumental reasons. In the present climate of performativity and obtaining value for money, considerations about people and social/organisational processes need to be part of the *MS* equation if corporate clients and funding agencies are to continue looking to *MS* for help.

Having said all of this, the traumas that I have experienced in coming to terms with this body of theory makes me wary of commending *AT* to management scientists on the grounds that it will lead to immediate improvements in the way that they practice *MS*. In some respects it makes the life of the practitioner more difficult since if he/she wants to connect with it in anything other than a highly superficial and/or fragmented way, it requires that his/her thinking about human, social and organisational processes be much deeper than I suspect it often is. Perhaps there is some comfort in knowing that it is often the case that things get worse before they get better. That was certainly true in my case.

Whether we are speaking of a broad philosophy at one extreme, or less momentous changes in the way *MS* is practiced at the other, or merely speaking about plugging gaps, it is important to state that there is no intention here to thrust *AT* on anyone. Although his published work often seems to convey a contrary intent, in face-to-face conversation Maturana maintains that he does not seek the agreement of others in presenting his ideas. Even less does he seek their obedience. Instead, he offers his thoughts in the spirit of an invitation to enter into a particular domain of discourse where the premises are fully disclosed.

That is how it should be, and I would urge any management scientist who might happen to stumble upon this work and contemplate how he/she might use it, to digest its contents in the same light. In any case, the principle of structure-determinism renders futile attempts at thrusting *AT* on anyone.

It follows then that if we accept that, in principle, *MS* - or any aspect of it - does welcome help, this should be provided on an individual basis, depending on user preferences and situational needs. At one level *AT*, is for those who find it a plausible explanation of their actual and deduced experiences and who accept the biological premises on which it is based and the wider epistemological and social ramifications of these. At another level it is for those who simply might use it in some meaningful and distinctive way as they go about their daily business.

## 12.2 Limitations, criticisms, and future research

### 12.2.1 Philosophical issues

*Paradigm incommensurability*

When someone employs an external frame that embodies its own epistemological assumptions that are at variance with those of the phenomenon that is being reflected on, the matter of paradigm incommensurability arises (see Kuhn 1970, Burrell and Morgan 1979, Jackson and Carter 1989). In this context since *AT* is premised on different philosophical assumptions to much of what is taken to be *MS*, this issue requires comment. Specifically the issue of paradigm incommensurability arises because *AT*'s constructivist epistemology stands in opposition to the objectivist epistemology that characterises much of the dominant hard *MS* tradition, and - while related - is not synonymous with the interpretive philosophy of the soft *MS* tradition.

This being the case, a hard-line supporter of the incommensurability thesis might claim that there is little point in using *AT* in the manner that I have. Specifically critics might claim that using *AT* in a critique mode is a rather pointless exercise, since it amounts to pitting one unproven philosophy against another.

The simplest response is to plead that the paradigm incommensurability thesis is not inviolable. Today there is a substantive literature which asserts that inter-paradigm exchange is not as problematic as was once thought (see, Smaling, 1994; Gioia and Pitre, 1990; Willmot, 1993a, 1993b). Indeed there are even management scientists and those who write about it who have constructed arguments to show that it can be beneficial (see Bryant, 1988; Mingers and Brocklesby, 1997).

Secondly, and more pragmatically, there is the rather obvious point that juxtaposing *AT* with *MS* can occur at different levels, philosophy being just one of these. While, it would be wrong in my opinion to discount the epistemological propositions of *AT* since these are a key defining feature of it, epistemology is not the be all and end all of it. It would therefore be unwise to discount the possibility that *AT* might be a source of theoretical and/or practical inspiration to a management scientist, simply on account of it having an epistemology that he/she might not be comfortable with.

In a sense, the incommensurability thesis is much less of an obstacle in juxtaposing *AT* with *MS* than it would be if we were looking to juxtapose two or more basic research traditions. Research, in the physical and social sciences for example, is primarily geared towards knowledge acquisition. The parallel in *MS* is when recommendations for action and intervention strategies proceed on the basis of some understanding of 'what is going on'. Hence, truth claims in *MS* are not unimportant. Yet as we have seen, the primary focus in *MS* is to accomplish specific purposes, which - depending upon the paradigm that we are speaking about - might include engineering a system according to some blueprint, aiding prediction and control, developing inter-subjective understanding among people, or exploring taken-for-granted system boundaries.

In such situations, what one takes to be the 'truth' and how one goes about revealing it is not necessarily a matter of pressing concern. Getting the job done in a

community of people and to their satisfaction is what counts. So assessing the validity of one knowledge claim over another may not come into it. In simple terms the object of the exercise in a good deal of *MS* is simply to find better ways of doing certain things.

Philosophical niceties aside, there is no compelling reason why people working in one paradigm cannot entertain ideas emanating from another. As Feyerabend puts it:

> *"Knowledge . . . is not a series of self constructed theories that converges towards an ideal view . . . It is rather an ever increasing ocean of mutually incompatible alternatives, each single theory, each fairytale, each myth that is part of the collection forcing the authors into greater articulation and all of them contributing, via this process of competition, to the development of our consciousness."* (Feyerabend 1993:21).

Even if the process does involve juxtaposing two seemingly incompatible epistemologies it does not necessarily follow that the debate that it generates must necessarily favour the one or other epistemology. As Morgan claims:

> *"Providing the debate . .. facilitates exploration rather than constraint , encouraging those involved to gain a measure of detachment from their usual presuppositions through reflection on the nature and implications of what they and others do in their research and the consequences of the knowledge they generate . . . the point is to learn from the process itself and to encourage the conversation to continue. In so doing, we are able to minimise the hegemony of a fixed evaluative stance or of any conventional wisdom that seeks to brush disagreement aside under the delusion that it can know what is true and right . . . in favour of an edifying exchange that thrives on self-conscious criticism, challenge and diversity."* (Morgan 1993:375)

Paradigm incommensurability then, is not an insurmountable obstacle that precludes using a set of ideas that are underpinned by one philosophical position to interrogate a field of inquiry and practice that is based on different principles. The proponents of paradigm incommensurability cannot dictate to others what are legitimate and non-legitimate sources of learning. This is a matter of observer judgment and, in many cases, will be based on more pragmatic considerations.

I submit then that there are sound *prima facie* reasons why there is value in using *AT* to interrogate the field of *MS* even though, technically this may breach the

incommensurability thesis. At the beginning of this work I said that application of *AT* to *MS* could be taken as a manifestation of Morgan's *'reflective conversation'* method, as one of Feyerabend's *'dreamworlds'*, or as a set of ideas to further develop our appreciation of the *MS 'Lebenswelt'* (Vickers). Which one of these does not really matter since the logic is the same; the process has involved employing an external frame to generate new insight, to promote learning, and to open up creative possibilities, it has not been to forcibly assert the validity of one philosophy over another.

*The dual role of frames*

The second philosophical question that is worthy of comment concerns the 'reality revealing' and the 'reality creating' role of frames that, broadly translated, are mirrored in the 'critical' and 'developmental' perspectives that I have used to structure the discussion. Recall that depending on the particular perspective used, it is possible to take *MS* to be a pre-existing entity that *AT* can examine, critique and comment on, or *AT* can be used to define a distinctive form of *MS* practice. These two possibilities arise out of Tsoukas's claim that frames not only " *reveal an otherwise mute, independent reality but, in an important way . . .* (they) *help to define that reality'* (Tsoukas 1992:18). In other words it is possible to employ frames as models *for* the situation as well as models *of* the situation (see Geertz, 1988). In the first case we are speaking of frames helping to organise the perceived facts of a situation in the mind of the person concerned; in the second case the frame guides the very creation of the situation. In similar vein, Morgan speaks of using frames on the one hand to 'see' and 'read' situations with a fresh perspective, and, on the other hand to 'write' situations in new ways (Morgan 1993:9).

Interestingly, none of those who recognize that there are these two distinctive ways of using frames claim that it is not possible to operate concurrently in both modes, although they do acknowledge that there are tensions between the two. To the extent that

Maturana's own constructivist philosophy sits much more comfortably with the 'reality creating' perspective than it does with the 'reality revealing' perspective, the tensions are undoubtedly present here. Yet in daily life, conversations often oscillate between terms dictated by first one and then by the other party. And, as Morgan (1993:374) says, in reference to his reflective conversation method, *"sometimes the conversation takes place on the researcher's terms, and at others, on those of the phenomenon being investigated. . .* ". In response to those who might claim that, from a philosophical perspective, I have confused the reframing process, I submit that the same oscillating process can be detected through this work, and that the research has been one such reflective conversation.

### 12.2.2 The methodological approach

It is possible to foreshadow two possible criticisms of the research approach, if not of the research itself. The first is that the research: 'to examine *MS* using the lens of *AT*', has been framed too broadly. Such framing, critics might justifiably claim, provides too much freedom, that it charts a path for wanderers rather than one for those who have a particular destination firmly in sight. My response to such a charge is to plead guilty while claiming also that there are extenuating circumstances.

In the beginning, I was reluctant to chart a course having so little knowledge of where *AT* might take me. Having eventually acquired a rudimentary understanding of *AT* that pays due regard to the breadth as well as the depth of understanding that it affords, I was then faced with the disconcerting thought that although it was manifestly clear to me that there was an interesting journey to be had, charting an exact course over the incredibly diverse terrain of *MS* was going to be an impossible task. Heading NNE while passing through the 'Land of Viable Systems' would make a good deal of sense, but heading SSW would be better while traveling through the 'Kingdom of Checkland'.

In the end I did what *AT* leads me to think is often how decisions are made in the organisational and social world, which incidentally is not analogous to reading a map and carefully plotting the most prudent route to a pre-determined location. Instead, the outcome closely resembled the process of structural coupling and ontogenic drift that Maturana and Varela claim accounts for the specification of natural species, and which I have applied in explaining the evolutionary drift of the exemplary firms in the Competitive Capability Project (Chapter 7).

In a nutshell it was a case of beginning the journey and making decisions about which way to turn as and when I encountered various intersections. This is not to say that there was no general destination in mind because there was, rather that the complete route was not planned in advance. Consequently like both the evolution of species and the developmental trajectories of the exemplary firms, the point at which I finish the journey reflects the decisions made *en route*. I can only trust that I have not missed too many attractions on the way.

Relatedly, critics might claim that the research it is not adequately grounded in the sense that I have not formulated a precise research question and then, through empirical means, set out to investigate and/or 'test' it. To some extent this is a redundant question, since the main explicit focus of the work has been largely theoretical, i.e. it has concentrated on developing a frame that sheds new light on existing understandings or creates new ones. However, I acknowledge that critics might claim that having constructed the frame, in particular having outlined what an *AT*-inspired approach to *MS* might involve, the next logical step is to actually go out and do it. Instead I chose to re-examine already completed interventions in the light of a limited subset of the emergent concerns.

Admittedly there are some disadvantages of the approach that I have taken. The two interventions reported on here were framed in the light of very different concerns,

hence my records of the interventions are by no means complete. Then there are the inevitable memory lapses that occur over a period of five years or so. And I no longer have direct access to the organizations in which the interventions were conducted nor to the individuals involved, virtually all of whom have moved on in their careers. I cannot therefore go back and check on their understandings of what happened, nor can I now pursue a different empirical research agenda.

In my defence, the main strength and contribution of this work is unashamedly theoretical. As I said at the outset the aim has been to raise questions not necessarily settle them. What empirical content there is, exists primarily for the purpose of illustration; to bring theoretical points to life. I have not sought to 'test' the veracity or even the practicality of what I have been discussing. Besides, given the time frame that I am required to commit to the project, designing an empirical investigation of the 'testing' sort would have been beyond the bounds of what is practically feasible.

Moreover, in the majority of thesis projects, the key questions are formulated related early in the piece, at which point it is possible to design and conduct the empirical part of the investigation. In the case of this research, it is only now, having given the juxtaposition of *AT* with *MS* a decent airing, that I am in a position to contemplate what 'testing' – to the extent that such a term has any meaning in the context of Maturana's philosophy – might involve.

Admittedly then, the work is incomplete. The line underneath it has been drawn for the simple reason that completion demands it.

### 12.2.3 New or recycled knowledge?

Beyond possible philosophical and methodological concerns, another criticism might be that some of the conclusions are merely restating what many practitioners –

experienced ones in particular – already know, or that the work simply rehearses what other scholars have already written about.

The first observation has some validity. Indeed I suspect that some of the insights into the practice of *MS* that I have spoken about, are already known to many. No doubt there are many experienced practitioners who would claim that you do not need *AT* to remind people about how people's emotions intrude upon the process of doing management science and on successfully implementing projects; that you do not need *AT* to work out an epistemology for using the VSM; and that those who facilitate problem structuring workshops are perfectly capable – without having gone through the hassle of understanding *AT* - of appreciating and dealing with the various group and social dynamics that occur when using SSM.

The critical question however concerns the form in which these things might already be known. Sometimes things are only obvious after someone has pointed them out. Writing before Maturana reached the same conclusion, and before Varela et al. coined the term *'embodiment'* to capture the tacit knowledge that is embedded in our subconscious actions, Michael Polanyi (1967:4) was proclaiming that we human beings always *" . . . know much more than we can tell"* .

If, despite there being gaps in the literature and in their training, experienced practitioners do 'know' about these things in a tacit or embodied sense, and, through apprenticeship and experience, have learned how to achieve a modicum of success in their respective domains, surely it is still worthwhile bringing these things more into focus. Surely that is a worthwhile activity.

More importantly perhaps, there are neophyte *MS* scholars to think about. While highlighting gaps in the literature and theorizing about them cannot possibly replace the value of apprenticeship and training, it is nevertheless beneficial. It might, for example, reduce the amount of time spent learning about these things. It might provide the

conceptual wherewithal that allows new recruits to the profession to think in advance about how specific 'real world' situations might best be handled. *AT* may not provide ready made answers, but I submit that it does help inexperienced practitioners in asking relevant questions.

The second observation - that 'listening' to *AT* in the domain of *MS* is very similar to listening to other voices that have been raised in recent years - also has some validity. Arguably the most obvious case in point revolves around *AT*'s anti-objectivist epistemology. This issue has arisen in debates about the applicability of postmodernism to *MS* (see Midgley, 1994; Mingers, 1994; Taket and White, 1993; Taket, 1994; White and Taket, 1994, 1996) as it does in some variants of 'critical systems' (see Flood, 1990; Midgley, 1995). Some might even go so far as to suggest that, to all intents and purposes, there is little substantive difference between Checkland and Maturana on this point.

The same point, but in a different context, has been put to Maturana (see Simon 1985), and my response to such critics would mirror his. He acknowledges that there are clear intersections between his work and others, but rejects the suggestion that what he is saying arises from aggregating a collection of disparate ideas generated elsewhere. Rather, "*. . . it is something that arises as a whole which has certain elements which resemble certain other people's work because they handled similar questions*" (Maturana 1985:36).

Similarities aside, I submit that *AT* provides a coherent theoretical frame that integrates some of the areas touched on by others, and for that reason – if for no other – it is not 'old hat'. Neither, as Birch (1991) puts it, does it amount to '*reinventing the already punctured wheel*'. I contend that there is a breadth, a coherence and a theoretical consistency in Maturana's work that is not always present elsewhere.

Combining this with a very explicit foundational basis adds up to a distinctive and novel contribution. Whereas, for example, the anti-objectivist sentiments expressed by

other critically inclined scholars is at best philosophically grounded, the anti-objectivist epistemology of *AT* – indeed the whole *AT* edifice – is scientifically grounded in experimental findings from the biology of cognition. Since the overwhelming majority of those who practice *MS* have been trained and socialized in the norms and values of science, then this distinctive feature of *AT* ought to appeal to them.

### 12.2.4 Avenues of further research

Since a major aim of this exercise has been to raise questions that might otherwise not be raised, effectively the whole discussion constitutes an agenda for future research. Indeed many of these have been flagged as the discussion has unfolded. That being the case, I shall now focus attention on three avenues of research that I believe are particularly interesting and worth giving some priority to.

*From the theory of practice to actual practice*

By and large, this research has focused on what *MS* has to say about itself, as this is reflected through its own literature. I have tried to link this with practice to the extent that I can, but I have not looked directly at *MS* practice itself. Logically this is the next step. For example, it is one thing to say that the *MS* literature tends to downplay the role of the agent in *MS*, but what about management scientists themselves? To what extent do they acknowledge the personal role that they play in the formulation and resolution of problems? It is one thing to say that much of the *MS* literature implies that interventions occur in the name of objectivity and truth, but to what extent do management scientists believe that this is the case? If they do present their findings in this way, to what extent is this an emotional/political strategy on their part to nullify resistance and facilitate implementation? It is one thing to say that the VSM lacks a clear epistemology for use, but how does this impact on VSM practitioners. Do they have explicit epistemologies; if

so, what are they? It is one thing to say that SSM gives insufficient attention to process-related issues. But how do experienced SSM practitioners handle process? Potentially there are many such practice-related questions that one could formulate and investigate empirically. This is an important next step.

*Reporting on interventions*

In Chapter 5, I indicated how taking account of the epistemology of *AT* might not only be manifest through a distinctive way of carrying out *MS*, but also through a distinctive way of reporting on practice through the literature. Specifically I argued that since the MS process can be construed as a whole sequence of choices and decisions made by particular people, at a particular point in time and in a particular location, it would be helpful to know more about whether and how these facets of the study had a bearing on the outcome. Borrowing a phrase from Watson (1994), I intimated that the audiences of written accounts of *MS* practice might be interested in seeing the puppet's strings as they watch the show, and learning from this.

My position is that research that is designed to investigate these partly hidden, but also partly explicit aspects of MS practice, is timely. The need for such inquiry is implicit in later versions of SSM and to some extent is reflected in the cases that appear in Checkland and Scholes (1990). However, I have in mind a much more thorough and explicit investigation of this line of inquiry rather than something that is couched as a by-product of the intervention process per se.

*Power*

One of the distinctive contributions of this research is that it repositions *MS* from something that a management scientist 'does', to something that 'belongs' to the relational circumstances in which it takes place. This subtle, but very important

distinction, brings the matter of power relations in *MS* fully into the spotlight, as it is in both the Habermasian and Foucauldian *MS* traditions (see Brocklesby and Cummings, 1997). What contribution then, might *AT* make to this debate?

Maturana has chosen to not focus a great deal on power, and that which he has said has attracted a good deal of criticism (see Berman, 1989; Mingers, 1997b). However the concepts and language of *AT* do open up theoretical space for a distinctive explanation of the relationship between power, discourse and meaning which is a key consideration in the two major critical systems/*MS* traditions just mentioned. The distinctiveness of the *AT* approach hinges on the relationship between networks of structural coupling, conversations, explanations and action. Since, in the human context, it would be erroneous to presume that the structural coupling between and among human beings is an equal one, since conversations reflect the nature of the coupling, and since meaning derives from languaging, then logically crafting an *AT* link between power and meaning in the *MS* context, ought to be relatively straightforward. This could provide an interesting angle on what has already been said by those MS writers who align themselves to the critical systems and/or postmodern traditions. It is therefore, another interesting research possibility.

### 12.2.5 The problem of circularity

Finally, at the beginning of this work, I made the point that in *AT* the problem of circularity is never far away. This being the case, it seems appropriate to finish on the same point. In *'looking at Management Science through the lens of Autopoietic Theory'* (the title of the thesis) I have used my construction of the latter to reflect upon my construction of the former. And I have done so within a very specific set of relational circumstances. Given my own constitutive limitations as a biological entity structurally-coupled to a medium, and, as an observer, this thesis reflects the person and the process

of its construction. In all honesty therefore, I cannot claim to be telling the story 'as it is'. As Maturana puts it, *'everything said is said by an observer'*.

# REFERENCES


Ackoff, R. (1974). "The Social Responsibility of Operational Research." Operational Research Quarterly 25: 361-371.

Ackoff, R. (1979a). "The Future of Operational Research is Past." Journal of the Operational Research Society 30(2): 93-104.

Ackoff, R. (1979b). "Ressurrecting the Future of Operational Research." Journal of the Operational Research Society 30 (3): 189-194.

Allison, J., A. Charnes, W.Cooper, T.Sueyoshi (1994). How Do the Construction and/or Interpretation of Models Affect our Decisions? Ethics in Modeling. W. A. Wallace. Oxford, Elsevier Science Ltd.

Argyris, C. (1982). Reasoning, Learning, and Action. San Francisco, Jossey-Bass.

Ashby, W. R. (1952). Design for a Brain. London, Chapman and Hall.

Atkinson, C. (1989). "Ethics: A Lost Dimension in Soft Systems Methodology." Journal of Applied Systems Analysis 16(1): 43-53.

Barabba, V. (1994). The Role of Models in Managerial Decision Making - Never Say the Model Says. Ethics in Modeling. W. A. Wallace. Oxford, Elsevier Science Ltd.

Bardmann, T. M. (1996). "Social Work: 'Profession Without Qualities'. Attempt to Link Social Work and Cybernetics." Systems Research 13(3): 205-214.

Bartlett, C. A. and S. Ghoshal (1990). "Matrix Management: Not a Structure, a Frame of Mind." Harvard Business Review (July-August): 138-145.

Bawden, R. (1993). I as in Academy: Learning to be Systemic (The Von Bertalannfy Lecture). International Society for Systems Sciences, University of Western Sydney, Hawkesbury.

Bawden, R. (1995). Systemic Development: A Learning Approach to Change. University of Western Sydney, Hawkesbury.

Beer, S. (1959). Cybernetics and Management. Kent, English Universities Press.

Beer, S. (1966). Decision and Control. Chichester, John Wiley and Sons.

Beer, S. (1972). Brain of the Firm. London, Allen Lane.

Beer, S. (1979). Heart of Enterprise, Chichester. John Wiley and Sons.

Beer, S. (1980). Preface to: Autopoiesis: the Organization of the Living. Autopoiesis and Cognition - The Realisation of the Living. H. Maturana and F. Varela. Dordrecht, D. Riedel Publishing Company.

Beer, S. (1981 2nd edn.). Brain of the Firm, Chichester. John Wiley and Sons.

Beer, S. (1985). Diagnosing the System for Organization. John Wiley & Sons.

Bennet, J. C. and R. J. Olney (1986). "Executive Priorities for Effective Communication in an Information Age." Journal of Business Communication 23: 13-22.

Bennett, P. (1990). Mixing Methods: Combining Conflict Analysis, SODA, and Strategic Choice. Tackling Strategic Problems: The Role of Group Decision Support. C. Eden and J. Radford. London, Sage.

Bennett, P. and S. Cropper (1990). "Uncertainty and Conflict: Combining Conflict Analysis and Strategic Choice." Journal of Behavioural Decision Making 3: 29-45.

Berger, H., T. Caywood, J.Engel, J.Magee, H.Miser, R.Thrall (1971). "Guidelines for the Practice of OR." Operations Research 19: 1123-1148.

Bergquist, W. H. (1992). The Four Cultures of the Academy. San Francisco, Jossey-Bass.

Berman, M. (1989). "The Roots of Reality: Maturana and Varela's 'The Tree of Knowledge'." Journal of Humanistic Psychology (2): 277-284.

Bhaskar, R. (1989). Reclaiming Reality. London, Verso.

Bilson, A. (1997). "Guidelines for a Constructivist Approach: Steps Toward the Application of Ideas from Family Therapy for Use in Organisations." Systems Practice 10(2): 153-177.

Birch, J. (1991). "Re-inventing the Already Punctured Wheel: reflections on a Seminar with Humberto Maturana." Journal of Family Therapy 13: 349-373.

Bolman, L. G. and T. E. Deal (1991). Reframing Organisations - Artistry, Choice, and Leadership. San Francisco, Jossey-Bass.

Boothroyd, H. (1978). Articulate Intervention: the Interface of Science, Mathematics and Administration. London, Taylor and Francis.

Brocklesby, J. (1993). "Methodological Complementarism or Separate Paradigm Development - Examining the Options for Enhanced Operational Research." Australian Journal of Management 18(4): 133-158.

Brocklesby, J. (1994). "Let the Jury Decide: Assessing the Cultural Feasibility of Total Systems Intervention." Systems Practice 7(1): 75-86.

Brocklesby, J. (1995). "Using Soft Systems Methodology to Identify Competence Requirements in Human Resource Management." International Journal of Manpower 16(5/6): 70-84.

Brocklesby, J. (1997). Becoming Multimethodology Literate: An Assessment of the Cognitive Difficulties of Working Across Paradigms. Multimethodology: Tthe Theory and Practice of Combining Management Science Methodologies. J. Mingers and A. Gill. Chichester, John Wiley and Sons.

Brocklesby, J. (2001). The Evolution of Competitive Capability: a Biological Perspective. The Evolution of Competitive Capability in New Zealand Firms. C. Campbell-Hunt, J. Brocklesby, S. Chetty and L. Corbett. Auckland, Auckland University Press.

Brocklesby, J. and S. Cummings (1996). "Designing a Viable Organization Structure." International Journal of Strategic Management: Long Range Planning 29(1): 49-57.

Brown, C. (1992). Organization Studies and Scientific Authority. Rethinking Organization. M. Reed and M. Hughes. London, Sage.

Bryant, J. (1988). "Framework of Inquiry: OR Practice Across the Hard-Soft Divide." Journal of the Operational Research Society 39(5): 423-435.

Burgoyne, J. (1989). A Behavioural Science Perspective on Operation Research Practice. Operational Research and the Social Sciences. M. C. Jackson, S. Cropper and P. Keys. New York, Plenum Press.

Burrell, G. and G. Morgan (1979). Sociological Paradigms and Organisational Analysis, London, Heinemann.

Cameron, K. and D. Whetton (1983). "Perceptions of Organisational Effectiveness over Organisational Life Cycles." Administrative Science Quarterly 26(4): 525-545.

Capra, F. (1996). The Web of Life - A New Synthesis of Mind and Matter. London, Harper Collins.

Carrier, H. D. and W. A. Wallace (1994). An Epistemological View of Decision Aid Technology with Emphasis on Expert Systems. Ethics in Modeling. W. A. Wallace. Oxford, Elsevier Science Ltd.

Chapman, C. B. (1992). "My Two Cents on how OR should Develop." Journal of the Operational Research Society 43(7): 647 - 664.

Checkland, P. (1981). Systems Thinking, Systems Practice, John Wiley & Sons.

Checkland, P. (1989). Soft Systems Methodology. Rational Analysis for a Problematic World. J. Rosenhead, John Wiley & Sons: 30.

Checkland, P. (1992). "Systems and Scholarship: the Need to do Better." Journal of the Operational Research Society 43: 1023-1030.

Checkland, P. (1995). Systems Theory and Management Thinking. Critical Issues in Systems Theory and Practice. A. G. K.Ellis, B.Mears-Young and G.Ragsdell. New York, Plenum Press.

Checkland, P. and A. Casar (1986). "Vickers' Concept of an Appreciative System: A Systemic Account." Journal of Applied Systems Analysis 13: 3-13.

Checkland, P. and L. Davies (1986). "The Use of the Term Weltanschauung in Soft Systems Methodology." Journal of Applied Systems Analysis 13: 109-115.

Checkland, P. and J. Scholes (1990). Soft Systems Methodology in Action. Chichester, John Wiley & Sons.

Checkland, P. B. and M. G. Haynes (1994). "Varieties of Systems Thinking: the Case of Soft Systems Methodology." System Dynamics Review 10: 189-198.

Churchman, C. W. (1971). The Design of Inquiring Systems. New York, Basic Books.

Churchman, C. W. (1979). The Systems Approach and Its Enemies. New York, Basic Books.

Clemson, B. (1984). Cybernetics: a New Management Tool. Tunbridge Wells, Abacus Press.

Cummings, S. and J. Brocklesby (1993). "The Classical System - Organisational Insights into What Made Periclean Athens Great." Systems Practice 6(4): 335-358.

Daellenbach, H. (1994). Systems and Decision Making: A Management Science Approach. Chichester, John Wiley and Sons.

Daellenbach, H. (1995). Debate. Newsletter of the United Kingdom Operational Research Society.

Daellenbach, H. G. and E. G. Read (1998). "Success and Survival of OR Groups - Where to from Here?" Journal of the Operational Research Society 49(4): 430-433.

Dando, M. R. and P. G. Bennett (1981). "A Kuhnian crisis in Management Science?" Journal of the Operations Research Society 32(2): 91-104.

Davies, J. and V. Mabin (2001). "Knowledge management and the framing of information: a contribution to OR/MS practice and pegagogy." Journal of the Operational Research Society 52: forthcoming.

Davies, L. (1990). "The Seduction of Systems: A Response to R L Flood's Editorial, "From Positivism to Critique"." Systems Practice 3(4): 383-400.

Davies, L. (1992). "On the Reality of Systems." Systems Practice 5(6): 601-628.

Davies, L. and P. Ledington (1991). Information in Action - Soft Systems Methodology. Basingstoke, Macmillan Educational.

Dell, P. (1982). "Family Therapy and the Epistemology of Humberto Maturana." Family Therapy NetWorker (July-August): 26, 39-41.

Dell, P. (1985). "Understanding Bateson and Maturana: Toward a Biological Foundation for the Social Sciences." Journal of Marital and Family Therapy 11(1): 1-20.

Dreyfus, H. L. (1996). The Current Relevance of Merleau-Ponty's Phenomenology of Embodiment. Perspectives on Embodiment. H. Haber and G. Weiss. New York and London, Routledge.

Eden, C. (1989). Using Cognitive Mapping for Strategic Options Development and Analysis (SODA). Rational Analysis for a Problematic World. J. Rosenhead, John Wiley & Sons.

Eden, C. (1990). Mixing Methods: Introduction. Tackling Strategic Problems. C. Eden and T. Radford. London, Sage.

Eden, C. (1994). "Cognitive Mapping and Problem Structuring for System Dynamics Model Building." System Dynamics Review 10: 257-276.

Efran, J. and M. Lukens (1985). "The World According to Humberto Maturana." Networker 9(3): 23-28 and 72-75.

Efran, J., M.Lukens and R.Lukens, (1990). Language, Structure, and Change - Frameworks of Meaning in Psychotherapy. New York, W.W.Norton and Co. Inc.

Eisner, E. W. (1990). The Meaning of Alternative Paradigms for Practice. The Paradigm Dialog. E. G. Guba. Newbury Park, Sage.

Eppen, G. D., F. J. Gould, and J. Schmidt (1988). Introductory Management Science. Englewood Cliffs, Prentice-Hall Inc.

Espejo, R. (1987). From Machines to People and Organisations. New Directions in Management Science. M. Jackson and P. Keys. Aldershot, Gower.

Espejo, R. (1989a). A Cybernetic Method to Study Organizations. The VSM : Interpretations & Applications of S Beer's VSM. H. Espejo. England, John Wiley & Sons.

Espejo, R. (1989b). The VSM Revisited. The VSM : Interpretations & Applications of S Beer's VSM. R.Espejo and R.Harnden. England, John Wiley & Sons.

Espejo, R. (1993). The Viplan Method: A Method to Study the Structure of an Organisation. University of Aston, Birmingham, Syncho Ltd.

Espejo, R., W. Schuhmann, M. Schwaninger, U.Bilello (1996). Organisational Transformation and Learning: A Cybernetic Approach to Management. Chichester, John Wiley and Sons.

Espejo, R. and R. Harnden (1989a). The VSM : Interpretations & Applications of S Beer's VSM, Chichester, John Wiley & Sons.

Espejo, R. and R. Harnden (1989b). The VSM: An On-Going Conversation. The VSM : Interpretations & Applications of S Beer's VSM, R. Espejo and R. Harnden, Chichester, John Wiley & Sons.

Fairtlough, G. (1982). "A Note on the Use of the Term 'Weltanschauung' (W) in Checkland's "Systems Thinking, Systems Practice"." Journal of Applied Systems Analysis 9: 131-132.

Fell, L. and D. Russell, Eds. (1994). Seized by Agreement, Swamped by Understanding. Sydney, Hawkesbury Printing.

Feyerabend, P. (1987). Farewell to Reason. London, Verso.

Feyerabend, P. (1993). Against Method. London, Verso.

Fiedler, K. (1982). "Causal Schemata: Review and Criticism of Research on a Popular Construct." Journal of Personality and Social Psychology 42: 1001-1013.

Fildes, R. and J. C. Ranyard (1998). "The Foundation, Development and Current Practice of OR: an Editorial Introduction and Overview." Journal of the Operational Research Society 49(4): 304-306.

Fiske, S. T. and L. M. Dyer (1985). "Structure and Development of Social Schemata: Evidence from Positive and Negative Transfer Effects." Journal of Personality and Social Psychology 48: 839-852.

Flood, R. (1990). "Liberating Systems Theory: Towards Critical Systems Thinking." Human Relations 43(1): 49-75.

Flood, R. L. and M. C. Jackson (1991). Creative Problem Solving. Chichester, John Wiley & Sons.

Flood, R. L. and S. A. Zambuni (1990). "Viable Systems Diagnosis: Application with a Major Tourism Services Group." Systems Practice 3(3): 225-248.

Foss, R. A. (1989). The Organization of a Fortress Factory. The VSM : Interpretations & Applications of Stafford Beer's VSM. R. Espejo and R. Harnden. Chichester, John Wiley & Sons.

Fox, S. (1997). "Situated Learning Theory Versus Traditional Cognitive Learning: Why Management Education Should Not Ignore Management Learning." Systems Practice 10(6): 727-748.

Fricke, M. (1997). In What Sense do Systems Exist? Systems for Sustainability: People, Organisations, and Environments. F. Stowell, R. Ison and R. Armeson. New York, Plenum.

Gadamer, H. (1975). Truth and Method. New York, Seabury Press.

Galliers, R., J. Mingers, M. Jackson (1997). "Organization Theory and Systems Thinking: The Benefits of Partnership." Organization 4(2): 269-278.

Gass, S. I. (1994). Ethical Concerns and Ethical Answers. Ethics in Modeling. W. A. Wallace. Oxford, Elsevier Science Ltd.

Geertz, C. (1988). Works and Lives: the Anthropologist as Author. Cambridge, Polity Press.

Gibb-Dyer, W. (1985). The Cycle of Cultural Evolution in Organisations. Gaining Control of the Corporate Culture. R. Kilmann, M. Saxton and R. Serpa. San Fransisco, Jossey-Bass.

Gioia, D. A. and E. Pitre (1990). "Multiparadigm Perspectives on Theory Building." Academy of Management Review 15(4): 584-602.

Gioia, D. A. and K. Chittipeddi (1991). "Sensemaking and Sensegiving in Strategic Change Initiation." Strategic Management Journal 12: 433-448.

Glaserfeld, E. v. (1996). "Farewell to Objectivity." Systems Research 13(3): 279-287.

Goolishian, H. A. and L. Winderman (1988). "Constructivism, Autopoiesis and Problem Determined Systems." Irish Journal of Psychology 9(1): 130-143.

Gregory, R. L. (1989). Touching Truth. Dismantling Truth: Reality in the Post-Modern World. H. Lawson and L. Appignanesi. London, Weidenfeld and Nicolson.

Habermas, J. (1984). The Theory of Communicative Action Vols. I and II. Oxford, Polity Press.

Habermas, J. (1993). Justification and Application. Cambridge, Polity Press.

Hames, R. (1996). Burying the Twentieth Century - Laying Down a Path of Whole System Change. Australian Systems Conference, Melbourne.

Heidegger, M. (1927). Being and Time. London, SCM.

Hejl, P. (1984). Towards a Theory of Social Systems: Self Organisation and Self-Maintenance, Self-Reference and Syn-Reference. Self-Organisation and Management of Social Systems: Promises Doubts and Questions. H. Ulrich and B. Probst. Berlin, Springer-Verlag.

Hesse, R. and G. Woolsey (1980). Applied Management Science: A Quick and Dirty Approach. New York, Science Research Associates.

Hoffman, L. (1988). "A Constructivist Position for Family Therapy." Irish Journal of Psychology 9(1): 110-129.

Husserl, E. (1900). Logical Investigations Vol. 1. London, Routledge and Kegan Paul.

Husserl, E. (1901). Logical Investigations Vol. 2. London, Routledge and Kegan Paul.

Introna, L. D. (1997). Management, Information and Power. Basingstoke, Macmillan Press Ltd.

Ison, R. and D. Russell, Eds. (2000). Agricultural Extension and Rural Development : Breaking out of Traditions. Cambridge, Cambridge University Press.

Jackson, B. and Parry, K. (2001) The Hero Manager: Learning from New Zealand's Top Chief Executives. Auckland, Penguin.

Jackson, M. (1982). "The Nature of Soft Systems Thinking, the Work of Churchman, Ackoff, and Checkland." Journal of Applied Systems Analysis 9(1): 17-28.

Jackson, M. (1985). "Social Systems Theory and Practice: the Need for a Critical Approach." International Journal of General Systems 10: 135.

Jackson, M. (1987). "Present Positions and Future Proposals in Management Science." Omega 15: 455-466.

Jackson, M. (1989). Evaluating the Managerial Significance of the VSM. The VSM : Interpretations & Applications of Stafford Beer's Viable Systems Model. R. Espejo and R.Harnden, Chichester, John Wiley & Sons.

Jackson, M. (1991). Systems Methodology for the Management Sciences. New York and London, Plenum Press.

Jackson, M. (1993). "Social Theory and Operational Research Practice." Journal of the Operational Research Society 44 (3): 563-578.

Jackson, M. (1994). "Critical Systems Thinking: Beyond the Fragments." System Dynamics Review 10: 213-230.

Jackson, M. (1997). Pluralism in Systems Thinking and Practice. Multimethodology: the Theory and Practice of Combining Management Science Methodologies. J. Mingers and A. Gill. Winchester, John Wiley and Sons.

Jackson, M. and P. Keys (1984). "Towards a System of Systems Methodologies." Journal of the Operational Research Society 35(6): 473-486.

Jackson, M. and P. Keys, Eds. (1987). New Directions in Management Science. Aldershot, Gower.

Jackson, M., S. Cropper, P. Keys, Eds. (1989). Operational Research and the Social Sciences. New York, Plenum.

Jacques, R. (1989). Post-Industrialism, Postmodernity and O.R. : Towards a 'Custom and Practice' of Responsibility and Possibility. Operational Research and the Social Sciences. M. C. Jackson, S. Cropper and P. Keys. New York, Plenum Press.

Kay, R. (1997). Applying Autopoiesis to the Facilitation of Organisational Change. Australian and New Zealand Systems Conference, Brisbane.

Kelly, G. (1955). The Psychology of Personal Constructs: A Theory of Personality. New York, Norton.

Kenny, V. and G. Gardner (1988). "Constructions of Self-Organising Systems." Irish Journal of Psychology 9(1): 1-24.

Keys, P. (1989). "OR as Technology: Some Issues and Implications." Journal of the Operational Research Society 40: 753-759.

Keys, P. (1991). Operational Research and Systems. New York, Plenum.

Keys, P. (1998). "OR as Technology Revisited." Journal of the Operational Research Society 49: 99-108.

King, J. B. (1994). "Tools-R-Us." Journal of Business Ethics 13:4.

Kirby, M. W. and R. Capey (1998). "The Origins and Diffusion of Operational Research in the UK." Journal of the Operational Research Society 49(4): 307-326.

Krogh, G. v. and J. Roos (1995). Organizational Epistemology. Basingstoke, Macmillan Press.

Krogh, G. v. and J. Roos (1996). Managing Knowledge: Perspectives on Cooperation and Competition. London, Sage.

Krogh, G. v. and J. Roos (1998). Knowing in Firms: Understanding, Managing and Measuring Knowledge. London, Sage.

Kuhn, T. S. (1970). The Structure of Scientific Revolutions. Chicago, University of Chicago Press.

Lakoff, G. and M. Johnson (1980). Metaphors We Live By. Chicago, Chicago University Press.

Lane, D. and R. Oliva (1994). The Greater Whole: Towards a Synthesis of SD and SSM. International Systems Dynamics Conference, Stirling.

Lawrence, J. and B. Pasternack (1998). Applied Management Science. New York, John Wiley and Sons.

Lawson, H. (1989). Introduction. Dismantling Truth: Reality in the Post-Modern World. H. Lawson and L. Appignanesi. London, Weidenfeld and Nicolson.

Lawson, H. and L. Appignanesi, Eds. (1989). Dismantling Truth: Reality in the Post-Modern World. London, Weidenfeld and Nicolson.

Leavitt, H. J. (1978). Managerial Psychology. Chicago, University of Chicago Press.

Little, J. D. C. (1994). On Model Building. Ethics in Modeling. W. A. Wallace. Oxford, Elsevier Science Ltd.

Luhmann, N. (1986). The Autopoiesis of Social Systems. Sociocybernetic Paradoxes. F. Geyer and J. v. d. Zoewen. London, Sage.

Maani, K. and R. Cavana (2000). Systems Thinking and Modelling. Auckland, Pearson Education.

Magee, B. (1987). The Great Philosophers - An Introduction to Western Philosophy. London, BBC Books.

Margolis, H. (1993). Paradigms and Barriers - How Habits of Mind Govern Scientific Beliefs. Chicago, University of Chicago Press.

Mason, R. O. (1994). Morality and Models. Ethics in Modeling. W. A. Wallace. Oxford, Elsevier Science Ltd.

Maturana, H. (1970a). Biology of Cognition. Autopoiesis and Cognition. H. Maturana and F. Varela, Reidel.

Maturana, H. (1970b). The Neurophysiology of Cognition. Cognition: A Multiple View. P. Garvin. New York, Spartan Books.

Maturana, H. (1980). Man and Society. Autopoiesis, Communication, and Society: The Theory of Autopoietic Systems in the Social Sciences. F. Benseler, P. M. Hejl and W. K. Kock. Frankfurt, Campus Verlag.

Maturana, H. (1981). Autopoiesis. Autopoiesis: a Theory of Living Organization. M. Zeleny. Elsevier-North Holland, New York.

Maturana, H. (1983). "What is it to See?" Arch. Biol. Med. Exp. **16**: 255-269.

Maturana, H. (1988a). "Reality: The Search for Objectivity or the Quest for a Compelling Argument." Irish Journal of Psychology **9**: 25-82.

Maturana, H. (1988b). Ontology of Observing: The Biological Foundations of Self Consciousness and the Physical Domain of Existence, http://www.inteco.cl/biology/ontology/index.htm.

Maturana, H. (1990). Science and Daily Life: The Ontology of Scientific Explanations. Selforganization: Portrait of a Scientific Revolution. W. Krohn, G. Kuppers and H. Nowotny. Dordrecht, Kluwer Group.

Maturana, H. (1991). "Response to Jim Birch." Journal of Family Therapy **13**: 375-393.

Maturana, H. (1992). Cognition and Autopoiesis: A Brief Reflection on the Consequences of their Understanding. State, Law, and Economy as Autopoietic Systems: Regulation and Autonomy in a New Perspective. G. Teubner and A. Febbrajo. Milan, Giuffre.

Maturana, H. (1993a). Biology of the Aesthetic Experience. Zeichen(theorie) und Praxis, Passau, Wissenschaftsverslag Rothe.

Maturana, H. (1993b). Videotaped Seminar - Language and Cognition. St. Kilda, Melbourne.

Maturana, H. (1995). The Nature of Time, http://www.inteco.cl/biology/nature.htm.

Maturana, H. (1997). Metadesign, http://www.inteco.cl/articulos/metadesign.htm.

Maturana, H. and P. Bunnell (1998). Biosphere, Homosphere, and Robosphere: What has that to do with Business? Society for Organizational Learning. Boston.

Maturana, H. R. and G. D. Guiloff (1980). "The Quest for the Intelligence of Intelligence." Journal Social Biol. Struct. **3**: 135-148.

Maturana, H., J. Lettvin, S.McCulloch, W.Pitts, (1960). "Anatomy and Physiology of Vision in the Frog." J. Gen. Physiol. **43**: 129-175.

Maturana, H., G. Uribe, S.Frenk, (1968). "A Biological Theory of Relativistic Color Coding in the Primate Retina." Arch. Biol. Med. Exp. **1**: 1-30.

Maturana, H. and F. Varela (1980). Autopoiesis and Cognition: The Realisation of the Living. Dordrecht, Reidel.

Maturana, H. and F. Varela (1987). The Tree of Knowledge - The Biological Roots of Human Understanding. Boston, Shambhala.

Maturana, H., J. Mpodozis, J.C.Letelier, (1995). "Brain, Language and the Origin of Human Mental Functions." Biol. Res. **28**: 15-26.

McLachlan, R. (1995). An Exploration of the Participative Use of Soft Systems Methodology. Faculty of Arts. Wellington, Victoria University.

Mendez, C., F. Coddou, H.Maturana, (1988). "The Bringing Forth of Pathology." Irish Journal of Psychology 9(1): 144-172.

Merleau-Ponty, M. (1961). Phenomenology of Perception. London, Routledge & Kegan Paul.

Midgley, G. (1989). "Critical Systems and the Problem of Pluralism." Cybernetic Systems 20: 219-231.

Midgley, G. (1990). Critical Systems Thinking and Methodological Pluralism. 34th Annual Meeting of the International Society for the Systems Sciences, Chicago.

Midgley, G. (1992). "The Sacred and Profane in Critical Systems Thinking." Systems Practice 5(1): 5-17.

Midgley, G. (1994). "Five Sketches of Postmodernism: Implications for Systems Thinking and Operational Research." Systemist 16(3): 187-202.

Midgley, G., and W. Gregory, (1992). "Guest Editorial - Developments in Critical Systems Thinking." Systems Practice 5(2): 125-128.

Mingers, J. (1980). "Towards an Appropriate Social Theory for Applied Systems Thinking: Critical Theory and Soft Systems Methodology." Journal of Applied Systems Analysis 7: 41-49.

Mingers, J. (1984). "Subjectivism and Soft Systems Methodology - A Critique." Journal of Applied Systems Analysis 11: 85-103.

Mingers, J. (1990). "The Philosophical Implications of Maturana's Cognitive Theories." Systems Practice 3(6): 569-584.

Mingers, J. (1992a). "The Problems of Social Autopoiesis." International Journal of General Systems 21(2): 229-236.

Mingers, J. (1992b). "Recent Developments in Critical Management Science." Journal of the Operational Research Society. 43(1): 1-10.

Mingers, J. (1994). "Separating the Wheat From the Chaff: A Modernist Appropriation of the Postmodern." Systemist 16(4): 255-260.

Mingers, J. (1995). Self-Producing Systems - Implications and Applications of Autopoiesis. New York and London, Plenum Press.

Mingers, J. (1996). Embodying Information Systems. Information Technology and Changes in Organizational Work. M. Jones, W. Orlikowski, G. Walsham and J. D. Gross. London, Chapman Hall.

Mingers, J. (1997a). Towards Critical Pluralism. Multimethodology: Towards the Theory and Practice of Combining Management Science Methodologies. J. Mingers and A. Gill. Winchester, Wiley.

Mingers, J. (1997b). "A Critical Evaluation of Maturana's Constructivist Family Therapy." Systems Practice 10(2): 137-151.

Mingers, J. (2001). "The Contribution of Critical Realism as an Underpinning Philosophy for OR/MS and Systems." Journal of the Operational Research Society 52 forthcoming.

Mingers, J. and J. Brocklesby (1997). "Multimethodology: Towards A Framework For Mixing Methodologies." Omega - International Journal of Management Science 25(5): 489-509.

Mingers, J. and S. Taylor (1992). "The Use of Soft Systems Methodology in Practice." Journal of the Operational Research Society 43(4): 321-332.

Miser, H. J. (1998). "What We Learned Early in the US Airforce about Establishing and Maintaining OR Groups." Journal of the Operational Research Society 49(4): 336-346.

Morgan, G. (1980). "Paradigms, Metaphors, and Puzzle Solving in Organization Theory." Administrative Science Quarterly 25: 605-622.

Morgan, G., Ed. (1983). Beyond Method - Strategies for Social Research. Beverly Hills, Sage Publications.

Morgan, G. (1986). Images of Organisation. Newbury Park, Sage.

Morgan, G. (1989). Creative Organization Theory. New York, Sage.

Morgan, G. (1989). Teaching MBA's Transformational Thinking. Paradox and Transformation : Toward a Theory of Change in Organisation and Management. R. Quinn and K. S. Cameron. Cambridge, Ballinger Pub. Co.

Morgan, G. (1993). Imaginization. New York, Sage.

Mulvey, J. M. (1994). Models in the Public Sector: Success, Failure and Ethical Behavior. Ethics in Modeling. W. A. Wallace. Oxford, Elsevier Science Ltd.

Munro, I. and J. Mingers (2000). The Use of Multimethodology in Practice - Results of a Survey of Practitioners. Warwick Business School.

Nicholls, M. P. (1987). The Self in the System: Expanding the Limits of Family Therapy. New York, Brunner/Mazel.

O'Hair, D., G. Friedrich, L.Shaver, (1998). Strategic Communication in Business and the Professions. Boston, Houghton Mifflin Company.

Ormerod, R. (1996a). "On the Nature of OR - Entering the Fray." Journal of the Operational Research Society 47(1): 1-17.

Ormerod, R. (1996b). "Viewpoint: New Methods for Old." Journal of the Operational Research Society 47(10): 1317-1326.

Ormerod, R. (1998). "Beyond Internal OR Groups." Journal of the Operational Research Society 49(4): 420-429.

Pidd, M. (1988). "From Problem-Structuring to Implementation." Journal of the Operational Research Society 39(2): 115-121.

Pidd, M. (1996). Tools for Thinking: Modelling in Management Science. Chichester, John Wiley and Sons.

Polanyi, M. (1967). The Tacit Dimension. New York, Routledge and Kegan Paul.

Pondy, L. and D. M. Boje (1981). Bringing Mind Back In. Frontiers in Organization and Management. W. M. Evan. New York, Praeger.

Quinn, R. E. and K. S. Cameron, Eds. (1988). Paradox and Transformation : Toward a Theory of Change in Organisation and Management. Cambridge, Ballinger.

Rennie, H. (1989). North Labrador and the Torngat Co-op: An Exploration of Checkland's Soft Systems Methodology Through its Application to Fisheries Development, Memorial University of Newfoundland.

Rosenhead, J. (1987). From Management Science to Workers' Science. New Directions in Management Science. M.Jackson and P. Keys. Aldershot, Gower: 109-129.

Rosenhead, J. (1989). Introduction: Old and New Paradigms of Analysis. Rational Analysis for a Problematic World. J. Rosenhead. Chichester, John Wiley & Sons.

Rosenhead, J., Ed. (1989). Rational Analysis for a Problematic World. Chichester, John Wiley & Sons.

Rosenhead, J. and C. Thunhurst (1982). "A Materialist Analysis of Operational Research." Journal of the Operational Research Society 33(2): 111-122.

Schein, E. (2000). "The Next Frontier: Edgar Schein on Organisational Therapy." Academy of Management Executive 14(1): 31-51.

Schon, D. (1982). Organisational Learning - a Research Perspective. Massachusetts Institute of Technology.

Schon, D. (1983). The Reflective Practitioner: How Professionals Think in Action. New York, Basic Books.

Schutz, A. (1964). Collected Papers 2: Studies in Social Theory. The Hague, Martinus Nijhoff.

Searle, J. (1983). Intentionality: An Essay on the Philosophy of Mind. Cambridge, Cambridge University Press.

Senge, P. M. (1990). The Fifth Discipline. New York, Doubleday/Currency.

Simon, R. (1985). "An Interview with Humberto Maturana." Family Therapy Networker 9(3): 36-43.

Smaling, A. (1994). "The Pragmatic Dimension - Paradigmatic and Pragmatic Aspects of Choosing a Qualitative or Quantitative Method." Quality and Quantity 28: 233-249.

Sparrow, J. (1998). Knowledge in Organizations. London, Sage.

Staw, B. M., Ed. (1991). Psychological Dimensions of Organizational Behaviour. New York, Macmillan.

Stubbart, C. I. and R. D. Smalley (1999). "The Deceptive Allure of Stage Models of Strategic Processes." Journal of Management Inquiry 8(3): 273-285.

Taket, A. (1994). "Undercover Agency? - Ethics, Responsibility and the Practice of OR." Journal of the Operational Research Society 45(2): 123-132.

Taket, A. and L. White (1993). "After OR: An Agenda for Postmodernism and Poststructuralism in OR." Journal of the Operational Research Society 44(9): 867-881.

Taket, A. and L. White (1994). "Postmodernism - Why Bother?" Systemist 16(3): 175-187.

Tomlinson, H. (1989). After Truth: Post-Modernism and the Rhetoric of Science. Dismantling Truth: Reality in the Post-Modern World. H. Lawson and L. Appignanesi. London, Weidenfeld and Nicolson.

Tomlinson, R. and I. Kiss (1984). Rethinking the Process of Operational Research and Systems Analysis. Oxford, Pergamon Press.

Tomlinson, R. C. (1998). "The Six Principles for Effective OR - Their Relevance in the 90's." Journal of the Operational Research Society 49(4): 403-407.

Torbert, W. R. (1989). Transformational Thinking Seen From a Developmental Perspective. Paradox and Transformation: Toward a Theory of Change In Organisations and Management. R. Quinn and K. Cameron. Cambridge, Ballinger Pub. Co.

Tsoukas, H. (1991). "The Missing Link: a Transformational View of Metaphors in Organisational Science." Academy of Management Review 16(3): 566-585.

Tsoukas, H. (1996). "The Word and the World: A Critique of Representationalism in Management Research." Warwick Business School.

Tsouvalis, C. and P. Checkland (1996). Reflecting on SSM: The Dividing Line Between 'Real World' and 'Systems Thinking World'. University of Humberside.

Tunstall, W. B. (1985). Breakup of the Bell System: A Case Study in Cultural Transformation. Gaining Control of the Corporate Culture. R. Kilmann, M. J. Saxton and R. Sherpa. San Francisco, Jossey-Bass.

Ulrich, W. (1982). Critical Heuristics of Social Planning: A New Approach to Practical Philosophy. Berne, Haupt.

Umpleby, S. A. (1994). The Cybernetics of Conceptual Systems. Institute for Advanced Studies, University of Vienna: www.gwu.edu/~umpleby/.

Varela, F. (1981). Describing the Logic of the Living: The Adequacy and Limitations of the Idea Autopoiesis. Autopoiesis: A Theory of the Living Organization. M. Zeleny. New York, Elsevier-North Holland.

Varela, F. (1984). The Creative Circle: Sketches on the Natural History of Circularity. The Invented Reality. P. Watzlawick. New York, W. W. Norton.

Varela, F., E. Thompson, E. Rosch (1991). The Embodied Mind - Cognitive Science and Human Experience. Cambridge, Mass., MIT Press.

Vickers, G. (1965). The Art of Judgement. London, Chapman and Hall.

Vickers, G. (1967). Towards a Sociology of Management. London, Chapman and Hall.

Vickers, G. (1970). Value Systems and Social Process. Harmondsworth, Penguin Books.

Wagner, H. M. (1972). "Commentary on ORSA Guidelines." Management Science 18(10): B609-B613.

Walker, J. (1990). "Diagnosis and Implementation: How a Large Cooperative Employed a Series of Proposals for Restructuring Based upon the VSM." Systems Practice 3(5): 441-453.

Walker, W. E. (1994). Responsible Policy Modeling. Ethics in Modeling. W. A. Wallace. Oxford, Elsevier Science Ltd.

Wallace, W. A. (1994). Introduction. Ethics in Modeling. W. A. Wallace. Oxford, Elsevier Science Ltd.

Watson, T. J. (1994). "Managing, Crafting and Researching:Words, Skill and Imagination in Shaping Management Research." British Journal of Management 5(June): 77-87.

Whitaker, R. (1996). The Observer Web: The Internet Nexus for Autopoiesis and Enaction, http://www.informatik.umu.se/~rwhit/AT.html.

White, L. and A. Taket (1994). "The Death of the Expert." Journal of the Operational Research Society 45(7): 733-748.

White, L. and A. Taket (1996). "The End of Theory." Omega, International Journal of Management Science 24(1): 47-56.

White, L. and A. Taket (1997). Critiquing Multimethodology as Metamethodology: Working Towards Critical Pluralism. Multimethodology: Towards the Theory and Practice of Combining Management Science Methodologies. J. Mingers and A. Gill. Winchester, Wiley.

Wilby, J. (1996). TSI as Critique: the Critical Review Mode. Critical Systems Thinking: Current Research and Practice. R. L. Flood and N. R. A. Romm. New York, Plenum Press.

Wilkins, A. L. and K. J. Patterson (1985). You Can't Get There From Here: What Will Make Culture-Change Projects Fail. Gaining Control of the Corporate Culture. R. Kilmann, M. J. Saxton and R. Sherpa. San Fransisco, Jossey-Bass.

Willmott, H. (1993a). "Breaking the Paradigm Mentality." Organization Studies 14(5): 681-719.

Willmott, H. (1993b). "Paradigm Gridlock: A Reply." Organizational Studies 14(5): 727-730.

Winograd, T. and F. Flores (1987). Understanding Computers and Cognition. Massachusetts, Addison-Wesley.

Wittgenstein, L. (1953). Philosophical Investigations. Oxford, Blackwell.

Woolgar, S. (1989). The Ideology of Representation and Role of the Agent. Dismantling Truth: Reality in the Post-Modern World. H. Lawson and L. Appignanesi. London, Weidenfeld and Nicolson.

Zeleny, M. and C. Hufford (1992). "The Application of Autopoiesis in Systems Analysis: Are Social Systems also Autopoietic Systems?" International Journal of General Systems 21(2): 145-160.

Zimmermann, H. J. (1998). "Some Observations on Practising Successful Operational Research." Journal of the Operational Research Society 49(4): 413-419.