THEORETICAL FOUNDATIONS OF OPERATIONAL RESEARCH

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S U M M A R Y

The conclusions of both Parts One and Two complement and reinforce each other. After outlining the ideals of OR, I set out in Part One to find and scrutinize the philosophical foundations upon which some leading operations researchers have claimed that these ideals could be implemented. In chapters 2, 3, and 4 I argue that adopting (respectively) the positivist, conventionalist and/or idealist philosophies as the theoretical foundations upon which to build an adequate theory of inquiry for the purposes of OR would force it to abandon its ideals. These philosophies are interpreted as attempts on the part of academic operational researchers to stave-off the open-ended ambiguity and anarchy of inquiry which an unqualified interpretation of OR's ideals could engender. These attempts to give substance to the ideals of OR all exert a strong bias against raising questions about the nature of the subject-matter with which OR deals, and it is largely on these grounds that they are rejected in chapter 5 because of the implications which this has for the ideals of OR. One conclusion of Part One is that OR needs protection from such philosophies, and that a realist-type alternative at least provides this. I conclude by raising the doubt whether philosophy can provide much more to OR. The other major conclusion is that OR needs to understand its subject-matter before it can reasonably hope to implement its ideals.

Given the general bias which we find in Part One against seriously considering the subject-matter of OR, we enter Part Two with some trepidation. Notwithstanding the philosophical bias against it, it is clear that OR must have a conception of the nature of its subject-matter. However, OR's ideals can just as easily be lost by inadequate attention to this task. In Part Two the biases discovered in Part One come home to roost. The first attempt to provide the ideals of OR with a substance on the basis of which its ideals can be implemented in an objective way turns out to be just that, i.e., metaphysical 'substance' in the guise of a theory of management. We see in chapter 6 that to the extent to which this theory moves beyond merely asserting that management would 'take care' of OR's need for an objective basis, it presupposes a social theory which would show how social systems by their nature (if properly constructed) embody this objectivity. This move is foreshadowed in chapter 3 where we see Kuhn (who is taken as an exemplar of conventionalist philosophy) finally resorting to this device to prop up his conventionalism against the growing weight of subjectivity under which it threatened to sag into the jaws of positivism. The social theory on which such claims rest is given detailed consideration in chapter 7.

In chapter 7 I give serious consideration to the possibility that OR's social theory, if it has one at all, will be developed in reaction to what it sees as the "problem of order", because this problem can be seen as but another way of stating its ideals in a specifically social way. Stating OR's ideals in this way orients them directly to at least one aspect of the question of the nature of OR's subject-matter. We see that by employing Durkheim's account of and solution to the social problem of order as a basis for comparison with OR (first as a homomorphism and later as an isomorphism) that we are able to gain quite a firm grip on OR's social theory (and, hence, its grasp of its subject-matter). We see that this theory, although providing a justification for OR's theory of management (especially in its modern form), it is itself inadequate. The basis of the inadequacy, most fundamentally, is that the theory in question presupposes the very thing that should be in question, namely, the nature of the social collective. I conclude with a specific illustration of the impact of this theory on the ideal of OR by analysing the inadequate treatment of power and conflict which it allows.

INTRODUCTION

I take it as an axiom throughout this thesis that the general ideal which operational research (hereafter referred to as OR) (1) should pursue is the implementation of reason in human affairs. In chapter one this ideal will be shown to underlie various writings on the basic nature of OR. This general ideal will appear there in the form of a set of more specific ideals which I take to be OR's distinctive interpretation of it. The task of OR when it is defined in this way is to find ways of implementing these derivative ideals.

Defining OR in this way is, ,of course, not at all typical. The literature on the techniques of OR is enormous in comparison with the literature on how it might go about implementing its broader aims (2). This fact might be taken as an indication that the aspect of OR on which I wish to concentrate is as yet too immature to warrant sustained analysis. This discouraging conclusion cannot, however, be drawn from small numbers alone. Those involved in attempting to state the basic nature of OR have by their considerable efforts constituted a formidable literature, and others are begining to follow their lead. Also, for the purposes of this thesis, whatever limitations might be imposed by the small numbers of writers involved is more than compensated for by the quality and influence of their work.

The writers who will receive most attention in this thesis are Ackoff, Churchman, Beer, and some members of the Institute for Operational Research (I.O.R.). The problem posed by their work is whether their various writings enable us to say with any confidence that OR can make

(1) Throughout I refer to the subject of interest in this thesis as OR. Others may wish to attempt to demarcate OR from Management Science or Systems Science, etc. These distinctions are not relevant for this work.
(2) The small numbers involved means that my repeated reference to OR in the abstract is to be taken as an idealistion which need not necessarily refer to an explicitly expressed majority opinion.

progress towards fulfilling its ideals. The conclusion reached at several points will be that as it is set at present its ideals are unattainable. This is so because the theoretical tools which they employ in the attempt to implement OR's ideals in fact prevent their attainment.

The major task of this thesis is the discovery and critical assessment of these theoretical tools. This thesis is persistently critical. The hope is that by such criticism OR may develop through the pushing back of theoretical barriers rather than through the filling out of inappropriately fixed boundaries. The need for criticism and discussion is crucial at these early stages of OR's development. The function of the criticism engaged in here is to help OR change course from the direction in which it is now heading. At times the criticism may appear savage, but no greater compliment could be paid to those whom I criticize than to take their work as deadly serious and worthy of sustained analysis.

The theoretical foundations which I shall discuss are those which are commensurate with OR's general ideal. In a broad sense the general ideal has two parts to it. One part relates to the role of reason. The major source which has been drawn upon to understand this has been philosophy. Part One of this thesis, therefore, looks at the theoretical foundations of OR which stem from its associations with certain philosophical traditions. I interpret the concern shown (sometimes implicitly) with philosophy as an attempt to understand the role of reason by the development of a theory of inquiry for OR. The other part of the general ideal relates to human affairs. The major source which has been drawn upon by OR to understand human affairs is social theory. Part Two of this thesis, therefore, looks at the theoretical foundations of OR which stem from its associations with certain traditions within the field of social theory. I interpret the concern shown (again, sometimes implicitly) with social theory as an attempt by OR to understand the object (or subject-matter (1)) of inquiry. In both parts I shall show that the traditions with which OR has become most closely associated exert biases which push it in unfruitful directions. It is lack of both space and time which prevents me from doing more than outlining the kinds of theoretical foundations which would bias OR in more fruitful directions. This is the major task for the future which emerges from this work. I shall not discuss those foundations--particularly mathematics and statistics--which do not seem directly to promote or to hinder the attainment of the general ideal. Subsequent developments may eventually show more clearly than it is currently understood how such foundations have an equivalently direct impact as the foundations which are of concern here.

Interest in the philosophical and social foundations of OR is a way of discussing the relationship between OR and 'science'. Science can be treated as an idealisation of objective inquiry or it can be treated as a current body (or bodies) of knowledge and problems. In most definitions and discussions of the basic nature of OR the first treatment dominates. In Part One it will be shown that the correct way for OR to relate to science is in both senses of the term. In Part Two I tackle the question of the relationship of OR to one specific science, namely, social science.

(1) Because I shall argue that an adequate theory of inquiry cannot be developed in isolation from an understanding of what it is that is being inquired into, the term subject-matter dominates the discussion. Grasping the nature of subject-matter is fusing the two aspects of the general ideal. Sometimes I use the term object of inquiry to emphasise the need for this.

OR has emerged from the early days of scientists using their intelligence to solve problems to become a subject of academic interest and practice. This work is addressed to these academics. It is not primarily addressed to the practitioner although it obviously has **implications** for him. If the ideal of OR has any meaning it is as a long-run theoretical aspiration for the practice of OR. However, there is no time or space to develop these implications here. The long-run hope of all interested academics must be that they can help in the development of an adequate theory of the practice of OR. I share this hope.

PART ONE

OPERATIONAL RESEARCH AND PHILOSOPHY

CHAPTER ONE

THE IDEALS OF OPERATIONAL RESEARCH

Introduction:

Our task is an evaluation of the 'theory of OR'. To do this we need some criteria against which to compare the extant theory that is uncovered with the kinds of theory that, in the light of the ideals, would seem most appropriate. It will often not be possible in this thesis to do more that indicate in general terms the kinds of theoretical development which seem called for. Much of the thesis is taken up with a critical exposition of the status of the theory of OR. However, even to do this we require as a minimum a set of criteria which can act as ideals. The task of this first chapter is an exposition of OR's ideals.

The ideals of OR which are presented here will function in two main ways. Firstly, they will suggest problems which require some kind of theoretical solution. Secondly, after an exposition of the theoretical solution typically adopted by OR, they will act as reference point against which this solution can be judged and, to a limited extent, will provide guides to the directions in which theoretical exploration could usefully be taken to remedy any deficiencies which are found by this comparison. Put more generally, the rest of the thesis following this chapter will be an attempt to follow the efforts of OR to 'implement' or 'operationalize' its ideals.

Without exception, all attempts to implement the role of reason in human affairs have pursued the general ideal of 'improvement'. At the highest level it is the distinctive interpretation of this ideal which has marked off one attempt from another. Thus, Churchman's (1974) definition of systems design (in which he included OR) as:

> "... implementing improvement in social systems
> by means of the best available method of
> inquiry" (p. 452)

does not, of itself, distinguish OR from any previous attempt to implement reason in human affairs, stretching from Plato to the present day. All are for improvement and all have chosen their 'best available method of inquiry' - usually called 'science' (as it is by Churchman elsewhere, cf. 1971). Thus, for example, to fully understand the Platonic approach to social reform in all its distinctiveness we have, as Popper (1945) has shown, to understand what exactly Plato understood by the term improvement. Only when we understand that for Plato improvement is to be interpreted as the ideal of a perfectly stationary state we can claim to truly understand his approach. Similarly, any claim to understand OR must be based on an understanding of its ideal of improvement. There is little point in asking whether a particular approach 'works' without first clearly grasping the notion of improvement against which it claims it ought to be judged. Having done this the analysis can proceed further by the erection of more general and comprehensive notions of improvement against which both the practice and the ideal of the approach in question can be judged. Failure to make these steps clear in his analysis, I believe, weakens an otherwise similar analysis by Boguslaw (1965) of a number of approaches to designing "utopias". If anything distinguishes OR from previous attempts to institute the role of reason in human affairs it is the great deal of attention which has been devoted to an explicit elaboration of the most general and comprehensive notion of improvement which is conceivable. This is something which Boguslaw ignores. It is so general and comprehensive that it is difficult, if not impossible, to seriously disagree with it. No attempt to do so will be made in this thesis. In an analysis of OR the two steps of understanding the 'internal' ideal of improvement and comparing this with a generally acceptable ideal of improvement are collapsed into each other. The 'OR approach', in distinction to most other attempts to institute reason in human affairs, is almost exclusively preoccupied with an explicit understanding of what is meant by improvement. In most other approaches the notion is left implicit.

Totality and Complexity:

Two central and closely related ideals of OR are that planned interventions into the world ('designs') can only be guaranteed to 'improve' the situation if due attention is paid to the 'total' or 'whole' situation and, what amounts to an expression of the same idea, all the interactions between the 'parts' in the situation are dealt with in their full complexity. OR is against 'incrementalism' (cf. Braybrooke and Lindblom, 1963) and 'satisficing' (cf. Simon, 1957). It is against notions of improvement which rest on the complementary ideas of fragmentation and simplicity. I shall turn in later chapters to the questions of whether, in fact, OR attempts to deal with totalities which are truly perceived to be complex and whether it deals with totalities at all. These questions, in fact, relate to another ideal which is also strongly related to the ideal of totality, namely, the idea that improvement is only truly secured if in formulating the intervention no 'arbitrary' limitations are placed on inquiry. By 'arbitrary' is meant limitations which prevent a grasp of the totality. The above questions can, therefore, be reformulated to ask whether, in fact, OR accepts arbitrary limitations on its inquiries by reason of its theoretical commitments.

That OR is preoccupied with improvement defined in terms of dealing with the whole 'system' is a point frequently and forcefully made by Churchman. Thus he reminds his fellow academic operations researchers,

> "... the joy of OR is that it is in the centre of the deepest mysteries of the human race, because, academically speaking, it has taken on the whole system" (1970b, p. B - 53).

Also, reiterating the same point, Mitchell has recently (1973) complained that OR has yet to make a significant contribution to a world-scale problem. The connection between dealing with a totality and dealing with complexity is brought out by the basic texts of OR.

According to Churchman, Ackoff, and Arnoff (1957) OR arose precisely as a response to the increased complexity of the management function which came about because of increased size and the increasing use of technology (both human and mechanical). As they put it, "during this period of differentiation and segmentation of the management function a new class of managerial problems began to appear and assert themselves, problems which can be called executive-type problems" (p. 4). The problem for the executive, Ackoff and Rivett suggest, is to "... integrat(e) the policies and operations of the diverse departments reporting to him, in order to obtain an overall operation that comes as close as possible to realizing the organisation's overall objectives". "This integrating function is quite complex", they note, "because of the conflict of interests that always develop between the units co-ordinated by an executive" (1963, p. 3). The job of the executive, then, is to "consider the effects of a policy on each department, but his evaluation should depend on the overall effect" (idem, p. 4); he has, that is, to "put ..... together the activities of parts of an organisation in such a way that the overall operation comes as close as possible to attaining the organisation's objectives" (ibid). The central idea is "... that the activity of any part of an organisation has some effect on the activity of every other part ... (and, therefore,) .... it is necessary to identify all the significant interactions and to evaluate their combined impact on the performance of an organisation as a whole, not merely on the part originally involved" (p. 10). As Churchman et al put it: "it is an objective of OR ... to provide managers of the organisation with a scientific basis for solving problems involving the interaction of components of the organisation as a whole" (p. 6). In a later chapter I shall consider what light can be thrown on the theory of OR by comparing this view with the sociological 'problem of order'.

OR is not content even if the problems which are presented to it are relatively simple; not likely! It adopts the "... system's orientation" and "... it deliberately expands and complicates the statements of problems until all the significantly interacting components are contained within it" (Ackoff and Rivett, op cit, p. 10, my emphasis). Recently Ackoff (1974) has restated this central idea of OR as the "... realiz(ation) that no problem ever exists in complete isolation. Every problem interacts with other problems and is therefore part of a set of interrelated problems, a system of problems"; such a system he calls a "mess" (p. 21). 'Problems', therefore, have to be 'deliberately expanded'; they have to be considered as a 'whole' because it is only as a whole that the problems of concern to OR exist. There may be other types of problems (although I think that some operations researchers would doubt this) but they are not of concern to OR. OR is concerned with complex problems and some, as Ackoff (1960) notes, "... feel that complexity is essential to OR" (p. 10). Ackoff apparently agrees with this characterisation for he goes on to note that "the complexity of the systems studies by OR lies not only in the large number of parts which they usually have but also in the types of interactions between the parts" (ibid). In view of his reference to OR's preoccupation with "... systems ... involv(ing) feedback control" Ackoff presumably would argue that OR deals with situations where the interdependencies are 'reciprocal' (Thompson, 1967), i.e., situations where all parts intereact with each other, and where, of necessity, all parts must be brought into harmony simultaneously. Thompson sees this type of situation being the most difficult, with less difficult situations being characterised by 'sequential' and 'pooled' interdependencies.

However, not only are the problems with which OR wants to deal complex, they are also difficult in another way. They are 'ill-structured'.

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Mitroff and Sagasti (1973) define the difference between 'well-structured' and 'ill-structured' problems in "... terms of the class of decision problems" to which they belong. They define "a decision theory problem" as a situation in which one has to "... choose from among a set of acts $A_1 \ldots A_m$ that A_i which optimizes (in some sense) the decision-maker's (Z's) return U_{ij}, where U_{ij} is the utility or value to Z of the outcome O_{ij} corresponding to the doublet (A_i, S_j) where (S_j) is the set of the 'states of nature'" (p. 120). They go on to say that "an <u>ill-structured</u> or 'wicked' decision problem is one for which <u>one</u> or <u>more</u> of the (A_i), (U_{ij}), (O_j) and (S_j) terms or sets is unknown or not known with any degree of 'confidence'" (p. 121).

Mitroff and Sagasti comment that "ill-structured problems ... are problems such that the biggest problem connected with them is to 'define the nature of the problem'". (Exactly the same point is made by Mason, 1969, p. B - 404). They go on to say that "ill-structured problems have an elusive quality that seems to defy precise methods of formulation" and that "many social problems seem to be of this kind or quality" (ibid). It may be true that many 'social' problems are of this type, but it is certain that all of the really significant problems which OR tackles are ill-structured. It is true, as Mason (op cit) puts it, that "to date the main thrust of the research effort in management science has been directed towards the task of finding 'optimal solutions' to planning type problems <u>once they have been formulated and defined</u>" (ibid), but this is clearly mere neglect on the part of operations researchers; it is not a result which has come about as a consequence of the typical perception of the 'nature' of the subject.

Thus, Churchman et al argue that "research should <u>begin</u> with the formulation of a problem" (p. 105, my emphasis). And what does 'formulating the problem' entail? Well ... precisely the activities which would turn an ill-structured problem into a structured one. Churchman, Ackoff, and Arnoff define a 'problem' in precisely the same terms as Mitroff and Sagasti define a 'decision theory problem', namely, as a situation involving a 'decision-maker' ('Z'), his 'objectives' ('U_{ij}'), "the system, or environment" in which the decision maker exists ('A_i, S_j' = O_j), and the "alternative courses of action" ('$A_1 \ldots A_m$'). If it is the absence of one or more of these which defines an Ill-structured problem then it is the job of the operations researcher to transform an ill-structured problem into a well-structured one because it is his job to "identify the decision-maker" (p. 108), "complete(ly) formulate objectives" (ibid), "understand the organisation and the resultant system" (p. 110), "get as complete a list of alternatives as possible" (p. 111), and develop new courses of action as required (p. 113).

None of these things may be done completely adequately, in practice the bulk of operations researchers (if they can be called that!) may neglect these issues ... never mind, this is what OR is about. As a statement of fact about the practice of OR it may be true that operations researchers typically deal with well-structured problems (referred to by March and Simon (1958) as "programmed" and Boguslaw as "established"), where no search activity is required. However, as a statement about the ideals of OR nothing could be further from the truth.

Confirmation of this is not hard to find. Consider the views of another leading figure in the OR world - Stafford Beer (1966). Beer sums up an "... attitude towards a potential OR job which one increasingly hears taken up by senior OR men, as well as by the managers they serve" (p. 496). This attitude is that "there is a well-defined problem; the facts appertaining to this problem are readily available, well-documented, accurate; there is an established measure by which to quantify the facts; there are well-known techniques which have proved reliable for solving similar problems; there seems to be no difficulty in making another application here; it follow that the outcome of the study will be of this certain form; there is a simple way of implementing the conclusions in practice. When all of this can be affirmed it is sensible to embark on some work; if any of these propositions is falsified then really we ought not to tackle the job" (ibid). Not surprisingly in view of what has been said about the ideals (if not the successes) of OR, to Beer "this outlook is anathema" (p. 497). "It is", he goes on, " a straightforward denial of the historical origins of the subject ... and has nothing at all to say about the future potentiality of science in management" (ibid). "There remain(s)", he says, "hosts of problems which are ill defined, the precise questions which ought to be answered being unknown either to the manager or to the scientists. They are characterised by an absence of facts, by inadequate recording of data, by unsatisfactory mensuration concepts, and perhaps by the absence of any relevant metric whatsoever. No-one has the faintest idea what would count as a solution". As it is, "... these problems which keep managers awake at night ... some group of scientists ought to attack ... (them) ... in collaboration with the management concerned" and if, he goes on, "in the consensus of OR opinion, to do this is not to do operational research, then we shall simply have to find a new term to describe the activity" (ibid).

Churchman goes so far as to make the tackling of "wicked problems" a moral precept. "The term "wicked problem"", he says, "refer(s) to that class of social system problems which are ill-formulated, where the information is confusing, where there are many clients and decision makers with conflicting values and where the ramifications in the whole system are throughly confusing" (1967, p. B - 141). "The moral principle" which he enunciates "is this: whoever attempts to tame part of a wicked problem, but not the whole, is morally wrong" (p. B- 142). Churchman puts forward this moral injunction as an ideal (in fact, Churchman has done more than almost anyone else in keeping the ideals of OR alive in what is in some respects a hostile environment - we shall see more of his endeavours later), for he realises that "such a moral principle would appear to be ridiculous to many a mangement scientist, who has been brought up to believe that he should only tackle 'feasible' problems"; this lack of concern, however, is merely a reflection of the fact that "... the profession has not yet taken itself seriously". However, "that the profession has a moral problem, nonetheless", he concludes, "there can be no doubt" (ibid). A similar moral injunction is made by other leading operations researchers. Beer, for example, carries on his argument (presented above) to attack "... the prudential policy that OR does only what it knows how to do" because, he goes on to say, if this philosophy is adopted "... the profession becomes stuck with the relatively small things it has already mastered ... (and) ... managers become used to these little activities and begins to draw boundaries to the scope of an OR group". This means, he believes, that "the most creative and adventurous scientists depart, and the group that is left 'become incapable of great things'". It is clear that this capability is seen as being central to OR's distinctiveness because he goes on to say that without it there is a "... de-naturing of what was originally operational research ...". What is required in his view is "adventure, hard work and the motive to get things done". Ackoff had expressed a similar theme in an earlier work (1960): arguing that although "OR has established its usefulness in dealing with problems of limited scope ... it is important that operations researchers continually try to deal with problems of broader scope (e.g., long-range planning), that they try to deal with obstinate aspects of organisational operations (e.g., marketing and research in industry), and that they try to enlarge the category of types of systems that are studied so as to include even problems of national planning" (p. 30). A similar vision of the "Need for Operational Research" was advanced by Zuckerman (1958). He argued that OR was crucially concerned with the design and evaluation of strategic policy in military affairs (for example, the ramifications of the use of atomic weapons) and his case for OR is summed up in the view that "the less one knows about a situation, the more, not the less, scientific attention it demands" (p. 16).

Churchman (1968a) has identified the whole system/complexity ideals as lying at the very heart of the ideal of improvement. One part of the "... very serious problem ..., (of)... defining the concept of improvement ... is ... how can we design improvement in large systems without understanding the whole system, and if the answer is that we cannot, how is it possible to understand the whole system?" (p. 2). His conclusion is that we cannot: "... we seem forced to conclude that anyone who actually believes in the possibility of improving systems is faced with the problem of understanding the properties of the whole system, and that he cannot concentrate his attention merely on one sector" (p. 4). The mirror image of the ideal of the whole system is the ideal of complexity. The ideal is, Churchman suggests, "the maximum-loop principle ... (which) ... is based on a monistic philosophy: there is one world of interconnected entities, not many" (p. 114). The claim is that to truly understand anything it must be understood in terms of all of its relationships. The principle of the maximum loop says that starting at any one point, inquiry, if it is to lead to improvement, must push out on all fronts until it finds the largest possible system within which the part makes 'sense'. Complexity is the corrollary of wholeness. We do not have a totality until the expansion of relationships does not make any 'significant' difference. It is, of course, the perception of the complexity of relationships which make up a totality which has often led to the call for OR teams to be interdisciplinary.

Free Inquiry: OR as 'Revolutionary' Science.

That OR aspires to grasp complex wholes - to deal with the "whole /10 system" - because through this step it has defined improvement, has become an essential part of OR's vernacular. It has become the enduring motif of the 'OR approach'. It is, however, an essentially vague and open-ended ideal. It poses many more questions than it answers, and I think that there must be some doubt whether, without substantial 'filling out', it answers any significant questions about the theory of the practice of OR. One approach towards giving some concrete substance to the ideals of totality and complexity is that of General Systems Theory. This approach has found some favour amongst those who were simultaneously oriented towards 'hard science' and the problems posed by the ideals. General Systems Theory, with its aspiration to discover the laws which were general to all systems, seemed to offer a scientific way of filling out the ideals of OR stemming as it did from the physical sciences. Sustained interest in General Systems Theory in OR circles seems, however, to be lacking and I shall consider it only indirectly to the extent to which a residue of its ideas have sedimented into the 'systems approach'. A major reason for this lack of sustained interest must be that the General Systems Theory is often little more than a restatement of OR's ideals; its injunctions are the same: deal with totalities (which implies that they have 'boundaries') as complex unities (which implies relationships between the parts which may even be 'law-like'). Although some explicit use has been made of the 'laws' of complex systems (particularly by those with a cybernetic inclination, for example, Beer), for the most part the reaction of operational researchers seems to have been merely that there is a happy coincidence of 'frameworks'. This being so, General Systems Theory could not offer, nor did it produce, answers to questions which the totality/complexity ideals posed about the practice of OR. In particular, it did not produce answers to the question of how one should conduct inquiry into wholes (systems). Granted that systems have boundaries and exchange processes, but by what means does on discover these? (1)

(1) Some attempts have recently been made to answer this in terms of systems theory. See: Buckley 1972.

The failure of General Systems Theory to provide answers to this practical question has meant that those operations researchers concerned with the general development of the subject have been thrown back on their own resources. They have provided a set of answers to the theory of the practice of OR, and it is these which are of major interest in the ensuing chapters.

Although I have said that the ideal of grasping a complex totality is vague and open-ended, it has had a profound effect on the general approach which operations researchers have taken towards the problem of improvement-oriented inquiry. An implication which has been drawn from it (but which need not necessarily have been, as we shall see) is that if improvement is to be understood in terms of complex totalities then any limitation on the scope or depth of inquiry is to be avoided. If improvement is to be understood in terms of the 'whole system' then for inquiry to be valid it too has to encompass the whole system. Taken to extremes, the ideal of the whole system implies that there can validly be <u>no</u> limitations whatsoever on inquiry. Although few operations researchers embrace the implication in this extreme form (although a few sometimes come close to it) it has had a profound effect on their attitudes towards such things as the role of techniques in OR; the role of (lower) management in the practice of OR, and, more generally, on the nature of OR as a science. A particular instance of this last attitude is the view typically taken towards the role of the established sciences in the practice of OR. Before I turn to look in detail at these attitudes, let me first say a few words about the general directions in which the argument will flow from here.

The history of the development of the theory of OR has been the history of attempts to avoid the complete emptiness of the extreme implications that, in the ideal, there are no limits on the scope of OR's inquiries, while at the same time attempting to ensure that the limits which are imposed do not frustrate the complex totality ideal. Various attempts have been made by leading OR theorists to impose 'acceptable' limitations on inquiry, with varying degrees of consciousness that this was, in fact, what they were doing, and at varying degrees of explicitness. The fact which is most curious about these attempts is that although the problem of valid inquiry for improvement stems from the problem of complex totalities, no serious effort has been made in OR circles to understand the nature of the totalities with which they deal. There has been philosophical speculation and a priori theorizing at various levels of sophistication (which has much in common with the rather 'other-worldly' theorizing of the more ardent General Systems Theorists, and both of which deserve Mills' (1959) not too friendly appellation 'Grand Theory'), but virtually no attempt has been made to ground OR in the nature of those particular systems - social systems - with which they are, by virtually common consent, concerned. Many of the more recent writings on OR (e.g., Ackoff, 1970; Ackoff and Emery, 1972; Churchman, 1974; Friend and Jessop, 1969; Friend, Power and Yewlett, 1974; Eden, 1976; Rivett, 1974; Mitchell, 1976; Stringer, 1967 & 1972) have in one way or another acknowledged [1] that social systems are the totalities with which OR is preoccupied, and yet even those who make this acknowledgement fail to grasp the implication which this has for the ideals and, potentially, the practice of OR. This failure can be traced partly to philosophical commitments (which I deal with first) and partly to 'pre-packaged' commitments about the nature of the social world which have largely been discarded by modern social scientists (I deal with this at the end of the

(1) I shall, of course, produce evidence for this assertion.

thesis). Throughout the question which I shall be attempting to answer is whether the theory of OR which has been erected (or, more accurately, borrowed) to fill out the complexity/totality ideal is coherent, that is, does it allow for and encourage an understanding of the 'whole system' or does it prevent it and discourage it?

The Freedoms of OR:

Let us consider some reactions of some leading operations researchers to attempts (some of which are their own) to tie them down to a conception of the whole system. Operations researchers display great tenacity in avoiding commitment about what it is that they are talking about. Eventually, however, they must provide us with some guarantees that they are talking about the whole system; then we can judge them on their own terms.

Let us start by a consideration of the views presented in an early paper by Ackoff (1960) entitled the "Meaning, Scope, and Methods of Operational Research". In it his "purpose ... (was) ... to accumulate, consolidate, and synthesize what has been said and written about the strategy of OR" (p. 4); and he notes that "at the heart of any discussion of the strategy of a scientific discipline is the following question: What is the nature of the discipline?" (ibid). In answering this question Ackoff notes his general agreement with Kendall (1958) (with which he thinks "... most operations researchers are in substantial agreement ..." (p. 6) who describes OR "... as an attitude of mind towards the relation between man and society" (ibid) and Beer (1959) who expands on this view to conclude that "operational research is not a science, for it is not about anything; it is science" (ibid). Whilst agreeing with this characterisation (we turn to consider Beer's views in more detail in a moment) Ackoff feels the need to distinguish what, if anything, is unique about OR that is, what is the "... special class of phenomena" that operational research scientists study. His conclusion is that "... in OR we study ... sets of interrelated acts ... (which he calls) ... operations" (p. 8). An operation, he tells us, is "... set of acts required for the accomplishment of some desired outcome" (ibid).

I think it will be admitted that this is not very specific: what are "required" and what are the "desired outcomes"? Ackoff refines the idea of operations by noting that "in general, OR has not been concerned with the operations of individuals but rather with those of a certain type of system" (p. 10), namely, an "organization". Apparently, then, it is "... the types of problems involving organizational operations which constitute the domain of OR" (p. 12). This, however, does not place very severe restrictions on the scope of OR. All we learn is that organisations are "essentially" groups of people with specialised responsibilities who communicate with each other and are controlled (pp. 11-12). Far from defining a "unique class of phenomena" (a criteria which, in fact, he comes to drastically weaken: see below) we find that all we have learned from this effort is that organizational operations can be changed by changing 'organizational content', 'organizational structure', 'communication' and 'control' (pp. 13-14), and that many other groups of scientists are concerned with these. Given the generality with which Ackoff talks about organizations it comes as no surprise to find him concluding that selecting an appropriate approach to "... improv(ing) an organization's performance ... can only be effectively solved by an integrated examination of the organization".(p. 17) (1) Thus, although

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(1) I shall return in the last chapter to a detailed criticism of Ackoff's subsequent developments of the substance of organizations.

Ackoff "... trie(s) to show that the aspect of organizations that OR has staked out for study is restricted to their operations and the decisions which control them" (p. 17), he actually ends up by saying that properly the organization should be studied from more or less all angles. This seems to me to be merely a way of saying that OR is the application of 'science' in organizations. He seems to be saying that all aspects of an organization are (at least) potential co-producers of operations, and that, therefore, all of them fall within the scope of OR's inquiry. The definition of OR as a science in terms of it discovering co-productive relationships in no way limits the scope of OR. In fact, the emphasis which Ackoff places on this concept (particularly in his more recent work with Emery, 1972) indicates a clear open-endedness in the scope of OR because, as Keat and Urry (1975) say in a discussion of the originator of it, J.S. Mill, "... almost anything can turn up in a list of causal conditions" (p. 32). In the final chapter I shall turn to ask how Ackoff in practice controls the proliferation of potential producers of operations. He does it, we shall see, by the implicit theoretical commitments which he (and others) makes about the nature of the social world. For the present we can note his disquiet about the "... absence of psychological and social variables ... in OR models ..." (p. 26). (1) In his view "a total system study would include consideration of all relevant objectives and all of the classes of action which make up the operation. That is, each choice would be considered as a controllable variable relative to all the organizational objectives" (p. 29). All forms of "problem reduction" (a term he thinks is more suitable than 'sub-optimisation') are to be avoided.

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(1) Which, as Hales (1974) notes, is seen in OR as a "... blatant case of sub-optimisation" (p. 13) and encouraged the adoption of the socio-technical systems frame of reference.

"Where ... (problem) ... reductions are imposed on the researcher by external forces no problem arises other than the one of determining whether the problem should be tackled under the imposed constraints", Ackoff says (p. 30). However, for him real "concern lies with reductions made by the researcher of his own free will, with his inclination to 'cut the problem down to size'" (ibid, my emphasis). It is, then, the researcher's own choice; nothing in OR limits him to this or that type of variable, or this or that type of relationship. In ackoff's opinion, "... the heart of the problem that has come to be known as "sub-optimization"" is the researcher's inclination to "... reduce the number of objectives, courses of action, or uncontrollable variables or modifying them in some way so that the resulting problem can be handled by familiar methods and techniques" (ibid). The danger which Ackoff sees is not that OR in engaging in problem reduction will solve problems a little less well than it otherwise might; it is a matter, no less, of survival of OR. Thus, Ackoff goes on to say that "if OR is to survive it must maintain a strong problem orientation, not a technique orientation. It must", he says, "expand its methods and techniques to fit the problems and not contract the problems to fit available methods and techniques" (ibid). Problem reduction clearly strikes at the heart of OR's raison d'etre. Other operations researchers have expressed as similar mistrust of reliance on the techniques of OR on similar grounds, and I turn to them in a moment. Although Ackoff says in this paper and elsewhere that what distinguishes science from common-sense "... lie(s) either in its subject matter or in method, or both" (1962, p. 2), his efforts on the question of subject-matter lead him to equivocation: "whether or not OR has a unique area of study, the question remains as to whether or not it has a unique methodology" (1960, p. 18). From his analysis there is clearly some doubt regarding the first, even though his intention had been to counteract the implication of Beer's remark (quoted above) that OR "... does not have and cannot develop a unique body of knowledge about the phenomenon which it studies" (ibid). This highlights the question of what distinguishes OR from any other possible discipline interested in the whole system. Does the same open-endedness apply to OR's methodology as well?

Again, Ackoff attempts to head off what he sees as the prevailing view in OR. He quotes Jessop's view that "... what constitutes operational research is not knowledge but know-how" (ibid). This clearly runs against the view that OR has a unique subject-matter but Ackoff draws consolation from the fact that the abstract model-building which Jessop advocates is in some sense scientific because "... an important characteristic of the scientific approach to problem solving is the act of abstraction which is involved" (p. 19). "Abstraction" as an attribute of OR's distinctiveness as "... unique scientific activity... (with a) ... methodology which equips... (its practitioners)... to study their subject matter more effectively than anyone else can" (p. 4) does not seem very distinct. Clearly, with an unrestricted subject-matter OR needs an unrestricted methodology. This is what Ackoff suggests. He defines methodology in exactly the same terms that he defines subject-matter. The subject-matter of OR is 'problems' and

> "Methodology can be considered to be a special type of problem solving, one in which the problems to be solved are research problems... (A)ny problem situation, and hence research-problem situations, can be represented by the following equation:
>
> $$V = f(Xi, Yj),$$
>
> where V = the measure of performance or accomplishment that we seek to maximize or minimize.

where Xi = the aspects of the situation we can control ...

where Yj = the aspects of the situation (environment of the problem) over which we have not control" (1962, p. 28)

Defining methodology in this way deliberately makes it as general as OR's subject-matter. This description does not help the outsider very much in his attempt to pin down what OR believes itself to be because the methodology is as non-specific as the subject-matter. Ackoff eventually resorts to the speculation that there is "... a trend toward models of greater generality... (and)... as these are developed consequences deduced from them may form a generalized body of knowledge about operations" (p. 20). One can only conclude that if, as he puts it, OR eventually found a "unifying body of knowledge" with its methodology it would, as he has described its outlines, be knowledge of everything: formulating a problem, he says, "... requires a complete identification of the components of the decision maker's problem..." (1962, p. 67, my emphasis).

Beer (1966) in his attack on "stereotyped" views of science, comes to an identical conclusion. The two particular stereotypes which Beer attacks that science solves 'given' problems and that the solutions which it does provide are found within the context of that given problem. In short, these stereotypes see science solving the problems it is given. To take the first stereotype, Beer argues that "the point to be made is this: operational research must encompass the whole of the problem situation, and management may not succeed in defining what this is" (p. 50). Beer will brook no interference to this freedom to define what the problem is. "Only the operational research team", he says, "can be held responsible for defining the scope of the problem" (ibid). (1)

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(1) Beer continues rather ruefully that "this is perhaps a revolutionary point of view; certainly it is one which management often resents as an abrogation of its prerogatives". "But", he concludes, "it is a rational attitude and one which management ought to embrace" (ibid). It is worth noting here a future major issue to which these remarks can be taken as a pointer.

The issue which will be of concern later is the ambivalence of OR towards management. It has been expressed by others, as well as by Beer. The Education and Research Committee of the Operational Research Society (1973) in its gloomy comments on 'The State of Research in OR' has drawn attention to this issue which it clearly sees as a central problem. It raises the "general question: what kind of subject do we think we are? Are we", it goes on, "to do the bidding of our paymasters, solving managers' problems by whatever ad hoc means seem best suited to the immediate circumstances, or are we indeed a science devoted to the greater understanding of the behaviour of complex socio-economic systems?" They go on to quote Tomlinson (1971) who points to what he sees as "... the OR dilemma, 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 so that major improvements can be effected in the future". This issue also concerns Popper, whose philosophy of science we shall look to for insights into the theory of OR. Popper, too, is concerned with the relationship between the free scientist and the achievement of objective science. At one point he looks for this anchorage in tradition and appeals to the scientist to "study the problem situation of the day" to continue a "line of inquiry which has the whole background of the earlier development of science behind it; you fall in with the tradition of science..." (1963, p. 129). There is, he says (and Beer agrees), no way to avoid the impress of tradition (cf. Magee, 1973, p. 70) and yet he elsewhere says (e.g., 1970) that being constrained by frameworks is poor science. Popper and the operations researchers never satisfactorily resolve this contradiction. Popper achieves a resolution, as Habermas (1974) shows, by neglecting to critically analyse his pivotal notion of 'criticism'; it is this which makes (as we shall see more fully in a moment) a proposal 'objective' and yet what eventually makes the criticism itself objective is unanalysed. The same complaint can also be levelled at OR, although the resolution arises there in a different form. However, Habermas neglects, after having raised a very pertinent question, to analyse on what grounds Popper thinks the resolution holds - if there is one this Popper cannot be accused of it is oversight. Analysis of both Popper and OR leads to the suggestion that in both cases the resolution is taken to hold because both rely on a largely undisclosed model of the social world which ensures that the processes of criticism are themselves objective. Very crudely, both envisage science as a social system with mechanisms which, if certain preconditions hold (Popper's 'open society'), continually evolve and adapt as entities to produce more and more perfect solutions. Under this view, then, it is perfectly acceptable to take 'tradition' as the starting point for inquiry - one would be crazy not to. The question arises, however, as to whether in fact this is the nature of science or of social systems in general. In any case, the point I wish to indicate here - I leave substantiation until a more appropriate time - is that it is on this sociological question that important issues for the successful implementation of OR's ideals hinge.

The reason for this is that "... the totality of what exists is an integrated system, and anything split off from the totality and considered separately is incomplete" (p. 53). This is the reason, he says, why to "... determine the boundaries of the problem ... is the most difficult problem in operational research"... because the "... scientist needs to enlarge the scope of his study in every dimension until the factors he is bringing in seem to make no difference to the answers he is getting" (ibid). The scope of OR's inquiries are as big as the problem: expansion of the scope of OR's inquiries "... is a process dictated to the scientist by the interrelated nature of the world" (p. 54, my emphasis). As Wittgenstein (1961) says in his investigation of the meanings of such language: "The world is all that is the case" (p. 5); which leads to the implication that "the sum-total of reality is the world" (p. 8). OR does not seem overly constrained by its investigation of the world (although we shall see that it is constrained by 'nature'). Beer sums up his argument thus: "The management problem has no stereotype. It is unique. It is malignant. It may involve all sorts of factors that no-one imagines to be relevant. The job of the OR man is to handle it. Do not tell him what the problem is, nor where his task may take him. Tell him what the trouble is, and send him to find the problem" (p. 54).

So much for given problems. Given solutions trespass on very much the same ground. For Beer the OR man is no more 'technologist', he is a free scientist. Beer argues that "it has to be accepted ... that the OR scientist works with true scientific comprehensiveness; he is not a pedlar of techniques". The OR man as scientist carries "the mark of the true scientist... (namely)... breadth of outlook" (p. 10). Techniques are relegated to second place, then, because they pre-empt inquiry. Thus, Beer says that what really happens when a decision technique is merely 'applied' is that "the mathematical model is a rigorous account of a very loosely formulated and altogether untested theory about the natural mechanism involved" (p. 224). "It is a limitation of the decision-theoretic approach", he says, "that it impetuously declares what the structure of a decision _ought_ to be without first investigating what it is" (ibid). Therefore, "one dare not slap a decision theoretic technique onto a problem like a poultice. The relevance of the mathematical model has to be examined with the greatest of care, and this in itself is a full-scale scientific investigation, involving the collection of facts, the formulation of hypotheses and the undertaking of experiments" (pp. 226-7). Beer sums up his argument by saying that the "... mathematical model does not exist sui generis" (ibid). This freedom is demanded because "operational research is an empirical science: it is concerned with actual situations and not with idealisations of them - and what is important to the manager may not have been present in the minds of those who developed the techniques" (p. 219). OR is normally to be thought of as a free and revolutionary science. It is only "... _within a given and accepted framework_ ... (that)... it may well be true that the problems of management can be matched to ... (techniques) ... for quick handling by someone trained in the appropriate mathematical technique... (whereas)... the whole purpose of Operational Research is to join in with the manager in questioning that very framework" (p. 404; cf. also Churchman, Ackoff and Arnoff op. cit. p. 12; and more recent expressions of this view by Wagner (1971) and Rivett (1974)).

A further illustration of OR's attitude towards limitations on its freedoms is provided by Beer's views on the proper place of lower management. I shall return to these views in a later chapter to draw different (but congruent) implications from them. Solutions are stereotyped when they lie, as Beer puts it, "... in the common phase space - that is, in the area of overlap between the phase spaces of the individuals who comprise the management team..." (p. 59). To explain: a 'phase space', as Beer describes it (pp. 57-8) is a "framework" within which, "... if it is properly selected, may be discovered the solution to the problem". When applied to an organisation Beer says that we need the concept of "thought block" - that is, the idea of the members of an organisation being "conditioned" so that the "... brain learns to find patterns in the world outside". "This process of conditioning", Beer says, "is vital for survival, for in its absence the world would appear entirely incoherent and unpredictable". With this framework or conditioned thought block the unruly and limitless universe is brought within manageable proportions: it provides stereotyped responses which reduce uncertainty and give direction, (1) and this is what Beer objects to. He concedes that people have different phase spaces (a notion very similar to 'role style'), but they are phase spaces nonetheless, and therefore even adopting a solution which lies within area of phase space overlap provides "... no guarantee that this gives the most appropriate answer" (p. 59). And why exactly? Because "the company itself has no brain. It has a phase space within which it always operates, but this can only be interpreted by human beings who inevitably get the company's phase space confused with their own" (ibid). This creates a difficulty for "the answer to a particular problem might... lie on the fringes of this space, in an area not belonging to the phase space of any one manager in the organization. The best answer to the problem would therefore be missed, because it would be strictly inaccessible to the management" (ibid). The same reason is advanced by Beer for the freedom

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(1) It is hard to resist, even at this early stage, alerting the reader to the strong similarity between Beer's characterisation of the functioning of organizations (more of which is to come) and that proposed by the Carnegie school of organisation theory, particularly Simon and his collaborators. Later on I shall go further than merely asserting a similarity between the conceptualisations of operations researchers and organisation theorists; I shall attempt to establish the case that operations researchers rely on such schemes. It is also worth noting here - I take it up in more detail in a moment - that Beer's 'phase space' is very close to Kuhn's notion of a paradigm, which is a device to enable problem solving to proceed. Parallels between OR's theory of inquiry and Kuhn's philosophy of science will be drawn and criticized.

claimed by OR to be the sole definer of the problem, and is implied by Churchman, Ackoff and Arnoff's comment that "... the initial statement of a problem provided by management to an OR team is more apt to be a revelation of symptoms than a diagnosis" (p. 68). As Beer goes on to say, the reason is "... that many of the problems that most need to be solved... are those very problems in which the solution has not only eluded management but has not even been envisaged... precisely because of the way in which a company is organised". That is, "... into clear-cut areas of responsibility; and the manager, whose authority derives from his position in one of those areas, is conditioned by the whole of his experience to seek solutions in which he can feel confident from personal knowledge, and over which he can exert personal authority" (pp. 50-51). Management (or at least operating management - as we shall see, Beer distinguishes 'higher' and 'general' management from other management) cannot be allowed to interfere with the investigations of OR - any intercession by management between 'nature' and the OR scientist corrupts the scientific process because management takes decisions "... based on mechanisms which, though rational, are not logical. They derive from biological necessity, not from intellectual processes, and result in decisions which have more to do with learning to survive than with... objective analysis..." (p. 1). The scientist, according to Beer, is essentially distinguished from the layman by the fact that the scientist can exercise "... his free will in <u>rigorous</u> choice" (p. 31). The rigour in the scientist's expression of his freewill ("self-consciousness", "intellectual") is imported by his methodology ("If we want to use words carefully", Beer says, "the method of science <u>is</u> method" (p. 29)). Rigour is provided by the "formal languages" (Beer's chapter 8) which are the highly generalized languages of "quantity", "probability", and "quality". These languages in themselves do not impose limitations on the scope of OR, however, because methodology is subservient to the freewill of the scientist. The scientist expresses his freewill in the self-conscious building of models and "hence models take pride of place over the formal languages used to make them rigorous" (p. 143). The scientist's freewill in choosing the problem definition could not, in any case, be much restricted by these formal languages; they are "abstract" and refer to nothing in particular and everything in general.

Conclusion:

These examples illustrate the reluctance which operations researchers show in accepting limitations on the scope of their inquiries. They accept no interference from lower management, from techniques, from methods, and from notions of subject-matter. This shows that operations researchers are equally reluctant to say precisely what it is they are talking about. The refusal to accept limitations imposed by a concept of OR's subject-matter is the key dimension along which freedom is claimed. The other restrictions flow more or less naturally and acceptably from the complex-totality ideal; the refusal to be pinned down to at least the attempt (we shall see that the attitude stretches to this extent) to grasp the nature of the subject-matter does not. We shall see in a later chapter that, in fact, there are very strong grounds for arguing that without a grasp of the nature of the subjectt-matter being dealt with there is no hope whatsoever of achieving the ideal of securing improvement in the context of complex-totalities. We shall see in the last chapter that insisting on this freedom finally takes its toll. It is impossible in practice to operate without at least an implicit grasp of the nature of the subject-matter being dealt with. This is an implication of Kuhn's work (and, to some extent, of Churchman's as well) but it is an implication which is not recognised, and consequently is not developed, for reasons I go into later. The problem is that because of OR's refusal to be constrained by subject-matter as a general principle, it has neglected the task of inquiring into the nature of <u>its</u> subject-matter. The result of this neglect has been that OR implicitly embraces a notion of its subject-matter which in fact prevents it from grasping the complexity and totality of the systems with which it deals, namely, social systems. We shall see that what is involved here is not merely the addition of "behavioural variables". Rather, I shall argue that even as it is seen by operations researchers themselves, what is required for there to be a theory of OR is that it should have an adequate theory of social systems. However, we must first inquire into the philosophical leanings of OR (its theory of inquiry) to discover (a) how it tames the potential anarchy of inquiry which is implied by its envisaged scope (OR's attitude is very similar to the anarchistic theory of knowledge advanced by Feyerabend (cf. 1970) which argues, contrary to OR's ideals, that there are no rational standards for science); (b) how these attempts to limit the scope of OR's inquiries have injected a bias against the desirability (or even possibility) of giving serious consideration to the subject-matter of OR, and, (c) how lack of consideration of the subject-matter of OR influences the possibility of it achieving its ideals. These questions are the conern of the remainder of Part One.

CHAPTER TWO

POSITIVISM, CONVENTIONALISM, AND OPERATIONAL RESEARCH

Introduction:

There are two sources which operations researchers turn to when they are searching for ways of controlling the potential anarchy of inquiry which is encouraged by the complex-totality ideal. These sources are philosophical positivism (which later turns into sociological positivism) and a philosophy which should (cf. Kolakowski, 1968, whose classic account of positivism I follow) be seen as an extension of this, namely, conventionalism (which appears in sociology as idealism). We shall see that both of these philosophies are paradoxical in that they attempt to simultaneously restrict inquiry (to make it scientific) and, at the same time, give it complete freedom. That philosophers should have pursued these seemingly incompatible goals is not surprising if we consider the historical circumstances in which these philosophies were for the most part worked out. For most of the history of these philosophies science was waging an ideological war against arbitrary impositions, mostly from ecclesiastical quarters. Science had to both assert its freedom and justify its claim to be the provide of 'controlled' knowledge. The working through of these historical concerns to a model of scientific inquiry has, however, left the paradox intact. The central weakness which this paradox has produced from our point of view is that, because of the preoccupation with freedom, philosophers have been willing to do no more than offer criteria by which we are entitled to say 'this is knowledge'. From their undoubtedly strong 'bargaining' position this was all that the philosophers on behalf of scientists were prepared to concede. Great reluctance has been shown by philosophers and scientists in disclosing or analysing how knowledge is acquired in the first place. This weakness seems to me to be fatal for OR which, if it needs any sound theory, needs a theory of inquiry itself. Churchman has, of course, made this the theme of much of his work (particularly, 1971). I shall give his work a detailed consideration in later chapters.

Let us see, then, what connections can be made between OR and positivism. No one person, nor even OR as a whole, has a consistent and completely worked out positivistic scheme of inquiry. Enough strands are evident, however, for the influence of positivistic thinking to be justifiably considered a significant thread in the theory of OR. It is (to carry on the metaphor) interwoven with the conventionalist thread in various ways to produce OR's philosophical garb.

<u>The Nature of Positivism:</u>

There are many varieties of positivism, and I cannot possibly deal with all of them here. Most often positivism appears as a dominant orientation which admits many interpretations (in this respect it is similar to a paradigm: see chapter 3). Positivism does, however, offer the prospect of control. Kolakowski observes that

> "Defined in the most general terms positivism is a collection of prohibitions concerning human knowledge, intended to confine the name 'knowledge' or 'science' to the results of those operations that are observable in the evolution of the modern sciences of nature" (p. 18).

Positivism, as a general philosophy, lays down four 'rules' of control (Kolakowski, pp. 11-19).

The first is the "Rule of Phenomenalism". This is the basic rule of positivism. According to positivism "we are entitled to record only that which is actually manifested in experience; opinions concerning occult entities of which experienced things are supposedly the manifestions are untrustworthy" (p. 11). This rule is designed to prevent questions about what, if anything 'lies behind' our observations, i.e., what generates them.

These questions are ruled out as 'meaningless'. The limits of knowledge, its 'raw material', are phenomena. Any attempt to extend the limits of knowledge beyond that which can be experienced is forbidden. This gives rise to the second "Rule of Nominalism".

"The Rule of Nominalism states that we may not assume that any insight formulated in general terms can have any real referents other than individual concrete objects" (p. 13). Theories, for the nominalist, are merely convenient and useful ways of ordering experience. They do not extend the limits of our knowledge; in themselves theories do not constitute knowledge. They 'represent' our true knowledge which is limited to our experience. Theories, in short, have no "ontological status" (cf. Keat and Urry, 1975, Chapter 1) in that they do not describe any reality beyond the experiences which they represent. "According to nominalism, in other words, every abstract science is a method of abridging the recording of experiences and gives us no extra, independent knowledge in the sense that, via its abstractions, it opens access to empirically inaccessible domains of reality" (pp. 15-16).

An extension of nominalism is the "rule that refuses to call value judgements and normative statements knowledge" (p. 16). Values do not exist of themselves except as subjective experience. Unless we can reduce them to phenomena they do not constitute knowledge.(1)

The final rule, which follows phenomenalism in particular, is "... a belief in the essential unity of the scientific method" (p. 17).

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(1) Ackoff and Emery (1972) have attempted to construct a methodology for behavioural research following this rule. I consider it in detail in the final chapter.

This rule also follows from positivism's general orientation towards only providing criteria for valid knowledge. If knowledge is restricted to phenomena, then it follows that all the sciences (that is, only those areas of inquiry which deal with phenomena) are subject to the same criteria. To admit that different sciences deal with fundamentally different realities (even while admitting that they deal with different phenomena) would be to deny the place of phenomena as the seat of knowledge. Because the origins of knowledge are irrelevant to the status of knowledge we cannot distinguish between the sciences by reference to their subject-matters.

Operational Research and Positivism:

The unity of science is an ideal which has inspired much thinking about OR's theory of inquiry. The latent (but unfulfilled) hope has been that such a theory could be provided by an application of a 'logic of science'. That is, in the spirit of positivism, an analysis of science which is conducted at a 'meta' level, independently of the "... actual and varying specific contents of different scientific theories" (Keat and Urry, op. cit., p. 25). In keeping with this general positivistic orientation Ackoff (1962) is "... not... concerned... with the body of information and knowledge which... (science)... has generated; that is, not with the specific theories, laws, and facts that have been developed in the various physical, life, and behavioural sciences". "Instead", he says, he is "... concerned with the procedures by which science generates this body of knowledge, the process of inquiry" (p. 5). This reference comes from a book dedicated to scientific method, so perhaps the bias is not surprising. However, the whole work rests on the assumption that for the purposes of OR (or, more generally, "applied science") what is really significant is what is general to science, rather than what can be learned from its accumulated knowledge. Ackoff has also made little attempt to redress the bias (an exception is his 1974 work which I discuss in the final chapter). This bias is common throughout OR circles, with many discussions on the nature of OR being restricted to "The Scientific Method" (Goodeve, 1957); "The Nature of Science and Methodology" (Ackoff, op. cit., Chapter 1); "Science and Operations Research" (Minas, 1961), and so on.

A phenomenalist and nominalist basis for this bias is clearly evident in the work of both Ackoff and Beer. Although we have seen Ackoff argue (rather half-heartedly we concluded) that scientific status depended in part on possessing a distinctive subject-matter, this view is all but abandoned when he comes to discuss the interdisciplinary nature of OR. Displaying his nominalist base he argues that "the division of science into disciplines was accomplished by man, not by nature" (1960, p. 24). The phenomenalist in him comes out when he immediately goes on to argue that "the disciplines cannot be individuated by a unique set of objects or events which constitute their subject matter, but they are distinguished by the aspect of phenomena to which they direct their attention " (ibid). For Ackoff all that is distinctive about a discipline is that it "... concern(s) itself with... (specific)... aspects of... phenomena" (ibid). If this is all that is significant about the different scientific disciplines, then OR as an 'interdiscipline' is a short and logically compelling step. The complex-totality ideal can be met by the employment of an interdisciplinary team because "in the operation of a system... the aspect... (of phenomena)... of the system which can be manipulated so as to improve its performance are likely to come from many different disciplines" (p. 25). The positivistic inspiration for the interdisciplinary team idea is that the team would cohere around the logic of science. In the ideal all subjects would be represented, and their contributions, by this line of reasoning, would be processed by a logic higher than that of the subjects themselves. Thus, Beer argues

> "... the OR man seeks other kinds of natural law than those normally investigated academically, namely those which are themselves interdisciplinary. For these are laws that apply to systems in general, and to control in general - these are the laws that management would like to discover" (p. 119).

In Part Two I shall amongst other things assess Beer's attempts to provide an account of such laws as it is formulated in terms of a theory of management and a cybernetic model of organizations.

Phenomenalism is for OR a way of claiming from, as Beer puts it, the stereotypes of science working on given problems with predetermined solutions. In Beer's view "the intellectual revolution of twentieth - century science... (which)... has been accepted by the scientist... (is that)... it is proper to his work to uncover the essential characteristics <u>of things</u>" (p. 4, my emphasis). That is, we have come to the "... realization that science is basically concerned with investigating how and why things are as they are". This means that "science, in fact, is organised knowledge about the world, not organised knowledge about itself" (ibid). It is merely a historical accident that the investigation of the world is grouped around 'subjects' and Beer sees this as an unfortunate "... hardening of the faculties" which means that investigation is 'slanted' (p. 5). This conception of science as investigation into a subject matter is merely a "... confusion in the public mind between science and technology". Only for the technologist is it relevant to ask "'What is your subject'" because the real scientist has to reply "'I investigate the world'" (ibid). "...(s)cience is not a thing done by physicists, nor a thing done by chemists" but is "the establishment of knowledge about the world" (p. 10). In Beer's view, then, "... nature is the subject-matter of science" (p. 55).

This phenomenalism is blended with a radical nominalism around the interdisciplinary team concept. Although in the previous chapter we have seen him argue that what distinguishes the scientist from laymen is his exercise of freewill, he is concerned that the scientist's training in practice "... gives him a bias" so that "the scientist, like those he seeks to advise, tends to be a prisoner within the accepted boundaries of the existing solution" (p. 48). We shall see shortly that this worry comes to dominate his thinking about science and the nominalism expressed here turns into conventionalism. To counteract this tendency he suggests that "... when a new area of study is being opened up, it is as well to use an interdisciplinary team" (ibid) because then the scientists "... do not play individually stereotyped roles" (p. 49). Apparently it is Beer's view that merely being in a team will mean that the scientists "... are neither stereotyped nor committed in advance to a point of view" (p. 50). The idea of an interdisciplinary team, therefore, is that by their interaction the many separate biases will be neutralised. OR searches for "completely novel solutions" (more on this in a moment) so that "... it cannot be predicted in advance which branch of science will be most useful in suggesting the breakthrough" (ibid). Conceptions of subject matter (theories) ought to be treated completely nominally. They are often not, so the next best thing is to, in some overall sense, neutralize biases to make them nominal in effect if not in origin. Another implication of the neutralizing hypothesis is that, in Beer's view at least, science is neutral as regards its subject-matter. This implication is borne out by his subsequent discussion about models (I have more to say about this in Chapter 5).

The strictures of both Beer and Ackoff that science is concerned with phenomena is the basis of their complaint against individual scientific disciplines: Phenomenalism "... require(s) that we distinguish between the true content of the data of experience (appearances, phenomena) and such illegitimate extrapolations from it as present the qualities we observe as qualities inherent in the nature of things..." (Kołakowski, op. cit., p. 20). For both Beer and Ackoff the scientific disciplines represent precisely these illegitimate extrapolations. Beer takes this point to the extremes of both nominalism and phenomenalism. Of the OR scientist (and one presumes he has a team in mind here rather than an individual) he argues that

> "he has no interest in arguing about the 'right' use of... (for example)... a psychological term, but only in the results. So the OR man is a special kind of scientist, for he does not have to bother with determining the laws governing basic natural phenomena such as learning. This is a preoccupation for the academic animal psychologist who has a special responsibility to define the terms to be used in his science, and to denote the mechanisms for which terms will stand. The OR man has, however, other concerns which do not preoccupy the academic scientist who normally regards himself as a specialist in a fairly narrow field. He must operate <u>across</u> the various scientific disciplines, being sufficiently knowledgeable and mentally agile to identify the model he needs..."
>
> (p. 119).

OR is not interested in the subject-matters of science; for OR they are purely nominal representations of phenomena. Combined with his earlier views a similar conclusion is implied by Ackoff's (1974) statement that "although concrete systems and their environments are objective things, they are also subjective insofar as the particular configuration of elements that form both is dictated by the interests of the researcher" (p. 84). I shall return in a later chapter to the concept of totality which is implied by these views. For the present we can begin to see what Beer means when he talks of "... uncovering the mechanism which underlies..." (p. 70) facts.

Insight is gained into OR's theory of inquiry by its links with the distinctly American philosophy of pragmatism. Elements of this philosophy underlie the views which we have so far considered, and they underlie others which we shall consider shortly. Pragmatism is linked to positivism in being, in some versions, an interpretation of positivism into a 'way of life' (Kolakowski, op. cit., Chapter 7). This extension of positivism is a natural one for OR to make being concerned, as it is, with the practical. Beer specifically links his conception of the nature of science to the well-known pragmatist Charles Peirce whose early work had marked positivist tendency. However, the extremity of Beer's views links him more closely with the interpretation of pragmatism given by William James. Beer shares with James (and most other operations researchers) the belief that there is an "... opposition between impartial explanatory knowledge and useful knowledge..." (Kolakowski, op. cit., p. 188). (Much of my later criticism of OR is in fact directed at just this distinction). James is very similar to Peirce in arguing that the meaning of our theories is their practical applications, and does not refer to unobservable underlying reality. However, he went further to assert that

"... we are right not only to behave <u>as though</u> the world looks this way or that, but also to entertain the idea that it is truly this way or that" (Kolakowski, op. cit., p. 189). For James the consequences of a theory exhausts its meaning and its truth; for Peirce (and for Beer sometimes) truth was still restricted to a "... correspondence between judgements and actual states of affairs" (p. 187). For both James and Beer

> "It makes no sense to ask, 'How are things really constituted?' but only, 'What do I get if I believe this or that?'... Science is not a collection of truths in any current, traditional, metaphysical, or transcendental sense, but a collection of practical directives that make sense when they can be carried out, and that are true when they further life, multiply energy, provide gratification". (p. 191).

We have seen Beer express a similar view as regards OR as an interdisciplinary science. The link with Jamesian pragmatism is forged with views such as the following:

> "It is worth remembering above all that facts are no use without a specification of why they are needed... Secondly, and this is a far more sophisticated point, the purpose behind data collection <u>alters</u> the facts that are collected" (p. 71).

> "The simple-minded notion of 'objective measurement' collapses under close examination. It turns out that measurements

are themselves based on theory, since there must be a purpose in mind when the measuring instruments are devised" (p. 96).

> "Science is certainly based upon fact and experiment; but the organization of its findings into coherent and useful generalizations is a subjective process... It therefore comes about that the so-called laws of nature are contingent, and not absolute; they are contingent on the languages we use to express them, both as to the structure of those languages and their frame of reference" (p. 121. Note that this statement is verging on a conventionalist view; I turn to this later).

Both Churchman (especially 1961a) and Ackoff (1962) show marked pragmatic tendencies. This can be seen in their treatment of defining, an activity which occupies a central place in their thinking. Both are preoccupied with operationalizing definitions, and both follow Peirce (and other positivists, particularly Bridgman and Stevens) who argues, as Kolakowski puts it, that "every work denoting a thing or a quality must be subjected to the pragmatic test before it can be legitimately employed. To know what it means we must state the practical steps by which we can verify whether a given object corresponds to the word in question" (p. 186). However, they too veer towards Jamesian pragmatism when they insist on the crucial role of purpose. Thus, Ackoff says that

> "To determine whether or not two operations are the same requires that we make

> explicit the sense in which we want <u>sameness</u> to be taken. This requires an explicit statement of the purpose for establishing difference or similarity between two or more operations" (1962, p. 143).

Without giving his work the detailed consideration it warrants, we can note that Churchman also adopts the pragmatic stance when he defines "measurement... functionally... (as)... the organization of experiences in such a way that they codetermine purposive decisions in a wide variety of contexts..." (1961a, p. 101). This definition follows on from his earlier discussion of the "theoretical content of facts" (1961a, Chapter 4) in which he poses the question: "... if the ultimate authority of observation or fact is removed, what has become of the objectivity of science?" (p. 87). He goes on:

> "Of course, to say that observation and measurement are theoretical constructs is not to say that they are fantasies of the mind or rational constructions without any ties to reality. But one must now think of reality and objectivity in different terms. Once the notion of "ultimate" authority is given up, one can no longer assert that the <u>real</u> is what is <u>directly observed</u>. Wherein lies the objectivity of science?
>
> The answer that most naturally comes to mind is that the objectivity of science lies - not in what it takes to be the ultimate authority - but in what it seeks to

accomplish" (p. 89).

The association of OR with nominalism in general and pragmatism in particular can be seen as an attempt to claim the freedom of inquiry which we encountered in Chapter 1. As Kolakowski says of it:

> "... pragmatist philosophy amounts to a kind of epistemological Jesuitism, a basic readiness to accept anything and everything, and boundless flexibility in applying evaluative rules in knowledge. Any other view is exposed to the objection of being rigidly dogmatic, of sacrificing the real values of life to abstract metaphysical fictions. This is indeed the case once it is granted that reality has no inherent qualities that can be interpreted as such, but is merely a collection of opportunities for individual success, and that this exhausts its possible meaning" (op. cit., p. 194).

In the pragmatist view the truth of anything depends on usefulness. This has the consequence that the "... scope of the truths we are entitled to accept is altogether unlimited, so long as they are useful to us in any respect whatsoever" (p. 193). With a pragmatic philosophical base, therefore, OR still has its unlimited freedom. Also, of course, it is still open to the charge that OR's theory of inquiry is that 'anything goes'. In response to this state of affairs two different strategies have been proposed. These two strategies, although both responding to the potential anarchy of pragmatism, have perceived this danger in slightly different ways. In one strategy the fear is that the scientist, because of his innate subjectivity, will persist (unless appropriate steps are taken) in having a narrow and biased view of reality (the whole system). The whole system is seen in strongly phenomenalist terms as all aspects of all phenomena. We have seen Ackoff express this view. As the danger is seen to come from the tendency of the scientist to reify his subject-matter, the strategy adopted here is to devise ways whereby the scientist (and/or science as a whole) is forced to de-reify subject-matter. The interdisciplinary team idea is a crude example of this. Popper and his followers (particularly Lakatos) have devised more sophisticated schemes. The objective of this strategy, in short, is that the scientist (or science) be forced to orientate himself to reality. In the second strategy the fear is not that the scientist (or science) will not be oriented to reality; it is accepted that scientists will inevitably and always reify their subject-matters. Direct access to reality is not seen as a possibility and so the second strategy does not attempt to achieve this. The fear which motivates the second strategy is that the inevitably reified schemes employed by scientists (sometimes called weltanschauungen, paradigms, or even just frameworks) will not, in some sense, 'match up' to the whole system. There is some doubt in this strategy as to the terms in which the whole system is perceived. I shall argue in the next chapter that the effect, if not the intention, of this strategy is the same as would have been achieved if it had straight-forwardly been perceived in phenomenalist terms. This notwithstanding for the moment, the second strategy may be summed up as the attempt to design some kind of system so that the reified schemes (or scheme, for science as a whole) at least progresses towards an eventual encompassing of the whole system. (Churchman, particularly 1971, has considered several such attempts). The interdisciplinary team idea can be interpreted in this way as well but, conceptually at least, they are distinct strategies. The first strategy is positivism proper, and I shall use the strategy put forward by Popper as an examplar of this. The second strategy is the conventionalist alternative which arises as a reaction to the positivist strategy but which, we shall see in Chapter 3, is subject to the same weaknesses. As an examplar for this strategy I choose Kuhn's work. For the remainder of this chapter I consider the positivist strategy and the conventionalist criticisms which have been made of it. I shall then turn to a more detailed consideration of the conventionalist alternative.

Strategies of Inquiry:

It follows from the pragmatist view of truth - what is true is what is useful- that straightforward phenomenalism has to be abandoned. We cannot say straightforwardly that our perceptions are true because we do not straightforwardly know that they will be useful. Because the whole system is seen as being immensely complex it is beyond the wit of any individual (or in Popper's version, for the whole of science for all of time) to grasp it.[1] Perceptions of phenomena, therefore, are bound to be partial, and we cannot rely on them in any straightforward sense to be a part of the whole. Phenomena may mislead us in their partiality. It is this view which is responsible for OR's demand for freedom from external impositions - subject-matter, management, and even given facts - on its process of inquiry. This view is also responsible for Popper's view that "... there are all kinds of sources of our knowledge; but none has

(1) As Popper (1963) puts it, we live in a world where "... our knowledge can only be finite, while our ignorance must necessarily be infinite" (p. 29).

authority" (1963, p. 24). To take what perhaps seems the clearest and most authoritative source of knowledge, namely, observation, Popper concludes that "... the programme of tracing back all knowledge to its ultimate source in observation is logically impossible to carry through: it leads to an infinite regress" (p. 23). This is so because "every witness must always make ample use, in his report, of his knowledge of persons, places, things, linguistic usages, social conventions, and so on" (p. 22). In other words, ".. all observation involves interpretation in the light of our theoretical knowledge..." (p. 23), and it is logically impossible to unravel all these 'theories' and reduce them to observational reports. Habermas (1974), to whom we turn in a moment for some insightful criticisms of Popperian epistemology, sums up Popper's central thesis in the following way: "... the sources of knowledge - pure thought, established tradition and sense experience - all lack authority. None of them.. (or any others) ... can lay claim to immediate evidence and primary validity and consequently to the power of legitimation. The sources of knowledge are always contaminated; the way to their origins is barred to us" (p. 199).

There is, of course, a fundamental problem which this recognition entails. It is that scientists may rest content with their partial views; then even the possibility of a whole system view would be lost. Popper airs this view in a discussion of Kuhn's notion of "normal science", which I shall discuss in more detail in the following chapter but which can be taken here, simply, to mean the acceptance by scientists of a partial view as a basis for their work. Popper describes this acceptance as

> "... the activity of the non-revolutionary, or more precisely, the not-too-critical professional: of the science student who accepts the ruling dogma of the day; who does not wish to challenge it; and who

> accepts a new revolutionary theory only if almost everybody else is ready to accept it - if it becomes fashionable by a kind of bandwagon effect. To resist a new fashion needs perhaps as much courage as was needed to bring it about" (1970, p. 52).

The consequence of this attitude Popper sees as nothing less than "... a danger to science and, indeed, to our civilization" (p. 53). Commenting on his recognition "... that facts, which are popularly supposed to be neutral, turn out to be purposive, because of the way they are assimilated into a situation", Beer goes on to make an identical point:

> "This state of affairs is dangerous to science, if it is not clearly recognized, for it can block new discoveries. Progress in science might well be defined as the overthrow of a model, and its appurtenances, that has exhausted its usefulness. The great scientist is one who sees the need and the moment to destroy a model - and who can also create its successor" (op. cit. p. 122).

As the last part of this quotation from Beer shows, he sees as a necessary condition (though not a sufficient one as we shall see in a moment) for progress in science that prevailing 'though blocks' be overthrown: although "... science collects more and more observations about the world, and undertakes more and more experimentation, discovery often awaits a conceptual breakthrough, a bursting of the thought blocks of the scientist himself" (p. 121). So, too, with Popper. It is a necessary condition for progress in science that scientists "... discuss fundamentals

- that is, the very framework of our assumptions" (op. cit., p. 56). In his view "... if we try, we can break out of our framework at any time" (ibid).

The metaphor which has readily come to mind to both highlight the necessity for overthrowing prevailing frameworks and to indicate the nature of the process (I come to this in a moment) is that of adaption. This is in line with the "... whole evolutionist current of positivism, the reduction of knowledge to a biological instrument of adaption..." (Kolakowski, op. cit., p. 246). This follows from the positivist denial of the possibility of "... faith in experience or reason if they are conceived of as capable of revealing something to us of 'the world's qualities'" (p. 247). Knowledge as biological behaviour is the only possible answer left to the question of the nature of knowledge if all questions of its origin are ruled out of court as metaphysical.

Popper (1973) distinguishes three levels of adaption: "genetic adaption; adaptive behavioural learning; and scientific discovery..." The latter, he says "... is a special case of adaptive behavioural learning" (p. 73) which "... may be regarded as a means used by the human species to adapt itself to the environment" (ibid). Beer (1966, Chapter 2) distinguishes two forms of adaption by combining Popper's first two types (although following Peirce, he distinguishes sub-classes of the first type). Beer's problem (as is Popper's here and elsewhere) is to distinguish 'scientific reasoning' from other 'modes of thinking'. The brain, Beer says, is merely a rather good adaptive device. Brains are "... constructed as machines for reinforcing successful combinations of neural events, and reducing the probabilities that alternative combinations will be tried" (p. 18). Normal thinking is merely an adaptation (Beer often refers to it as 'conditioned') to external events and contingencies. However, this puts man in a precarious position because "... the device which works so well in adapting the animal to its environment is no blessing when an intellectual breakthrough is required" (p. 20). Then "... the brain is not always equal to the task" (p. 21). Everything is fine "... so long as the social, economic and industrial environments change slowly..." but the problem is that they don't. This means that "unless those responsible for policy-making abandon this method, and turn to other ways of exploiting their cerebral equipment, our society will not adapt sufficiently quickly, and we shall become economically extinct" (ibid). The message is clear: what is required is some form of super-adaptive process - enter OR as science to the rescue.

Hence the raison d'etre of OR: managers (unless they are trained as OR scientists - Ackoff and Rivett, (p. 96) - or unless they are 'top' management) cannot do OR if they have the time from normal duties (See: Beer, op. cit., p. 51) because their normal duties of adaption make them completely unsuited to undertake the super-adaptation of the scientist. However, in a 'complex' world mere 'managing' is clearly not enough, and it is in recognition of this fundamental that Beer (and many others) seeks to find for OR "... its meaning in its origin and early days" (p. 33). He refers, of course, to 'war-time OR', a period of activity to which many operations researchers turn to discover the 'essence' of OR, and Beer explains why it serves so well as an examplar for present day OR. It does so because it illustrates, par excellence, revolutionary science in action in response to the failure of traditional ways. Beer explains: the situation was clearly one of crisis; things were going drastically wrong, and all this in a situation where, as he puts it, "... a large number of sacred cows had laid down and died" (p. 34). In a war situation, he says, which is entirely analogous to that in an industrial undertaking, "... it turns out that nothing happens exactly as was expected" (p. 38). In this perfectly typical situation, he goes on, "the scientific description of the whole situation has to be rewritten, which is a job for operational research".(p. 41). This war-time situation suggests to him "... a remarkably characteristic feature of an operational research solution. It often", he says, "lies outside the framework of possibilities ever contemplated by the managerial solution. This means that the mixture of experience, knowledge and straight thinking by which the management's policy has been reached, has managed to delineate a range within which the answer is expected to lie, and the answer chosen is roughly in the middle of this range. The inexplicable failure of the management's policy, which is normally the signal for introducing operational research, often means that the best answer has unfortunately been excluded from the collection of plausible policies that management is prepared to belive offer solutions that really count". (pp. 44-45). It is on the basis of this view of the adaptive function of OR that Beer (as we have seen) attacks the stereotypes of science. It follows from 'complex world-simple man' view and it provides the "... reason why it takes an objective,, scientific, interdisciplinary investigation to arrive at a solution outside the phase space - a study undertaken by men who are by definition not prone to this particular thought block" (p. 61).

Many other leading operations researchers define knowledge, from an OR point of view, in terms of its adaptive function. Thus, Churchman, Ackoff and Arnoff (op. cit.) remark that "... one of the functions of OR with its mixed discipline teams is to increase an organization's memory - by bringin in a collection of knowledge different from that of the organization's routine - and to aid its consciousness (the executives) in developing and evaluation alternatives for action" (p. 85). An important role for OR is the enhancement of the "self-repair" capability of the organization which, although it is a 'high level' 'reflective goal-changing unit' which consciously adapts itself - in that it can engage in "conscious learning... (and)... can be selective and take, from a wide range of external information sources, that information relevant to the organization's survival or other major goals" (p. 83) - that adaptation may be 'faulty'. Thus they say that "the consciousness could be expected to show all the faults, in its operation, that we might find in humans or executive groups which run organizations: delusions, faulty direction, misinterpretation of messages, lack of awareness of new opportunities, poorly defined operating goals, and the rest" (pp. 84-5). With this conception of the capabilities of the organization without the input of OR it is not surprising that they earlier advocated the 'team approach' partly on the grounds that "those in control of a system may be unaware of one or more of... (the)... aspect... (of a problem)... and hence have an incomplete picture of their system" (p. 10).

Very few operations researchers write in such a direct way as Beer and Churchman, Ackoff and Arnoff, and yet many more advocate revolutionary science as the raison d'etre of OR and implicitly rely on the knowledge of adaptation. Relatively recently there have been published articles by two leading operations researchers (Wagner, 1971, and Rivett, 1974) which, in their strong criticism of the conception of OR as techniques and their refinement, reflect this fundamental belief. It is expressed in the view that OR is a revolutionary science dedicated to the formulation and solution of problems beyond the reach of normal management. Rivett in his article 'Perspective for Operational Research' commences with the claim that "... growth of... organizations... is one of the fundamentals of all organizations" (p. 225) and later adds "... the basic need for survival which, together with growth, underpins all organizational objectives" (p. 227).

He believes that OR has concentrated on the development and use of techniques and that this has prevented it from effectively helping to achieve these goals. More generally, he believes that it has hindered OR in the achievement of its historic promise. The promise is that "... when we look at a particular area of an organization's activity and try to create a model which represents that activity we are doing so not in order to descrive that activity in some logical connected way as is usual in science at large but rather in order to enable a better achievement of the organisation's objectives to be made" (pp. 226-7). To discover the objectives of an organization we must not, he says, be misled by individual pronouncements (ibid).

Wagner diagnoses a 'coming battle for survival' in OR, and the response which he considers appropriate is a strong shift towards a 'professional' rather than a 'technical' outlook. The background justification for this shift is the remark made back in 1953 by Levinson that "... the successful operations-research department can render an invaluable service by keeping top management informed of significant trends, either inside the business or in the business's relation to its environment, which may have an impact on executive decisions" (Wagner, 1971, p. 1261).

The implication flowing from all these views is that OR starts where normal processes of adaption fail. The truths which operational researchers seek are pragmatic, and there is nothing quite so pragmatic as survival. As a sympathetic reviewer of Popper's philosophy notes, "... Popper's theory of knowledge is coterminous with a theory of evolution. Problem-solving is that primal activity: and the primal problem is survival" (Magee, 1973, p. 57).

The pragmatic view of truth immediately directs formulations of theories of inquiry towards process models. Clearly, if truth is what is useful for adaptation we can never have truth itself because the environment (including our purposes) may change and negate what was once 'the' truth. Thus, "Popper's notion of 'the truth' is very like this: our concern in the pursuit of knowledge is to get closer and closer to the truth, and we may even know that we have made an advance, but we can never know if we have reached our goal" (Magee, op. cit., p. 28). A guarantee that progress is in fact being made is required by this view of truth, and such guarantees are sought by all. From the OR side it has been Churchman above all others who has persistently argued this point. The nature of the guarantees demarcates for us the positivist from the conventionalist strategies. Within the positivist frame of reference there are two major alternatives for the role of guarantor. Firstly, one may choose the phenomenal world as the guarantor. In his direct intercourse with the facts the scientist is prevented from persistently pursuing non-adaptive ideas. Alternatively, one may focus on the adaptive process itself and concentrate on the usefulness of the knowledge as a basis for its acceptance irrespective of the empirical compulsion to do so. Because of the nature of the process itself science is compelled to accept useful ideas and reject those which are not useful.

Facts as Guarantor:

The metaphor of adaption carries with it the implication that there exists 'selective pressure', an 'environment' which vetos non-adaptive mutations, and rewards useful ones. Popper's best known contribution to the philosophy of science is his statement of view that for science the environment can only be a sure guarantor in its vetoing function; we get very little guidance on our successes. This view follows partly from his belief in man as a free agent who exploits nature. Its main impetus however, comes from his conception of the enormity of the whole system and the intellectual impossibility of doing more than correcting mistakes (see, particularly, 1957). His argument for falsification and against induction (or verification) follows from this, and so his "... propos(al) to replace... the question of the sources of our knowledge by the entirely different question: 'How can we hope to detect and eliminate error?" (1963, p. 25).

Popper's limitation of the 'language' of adaption to one or two word utterances - 'no' or 'not disproved' - does not affect the positivistic nature of his scheme, even though he argues that he is not a positivist (by which he appears to mean inductivist). The fundamental claim made by Popper is that progress in science is guaranteed by the ability of the scientist to directly compare the predictions of theories with the 'facts'. Thus, he says, "the proper epistemological question is not one about sources; rather, we ask whether the assertion made is true - that is to say, whether it agrees with the facts... And we try to find this out, as well as we can, by examining or testing the assertion itself; either in a direct way, or by examining or testing its consequences" (op. cit., p. 27). A fundamental implication of this view is the further one that facts and theories are independent in the sense that theories can vary independently of the facts. This further view is indicated by Popper's insistence that the philosophy of science is solely concerned with the 'logic of science' implying that science as a whole talks with a metalanguage (cf. Beer, p. 208) within which disputes between theories can be resolved. The metalanguage which Popper has in mind is what has been dubbed the 'neutral observational language'. Kuhn has observed of Popper's scheme that his falsification rule "... require(s) that both the epistemological investigator and the research scientist be able to relate sentences derived from a theory not to other sentences but to actual observations and experiments" (p. 15). Later on, in replying to his critics, he clarifies his view of the Popperian scheme. "The point-by-point comparison of two successive theories demands", he says, "a language into which at least the empirical consequences of both can be translated without loss or change". This means, he goes on, that "... theories can be compared by recourse to a basic vocabulary consisting entirely of words which are attached to nature in ways that are unproblematic and, to the extent necessary, independent of theory" (p. 266, my emphasis).

Habermas has made an identical point. Habermas wages war on Positivism, and whilst noting that Popper has done much to rid us of the idea of 'immediate knowledge', he argues that"... for all his criticism, he shares in the last analysis a deep-seated positivistic prejudice. He assumes the epistomological independence of facts from the theories which should descriptively grasp these facts and the relations between them. Accordingly, tests examine theories against 'independent' facts". (1974, p. 200). As Kuhn (ibid) implies, the independence or neutrality of the required language for theory comparison is ideally based on a language "... consist(ing) of pure sense-datum terms plus syntactic connectives".

Beer, too, resorts to a neutral observational language to salvage his conception of science from the ineptitude of the subjective scientist. It is Beer's view that the subjectivity of the scientist notwithstanding, for some reason a scientific "... model is simply a reflection of whatever is the case which is explicitly made available for experimentation" (p. 101). The reason is that "... science itself is in the long run protected against the dangers of its own intellectual apparatus. In the first place", he says, "it is founded on observation and experiment. Eventually the model that is no longer useful (we say 'that is seen to be false'), and the model that can no longer encompass the scope of the scientist's insight (we say 'that is seen to be trivial') are overwhelmed. For if objectivity and neutrality in science are nowadays regarded with more suspicion than our fathers showed in them, something of the objective and neutral breaks through into our work from outside ourselves" (pp. 122-3). In short, science has "... an objective language of fact and... models of reality..." (p. 123), and this is why Beer can so confidently assert that "... the laws that govern systems and indeed the laws that govern nature itself - and therefore scientific enquiry" (p. 120) even though "... the so-called laws of nature are contingent... on the languages we use to express them..." (p. 121). Churchman (1970b) has concluded that the view that the facts act as a guarantor is widespread in OR texts. He recounts his experience: "In almost every text I have examined, the discussion of data collection seems very strongly to imply that the data items are to be obtained by observation, i.e., that... (the data gathering step in 'doing OR')...is based on the classical empiricist notion that the true events of external nature are to be learned through our senses" (p. B - 41).

Before moving on to a brief consideration of conventionalist criticisms of this strategy and the alternative which it proposes (which are considered in more detail later) it is worth mentioning another feature of Popper's scheme which will be considered in an OR guise in Part Two, namely, the role of tradition. Although he sees science as "... consist(ing) of bold conjectures controlled by criticism and that it may, therefore, be described as revolutionary" (1970, p. 55), he has also "... always stressed the need for some dogmatism" (ibid). Because of the enormity of the whole system and the infinitude of possible speculations there has, for efficiency's sake, to be a degree of restraint imposed on scientist other than that provided by the facts. For Popper this restraint is provided by having scientists oriented to tradition as the repository of the results (knowledge and problems) of past adaptions. For the organism of science to adapt efficiently it must have the direction provided by its 'basic nature'. This basic nature is, as far as Popper is concerned, the accumulated heritage from the past. As Popper sees it, science works most efficiently when it presupposes the basic correctness of previous non-refuted theories. Subsequent science modifies but does not subvert. As Popper puts it, "... a scientific revolution, however radical, cannot really break with tradition, since it must presume the success of its predecessors" (1973, p. 93). "This is why", he says, "scientific revolutions are rational" (ibid). They are rational because they refine the rationality embodied in tradition. For Popper, then, tradition plays an important role in rationally curbing the otherwise boundless freedom which he, like many operations researchers, claims for science. In Part Two we shall examine very closely a similar device employed by some operations researchers (for similar ends) as it arises in two forms. First, I will consider some attempts to give OR a rational basis which rest their case on a 'theory of management'. Second, I shall consider the rather more implicit attempts to achieve the same result which rely on a theory of the social world.

Conventionalist Criticisms:

Rivett (op. cit.) remarks on "... one of the difficulties which we face in carrying out Operational Research". It is a difficulty which, he says, "... probably exists in all sciences, but in none... does it exist in such a severe or constraining form as in our own". The difficulty is that "there is, of course, no such thing as an Operational Research problem. There are not even such things as problems, in a concrete form ... The problems only exist in our own minds. Hence there is a purely subjective basis to our subject..." (p. 226). Conventionalists take such views very seriously and make them the cornerstone of their preferred strategy of inquiry. Conventionalists take for granted the nominalism of theories and their reification and ask, given these inevitable features of science, how things can be arranged such that the selection of theories is justifiable. As Kolakowski puts it:

> "The fundamental idea of conventionalism may be stated as follows: certain scientific propositions, erroneously taken for descriptions of the world based on the recording and generalization of experiments, are in fact artificial creations, and we regard them as true not because we are compelled to for empirical reasons, but because they are convenient, useful, or even because they have aesthetic appeal. Conventionalists agree with empiricists on the origin of knowledge, but reject empiricism as a norm that allows us to justify all accepted judgements by appealing to experience, conceived of as a sufficient criterion of their truth" (op. cit., p. 158).

For the 'naive' positivist theory choice is determined by the facts alone. For the conventionalist the facts are claimed to be only a necessary condition for theory choice; other criteria are, they assert, more important, and it thus falls to the conventionalist to demonstrate the rationality of these criteria and their application. The conventionalist critique starts with an analysis of the positivist conception of 'facts'. Conventionalists attempt to destroy the concept of fact as a basis for theory choice and in so doing remove the opacity which they see surrounding the concept as it appears in positivist's hands. I shall be concerned in the later chapters of this first part with the question of whether this attempt is successful, and also with the further question of the consequences of (as it turns out) its failure on the ideals of OR.

As we have seen, positivists assert the independence of facts from theories. That is to say, they claim that facts and theories constitute qualitatively distinct domains. It is possible, they claim, to move between these two domains by meas of the metalanguage of a neutral observational language, but the fundamental distinction between them is unaltered by this possibility. Conventionalists argue, on the contrary, that for the purposes of theory choice facts and theories constitute the same domain; they are qualitatively identical. Thus they claim that a neutral observational language is an impossibility. As Kolakowski puts it:

> "The conventionalists deny that there is any such thing as 'pure experience', i.e., facts that do not involve theoretical presuppositions, but are recorded directly 'from nature', so to speak" (p. 160).

Acceptance of the interrelationship of fact and theory (sometimes posited on psychological grounds, sometimes on the basis of the properties of language, and sometimes on logical grounds) implies the impossibility of either proving or disproving theories by means of the 'facts' because they, in various ways, presuppose theories. Thus, Churchman's criticism of OR texts which imply that data are what are apprehended directly by the senses is based on their neglect of the "... strong philosophical tradition which says that all data are a combination of theory and sensation" (1970b, p. B - 42). Conventionalism is also the basis on which Churchman criticizes views, such as are expresses by Beer, that "laws" "... 'are conceptual devices by which we organize our empirical knowledge and predict the future'" (Beer, op. cit., p. 171, my emphasis). Churchman criticizes the positivism betrayed in such views because they rest on the assumption that facts are independently given to be organized (cf. Churchman, 1971, p. 60). As far as Churchman is concerned the "... "facts"... are based on assumptions about how the whole system should work or inevitable does work" (1970b, p. B - 42). Thus, "the inconsistency of an hypothesis with an observation does not necessarily mean that the hypothesis is to be rejected: the measurements themselves may sensibly be rejected..." (Churchman, 1961a, p. 78). This is so because "... all description methodologically entails prediction... (and)... in general all measurement takes place within the context of a theory" (op. cit., p. 85: see also Ackoff, 1962, pp. 22-24 who follows Churchman).

For OR the conventionalist criticism is a powerful one with profound implications. The criticism amounts, in Kolakowski's view, to the argument that "...the data of experience always leave scope for more than one explanatory hypothesis, and which one is to be chosen cannot be determined by experience" (op. cit., p. 159). Or, as Keat and Urry (op. cit.) put it, "there is the view that the truth or falisty of theories is 'under-determined' by empirical data" (p. 61). Positivism claimed to provide a way to grasp the whole system by reducing it, through its phenomenalist orientation, to, as Kolakowski puts it, a "hypothetical unity" (p. 41) which was to be found expressed in the direct experience of phenomena. Conventionalism's rebuttal of the purity of experience is also, then, a rebuttal of the claim that we can grasp the whole system, so to speak, in 'parts'. In fact, conventionalists argue, we never understand things as isolated parts. Our very designation of the facts presupposes a larger view within which such facts are produced, as we have seen Churchman (1970b) argue, and it is a point which crops up regularly in his work (see for example: 1968a; 1971). If facts presuppose a view of the whole system then the positivist position, far from providing an adequate theory of inquiry for OR with which it can live up to its ideal of grasping the whole system, has merely pushed the most important question into the background. The question that arises from the conventionalist critique is: how can we justify the assumptions which we inevitably make about the whole system? For the conventionalist, the refusal of positivists to face this question makes their strategy of inquiry opaque. Habermas (1974) who although in many important ways is not a conventionalist, sums up the conventionalist concern very clearly. Commenting on Popper's scheme he concludes that in it "... whole problem-areas would have to be excluded from discussion and relinquished to irrational attitudes, although, in... (his) ... opinion they are perfectly open to critical elucidation" (p. 196). In his view in the Popperian scheme "... the meaning of the empirical validity of factual statements... is determined in advance by the definition of the conditions of refutation" (p. 201) and, therefore, "... positivism... (is)... a fetish which merely grants to the mediated the illusion of immediacy" (ibid).

At one point in his work Churchman phrases the concern in the form of the question: "What assures us that in our attempts to improve social systems we have considered all that it is possible to consider?" (1968, p. 171). The failure by the positivist to answer this question because of the undertermined nature of facts rules positivism out as a serious contender for the development of OR's theory of inquiry because, fundamentally, operating within the positivist frame of reference inquiry is out of control. The facts, in this frame of reference, represent a denial of OR's ideal of free inquiry. They represent an arbitrary limitation on inquiry because they have to be taken as 'given', that is to say, they have to be taken as preconstituted entities the inquiry into which is forbidden.

A popular conventionalist reaction to this imposition from positivism is to argue for an idealization of the fact. That is, instead of allowing the assumptions behind the facts to lie dormant and implicitly constitute the facts, an attempt is made to bring the assumptions to consciousness (cf. Ackoff, 1962, Chapter 5) and in so doing explicitly determine the facts. Facts are thus determined by ideas and hence are idealisations. As Churchman says of the idealist who dominated his work:

> "For him the question "What is real?" means "What is realizable?" Since the realizable is an idea, he believes that ideas are primary; what we observe is a result of what we think about. Hence he interprets the first challenge to be a challenge of thought: How can we be sure that we have thought about everything that is relevant?" (1968a, p. 172).

As facts are determined by idealized conceptions, the only way to determine the whole system is to conceive it through an idealization of the whole system. Churchman recognizes that this is an impossible demand which is as open-ended as the pragmatic philosophy we encountered earlier. In fact, pragmatism is often conjoined with idealism to produce, for example, the notion of idealized operational definitions. Under this notion we should define a variable (for example) such that the operations whereby it is to be measured are specified in the definition in ideal terms. That is, the conditions of measurement are specified in terms of the "best conceivable". Actual measurements are then compared to these "standard conditions" and attempts are made to adjust the actual data to the data which would have been produced were the conditons actually ideal. This notion is advanced by both Churchman and Ackoff. However, using "... the ideal conditions and procedures... (to)... act as a standard by means of which we can compare observations made under different conditions using different operations..." (ackoff, 1962, p. 162) entails that we measure the standards themselves. So we need another set of idealized definitions to enable us to measure the standards. Similarly, we need to measure the standards at this higher level... and so on. We shall encounter this problem in a particularly vicious form in the final chapter when considering the work of Ackoff and Emery (1972). The point to be made here, however, is that there is a clear need to provide some justification for the idealizations in a way that removes this open-endedness. The conventionalist has to convince us that his conventions (idealizations) are objective.

The way forward for the conventionalist, having destroyed the positivist notion of fact, is to argue that it is the process whereby the conventions are chosen which determines the objectivity of science. If it is theories which determine the facts and conventions which determine the selection of theories, then it is vitally important to have the theories selected by the 'best' conventions. Thus, Churchman argues that the real meaning of the claim that facts have a theoretical content is that "... all propositions of science are - methodologically interpreted - theories" (1961a, p. 81). By this Churchman does not mean

"... that 'facts' are 'theories' in any semantical or syntactical sense" (ibid). What he is interested in from the claim is the "... consider(ation) ... (of)... propositions in terms of the decision-processes by which they are accepted or rejected" (op. cit., p. 80). It is this process which determines their "methodological meaning" which is of crucial importance because for Churchman it "... seems impossible to discuss the scientific verification of recommendations without at the same time discussing the problem of how science itself should make decisions" (p. 78).

Churchman's favourite solution to the obviously threatening open-endedness of this proposal is to attempt to design a system of inquiry which has objectivity 'built in', that is, guaranteed by its very structure. After demonstrating that "... the validation of simple sensations can only be designed within the context of a community of inquiring systems; in other words, "X is yellow" can be validated by an inquiring system only if there are other inquiring systems" (1971, p. 103), he concludes that the major problem is that the inquirer turns out to be "conventional". That is, it is "... capable of receiving any "information" and handling it in exactly the same way as it does imputs from any other source" (op. cit., p. 123). It has, in short, a "reality" problem the only solution to which "... lies in the design of a community of inquirers" (ibid). I shall consider Churchman's attempt to provide an adequate community design to make conventionalism objective, by its interpretation within Hegelian philosophy, in Chapter 4.

Before that, however, in Chapter 3, I shall give extended consideration to the work of Thomas Kuhn (particularly, 1962) probably the most widely known contemporary conventionalist. His work provides a blueprint for a possible conventionalist strategy for the development of a theory of inquiry for OR. Kuhn, as a committed conventionalist, is dedicated to the task of putting some muscle behind the basic conventionalist view that the facts do not provide an adequate basis for theory choice. He wants to show that scientific activity can be seen to be objective without recourse to the positivist's opaque conception of facts. As he says, although many philosophers of science wish to "... compare theories as representations of nature, as statements about 'what is really out there'", Kuhn believes that such approximations to the truth are not to be found. "On the other hand", he goes on, "I no longer feel that anything is lost, least of all the ability to explain scientific progress, by taking this position" (1970, p. 265). Kuhn attempts to explain the progress of science - its rationality - virtually solely in terms of its social structure (cf. 1970, p. 21), that is, in terms of the values, norms, ideology and social organization of science.

As conventionalism is an obvious contender for the attention of operations researchers (and more interest is currently being shown in it, as we shall see later) it is worth closer consideration. The major question I shall be seeking to answer is whether it is as practically comprehensible as its advocates claim. That is, does it too, even in the hands of its most sophisticated exponents, ask us to unquestioningly accept the given facticity of the object of inquiry to provide the final quarantor of objectivity? Does conventionalism let positivism slip in through the backdoor? It is to these difficult questions that I turn in the remaining chapters of this part.

<u>Conclusion: The Realist Option:</u>

In this chapter I have considered two major strategies of inquiry - positivism and conventionalism - which have some support in the OR literature. So far I have subjected the positivist alternative to the criticism made of it by conventionalists, but we have still to see whether similar criticisms from a non-conventionalist viewpoint can be made of conventionalism itself. To show how this might be the case it will be useful at this stage to briefly outline the criticisms of positivism which are very similar to those made from the conventionalist viewpoint but which stem from what could loosely be called the realist viewpoint. Realism is as yet a relatively undeveloped philosophical viewpoint which has arisen as a reaction against both positivism and conventionalism. Later on I shall pick up some of the essential threads which I outline here under the title of realism in terms of other philosophical positions with which realism is closely associated - particularly the structuralist viewpoint. Here I shall draw generously on the account of realism given by Keat and Urry (op. cit., particularly Part One), which is itself a synthesis of much of the available literature.

Realists are preoccupied with the problem of adequate causal explanation, and it is on the grounds that both the positivist and conventionalists cannot provide adequate causal explanations that they are attacked by realists. Because of their phenomenalist and nominalist commitments, positivists do not accept that theories add any truth value to the phenomena which they summarize and organize. Thus, they attempt to devleop 'correspondence rules' and neutral observational languages to translate theoretical terms into their phenomenal equivalents. Positivists, therefore, cannot (and do not) use theoretical terms as a constituent part of causal explanations. Theoretical terms enter into causal explanations for the positivist only as general laws which summarize, in a universal form, regular connections (so far as not yet disproved from a Popperian viewpoint) between discrete phenomenal 'events'. For the positivist a causal explanation has been established when a regular connection between events has been verified or not refuted. We shall see evidence of this view of explanation being entertained in OR circles (by Ackoff and Emery, 1972 at least) in the final chapter.

The realist finds the positivist account of causal explanations inadequate because of the positivist's insistence that all that is real (for science) is phenomena. If this is so, the realist argues, then as we often find regular relations between events which are accepted not to be causal (for example, the regular relationship between barometer readings and changes in weather conditons: see Keat and Urry, p. 12), there is a gap in the positivist's account. The positivist does not tell us, though he ought to, how we are to distinguish regular relationships which are indicative of truly causal relationships and those which are not. Because the positivist is prevented from looking beyond the phenomena he cannot distinguish causal from non-causal laws. All regular connections between events have (strictly) to be treated by him as causal. The realist argues that we should distinguish between explanations that are causal and those that are not on the basis not merely that a regular connection has been found between events, but additionally and crucially, on the basis that we can provide a description (and to the extent possible, a demonstration) of underlying mechanisms and processes which are responsible for the regularity. Keat and Urry put it this way:

> "For the realist, adequate causal explanations require the discovery both of regular relations between phenomena, and some kind of mechanism that links them. So, in explaining any particular phenomenon, we must not only make reference to those events which initiate the process of change: we

> must also give a description of that process itself. To do this, we need knowledge of the underlying mechanisms and structures that are present, and of the manner in which they generate or produce the phenomenon we are trying to explain" (p. 30).

For the positivist there are no underlying realities. Insofar as he countenances the use of theories and models they are treated as heuristic devices to summarize or suggest regular relations. For the realist theories and models are attempts to "... describe structures and mechanisms which are often unavailable to observation..." (Keat and Urry, op. cit., p. 34). Thus, like the conventionalist, the realist argues that theoretical terms are in some sense real.. However, the reality of theoretical terms for the realist lies in their attempted description of real processes; the reality of theoretical terms for the conventionalist lies in their belief that theoretical terms are themselves the basic reality for science. For the conventionalist definitions determine reality. For example, the conventionalist view of "... the proposition, 'Phosphorus melts at the temperature 44 degrees centigrade' - a famous example - is... (taken)... not... (as)... the account of any observation, but a definition or partial definition of phosphorus". (kolakowski, op. cit., p. 163). A similar view of definitions is evident in the work of Ackoff and Churchman, and we shall see it in operation particularly in the final chapter as regards Ackoff in particular. Kolakowski comments on the conventionalist view that the proposition does not "... stand here for ' a body that melts at 44 degrees centigrade', but a body with certain known physical and chemical properties" (p. 167). He goes on: "The proposition in question establishes the coincidence of these properties with the melting point, which cannot be decreed by any definition. Therefore this proposition is not a definition, for we assert it only when we know that the object it refers to is really present" (ibid). In this comment of Kolakowski's we can detect the seeds of a realist-type criticism of conventionalism. I shall attempt to germinate it in the following three chapters.

However, even at this point we can see that the realist, unlike both the positivists and the conventionalists, argues that theories deliberately posit the existence of entities which although possibly non-observable are real nonetheless. The fundamental weakness of positivism for the realist is that it forbids this (an illustration of the consequences of this will be given in the final chapter), and so cannot possibly claim to provide a way of grasping the whole system. Positivists, in their claim to have provided causal explanations, must do so, from a realist point of view, by presupposing the properties of the very underlying reality which the realist would like to discover. The conventionalist, because of his refusal to consider the reality underlying adequate theories, must do likewise. We shall see that he typically does so in a way which makes him indistinguishable from the positivist. In their concern to banish the metaphysical from science, both positivism and conventionalism have succeeded, we shall see, in banishing the real world by banishing any consideration of the real nature of the subject-matter of inquiry. Positivists do this by limiting facts to phenomena, conventionalists by claiming all reality there is for theories. The conclusion to this first part will therefore be that the association of OR with these philosophies is destructive of its ideals. In support of this conclusion I shall argue that the adoption of a realist-type alternative would help OR achieve its ideals.

CHAPTER THREE

THE CONVENTIONALIST OPTION FOR OPERATIONAL RESEARCH

SECTION A

OPERATIONAL RESEARCH AND KUHNIAN SCIENCE

Introduction:

Chapter Two has laid to rest any hope that OR could turn to positivism as a source for the construction of a theory of inquiry which would be consistent with its ideals. It is, indeed, surprising that positivism should be entertained in OR circles at all. However, the realist-type viewpoint arises mainly as a reaction against the destructive implications of the most sophisticated positivist of them all - David Hume - who took positivism to the only conclusion that was logically consistent with its premises, namely, that rational knowledge was an impossibility. Starting from an extreme phenomenalism and nominalism, Hume argued that the 'necessity' involved in any causal explanation was purely a psychological phenomenon. We feel that events are necessarily connected but this feeling is based only on the habits produced by past experience. The more frequently events occur in regular relationship the stronger is our psychological feeling of the necessity of the relationship, but the logidal grounds for doing so do not increase. As Kolakowski puts it:

> "Consequently what can really be asserted beyond all doubt is limited to individual accounts of immediate observations; assumptions concerning the nature of the world 'given' in those observations, whether touch-its reality or the nature of the observing subject, are excluded. It is easy to see that, in this conception of knowledge, that which we truly know is utterly barren and unproductive, whereas that which helps us to live, create a science, and enrich our store of information generally is no longer

> knowledge in the proper sense of the term.
> In the last analysis, according to Hume,
> there is no such thing as rational knowledge about the world" (op. cit., pp. 51-2).

It is this underlying logic which accounts for the prevalent attitude in OR circles that the knowledge of the various sciences is not really knowledge in the full sense but, rather, is "... a collection of guidelines, useful and indispensible in practice, but devoid of truth value" (ibid). We have seen this view expressed particularly strongly by both Beer and Ackoff, and it is reflected in the training of operations researchers.

The same tendency to undermine the ideals of OR is to be found in the work of Popper, probably the most sophisticated contemporary positivist. Popper (particularly, 1957) openly declares war on those who would attempt to gain knowledge of totalities. He confines valid knowledge to the small scale "piecemeal" fragments of experience which can be falsified. In view of this it is perhaps surprising that he has received even those attentions that he has (e.g. Boothroyd, 1974; Bevan, 1976; Eilon, 1975) from operations researchers. Nevertheless, in this chapter I shall use the Popperian scheme as a representative of the positivist option for OR. It has at least face-value relevance particularly on the grounds that Popper insists that science should continually be revolutionary in its outlook and practice. This feature, at least, coincides with the ideals of OR that we found in Chapter One.

As I suggested in Chapter Two, a major alternative, and apparently, competing, option for OR is the adoption of some form of the conventionalist viewpoint, and we have found some support for this viewpoint in OR circles. As I have already suggested, a convenient choice for an examplar of this option is the work of Kuhn. The conventionalist option does not, at least at first sight, have the fatal defect of obviously undermining the ideals of OR, and we will find support for Kuhn's version of it in the writings of some operations researchers. The major task of this chapter is to discover whether the conventionalist option in Kuhn's hands can provide an adequate basis for the development of a theory of inquiry for OR. In the following chapter I shall ask the same question of the conventionalist option as it is treated by Churchman.

Kuhn's option for OR:

Of all the reasons why Kuhn's work might be adopted to provide an exemplar scheme of the practice of science on which OR's strategy of inquiry could be built, the following are perhaps the most compelling:

1. Kuhn appears to offer a radical alternative to the Popperian scheme, which, in the light of the latter's failure provides at least the basis for interest in his scheme.

2. This interest is rewarded for, as we shall see, Kuhn appears at one and the same time to remedy what is deficient in Popper without losing Popper's most valuable feature, namely, his revolutionary outlook.

3. Kuhn achieves this favourable resolution by offering (implicitly, for OR) an intellectual division of labour which both has a good deal of support in the OR literature and avoids the worrying aspect of Popper's scheme which argued for the necessity of continually overthrowing total theoretical schemes in order that progress can be said to have been made.

In the barest outline, these reasons make Kuhn a most attractive option for OR. In our exposition of these bases of attraction, let us start with a seemingly very practical point: Popper's scheme is downright impractical whereas in Kuhn's work there is a strong breath of realism injected into thinking about the practice of science and its progress. At last scientists feel that they can recognise themselves in Kuhn's account, whereas many previous attempts to theorise about the nature of science have left them cold.

A major source of the realism which one suspects scientists find in Kuhn's account is to be found in the very structure which he lays down for scientific activity. Unlike Popper, who stresses the necessity (and claims the actuality) of continual and fundamental revolution in science Kuhn distinguishes between two major types of activity in the progress of science only one of which matches Popper's all-embracing notion of revolutionary science. According to Popper science "... progresses by bold ideas (such as the theory that the earth is not flat, or that 'metrical space' is not flat), and by the overthrow of the old ones" (1945, Vol. 2, p. 12). The structure which Kuhn, on the other hand, employs to describe the progress of science (we shall see later that Kuhn takes for granted that science progresses, so we can use 'activity' and 'progress' synonymously) has this revolutionary (or 'extraordinary') research as a major component, but Kuhn firmly distinguishes this from what he claims is the dominant 'normal' activity of science. The structure of scientific progress which Kuhn lays down is one with fairly long periods of 'normal' research interspersed with relatively infrequent periods of revolutionary research (note that all that is necessary for Kuhn's view is that these different types of research <u>alternate</u>; it is merely his empirical judgement that 'normal' research dominates as a proportion of time).

Thus, whereas Popper argues that for true scientificity "... a mistake infects an entire system and can only be corrected by replacing the system as a whole" (Kuhn, 1970, p. 12) Kuhn argues that typically it is not whole theories which are tested but the ingenuity of the scientist in solving tricky and interesting 'puzzles' which are posed within a pre-existing theory. That is, it is, according to Kuhn, the scientist and not the theory which is falsified (!) by a 'mistake'. It is the scientist's attempt to solve the puzzles posed by the currently accepted theory - or, as Kuhn prefers, for reasons we go into later, to call the 'paradigm' - which Kuhn argues preoccupies most scientists for most of the time, a conclusion which he believes is entailed in Popper's description of science as well as his own. For Kuhn it is a logical truism that revolutions in science require normal (non-revolutionary) science as their counter-part: it is not possible to make 'mistakes' or to 'test' theories by attempting to 'falsify' them outside the context of a pre-given framework (see: 1970, pp. 1-23), and once this much is accepted, normal science follows as a corrollary.

Apart from the fact that Popper's criterion of falsifiability would reduce science to impotence,(1) Kuhn's major point of contention against the Popperian view of 'progress' is that without normal science (that is,

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(1) Because it "... requires that we first produce the class of all logical consequences from the theory and then choose from among these, with the aid of background knowledge, the classes of all true and of false consequences. None of these tasks can", he adds, "be accomplished unless the theory is fully articulated logically and unless the terms through which it attaches to nature are sufficiently defined to determine their applicability in each possible case" (Kuhn, 1970, p. 16). Needless to say, "... no scientific theory satisfies these rigorous demands..." (ibid).

research which is not progressive in Popper's terms) no progress, revolutionary or otherwise is possible. We come to this shortly. Here it is of interest to note that Kuhn's conclusions regarding one of the unrealistic implications of Popper's criterion of scientificity has been noted by at least one operations researcher.

Eilon (1975) in his editorial descussion on the question of "How Scientific is OR?" draws upon the Popperian option as an exemplar of the scientific approach, and on the basis of the parallel which he draws between the major features of Popperian science and what he takes to be the practice of OR, he concludes that when set within the Popperian "... context... OR may be regarded as a scientific activity" (p. 7). However, after having completed this 'matching' exercise some doubt about the practical implications of Popper's scheme sets in. Eilon goes on to observe that there "... is... (an)... aspect in which "experimentation" in OR fails to attain the rigour demanded by the hypothetico-deductive method... (of Popper)" (p. 8) because he "... recall(s) that the purpose of scientific experimentation... (in the Popperian view)... is to subject a given hypothesis to the most crucial and demanding test that can be devised, since the object of the exercise is to refute the theory". "In a managerial context", Eilon concludes, echoing Kuhn's comments on the 'management' of science, this "is not tenable, since when socio-economic systems are placed in extreme critical conditions they may fail altogether; and while such experiments may be entirely satisfactory in refuting given hypotheses, they could be catastrophic from a managerial viewpoint" (ibid). Again recognising the lack of realism in Popper's account from a Kuhnian point of view, Eilon goes on to observe that "in the majority of cases... implementation does not constitute a crucial test of accepted beliefs, but a relatively minor experimentation exercise" (ibid; cf. Popper, 1945, Vol. 2, p. 12). In other words, the practising operations researcher in his day to day work in modifying and improving organizations, typically takes 'accepted beliefs' for granted and contents himself with puzzling out improvements within this framework. What Eilon has in mind, I believe, is something closely akin to Kuhn's normal science.

However, there is a potential contradiction here. As Eilon notes, "the hallmark of the hypothetico-deductive approach is that of conjectures and refutations... (p. 5) and further that "the elements... (found in the Popperian model)... of formulating a hypothesis, of prediction and of experimentation are clearly discernible in the OR process; the feedback mechanism that requires the definition of the problem, the assumptions, the model structure and the conclusions to be continuously reviewed and adapted in the light of new information and new experience is analogous to the continuous evaluation that takes place in scientific... (i.e. Popperian)... inquiry" (p. 7, my emphasis). In the Kuhnian scheme there is no such continuous evaluation; 'normally' scientists work within 'accepted beliefs'. How then can we have our cake and eat it as Eilon appears to attempt? How, that is, can we have both revolutionary ('bold conjectures') and non-revolutionary science? Eilon detects but does not make the contradiction between these two modes of activity explicit, which accounts, I believe for his attempted compromise through the advocacy of 'satisficing' (see: 1972). Kuhn handles this contradiction very easily through his advocacy of role specialisation: some are concerned with revolutionary activity; most are concerned with puzzle-solving. However, before concretely making the connection between Kuhn and OR on this basis it is worth showing how it is that Popper fails to see the impracticality of his proposals and why, therefore, Eilon is not unduly disturbed by the contradiction which his comments point towards.

Eilon could 'match' OR and Popperian science because at the level of ideals, and apart from the correspondences over such things as rigorous specification of predictions and testing through the comparison of predictions with 'experience', both OR and Popper share the commitment to radical rethinking as an essential attribute of scientificity. However, as we have seen, OR is also committed to the idea of radically rethinking the whole whereas Popper is well-known for his opposition to holism. Therefore, both Popper's and Eilon's advocacy of falisification only become comprehensible (but not, of course, thereby justified) when we realise that it is meant to apply to 'piecemeal social engineering'.

Thus, although 'whole' theories are in Popper's scheme continually exposed to the risk of revolutionary overthrow, the theories themselves are thought to be rather small-scale. By way of added explanation it should be noted that Popper acknowledges his debt to micro-econimics (via, particularly Ludwig von Hayek) for major features of his scheme. Popper's model can be seen, then, as an elaboration of 'intellectual perfect competition'. In the micro-economic model, of course, with the 'market' being composed of units so small that they are 'insignificant' with respect to the total market so that the failure of one unit (its economic 'falsification', so to speak) has no catastrophic consequences. In the classical economic model the individual units are related only through the information provided by the impersonal price system: in other words their level of connectivity is low. When the level of connectivity increases (as it has, of course, in our 20th Century economy) the failure of one unit does have catastrophic, that is, widespread, consequences, as recent history has amply demonstrated. Looked at in this way the lack of realism of Popper's scheme is its location in 19th Century thinking. Eilon achieves the same degree of low connectivity through a slightly different route: he recognises complexity but denies its importance through his belief that performing at a 'satisfactory' level is the most that can reasonably be expected. As Cyert and March (1963) have shown, the satisficing criterion uncouples connections in a totality and with this low level of connectivity one can understand very well how someone could simultaneously advocate falsification and piecemeal change.

Kuhn's scheme should be attractive to OR as an exemplar because it effects a synthesis between the desirability of revolutionary science and the necessity of normal science through a division of responsibility. This view is remarkable for its correspondence both with a popular view in OR (and we shall see later in modern organization theory). Kuhn's viewpoint is realistic because it avoids extremes: firstly, if we look at it as a managerial philosophy, it recognises that revolution is not all there is to scientific progress. As Kuhn himself remarks, "by their nature revolutions cannot be the whole of science: something must necessarily go on in-between... (and)... since the science which I call normal is precisely research within a framework, it can only be the opposite side of a coin the face of which is revolutions" (1970, p. 242). Secondly, by this recognition, the Kuhnian scheme creates a clear role for specialists in revolution, that is, specialists in the creation of frameworks. Thus, in Kuhn's scheme we have roles for both OR and top management, and middle management, and to make the connection between the views of Kuhn and those expressed in OR circles I need do no more than recall the views of Beer set out in Chapter 2: OR, responding to a crisis situation (which we shall see is an integral part of Kuhn's structure of scientific progress), comes in and recasts the framework within which it is management's task to manage. To reinforce the connection it is perhaps worth stressing that in common with OR, as seen by Beer and others, Kuhn is centrally preoccupied with the revolutionary side of science. As he says in response to critics who accuse him of 'down-valuing' scientific revolutions: "Discovering the puzzling nature of revolutions was what first drew me to history and the philosophy of science in the first place" (op. cit., 241-2). Also, in case there is any doubt about it, it is also worth pointing out that Kühn (in common with both Popper and OR) sees his work as having definite normative implications: this can be seen from his commitment to a better understanding of the notion of rationality (cf. 1970, p. 264) which he equates with science and his view that the normative and descriptive are inextricably linked. Thus his belief that his "... theory has consequences for the way in which scientists should behave if their enterprise is to succeed" (1962, p. 207).

<u>OR and Normal Science:</u>

Let us look first at the role envisaged for the normal scientist - the equivalent role in OR thinking being played by the 'average' manager. We have seen in Chapter 2 that Beer distinguished the different mode of thinking employed by the OR scientist and the average manager. The manager was prone to all types of 'thought blocks', the major one coming from the limited perspective generated by his formal organizational role. The formal organizational role was, in turn, a derivative from the 'organizational phase-space': it constituted, in Beer's rendering, the particular manager's 'phase-space'. This distinction between organizational and particular phase-space provides the basis for Beer to distinguish two different types of roles: one set of persons is concerned with the organizational phase-space (the OR scientists and top management) and another set is concerned with particular phase-spaces ('average' managers).(1) As Beer himself puts it: "It is not a question of the scientist being cleverer than the manager, nor of the manager being obstinate and old-fashioned. The two men have quite different roles to play in quite different jobs" (1966, p. 84). It is on the basis of this strict demarcation of outlooks and responsibilities that Beer argues that OR is something that a manager could not do even if he had the time: these two roles are seen as being <u>intrinsically</u> different. And wherein does this intrinsic difference lie?

The key to the distinction between these two roles lies in the different types of commitment which they necessitate, a difference which, as spelled out by Beer, is mirrored by Kuhn's distinction between normal and revolutionary scientists. Beer, continuing the quotation above, goes on to say that "It is not the manager's job to change his own conceptual framework, but to get on with the responsibilities which he exercises within it". A phase-space is a conceptual aid to problem solving (p. 58) and thus the good manager is someone who accepts his role and solves problems within it. Also, clearly, the reverse of Beer's strictures to managers applies: it is not the job of scientists to solve these small-scale and detailed problems. The scientist provides the possibility and the manager realises the actuality. This is Kuhnian science. As Kuhn says of paradigms: being "... very limited in both scope and precision... at the time of its first appearance..." a paradigm in this crude state is "... an object for

(1) I shall deal with the implications of Beer's views for a'theory of management' in Part Two.

further articulation and specification under new or more stringent conditions" (1962, p. 23).

For Kuhn,as for Beer, "normal science consists in the actualization of that promise, an actualization achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself" (op. cit., p. 24). In other words (Beer's in fact), the OR scientist is concerned with form and the manager with content.

OR is not concerned with normal science: "The cybernetician seeking to account for the behaviour of a company and its controls does well to redefine the boundaries of world situations, and ought not to accept uncritically the conventional boundaries he finds... Even when the boundaries are drawn it will be necessary to remember that they are quite arbitrary... For all anyone knows, nature may have a different idea of where these boundaries lie" (Beer, 1966, p. 303). Also, "... the manager ... (is)... dealing with a given world situation which it... (is)... his responsibility to manage. In the higher functions of management... it is precisely the structure of the world situation which has to be managed" (pp. 345-346), and the "... whole purpose of OR as described here is to join with the manager in questioning...(the)... framework" (p. 404). This type of relationship between the scientist and the manager also provides the foundation for Ackoff and Rivett's envisaged eventual 'marriage' between OR and management. Ackoff and Rivett are correct in designating the relationship in this way as a marriage; normal science and the provision of paradigms cannot be separated.

One of the major puzzles with which Kuhn was concerned in his work was how, contrary to common belief, was it possible to account for the success of science given that it is "... the abandonment of critical discourse that marks the transition to a science" (1970, p. 6, my emphasis). Moreover, in Kuhn's view, a view that we have seen expressed by Beer, "preconception and resistance seem the rule rather than the exception in mature scientific development... (and)... furthermore, under normal circumstances they characterise the very best and most creative research as well as the more routine" (1963, pp. 81-2) and are "... symptomatic of characteristics upon which the continuing vitality of research depends... (namely) ... the dogmatism of mature science..." (ibid, my emphasis). Kuhn offers us reasons for both why scientists normally behave in this way and why it is associated with success.

Kuhn's answer to why it is that scientists normally behave in this narrow and myopic way is straightforward: in distinguishing different meanings of his notion of paradigm Kuhn emphasises that " a paradigm governs, in the first instance, not a subject-matter but rather a group of practitioners" (1962, p. 180). On the one hand, therefore, a paradigm "... stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community" (1962, p. 175), my emphasis). The word community was emphasised because, in Kuhn's view it is its dominance which cultivates and enforces "... shared values... (which act as)... important determinants of group behaviour" (1962, p. 186): a paradigm, through its social enforcement via the community becomes, as he puts it, a "... time-tested and group-licensed way of seeing" (1962, p.189). The hapless scientist falls prey to the community just as the manager in Beer's account is 'conditioned'. For Kuhn, then, an important corrollary of the notion of paradigm is that it is "... an accepted achievement in the sense that it is received by a group whose members no longer try to rival it or create alternatives for it. Instead, they attempt to extend and exploit it in a number of ways..." (op. cit., p. 91): in other words, the "... scientific community acknowledges... (it)... for a time as supplying the foundation for its further practice" (op. cit., p. 10). Realising, then, that it is the "existence of... (a)... strong network of commitments - conceptual, theoretical, instrumental, and methodological - ... (which) ... is a principal source of the metaphor that relates normal to puzzle-solving" provides the explanation why so much of a scientist's normal activity can be appropriately viewed as a "...mopping-up operation" (op. cit., p. 24), and it provides an answer to the "... most striking feature of normal research problems...(which)... is how little they aim to produce major novelties, conceptual or phenomenal" (op. cit., p. 35). The same feature of problem-solving in organizations is explained by Beer in the same way in a passage which we have already examined from a different angle in Chapter 2.

> "So what is the best solution for the company? The solution normally adopted quite obviously lies in the common phase-space - that is, in the area of overlap between the phase-spaces of the individuals who comprise the management team - but there is no guarantee that this gives the most appropriate answer. The company itself has no brain. It has a phase space within which it always operates, but this can only be interpreted by human beings who inevitably get the company's phase space confused with their own. But suppose the company had its own brain and was aware of its own phase space. The

answer to a particular problem might then lie on the fringes of this space, in an area not belonging to the phase space of one manager in the organization. The best answer to the problem would therefore be missed, because it would be strictly inaccessible to the management. The thoughts of a man concerned with this particular problem cannot break through the barriers of his own facilitated pathways to reach the goal" (1966, p. 59).

We cannot, then, expect novelty from management and this "... is the reason why it takes an objective, scientific, interdisciplinary investigation to arrive at a solution outside the phase space - a study undertaken by men who are by definition not prone to this particular thought block" (op. cit., p. 61). Conditioning, or some such notion, then, is seen by both Kuhn and Beer as being responsible for myopic normal behaviour: the framework (phase-space/paradigm) provides the rigid context for problem/puzzle-solving, and then "... the reason for employing operational research at all is to see whether there is a solution outside" (Beer, op. cit., p. 61). Beer is hinting here that there are benefits associated with this seemingly irrational behaviour, and I shall turn to these in just a moment. Clearly, however, there are dangers as well; the major one being that change, when desired, is inhibited. Both Beer and Kuhn recognise this, and both provide interesting descriptions of situations where the thought blocks which we shall see are essential to progress on the one hand threaten to block it on the other (Kuhn, for example, points out that Priestley went to his deathbed believing tenaciously in the phlogiston theory). Churchman

(1971) also recognises the dangers involved in a strikingly similar way. The lesson which Churchman draws from it is one that I shall return to later, namely, that OR's whole system has to be defined in terms of implementation as well. Thus:

> "The management scientist often discovers that what he considered to be a sufficiently closed system for the development of an optimal plan turns out to have facets and modifications that he never suspected. He finds, for example, that people identify with their roles in a certain manner, so that when new courses of action are suggested, their most typical reaction is to look at their particular role and the way in which it will be changed if the new plans are adopted. Top management may believe that the total organization is striving to maximize some function of money. (In the case of Kuhn's scientists, the maximization of truth). But the individuals in the organization may also attempt to maximize a particular kind of relationship they hold to the organization. The latter may well be threatened by a plan which increased the organization's effectiveness. These people therefore, resist any suggestions for change". (p. 227, my insert).

Whether or not Churchman would adhere to the detailed explanations provided by Beer and Kuhn the conceptual split between qualitatively different types of roles is clear and consistent with the main thrust of Beer and Kuhn (and I say this notwithstanding that Churchman argues for 'exoteric inquiry', as will be seen later on).

And the benefits from myopic role performance? The answer given by both Beer and Kuhn (and, implicitly by Churchman: see his generalized model of a system's anatomy, 1971, Chapter 3, p. 43) is the age-old one which has been provided by virtually all social commentators since Plato, and perhaps beyond that, namely, the virtues of specialisation. Beer is straightforward about this: "For practical managerial reasons, a company is divided into clear-cut areas of responsibility; and the manager, who is conditioned by the whole of his experience to seek solutions in which he can feel confident from personal knowledge, and over which he can exercise personal authority" (op. cit., p. 51). And further: "The existing organization is likely to come very close to a good answer which does lie within its own phase-space of solutions acceptable to all the managers, because there will be no thought block to prevent its so doing" (op. cit., p. 61). Beer's reference to the necessary confidence which a narrow role encourages is central to Kuhn's thinking. He argues that in all cases "... a commitment to the paradigm... (is)... needed simply to provide adequate motivation. Who", he asks, "would design and build elaborate special-purpose apparatus, or who would spend months trying to solve a particular differential equation, without a firm guarantee that his effort, if successful, would yield the anticipated fruit?" (1963, p. 95). Kuhn earlier in the same article had provided the answer: "That sort of work is undertaken only by those who feel that the model that they have chosen is entirely secure... (and therefore)... in receiving a paradigm the scientific community commits itself, consciously or not, to the view that the fundamental problems there resolved have, in fact, been solved once and for all" (op. cit., p. 86). Beer will countenance no concern by managers for fundamentals. In stressing that a manager who has failed to find an answer to a problem which lies outside his phase-space should not be labelled 'culpable' or "... incompetent, for if managers allow themselves too readily to wander imaginatively outside the phase-space set up for them by their own knowledge and experience, they become rather dangerous" (op. cit., p. 61). One of the reasons for him thinking that this type of behaviour on the part of managers is dangerous is that without "this process of conditioning... (which)... is vital for survival, ... the world would appear entirely incoherent and unpredictable" (op. cit., p. 58). The manager would, if the framework were always open to revision by himself and others, have no confidence in burrowing deeply into what the OR scientist sees as esoteric detail. As Kuhn says, following James, the world would become a "bloomin', buzzin' confusion".

Microscopic burrowing is then the major advantage of rigid role specialisation. The function of a paradigm (phase-space, or, in the case of Churchman, a Weltanschauung) is, as Kuhn puts it, to provide a pervasive foundation which "... tells them about the sort of entities with which the universe is populated and about the ways the members of that population behave; in addition it informs them of the questions that may legitimately be asked about nature and of the techniques that can properly be used in search of them. In fact", Kuhn goes on, "a paradigm tells scientists so much that the questions it leaves for research seldom have great intrinsic interest to those outside the profession" (1962, p. 93) because "given the paradigm and the requisite confidence in it, the scientist largely ceases to be an explorer at all, or at least an explorer of the unknown" (op. cit., p. 96-97). And because there are no "... deep debates over legitimate methods, problems and standards of solution... "

(op. cit., p. 48) OR loses interest.

OR loses interest because it is not management. OR's interest lies almost exclusively with the creation of the framework. Thoughtful operations researchers rightly recognise that an OR which is preoccupied with puzzle-solving cannot be revolutionary and, by the same token, as puzzle-solving relies on a taken for granted acceptance of the framework, accepting the role of normal science would rule out any possibility of justifying that OR solutions were truly progressive. This is so because it is clear that paradigms define problems and acceptable solutions in advance of inquiry and, in so doing, normal science would negate OR's ideals. We have here, of course, another entry point into the argument against OR as techniques.

OR and the Kuhnian version of "Revolutionary Science":

OR's attitude towards mere puzzle-solving means that without a doubt in Kuhnian terms it has to identify with revolutionary inquiry. We have seen in Chapter 2 that indeed OR does adopt the self-image of the revolutionary and the question to be answered here is whether this image can be given a Kuhnian interpretation. If it can we shall be in aa strong position to go ahead and trace out the implications of Kuhn's position and this will give us a vantage point from which to comment on the commitments which such a strategy of revolutionary inquiry would imply for OR's ideals. If we find - as we shall - that Kuhn's account of revolutionary inquiry has serious flaws in it as a normative scheme of inquiry, we shall be led to ask whether those features are also to be found in some conventionalist schemes of inquiry embedded in the literature of OR. Our use of Kuhn, then, is as a catalyst. A detailed understanding of his scheme will provide us with valuable insights into understanding what issues OR should (but at present does not) face to be a credible science of improvement.

OR, as we have seen in Chapter 2, has a clear attitude towards the use of techniques: they are only justified given the larger framework. On this point Kuhn is adamant too. Normal science, he says, is not a "... methodological prescription..." (1962, p. 245) which will act as "... therapy to assist the transformation of a proto-science to a science..." (ibid). On the contrary, it is only after the occurence of "... the transition to maturity... (that)... progress become(s) an obvious characteristic of the field" (ibid). Thus, "... normal science is predicated on the assumption that the scientific community knows what the world is like" (op. cit., p. 5) and, as OR argues, this cannot be taken for granted. It is not hard to show that OR conceives of revolutionary science in precisely the same way as Kuhn; nor is it hard to understand what is meant by these versions of revolutionary science. What is hard to understand is why this revolutionary activity is believed to be progressive. Kuhn tries hard to make his account explicit on this score and we shall follow it through (after having made the above mentioned connection) to see if he can. Later I shall consider whether OR has been able to show why its revolutionary outlook is progressive. Kuhn's eventual failure to provide a satisfactory answer should provide some lessons for OR.

For Kuhn "... scientific revolutions are... taken to be those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one" (1962, p. 92). Kuhnian revolutions, in fact, involve nothing less than the overthrow of reality. Thus: "successive paradigms tell us different things about the populations of the universe and about that population's behaviour" (op. cit., p. 103); again, "... when paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions" (op. cit., p. 109) and "... after a revolution scientists are responding to a different world" (op. cit., p. 111). If we understand by 'reality' that which cannot itself be questioned, then it is clear that Kuhn is indeed talking of revolutions as the overthrow of reality for looked at from the point of view of the scientist (or manager in OR's account) reality is seen by Kuhn to be mediated throught the conceptual framework provided by the paradigm. Asserting the primacy of 'sensation' over 'stimuli' Kuhn argues that"... deliberative processes ... analyse what is for us the given" (1962, p. 195). Deliberation is for Kuhn that sophisticated form of analysis which he calls normal science and for him we cannot deliberate or analyse "... until after we have had a sensation... (that is) ... perceived something" (op. cit., p. 194). For Kuhn, then, the paradigm provides the essential outlines of reality (he says it provides a map and instructions for map-making: see Masterman, 1970) and changes in it change that reality. In essence Kuhn says in his discussion of the correspondence of his views with those of Imre Lakatos (see: 1970), a paradigm provides a 'programme' for future research, and in fact a view of OR as the provider of such 'action programmes' has recently been expressed (Boothroyd, 1974). This view is in keeping with a fairly long tradition within OR as to its essential role.

Churchman as long ago as 1961 had outlined his views on what he understood to be an "... important future task of operations research with respect to top management" (1961b, p. 6) and, as we shall see later, much of his later work can be seen as an attempt to understand and solve the problems of successfully carrying out such a task. The task is to institute revolutionary change in organizations. Thus his "... paper is concerned with the problems of the creation of new goals in an organization. It intends to suggest that operations research can be viewed not only as an attempt to solve problems via research methods but also to develop new problems and to establish the importance of these within an organization" (ibid). The task, then, is to provide the organization with a 'long-range plan' under which shorter-term goals are pursued on the belief that one should not "... construe the goals of an organization over its history as attempts to add increments to and endless pile" (op. cit., p. 8). Kuhn is, of course, well known for his opposition to the 'accretion' theory of the growth of knowledge for precisely the same reasons as Churchman, namely, that it is seen to be based on a "... primitive form of logical induction(ism)... (which is)... as naive in business as it is in science" (op. cit., p. 9; cf. Kuhn, 1962, p. 96). Conforming very closely to Kuhn's view of a paradigm as a programme, Churchman argues that we should properly view organizations as "ideal-seeking". This implies (as Ackoff and Emery, 1972, p. 237, spell out) that the "... objective is not so much to solve problems as it is to create more challenging and important problems to work on..." They go on to note that "ideal pursuit can provide cohesiveness and continuity to extended and unpredictable processes..." (ibid). This is precisely one of the main functions which Kuhn ascribes to a paradigm. We can also refer to Mason (1969) for another view of the nature of a strategic plan. Referring to widely respected sources Mason describes an organization's strategic plan as the "... particular set of beliefs or assumptions about the world that an organization adopts... (to)... guide its activity..." Furthermore, a "... strategy is a statement in broad conceptual terms of 'what business the company is in or is to be in and the kind of company it is or ought to be'": a strategy embodies the basic "... assumptions... (which)... represent the world of his business as the manager sees it. They underlie every decision he makes... (and)... a rational manager uses these assumptions to interpret the data provided by his information system and thenusing logic he concludes the best plan of action" (p. B - 403-4). This is straightforwardly a Kuhnian-type view. Kuhn argues that something must "... bridge what would otherwise be gaps in the specification of the content and application of scientific theories" (ibid). The 'something' which Kuhn has in mind for the natural sciences is, of course, a paradigm. This is a conceptual device which, as Masterman (1970) says "works when the theory is not there" by answering such questions as: "What are the fundamental entities of which the universe is composed? How do these interact with each other and with the senses? What questions may legitimately be asked about such entities and what techniques employed in seeking solutions?" (Kuhn, 1962, pp. 4-5). In a similar fashion Churchman argues that:

> "... scientific method must include a philosophy of the whole system, however vague, however inadequate, however difficult to defend. It is what the Germans call a *Weltanschauung*, a perception of what reality is like. It becomes an integral part of the applied scientist's behaviour. This, above all, is why the applied scientist is not merely applying the results of pure research; he is also applying his Weltanschauung" (1968a, p. 133).

Elsewhere, (1970b as we have seen) Churchman, in referring to a Weltanschauung as 'systemic assumptions', makes the same point when he says that they refer to an "... understand(ing)... (of)... the purposes and structure of... reality" (p. B - 43) which tells the possessor "... how the whole system should work or inevitable does work" (p. B - 42).

The role of the operations researcher is then to formulate an appropriate world-view. Recall Beer's insistence that the operations researcher should not be burdened with a definition of the problem in advance of his inquiries, and that if he is to be a truly revolutionary scientist it is up to him to define the 'facts' (to define the nature of reality) and not meekly bow down before the facts as they are presented to him. Churchman and Emery (1966) have, it seems to me, gone further than anybody in arguing for the full-blown Kuhnian view. They recognise the strong interconnection which exists between the approaches of the of the behavioural scientist and the operational research scientist to the study of organizations, just as Kuhn has argued for the integration of epistemological and sociological approaches if we are to have an adequate understanding of the progress of science. Churchman and Emery realise that setting up the problem of the growth of knowledge within organizations in terms of what they, like Kuhn, see as the 'complementary' approaches of behavioural science and operational research immediately poses the problem of their 'combination', which they recognise is itself an organizational concept (see: p. 77). Their final resolution of this problem of the appropriate basis of combination is to argue for "... engage(ment) in institution-building so that agreement about his research concerns can be actively pursued and powerfully sanctioned" (op. cit. p. 81; cf. also Stringer, 1967). Compare Kuhn (1970) outlining the basic commitments his research entails:

> "Already it should be clear that the explanation... (of science)... must, in the final analysis, be psychological or sociological. It must, that is, be a description of a value system, an ideology, together with

> an analysis of the institutions through which that system is transmitted and enforced. Knowing what scientists value, we may hope to understand what problems they will undertake and what choices they will make in particular circumstances of conflict". (1970, p. 21).

The concern of Kuhn with an understanding of the values etc. of the scientific community to understand progress in science is a typical expression of the conventionalist's concern for the rationality of the processes of theory choice. A similar background motivates Churchman and Emery's concern to base organizational functioning on the "... very pervasive strands of common interest" which make "societies as admittedly full of conflict as ours.. hang together..." 'Churchman and Emery, op. cit., p. 81). Compare Kuhn's suggestion that one possible answer to the problem of progress in science is that it is "... important that group unanimity be a paramount value, causing the group to minimize the occasions for conflict and to reunite quickly about a single set of rules for puzzle-solving..." (ibid).

The assumption that 'hanging together' is evidence of common values is that one can be challenged, and I take up this as an issue in a later chapter. The point to be drawn from it here is that sharing it implies that Churchman and Emery and Kuhn have a common outlook.

Conclusion:

There are, then, strong parallels between OR and the Kuhnian scheme, both as regards normal science and revolutionary science. OR is particularly strongly attached to Kuhn's notion of revolutionary science which involves the creation of facts,[1] and not merely their acceptance. Siding with a Kuhnian version of revolutionary science would appear, therefore, to overcome the difficulties which beset positivism and, particularly, the revolutionary science of Popper. However, we still have no guarantee that the revolutionary science of which Kuhn speaks is guaranteed (or even likely) to be progressive. This must be the acid test of this scheme, and we shall have to pursue matters further to discover what guarantees Kuhn can offer us from within his conventionalist viewpoint. Our best way of proceeding is to examine how Kuhn deals with what has come to be seen as the central problem of theories of inquiry, namely, the relationship between the subject who acquires the knowledge and the object of his inquiry. It is only by gaining a deep appreciation of the many pitfalls presented by the commonly made 'subject-object distinction' that self-reflective progress can be made in the development of an adquate theory of inquiry for OR. I shall, therefore, make a brief detour to examine some of the problems which arise because of this distinction. I shall then turn to the question of whether Kuhn's scheme provides an adequate theory of inquiry.

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(1) We will see more of this later in his model of the psychology of the subject.

SECTION B

THE FOUNDATIONS OF KUHNIAN SCIENCE

The Problem of Knowledge: Process versus State:

Interest in theories of inquiry means that one cannot avoid dealing with the problem of the natures of subject and object for this, with their relationship, is the most basic definition of knowledge possible. Because the issues involved in this definition of knowledge are rarely given explicit consideration, theories of inquiry often have embedded in them covert resolutions. These resolutions often provide a foundation for the acquisition of knowledge which effectively make it impossible to attain. Therefore, the views taken on the resolution of the subject-object distinction may negate the whole enterprise: they may make knowledge impossible to acquire by the very way in which they approach the problem.

This possibility is, in fact, actualised by the way in which some operations researchers approach the problem of knowledge. This is regrettable because our analysis in the first chapter has led us to the conclusion that the distinctive feature of OR is its resolute commitment to unfettered inquiry into social organizations. OR stands or falls as the guardian of sound knowledge acquisition. The application of knowledge (via, for example, techniques) is seen as a secondary activity. In this section I wish to raise the subject-object problem and apply some general thoughts on the matter to the Kuhn-Popper debate. This analysis will put us in a favouable position to carry the analysis over to Operations Research.

There has for some time been a growing recognition that the appropriate stance towards the problem of knowledge is not to ask 'What is knowledge?' or 'How is knowledge possible?' but, rather, to conceive of knowledge interms of its growth, that is, as Piaget (1972, p. 6) puts it, to consider "... knowledge... always... (as)... a process rather than a state..." (ibid). This recognition is now fairly widespread, and even affects some views affects some views within OR on the current status of its practice (and, as we shall see in a later chapter, this stance has infused virtually all modern thinking in the behavioural sciences). It is, of course, central to the outlook of Kuhn's work.

The common recognition of viewing the problem of knowledge as a problem of growth or process has, however, not been taken very far by many writers. In particular, many have been reluctant to accept that knowledge is only to be understood in terms of process. There, are, of course, the products of the knowledge process, but it is possible that the reason they are validly called knowledge at all is because of their relation to a process of production. Many writers who adopt the process stance have attempted to avoid this implication. Hegel's notion of knowledge as process, for example, reveals on close examination (which we undertake later) to have concealed in it the view of knowledge as the state of 'Absolute Knowledge'. Similarly, Kuhn's version will be seen to rest on an implicit view of knowledge as a state. Also, (in both the next and the final chapter) examples from the literature of operations research will be found to have surreptitiously imported a notion of knowledge as a state, even though their schemes are presented as representatives of the process schema.

We should view adequate knowledge in terms of the process of its growth because as Piaget says "the theory of knowledge is... essentially a theory of adaptation of thought to reality, even if in the last instance this adaptation (like all adaptations) reveals the existence of an <u>inextricable interaction</u> between the subject and object..." (op. cit., p. 18, my emphasis). As I have emphasised in this quotation, looking at the growth of knowledge as process implies most fundamentally that subject and object cannot be torn apart; it is only their combination which constitutes knowledge and, as obvious as this sounds, some very subtle way of distinguishing subject and object and then allowing them to relate have been attempted. The product of their relationship has then been called knowledge. That is, the problem of knowledge has not often been seen as the relationship between the subject and the object but, instead, it has been seen as the 'subject' and 'object' relating. Few have resisted the temptation of believing that one first defines the subject or the object (or both) and then one lets the knowledge process proceed. They have done this without apparently realising that what they have in fact achieved was not a definition of knowledge as process, but a definition in advance of the process of knowledge itself. If this procedure is followed the process becomes merely the vehicle for realising this predefined knowledge either as the given object (positivism) or the given subject (conventionalism).

Defining the bounds of knowledge in advance is, of course, the direct antithesis of the declared ideal of OR and all serious epistemologies of true discovery. In an important sense, however, it is hard to lay the blame for this unwarranted move at the doorstep of the proponents of the epistemologies themselves. Much of the blame has to be placed fairly and swuarely on the language which we use to think about the problem of knowledge, a conclusion which Wittgenstein (amongst others) came to. Mere mention of the problem of knowledge in terms of subject and object seems to encourage the paralysis of thinking that the problem of knowledge can be unlocked by discovering they key in the subject or in the object. No suitable language seems available to think the problem through without lapsing into an essentialism[1] of one or the other. The search is always

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(1) That is, that knowledge is 'essentially in the object or the subject.

for an unassailable foundation of knowledge, where in fact none is to be found.(1) We have seen, in the introduction to this chapter, the kind of position which can result if the attempt is made, when we briefly looked at the implications of Hume's work.

Let me mention some direction which will follow from the stance towards the problem of knowledge outlined above.

Firstly, it may be thought that I have opened up a nihilistic line of thought which will end with the condemnation of all attempts at attaining adequate knowledge. Not so. My aim is to expound and, to the extent possible clarify some issues which must be faced by OR. I hope to clear away nonsense. It is altogether another task to provide a clear and unambiguous theory of inquiry for OR, although the guidelines I provide later at least make this an approachable ideal. The views which I shall attack do not.

Secondly, although much of the blame for the mistakes which the following will reveal must, as I say, be laid at the doorstep of the very language we use to think about the problem of knowledge - particularly talk of 'subject' and 'object' - I shall still retain this vocabulary for it seems to me that one advantage of doing so is to emphasis one very important feature of any adequate account of the growth of knowledge. That is, that both subject and object are to be taken seriously. They are both to be treated in non-essentialist ways, certainly, in that they are not

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(1) This is even true of Popper - his views are pessimistic forecasts of the amount of knowledge we can acquire, they do not weaken the argument tha knowledge is absolute for him because it is so in principle. (See: 1945).

endowed with some inscrutable, independent 'giveness'. In fact, we only truly take subject and object seriously when they are considered in relation to process. It is not the essentialist treatment of subjects or objects which brings them properly into account in any theory of the growth of knowledge. On the contrary, it is by their very treatment of them as 'essences' that they are forced out of the picture. The neglect of the nature of the object - what I have previously called subject-matter: for our purposes here the terms can be used interchangeably - is the central pre-occupation of this thesis. I shall later on only have some general remarks to make on the nature of the subject as he is implicated in the process of inquiry. Some interesting thoughts on the role of the subject have beem published by Churchman and Schainblatt (1965), and there is a growing interest in the problem of implementation (see: Huysmanns, 1970; Shultz and Slevin, 1975), but this work is marred by a general failure to conceive of the nature of the subject within the context of an adequate understanding of the nature of the object. Part Two of this thesis is devoted to an analysis of the typical conception of the object which has popular support in OR circles. The question asked (but not adequately answered) by this thesis is what precisely is the nature of the subject as it is conditioned by the nature of the object? In simple terms, what would OR look like if it had an adequate grasp of the nature of its subject-matter?

We turn first in our quest to understand the problem of knowledge to a consideration of the work of Louis Althusser (1970). Althusser's account of the problems of epistemology is interesting because he shows that the 'empiricist problematic', by commencing with a given subject and a given object, effectively makes knowledge the tautologically inevitable outcome of the processes set up whereby 'knowledge' is to be acquired.

We shall in a moment want to distinguish two variants of the way in which this is achieved. It is this feature of the schemes of Popper and Kuhn - the inevitability of valid knowledge - which above all else is the major thrust of the criticism which I wish to make of them. Later on I use the results of this analysis to criticize some writing on the nature of OR. What other criticism could be more fundamental?

To turn to Althusser's account: he says that:

> "The empiricist conception of knowledge presents a process that takes place between a given object and a given subject. At this level, the status of this subject (psychological, historical, or otherwise) and of this object (discontinuous or continuous, mobile or fixed) is not very important. This status only affects the precise definition of the variants of the basic problematic, while the basic problematic itself is all that concerns us here. The subject and object, which are given and hence pre-date the process of knowledge, already define a certain fundamental theoretical field, but one which cannot yet in this state be pronounced empiricist. What defines it as such is the nature of the process of knowledge, in other words a certain relationship that defines knowledge as such, as a function of the real object of which it is said to be knowledge".
> (op. cit., p. 35).

Althusser goes on to say that:

> "The whole empiricist process of knowledge lies in fact in an operation of the subject called abstraction. To know is to abstract from the real object its essence, the possession of which by the subject is then called knowledge... (T)he essence is abstracted from real objects in the sense of an extraction, as one might say that gold is extracted (or abstracted, i.e., separated) from the dross of earth and sand in which it is held and contained" (op. cit., p. 35-6).

Accounting for empiricist philosophies in this way leads Althusser to two conclusions: firstly,

> "Knowledge: its sole function is to separate, in the object, the two parts which exist in it, the essential and the inessential - by special procedures whose aim is to eliminate the inessential real (by a whole series of sortings, sievings, scrapings and rubbings), and to leave the knowing subject only the second part of the real which is its essence, itself real, Which gives us a second result: the abstraction operation and all its scouring procedures are merely procedures to purge and eliminate one part of the real in order to isolate the other" (ibid).

The point is that schemes for the acquisition of knowledge (the sortings, sievings, etc.) which commence from a given subject and a given object claim, by the way in which the object is conceptualized in the first place (and this may be done unconsciously, of course), that their 'procedures' will deliver merely by the nature of those procedures, the essence of the object. One insight which follows from this is that we cannot accept on face value the often-made claim that the scientist is not seeking absolute knowledge, since the search process continues indefinately. Hegel, as I have mentioned, and as we shall see in detail later, points to infinite, unending process without revealing that in fact absolute knowledge is ever-present even in the working out of that process. Absolute knowledge (of essences) is given in the procedures even though we may not grasp the essence as such until the end. In other words, what the procedures guarantee is 'progress'. They guarantee this, as Althusser points out, because they define the real object as having a structure which "... concerns precisely the respective positions in the real of the two constitutive parts of the real: the inessential part and the essential part... (where)... the inessential part occupies the whole of the outside of the object, its visible surface; while the essential part occupies the inside part of the real object, its invisible kernel" (op. cit. p. 37).

With this pre-given structure of the real object it must follow that removal of the inessential is merely the corrollary of revelation of the essential. The move in any scheme of knowledge acquisition which defines the structure of the object in this way, therefore, is always seen as 'progressive' without the goal ever having to be reached. Progress, therefore, can be defined and equated with pure process because process is merely an instrumental operation on a given structure which by its construction is defined so that it must yield the sought after fruit merely through its application. Althusser sums up in this way:

> "What represent knowledge... is completely inscribed in the structure of the real object, in the form of the difference between the inessential and the essence... Knowledge is therefore already really present in the real object it has to know, in the form of the respective dispositions of its two real parts! Knowledge is completely and really present in it: not only its object, which is the real part called the essence, but also its operation, which is the distinction and respective positions that really exist between the two parts of the real object, of which one (the inessential) is the outer part which conceals and envelops the other (the essence or inner part)" (op. cit., pp. 37-8).

Popper and the Problem of Knowledge:

How do these thought apply to Popper's scheme, taking his an an exemplar of positivism? It is clear from them that we should be highly suspicious of schemes which claim that they are progressive merely because they are corrective of inadequate knowledge. We should be suspicious because we are entitled to ask 'inadequate with respect to what, exactly?', and if all we receive in reply is a mute pointing to a backdrop of 'reality', we can be sure that we are not talking of knowledge acquisition as such but of knowledge as essence. This, of course, is not to say that we should not welcome the correction of inadequate knowledge of reality. The concern here is with the schemes of inquiry whereby adequate knowledge is claimed to be acquired.

Take Popper's notion of falsification as the key-stone in what he considers to be the '<u>Logic</u> of Scientific Discovery'. I emphasise that Popper considers his scheme to be merely concerned with the acquisition of knowledge by dint of the logic of its procedures alone. Because scientific knowledge for Popper is solely the <u>outcome</u> of the application of the procedures of falisification,(1) we are talking of knowledge as the <u>state</u> achieved through their application. We are definitely not talking of the process knowledge acquisition as such when we conceive of knowledge in this way. Popper's main concern seem to be to avoid situations where we too easily accept theories as knowledge and so he sets up the criterion of falsification to place the most rigorous demands possible on a contending theory. This rigour is useful as a value to which scientists should adhere but it cannot describe the acquisition of knowledge because of its reliance upon (as Kuhn terms it) a neutral observational language which allows, by the application of his procedure, the removal of the inessential real and thereby progress towards the essential. Recall that Popper's model (for which we have seen some support in OR) involves the subject pervaded by his subjective conjectures which colour his expectations on the one hand, and on the other there is the world - the object - which can be addressed in terms of the utterances 'no' or 'not disproved'. Within this framework the problem of knowledge is the removal of false conjectures

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(1) He repeatedly says that he concedes that knowledge can be achieved by other means - but that this is of no interest to him.

and their replacement by 'not false' conjectures. But that he sees this movement as progressive says much for his view of knowledge. He shows not the slightest concern for how knowledge is actually acquired (see: 1970, pp. 57-8 for his bitter criticisms of Kuhn's turn to psychology and sociology).

Thus Popper says:

> "... we have learned not to be disappointed any longer if our scientific theories are overthrown; for we can, in most cases, determine with great confidence which of any two theories is the better one. We can therefore know that we are making progress... Thus we can say that in our search dor truth, we have replaced scientific certainty by scientific progress".
> (1945, p. 12).

Popper appears to believe that by avoiding all talk of the origins of knowledge and by restricting himself solely to questions of the progress of knowledge he avoids the incomprehensibility of essentialism. He believes, and rightly, that it is impossible to talk rationally of the origins of knowledge in essentialist terms, but how comprehensible do we become by remaining silent? What are we left with in Popper's scheme is the capitulation to the essentialist scheme by default, because Popper cannot avoid at least 'pointing' to the very thing that the essentialists directly assert. Popper is correct in his insistence that talk of the <u>ultimate</u> origins of knowledge is misplaced; he is wrong in carrying this over to a silencing of all talk of the process of knowledge acquisition.

The lesson which OR should take away from this is that the search for the panaceas of applied research in terms of 'criticism', 'falsification', 'rigour', 'dialectics' etc., are in themselves meaningless with respect to the problem of knowledge. All such 'keystones' of 'science' often involve the totally unwarranted implication that 'reality' is immanent and the inessential has only to be removed.

Kuhn and the Problem of Knowledge:

Demonstrating that Popper's scheme is essentialist is not difficult; it fits clearly within the empiricist problematic as described by Althusser. Showing that the same is also true of Kuhn's scheme is, on the face of it, much more difficult because it is widely believed that Kuhn offers (to use his own terminology) a 'paradigmatic' alternative to the Popperian view (cf. Sklair, 1973; Masterman, 1970; Magee, op. cit., and most of Kuhn's critics in Lakatos and Musgrave, 1970). The task looks especially daunting in view of the fact that, as Masterman claims, Kuhn has amended Popper's account where we have found it wanting most, namely, with regard to the origins of knowledge. Nevertheless, in spite of all this opinion to the contrary, we may be encouraged in our task by the fact that of all the people involved in the so-called 'Kuhn-Popper debate' one leading character, Kuhn himself, has consistently expressed surprise at the gap his and Popper's followers have zealously attempted to place between them. I think we should pay Kuhn the honour of taking him at his word when he says that "... Sir Karl's view of science and my own are very nearly identical" (1970, p. 1) and believe him when he writes claiming to wish to clear up 'misunderstandings' (op. cit., passim). Much, I believe, is to be learned about Kuhn's scheme from examining the basis of this overlap.

Although I have said that it appears difficult to fit Kuhn into

Althusser's 'empiricist problematic', in one sense, if we were prepared to forego the lessons which extended analysis could provide, it is very easy to dismiss Kuhn's scheme in these terms. This dismissal could follow naturally from the very way in which Kuhn sets up his problem. Kuhn's problem is to account for progress in science, and he quite rightly sees an essential role in this for a study of science 'as it is'. However, he goes much further than this open-ended approach would seem to imply when he takes as his starting point the common-sense assumption that science as it is is successful. Thus he says: "...we must explain why science - our surest example of sound knowledge - progresses as it does, and we must first find out how, in fact, it does progress" (1970, p. 20). If we discount for the moment the latter part of this sentence, which shows that at least Kuhn intends to uncover the processes by which science acquires knowledge, we might be forgiven for concluding that he is attempting precisely the same kind of analysis as Popper. Starting from the supposition that the knowledge which science has is adequate, concern shifts from an analysis of why this knowledge can be taken as adequate, in terms of an analysis of the process by which it was acquired. Instead, what we are given (certainly in Popper's account, but not so clearly yet in Kuhn's) is a functional or teleological account of how the given end (adequate knowledge) could or must have been achieved. Certainly, teleological and process accounts are often compatible, but a teleological account cannot stand as a substitute for a process account. Instead of being given an account of why the process of knowledge acquisition produces adequate knowledge (that is, an understanding of what is adequate knowledge) we are given an account of the adequacy of theprocedures in achieving a given end (which is merely assumed to be adequate knowledge).

Kuhn has amended Popper's account in several interrelated ways.

His major innovations, however, lie in two areas which, I believe, he then works through, within the essentialist problematic shared by Popper, to produce his distinctive scheme.(1) The two areas of innovation are firstly, in his extension of Popper's psychology of the subject to a psychology (Kuhn says that he prefers 'sociology') of the 'fact' - this is by way of a logical extension of what was already inherent in Popper's scheme. Secondly, on the basis of this extension, he attempts to provide an account of the origins of a science. Considering that operations research in effect claims to be creating a new 'science' each time it investigates an organization, it should be very interested in such attempts and the grounds on which they might fail.

Science as a Dynamic Process:

Whereas Popper offers us the 'comparative statics' of the growth of knowledge, Kuhn offers us a 'dynamic' analysis. Whereas in Popper's scheme theories change in a time-independent way, for Kuhn paradigm shifting is a time-dependent process. Both schemes are 'processual', but for Kuhn the process is materially altered by having to take place over time. For Kuhn a central observation is that "... discovery is a process and must take time(1962, p. 55). The view of the growth of knowledge as time-dependent follows logically from the deduction that "only when all the relevant conceptual categories are prepared in advance... can discovering

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(1) Using his own terminology, Kuhn would call this paradigm elaboration, that is, puzzle-solving.

that and discovering what occur effortlessly, together, and in an instant" (ibid). (We can note just in passing that March and Simon (1958) would concur with Kuhn on this point, calling Kuhn's paradigm shifting 'non-programmed decision-making' and his notion of normal science 'programmed decision-making'). Kuhn sees in his acceptance of the time-dependent nature of the growth of knowledge the answer to a fatal weakness in the Positivist view of science. Arguing that successive paradigms must of necessity be 'incommensurable' - because (and this is an argument to which I shall return) a new paradigm by definition deals with the problems with which the earlier one could not - Kuhn dismisses the prevalent view of "... science-as-accumulation..." (op. cit., p. 96). This is a view which he sees as being "... entangled with a dominant epistemology that takes knowledge to be a construction placed directly upon raw sense date by the mind" (ibid). As he correctly goes on to say:

> "(This)... interpretation, closely associated with early logical positivism and not categorically rejected by its successors, would restrict the range and meaning of an accepted theory so that it could not possibly conflict with any later theory that made predictions about some of the same natural phenomena" (op. cit., p. 98).

If knowledge is merely constructions (or representations) placed on 'raw' sense data then dynamic change is thereby ruled out because the logical implication is that:

> "... the range of application... (of scientific theories)... must be restric-

> ted to those phenomena and to that precision of observation with which the experimental evidence at hand already deals. Carried a step further (and the step can scarcely be avoided once the first is taken), such a limitation prohibits the scientist from claiming to speak 'scientifically' about any phenomenon not already observed. Even in its present form the restriction forbids the scientist to rely upon a theory in his own research whenever that research enters an area or seeks a degree of precision for which past practice with the theory offers no precedent. These prohibitions are logically unexceptionable. But the result of accepting them would be the end of research through which science may develop further"
> (op. cit., p. 100).

Forbidding scientists to go beyond phenomena (as all positivists do) would strip the process of knowledge growth of its obviously time-dependent nature because accepting that normal science is conducted within the promise provided by a paradigm, means (and we go into this in a moment) that the 'facts' are in some sense 'found' in advance of their discovery. Therefore, Kuhn concludes time is needed both to articulate the paradigm and to discover the scope of its application. Kuhn's scheme of the dynamics os science runs: initial paradigm, articulation, the discovery of 'anomalies', the onset of 'crisis', the resolution of anomalies through the use of a new paradigm. If, as the positivists suggest, facts are unambiguously 'given', this time-dependent process is truncated. As Kuhn puts it:

> "If positivistic restrictions on the range of a theory's legitimate applicability are taken literally, the mechanism that tells the scientific community what problems may lead to fundamental change must cease to function" (op. cit., p. 101).

This attack on positivism combined with his views on the nature of scientific revolutions gives us, perhaps, some hope that Kuhn will avoid the totally unwarranted presupposition of positivism that improvement can be defined interms of uncovering phenomena. Making a statement with which Kuhn (and some operations researchers) should agree Harvey (1973) points out, that "... positivism draws its categories and concepts from an existing reality with all its defects..." (p. 130). Thus, we could perhaps assume that the Kuhnian scheme, in remedying this defect in positivism, will provide us with the possibility of outlining a scheme of inquiry which is truly revolutionary in the OR sense, i.e., only the 'real' as a complex totality is given.

With the firm commitment to viewing scientific progress as a dynamic process, Kuhn then looks to the problem of uncovering and supporting the bases upon which such a commitment could rest. And here, not surprisingly, Kuhn resorts to the standard currency of the philosophy of science and social science of his day, and he skilfully interprets them within his process view.(1) If the process of scientific progress takes time to work

(1) But they do not change their basic nature because of this, as we shall see.

through then Kuhn's first problem is to explain the machanics of the process: specifically, he has to explain the commitment which scientists display towards theories, because commitment is a necessary corrollary to the activity of working out the promise provided by a paradigm to the point where crisis is overwhelming and change ensues. Kuhn achieves an explanation of this commitment by 'sociologizing' facts.

Kuhn extends his criticism of positivism's belief in the self-evident giveness of facts (a view which he sees as being responsible for their non-dynamic view of scientific progress) by giving facts a sociological status in two ways. Firstly, at the level of the individual, he argues for a Kantian model of man, whereby the very perception of the world is seen to be predicated upon given (<u>a priori</u>) conceptual categories. (1) Secondly, at the level of the scientific community, to explain why individuals both share and continue to employ a particular set of conceptual categories, that is, a paradigm, Kuhn argues for something very close to sociological functionalism (of which I shall have more to say in the final chapter) whereby the individual is seen to be 'socialized' by his group into accepting a particular set of conceptual categories. Given these two sociological innovations in the explanation of scientific process Kuhn claims that he has explained its characteristic dynamism. Whether this model also explains scientific progress is quite a different matter, as we shall see.

The Kantian basis to Kuhn's work can be seen both in the emphasis which he gives to the subject (as evidenced, for example, by his criticisms

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(1) This is an option that Churchman (1970a and 1971) considers. Although he finds some fault in it - as, indeed, does Kuhn - it has a lasting influence on his views. Note that the Kantian model of man will reappear in Part Two as an attempt to treat the members of social systems as 'purposeful' individuals.

of Popper's notions of 'mistakes' etc.) and in his description of the subject (i.e., the scientist). On the central question of theory choice Kuhn says that he shares with some other philosophers "... a common conviction of the relevance of the philosophy of language and of metaphor" as compared with Popper and his followers who "... share with the more traditional philosophers of science the assumption that the problem of theory-choice can be resolved by techniques which are semantically neutral" (1970, p. 234). It is above all around the question of the existence of a 'neutral observational language' that "... the central constellation of issues which separate... (Kuhn)... from most of... (his)... critics" exists (op. cit., p. 266). Kuhn is emphatically opposed to the idea of a neutral observational language because in his view "... languages cut up the world in different ways, and we have no access to a neutral sub-linguistic means of reporting" (op. cit. p. 268). With Kuhn's denial, however, comes an affirmation: "... knowledge of nature... is built into language" (op. cit., p. 270).[1] In discussing scientist's acquisition of the prevailing conceptual categories through being presented with 'exemplars', Kuhn argues that "...doing problems is learning the language of a theory and acquiring the knowledge of nature embedded in that language" (op. cit., p. 272). This knowledge is pre-linguistic in that "... relat(ing) words to other words... can function only if we already possess some vocabulary acquired by a non-verbal or incompletely verbal process" which involved some kind of "... direct matching of whole words or phrases to nature" (op. cit., p. 270-1). Language, it seems, sets the limits to knowledge. This is so because "... our

(1) Both Churchman and Beer argue this point.

world is populated in the first instance not by stimuli but by the objects of our sensations" (1962, p. 193), so that we have a "... language conditioned or language-correlated way of seeing the world... (without which)... we do not see the world at all" (1970, p. 274).

With this strongly Kantian model of the subject Kuhn has both given the basis for explaining the commitment (of which I spoke earlier in terms of 'role specialization') and has also apparently plugged the hiatus in the positivistic account, namely, its inability to explain the origins of a science other than his reference to unanalysed, primitive phenomena. This remedy is highlighted - with Kuhn's approval - by Masterman's (1970) account of what she sees as Kuhn's "central originality".

An analysis of the usages to which Kuhn puts the notion of paradigm leads her to conclude that "... the paradigm's basic property... (is its) ...concreteness or 'crudeness'" (op. cit., p. 67). She argues, against the "aetherial philosophicalness of the 'falsifiable metaphysics' view" (ibid) that Kuhn has been concerned with "... the whole process of human beings trying to achieve a scientific explanation" (op. cit, p. 68). That is, Masterman argues, Kuhn has been concerned, as no-one has before him, with the problem that if theories do not attach to nature in a non-problematic way, how can science work when the theory is not there? It is the neglect of this question which accounts for the "... gross <u>lacuna</u> which ... (she)... asserts to be in the Popperian view - namely, that Popper cannot account for how any new research line starts up..." (op. cit. p. 71). The "central originality" of Kuhn, therefore is, Masterman argues, his "... investigation into the crude origins, and early stages, of any science..." (op. cit., p. 73). And the crude origin of a science is, as Masterman describes it, an "... initial practical trick-which-works-sufficiently-for-the-choice-of-it-to-embody-a-potential-insight..." (op. cit., p. 70).

Science in this view starts from an 'artefact' or 'construct' which enables puzzle-solving to proceed by 'representing' an object as 'like' something else without necessarily providing the basis for saying with what exactly the constituted object is similar (Kuhn, 1970, p. 275). Masterman's analysis, therefore, provides a place for Kuhn's 'tacit' (i.e., non-verbal) knowledge in a philosophy of science.

Starting from a potentially productive insight, Kuhn's dynamic description of science follows: by the achievement of concrete results (which Masterman calls the 'sociological paradigm') and the development of a scientific community which is dedicated to the fulfilment of the promise of the insight and its achievements, normal science blossoms into a "disciplinary matrix" of norms, values, concepts, and results. Thus, unlike Popper who is exclusively concerned that theories should not too easily be accepted, Kuhn is preoccupied with explaining mechanisms which prevent theories from too easily being thrown away. It is to this end Kuhn musters the formidable combined forces of a priori perception and social conditioning.

These concepts can certainly explain commitment, but how do we then explain change? We might be forgiven for suspecting that Kuhn has worked himself into the same corner occupied by the structural functionalist sociologists, namely, the inability to explain change (see, for example, Gouldner, 1970; Lockwood, 1970; Mills, 1959) except in terms of the metaphysics of the 'system'. This is an option which Kuhn finally adopts. However, prior to this he employs another option, one which is provided for him by his Kantian subject. This is to make explicit what is implicit in Kant, namely, a taken-for-granted object world. Kant's a priori categories are a priori of something and this thing is taken for granted when we focus exclusively on the subject. Kant wanted to set the bounds of 'pure reason' much in the way in which Kuhn wishes to set the bounds of

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'logic' and all the characteristic reasonings of normal science. Kant in his 'Critique of Pure Reason' lays down the bounds of pure reason as subsisting within the limits of a priori knowledge, just as we have seen Kuhn draw limits by the use of his notion of paradigm. The Kantian categories by defining the nature (the 'logic') of the world (from the point of view of inquiry) presuppose the existence of that world, which would not matter at all if it were not for the fact that 'it' is continually referred to by Kant, Kuhn and some operations researchers, in an unexplained way as a guarantor of progress.

As Husserl (1970) says of Kant:

> "Naturally, from the very start in the Kantian manner of posing questions, the everyday surrounding world of life is pre-supposed as existing - the surrounding world in which all of us... consciously have our existence..." (p. 104).

And referring back to Kuhn's thoughts on the nature of the contact between the subject and the world, we can refer to Mepham's (1973) point that:

> "Not only is it impossible to think an object world as the source of systems of meaning, it is also impossible to identify this source as subjective. For there is not first a subject and a world, and then a language created at their point of contact. Because to be a subject is already to live in a world

> ... No doubt it is possible for a person to have 'wordless thoughts' but this is to miss the point. For to be a person is to live in the world... Where there are yet no systems of meaning there can be no subject... Thus we cannot look for the determining conditions which make language and culture possible at the level of the subject. We cannot, in a Kantian manner, discover organizing powers of consciousness attributable to a transcendental subject" (op. cit., p. 126-7).

Thus, Kuhn should not claim that it is in the subject that the origins of science are to be found. And neither does he. On the critical question of paradigm change he is forced to virtually abandon the central place of paradigm in his scheme and reluctantly to refer to something very close to the positivist's idea of phenomena. We shall see that, as Kuhn describes it, paradigms merely organize phenomena, and progress is defined as increasingly better organizations. However, in an attempt to avoid the positivist trap which has has by this move set for himself he reintroduces the supremacy of the subject in a new guise: the 'supra-subject' in the form of the 'community'. This final move, we shall see, does not make his theory of inquiry any more understandable.

Kuhn's Theory of Revolutionary Inquiry:

(i) Sociological Relativism:

With sociologizing of the fact comes, as I have suggested, the need for an explanation of paradigm change. Since paradigms determine the facts, paradigm change, it might appear, could just as easily be called 'fact-change'. This looks the very epitome of revolutionary, anti-positivistic science. But the interesting question is whether Kuhn can explain change in terms of the same concepts which he employed to explain stability or whether he has to revert to other modes of explanation. Because the concepts which explained stability were the anti-positivistic ones, the interest in this question is whether concepts which explain the basis of progressive change are also anti-positivistic.

Kuhn initially describes scientific revolutions in terms of paradigm changes or changes in "world views", as we have seen. However, this is clearly not an explanation of the change, not yet does it explain why a change can be thought of as progressive. To give explanations of these two aspects of scientific revolutions necessitates an explanation of both the relationship between successive paradigms and the basis on which the earlier one was rejected, and the later one selected. Recall that what is involved is a "... decision between alternative ways of practicing science..." (Kuhn, 1962, p. 157). Kuhn attempts initially to explain both of these aspect of scientific revolutuions by the single concept of "situation". The situation which Kuhn invokes to fulfil this explanatory role is one of 'crisis'. Crises arise, according to Kuhn, because eventually the continual exploitation of the promise of the initial paradigm leads to the discovery of 'anomalies', that is, puzzles which have not yielded to solution within the terms set by the prevailing paradigm. A crisis arises not merely because of the existence of unsolved puzzles. Kuhn distinguishes two possible reactions to 'anomalies'. On the one hand they can be viewed as outstanding puzzles which are expected to yield given enough effort and ingenuity. On the other hand they may be viewed as 'counterinstances', that is, unsolved puzzles which are seen to undermine the very paradigm in which they are posed. It is only with the second reaction thattrue crisis occurs and a paradigm shift occurs. As Kuhn says, "... novel theory emerge(s) only after a pronounced failure in the normal problem-solving activity" (op. cit., p. 74-5). Given that a real crisis exists, the selection criterion and the relationship between competing paradigms is determined. As Kuhn says:

> "Probably the single most prevalent claim advanced by the proponents of a new paradigm is that they can solve the problems that have led one to a crisis. When it can legitimately be made, this claim is often the most effective one possible".
> (op. cit., p. 153).

The ability to solve the anomalies of the old paradigm provides the selection criterion, and it also determines the relationship between new and old.

> "If new theories are called forth to resolve anomalies in the relation of an existing theory to nature, then the successful new theory must somewhere permit predictions that are different from those derived from its predecessor. That difference could only occur if the two were logically incompatible. In the process of being assimilated, the second must displace the first" (op. cit., p. 97).

This logical incompatability Kuhn calls "incommensurability". In this version of progressive change, new paradigms are progressive because they solve problems which the old one could not. However, Kuhn does not at this stage of his explanation wish to stress actual problem-solving ability. He points out that often this claim is unfounded - demonstrations are usually yet to come. His stress, rather, is on the psychological/ sociological aspects (the incommensurability) of the change. At one stage in his argument it is this which is held to be the basis of progressive change. However, this leads, obviously enough, to great difficulties. Unless something intervenes to guarantee the new logic provided by the new paradigm, we are correct to feel suspicious of his attempts (evidence of which is given below) to provide an explanation of progressive change solely in terms of sociological concepts (as Popper, indeed, is). One reason for suspicion is that explanation solely in terms of the sociological concepts which were employed to explain stability, appears to rule out even the possibility of change, regardless of the question of whether they can explain progressive change. This Kuhn recognises.

Kuhn faces the same problem of explanation which operations researchers face over the problem of 'implementation'. A paradigm shift being a new way of practicing a science is akin (as I argued earlier) to the provision by an operations research team of a new strategic plan. Most thoughtful operations researchers agree that for real improvement the implementation of a new strategic plan involves organizational changes and all of them recognise that this involves crossing vested interests, and the like. The new way of operating promised by either a paradigm or a strategic plan cannot, as all agree, be realised through mere logical argument and demonstration. Kuhn is, therefore, talking like a well-seasoned operations researcher when he observes:

> "... if a new candidate for paradigm had to
> be judged from the start by hard-headed

people who examined only relative problem-solving ability, the sciences would experience very few major revolutions. Add the counter arguments generated by what we previously called the incommensurability of paradigms, and the sciences might experience no revolutions at all" (1962, p. 157).

The simple Popperian-type solution then is avoided by Kuhn - the facts do not straightforwardly arbitrate between competing theories, but the consequence of reasserting the sociological aspect of theory choice (1) has the awkward implication that change might not occur at all.

And yet, even in the face of this implication, Kuhn is not prepared to give up the dominant role of the subject as he straightforwardly asserts:

"But paradigm debates are not really about relative problem-solving ability, though for good reasons they are usually couched in those terms. Instead, the issue is which paradigm should in the future guide research on problems many of which neither competitor can yet claim to resolve completely... The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems

(1) Note that Kuhn comes very close to the 'organization of a self-determining institution' view, a view which is an off-shoot of structural-functionalism. See Perrow, 1972, Chapter 5.

that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith" (op. cit., p. 157-8).

Earlier on Kuhn had argued, consistently with his initial sociological outlook, that mere anomalies could not by themselves account for crisis and change because it is, he recognised, always possible that scientists "... do not... treat anomalies as counterinstances..." (op. cit. p. 77). From his sociological line of argument, Kuhn has no hesitation in saying that "... no amount of observation could refute" a paradigm and also that "... every problem that normal science sees as a puzzle can be seen, from another viewpoint, as a counterinstance and thus a source of crisis" (op. cit., pp. 78-9). Kuhn's sociology is clearly in danger of ushering in the unrestrained relativism which we have seen is always latent in OR, a criticism which is persistently levelled at him by both his positivist and non-positivist critics (Lakatos and Musgrave, 1970; cf. Giddens, 1976; Keat and Urry, 1975). Kuhn's response to the problem of relativism is a reversion to the positivistic notion of phenomena as a provider of objectivity.

(ii) Phenomenalism: the given object of inquiry:

With Kuhn's immensely rich style of writing (Masterman found for example, 21 different usages of the word paradigm!) it is possible to find early warning of chahges which are to come as he is continuously wrestling with the difficulties of positivism and conventionalism. His work should be seen as an attempt to curb the excesses of both. Thus, when I say that Kuhn changes direction it should really be understood as a change of emphasis.

The swing to phenomenalism can be seen in his remark that:

> "The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgement leading to that decision involves <u>the comparison of both paradigms with nature and with each other</u>" (op. cit., p. 77, my emphasis).

This remark is made in the context of a preamble to:

> "...what will ultimately be a central point - that the act of judgement that leads scientists to reject a previously accepted theory is always based upon more than a comparison of that theory with the world" (ibid).

Kuhn is leading up to the point that:

> "No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature" (ibid).

Thus, when he comes to ask: "... is sensory experience fixed and neutral? Are theories simply man-made interpretations of given data?" (op. cit., p. 126) and finds the traditional answer of "... an immediate and unequivocal Yes!" (ibid) unsatisfactory. However, we can guage the distance which he has progressed from this unacceptable position by his further comment that "in the absense of a developed alternative, I find it impossible to relinquish entirely that viewpoint" (ibid). Although there is <u>more</u> to theory choice than ' a comparison of a theory with the world' there is still the operation of the 'world' (or 'nature') in theory choice, and we start to appreciate that Kuhn was not really worried that the object was pre-given. On the other hand he <u>was</u> concerned that this way of viewing progress in science "... no longer <u>functions</u> effectively, and attempts to make it do so through the introduction of a neutral language of observations now seem(s)... hopeless" (ibid, my emphasis). For Kuhn, paradigm choice is not made 'merely' by comparison with the given world, but it is nonetheless crucially made in the context of the <u>given</u> world. Kuhn attempts to get around the positivistic implications of such a view by, for example, using the word 'world' in two senses: one refers to the sociological nature of a paradigm, namely, "practicing in different worlds, the two groups of scientists see different things when they look at the same point in the same direction" (op. cit., p. 150). The other usage, following hot on its heels, refers not to the <u>way</u> people look, but the <u>object</u> which they are looking at. Following the previous quotation, Kuhn remarks that: "Again, that is not to say that they can see anything they please. Both are looking at the world, and what they look at has not changed" (ibid). One is forced to suspect that Kuhn hopes by this double usage to conflate subject and object, but he cannot keep them together. Letting nature back in by going so far as to say that "most of the puzzles of normal science are directly presented by nature, and all involve nature indirectly" (1970, p. 163) is a surprising and important concession from a view which had earlier claimed that the origins of science lay in paradigms.

Kuhn wishes to avoid saying that theories are 'simply' "... man-made interpretations of given data" (ibid). However, by arguing that "though different solutions have been received as valid at different times, nature cannot be forced into an arbitrary set of conceptual boxes" (ibid) he comes very close to it especially as he consistently speaks of the "... scientific revolution as a displacement of the conceptual network through which scientists view the world" (1962, p. 102, my emphasis). From such views the unavoidable conclusion is that Kuhn's model of scientific process is one where a mind (albeit Kantian) operates in an instrumental way on a given reality. The only way in which this conclusion could be avoided would be to foreswear speaking of the object as an unspeakable essence! Kuhn, quite plainly, does not.

Although theory choice in the context of the given world is indispensable in Kuhn's scheme to save it from the relativism of solipsism, his reaction to this realization is to ban all talk of it. In introducing an essay (1970, p. 1-23) he notes that:

> "...(Popper and I)... both insist that scientists may properly aim to invent theories that explain observed phenomena and that to do so in terms of real objects, whatever the latter phrase may mean" (op. cit., p. 2, my emphasis).

Again, after asserting the crucial 'eventual' arbitrating role of nature (op. cit., p. 263) he goes on to insist that it is fruitless to talk of 'truth' in other than an "intra-theoretic sense", and he castigates his critics for wanting

> "... to compare theories as representations of nature, as statements about 'what is really out there'. Granting that neither theory of a historical pair is true, they nonetheless seek a sense in which the latter

is a better approximation of the truth. <u>I believe that nothing of that sort can be found</u>. On the other hand, I no longer feel that anything is lost, least of all the ability to explain scientific progress by taking this position" (op. cit., p. 265, my emphasis).

As a part of this process of pushing the object out of mind Kuhn argues that when we come to see things correctly "... the phrases 'scientific progress' and even 'scientific objectivity' may come to seem in part redundant" (1962, p. 162). Kuhn even goes so far as to rule talk of objectivity out of bounds <u>on principle</u>. Thus, his discovery of the sociological fact that there is "... no theory-independent way to reconstruct phrases like 'really there'", means to him that "... the notion of a match between the ontology of a theory and its "real" counterpart in nature... (is)... illusive in principle" (op. cit., p. 206).

This is a return to his view that "the competition between paradigms is not the sort of battle that can be resolved through proofs" (1962, p. 148).

If paradigms cannot guarantee their objectivity, and he will not hear talk of the real world guaranteeing it, the only possible source of objectivity left open to him is to look for something which is external to paradigms and has its own guarantee of objectivity 'built-in'. Kuhn finds this guarantee of objectivity in the properties of science as a social system. I shall turn to these in a moment. Before I do so I wish to suggest that no matter what the process is by which paradigms are compared, in Kuhn's scheme phenomenalism is not thereby avoided. Kuhn appears to argue that it is when he says that "in the sciences the testing situation never consists, as puzzle-solving does, simply in the comparison of a single paradigm with nature. Instead, testing occurs as part of the competition between two rival paradigms for the allegiance of the scientific community" (1962, p. 145). If paradigms are, as he frequently stresses, incommensurable, it is impossible to fully translate the <u>concepts</u> of one into the <u>concepts</u> of the other, within what context does the competition take place? Not solely at the limited points of conceptual contact surely? If this were so very limited confidence could be placed in the final resolution of such competitions. A clue to the nature of the context is given by the facility with which Kuhn himself feels able to distinguish 'viewpoints'. How is it, given incommensurability, that Kuhn can say (as he often does) that this is one view and this is another view? What is it that enables both Kuhn and the scientists whom he discusses to differentiate different views at all? Kuhn seems to argue that views can be differentiated by referring to the 'field' - the views are different views of the 'field'. For example, after a paradigm shift "for them the field will no longer look quite the same as it had earlier" (op. cit., p. 83). But if we take Kuhn's early views seriously the 'field' is nothing but the paradigm in its determination of the world. If we followed the subjectivist path which Kuhn lays down we would reach the absurd conclusion that having views of the field should be read as having paradigms of paradigms! Quite clearly, when Kuhn refers to the field we are to read this as something other than a paradigm. Clearly, again, a paradigm could not be the origin of a science because the 'field' must in some way be independent of paradigms for there to be 'views' of it. The very possibility of comparing paradigms thus requires, if not a fully neutral observational language, then a language which exists outside of paradigms. It is to Kuhn's credit that he recognised that this language could not be a neutral observational language, but it is to his detriment that having achieved this step he submerged the whole issue by deifying the subject because in so doing he left behind an implicit essentialism of the object. Making a somewhat similar point Harvey (1973) comments on Kuhn's work:

> "Underlying Kuhn's analysis is a guiding force which is never explicitly examined. This guiding force amounts to a fundamental belief in the virtues of control and manipulation of the natural environment. The leap of faith, apparently, is based on the belief that the new system will allow an extension of control over some aspect of nature. <u>Which aspect of nature</u>? Presumably it will once again be an aspect of nature which is important in terms of everyday activity and everyday life at a particular point in history" (op. cit., p. 121, my emphasis).

Because Kuhn refuses to talk about the nature of the object of inquiry he cannot bring it in to account for the objective progress of science except in an implicit way. As far as the individual scientist is concerned, Kuhn leaves him stranded in his own subjectivity. Instead of turning to an explicit analysis of the role of the object of inquiry in securing objectivity, Kuhn turns to the scientific community. In making this move Kuhn offers us a brand of speculative metaphysics which will occupy our attention in Part Two.

(iii) Science as an Adaptive System:

Turning to the nature of the scientific community Kuhn claims to be able to

"... say that... (a)... later theory was the better of... two as a tool for the practice of normal science, and... hope to add enough about the senses in which it was better to account for the main developmental characteristics of the sciences. Being able to go that far, I do not myself feel that I am a relativist" (1970, p. 264-5).

The supreme importance of the scientific community is shown when he says that "The very existence of science depends upon vesting the power to choose between paradigms in the members of a special kind of community" (op. cit., p. 167). As he had earlier put it: "As in political revolutions, so in paradigm choice - there is no standard higher than the assent of the relevant community" (op. cit., p. 94). It is in terms of the scientific community that all questions of progress are answered, all doubts dispelled:

"... the nature of such communities provides a virtual guarantee that both the list of problems solved by science and the precision of individual problem will grow and grow. At least, the nature of the community provides such a guarantee if there is any way at all in which it could be provided. What better criterion than the decision of the scientific group could there be?" (op. cit., p. 170).

Recalling the implicit essentialism of the object in Kuhn's work and the remarks of Althusser noted earlier, it comes as no surprise at all to hear Kuhn say that "the scientific community is a supremely efficient instrument for maximizing the number and precision of the problems solved through paradigm change" (op. cit., p. 169, my emphasis).

What are the properties of the scientific group which provide this virtual guarantee? "... (T)he intuitive notion of community that underlies much in the earlier chapters of... (his)... book ... is a notion now widely shared by scientists, sociologists, and a number of historians of science" (op. cit., p. 176). The notion is that the scientific community is an adaptive system which means by the very definition an adaptive system which survives that "... a sort or progress will inevitably characterise the scientific enterprise so long as such an enterprise survives" (op. cit., p. 170). Thus, Kuhn says, although "the analogy which relates the evolution of organisms to the evolution of scientific ideas can easily be pushed too far", (although he does not say how), "... it is very nearly perfect... (because)... the resolution of revolutions is the selection by conflict within the scientific community of the fittest way to practice future science" (op. cit., p. 172).

The idea of an adaptive organism may be a useful analogy in some contexts, but used as a way of guaranteeing the growth of objective knowledge it is very hard to avoid making a definition of objectivity based on it tautologous. Thus objectivity is guaranteed by the community; the community is an adaptive system; adaptive systems survive; therefore survival is the criterion of objectivity. No wonder that as far as Kuhn is concerned "... to a very great extent the term 'science' is reserved for fields that do progress in obvious ways" (op. cit., p. 160) because if it exists it has to be progressive! It is this reasoning which lies behind Kuhn's argument that we need not consider objectivity as at all related to truth. Thus he argues that by viewing the scientific community as an adaptive system we can "... relinquish the notion, explicit or implict, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth" (op. cit., p. 170). And if we accept his viewpoint we can also accept that so long as "... the scientific enterprise... survives... there need not be progress of another sort" (ibid). However, the adoption of this analogy means that Kuhn's model is not so much revolutionary as evolutionary. Considering Kuhn's initial aim was to refute the 'accretion' view of knowledge, the selection of this evolutionary model is surprising indeed. Evolution of the species (which reference to Darwin and not, for example, Spencer suggests is Kuhn's model) is quite clearly cumulative. Species, unlike individuals, do not 'die'; no fundamental, qualitative, changes of species are possible if that species is to survive. For science as a whole, then, as species, the struggle is not for survival but for structure (see: Buckley, 1967, p. 12). Scientific revolutions, then, can only be seen as 'mutations' which do not change the 'critical variables' themselves, but only their range of feasible values. As Kuhn argues for a pre-given object-world which does not itself change with revolutions, as could the environment of a biological species. Scientific revolutions, it seems, are to be seen as progressive evolutionary mutations towards harmony with the environment. The 'structure' of science can change (which at the level of the individual can be legitimately seen as 'revolutionary') but only on one direction: science, in the evolutionary view, has to become 'more and more like it really is'.

This view goes a long way towards explaining why it is that both Popper and Kuhn "... insist that adherence to a tradition has an essential role in scientific development" (kuhn, 1970, p. 2). Both see tradition as a prime source of knowledge for scientists both as framework for research and as a point of departure: science's adaptations are directed towards the resolution of difficulties - points of misfit - in the relation between theory and reality in such a way as to conserve what is good in the old and remedy what is bad. The difference between them is that Kuhn points out that remedying the bad may have repercussions for our views of the good as well. Viewing the community as adaptive, therefore, legitimates our acceptance of present knowledge as 'essentially' right but open to modification. This is a view which we shall find very clearly expressed in Beer's work, for example, which I consider in Chapter 6. Note that Kuhn does not believe that scientific knowledge is fundamentally correct in any ontological sense - he does not believe that it progressively uncovers the real structure and processes of the world. Scientific theories have phenomena underlying them - although they are not directly accessible - and in this sense they are fundamentally correct, although incomplete.

Kuhn's Relationship to Sociological Functionalism:

The reason why Kuhn believes that scientific knowledge is basically correct is because he believes that the scientific community has social properties which make it adaptive.

Kuhn has by no means stumbled upon the adaptive systems view by change. His contact with behavioural scientists and sociologists has, I believe, provided him with what has relatively recently (Silverman, 1970; Couldner, 1970; Friedrichs, 1970) become recognised as the systems paradigm which in sociological circles has, under the sponsorship of Talcott Parsons, become most widely known as structural functionalism.

Although I shall go more deeply into this paradigm later on, Kuhn's social theory has all the main features of the structural-functional model. The feature of the scientific community which make for its 'progressive' adaptiveness is primarily the existence of common values. This feature is probably the most crucial as it is the pre-requisite of a high level of 'functional interconnectedness', a feature highlighted by Gouldner (1959)as the cornerstone on which adaption is taken to rest. Kuhn depicts the scientific community as follows:

> "A scientific community consists... of the practitioners of a scientific speciality. To an extent unparalleled in most other fields, they have undergone similar educations and professional initiations; in the process they have absorbed the same technical literature and drawn many of the same lessons from it... As a result, members of a scientific community see themselves and are seen by others as the men uniquely responsible for the pursuit of a set of shared goals... (which means that)... within such groups communication is relatively full and professional judgement relatively unanimous" (op. cit., p. 177).

Full and free communication and the responsiveness which shared values provides is the cornerstone of the community's adaptiveness: it is within this structure of the free, full and dedicated scrutiny of competing paradigms that only the fittest survive. At this level there is little to distinguish Kuhn's scheme from Popper's intellectual 'free competition' (1)

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(1) Compare the view expressed by Popper that "there can be sociological laws, and even sociological laws pertaining to the problem of progress; for example, the hypothesis that, wherever the freedom of thought, and of the communication .... cont. overleaf...

Also, like Popper, Kuhn is careful to point out that the functioning of such a system can be upset by anything which hinders either the flow of information or its creative and fully dedicated scrutiny. So power has to be excluded as a possibility: "If authority alone, and particularly if non-professional authority, were the arbiter of paradigm debates, the outcome of those debates might still be a revolution, but it would not be a <u>scientific</u> revolution " (op. cit., p. 167). Kuhn apparently believes that it has not been a problem since Hellenic Greece (again in company with Popper). What is a pre-requisite with Kuhn becomes with others (if not with Kuhn himself - cf. Harvey, op. cit., and Giddens, op. cit.) a myopia towards questions of power and, its corollary, conflict. As regards creativity and dedication, Kuhn, along with many others including, of course, micro-economists, sees an important role for self-interest. Common values are crucially important, but should not be pressed (or expected to be effective) to such an extent that they stifle individual initiative.

In Chapter 7 these features will be shown to be crucial elements of OR's social theory. I postpone criticism of them until that time.

<u>Postscript: The Viability of Conventionalism as a basis for OR's Theory of Inquiry:</u>

Kuhn attempts to overcome the weaknesses of positivism and fails. He fails because although he banishes the positivist conception of facts as they relate to the practice of science, to convince us that science is objective he relies on an implicit phenomenalism Thus, as Kolakowski

(1) ...cont.
of thought, is effectively protected by legal institutions and institutions ensuring the publicity of discussion, there will be scientific progress" (1945, p. 322). This 'hypothesis', in fact, is a major idea underlying his work

puts it, although "... conventionalism sets out to criticize... this... (positivist)... notion of 'the given'" (op. cit., p. 177) "... this situation may be interpreted as a victory of positivist thought: ... even its adversaries regard its results as irreversible, i.e., have resigned themselves to the fact that scientific knowledge cannot have metaphysical pretensions, and that metaphysical aspirations must consequently find justifications other than those scientific knowledge can provide" (p. 179). Historically, conventionalists have turned to religion for this kind of knowledge. Kuhn, as we have seen, turns to the social system. (Churchman turns to religion). The objectivity of science, he says, is not to be found in its relationship to its object of inquiry. This option is open to Kuhn by reason of his very sensitive historical researches. However, his conventionalist commitments do not allow him to take it. As Keat and Urry (op. cit.) say of Kuhn's treatment of the discovery of oxygen,

> "... whereas, for Kuhn, Priestley and Lavoisier are seen as 'working in different worlds', the realist maintains that they worked in the same world, whilst their theories made conflicting claims about the nature and existence of some of its constituents" (p. 62).

To make a firm break with positivism Kuhn would have to be able to distinguish between the 'real' object as some unexpressible essence, and the 'object of knowledge'. Kuhn cannot make this distinction because for him, as an implicit phenomenalist, all objects are essences: hence his common-sense reaction of refusing to speak about the unspeakable. That we should distinguish between the real object and the object of knowledge is a point made by Althusser:

"... knowledge never, as empiricism desperately demands it should, confronts a pure object which is then identical to the real object of which knowledge aimed to produce precisely... the knowledge. Knowledge working on its 'object', then, does not work on the real object but on the peculiar raw material, which constitutes, in the strictest sense of the term, its 'object' (of knowledge), and which, even in the most rudimentary forms of knowledge, is distinct from the real object" (op. cit., p. 43).

Kuhn attempts some kind of dialectic between positivism and conventionalism, but they are not really distinct points of view in the first place. Therefore the result is a foregone conclusion. Both treat knowledge as knowledge of essence. Using different designations for conventionalism and positivism, Bamborough (1966), through an analysis of Wittgenstein's solution of the problem of universals, has come to a similar conclusion on different grounds. Bamborough designates (rather unconventionally) as 'nominalism' what I have termed conventionalism, and calls 'realist' what I have called positivist. The latter is a common convention which I have not followed (see: Keat and Urry, op. cit., pp. 1-2), for obvious reasons. The difference in the grounds on which Bamborough argues that both philosophies are essentialist is that he does not deal with the implicit phenomenalism (realism, in his terms) of conventionalism. He does not, that is, deal with the guarantor problem faced by conventionalism. Nevertheless, his account of Wittgenstein is of interest to us because he reaches an identical conclusion on the necessity of distinguishing between the real object and the object of knowledge, and he gives us some insight into the nature of the latter. As Bambrough puts it, both 'realism' and 'nominalism' seek to provide knowledge which is universal in the sense that it provides knowledge of what it is that is common across a set of objects. The nominalist claims that what is common across a set of objects is our designation of those objects. The realist claims that what is common across a set of objects is something real about those objects themselves.

According to Wittgenstein both views are mistaken in that they merely complement each other from different essentialist stances. Wittgenstein solves the problem of universals by denying that the problem has any status. He says that the problem of universals arises from the mistaken belief that for us to be able to claim knowledge that knowledge has to be of the world in general (cf. Louch, 1966). This naturally leads us on to look for a general property somewhere which will sustain our general knowledge. In making this step we have ushered in essentialism by directing out attention away from our knowledge to the thing which sustains its generality. Instead of universals, all that we can hope for and ever find Wittgenstein says, are family resemblances. Thus, if we take a family and say of one that she has the 'family' nose, of anther that he has the 'family' eyes, and of yet another that she has the 'family' mouth, etc. we can exhaust all their characteristics and never find the one essence to which they all correspond. There is nothing in 'essence' which accounts for them belonging to the same family: all that we have are a set of criss-crossing and overlapping <u>relationships</u> between the characteristics which determine 'the family'. Having achieved this understanding we can say that the only thing which the members of the family have in common is that they are members of the family. This notion of family resemblances in opposition to universals seems ~~to me~~ (though again I do not want to argue it in detail) identical to the distinction made by Christopher Alexander (1964) between, respectively, 'semi-lattice' structures and 'tree' structures, and I mention it at this stage only to indicate one feature of Wittgenstein's notion which will be of importance later. This feature is the inherent complexity which semi-lattice structures display compared to tree structures. Dealing with complexity, we saw from Chapter 1, is central to OR's ideals, so we shall look (in Chapters 5 and 7) with interest at its attitudes to the requisite structure of its knowledge of social systems: is it the simplicity of universals, or the complexity of family resemblances?

Wittgenstein's favourite example of the problem of universals is the question of what it is that games (e.g., chess, badminton, the olympic games, etc.) have in common.

> "Wittgenstein... denies at one and the same time the nominalist's claim that games have nothing in common except that they are called games and the realist's claim that games have something in common other than that they are games. He asserts at one and the same time the realist's claim that there is an objective justification for the application of the word "game" to games and the nominalist's claim that there is no element that is common to all games. And he is able to do all this because he denies the joint claim of the nominalist and the realist that there cannot be an objective justification for the application of the word "game" to games (universalia in rebus) or a common relation that all games bear to something that is not a game (universalia ante res)" (Bamborough, op. cit., p. 199).

Wittgenstein is not a nominalist in that he does not accept that names can be assigned to random objects, for then there would be no set of relationships which defines a family: the properties of the objects in terms of their resemblances to each other are crucially important to understanding. But, on the other hand, he will not agree with the realist (in Bamborough's terms) that what is common to a set of objects is something other than that they are a set of objects. The nominalist is right when he denies the existence of this other property, but is wrong in saying that there is nothing else; the realist is right that there is something else, but is wrong in saying that this something else is something common to all:

> "Because the nominalist and realist are both right and both wrong, each is driven into the other's arms when he tries to be both consistent and faithful to our language, knowledge and experience. The nominalist talks of resemblances until he is pressed into a corner where he must acknowledge that resemblance is unintelligible except as resemblance <u>in a respect</u>, and to specify the respect in which objects resemble on another is to indicate a <u>quality or property</u>. The realist talks of properties and qualities until, when the properties and qualities have been explained in terms of other properties and other qualities, he can at last do nothing but point to the <u>resemblances</u> between the object that are said to be characterized by such and such a property or quality"
> (Bamborough, op. cit., p. 205).

With either positivism or conventionalism (to revert to our terminology) the problem of knowledge is abandoned. Let us now turn to consider probably the most philosophically sophisticated member of the OR community, C. West Churchman. Churchman has been preoccupied with the problem of 'designing adequate inquiry' (as he might put it). We will be concerned to see if he has avoided the pitfalls which have diverted Kuhn from his intended path, i.e., the development of a theory of inquiry.

CHAPTER FOUR

CHURCHMAN'S PHILOSOPHICAL BASIS FOR OPERATIONAL RESEARCH

Introduction:

We have seen (in Chapter 2) that Churchman has strong elements of conventionalist philosophy informing his work, and some connections (in Section A of Chapter 2) between his view of OR and Kuhn's view of science have been drawn. Further connections between the views of Kuhn and Churchman will appear in this chapter. However, Churchman is not straightforwardly a conventionalist (we have, for example, noted pragmatist strains in his thought). The conventionalist basis to his work leads (as it did with Kuhn) to difficulties of guaranteeing objective progress in inquiry so he turns to Hegelian idealism for help. To the extent to which Churchman offers us clear guidelines to the construction of an adequate theory of inquiry, they are to be found in his Hegelian interpretation of conventionalism. Kuhn's guidelines to the development of an adequate theory of inquiry are to be found in his sociological-functionalist interpretation of conventionalism. Whereas Kuhn finds his guarantor in the social system, Churchman find his in Hegelian idealism. For him, all OR is in some sense 'social OR', he is sceptical of much direct help being forthcoming from the social and behavioural science in their present state of development. He is, for example, distrustful of cybernetic 'organismic' analogies (1968a p. 87). This is not to say that he rules out an eventual intimate tie up between OR and the social sciences. Far from it, as his paper with Emery (previously referred to) demonstrates. His worry seems to be that it is premature to involve oneself in theories of social process while the philosophical foundations of these theories, and particularly the implications which they may have for inquiry into social systems, remains unclear. This, again, is not to say that he sees a clear temporal priority with social theories being 'based on' philosophically sound strategies of inquiry: there is room in his thinking for some kind of joint progress. Churchman has, however, been preoccupied with philosophical issues and has by and large steered clear of direct discussion of social issues - both theoretical and practical - preferring to refer selectively to particular connections as illustrative material to the general line of argument he is pursuing. Nevertheless, Churchman's work can fruitfully be seen as an attempt to provide the philosophical foundations of OR's social theory (which is discussed in Part Two).

Although Churchman acknowledges indebtedness to Hegel, and his preferred "Dialectical Inquiring System" (1971) is more or less straightforwardly Hegelian, his indebtedness goes further than this. Showing this, however, raises considerable problems of exposition.

Hegel's writings are vast and too multifarious to be treated as a whole for the purposes of this chapter. What is required are Hegel's views on the overall outline and purpose of his scheme, and such are available in his 'Introduction to the History of Philosophy'. It is here that Hegel tells us what philosophy is and, more importantly from our point of view, in doing so spells out the role of the philosopher in the history of knowledge. I intend, therefore, to use this work as my major source of authority on Hegel's scheme. I shall refer to the translation and commentary of Quentin Lauer, S.J. ('Hegel's Idea of Philosophy', 1971). As regards Churchman's works I shall largely restrict myself to his later work on the 'Design of Inquiring Systems' (1971), where we find the fullest treatment to date of what he considers the major philosophical issues of OR (as a 'planning science').

Showing the deep relationship between Churchman's work and Hegel's philosophy has a very important implication. Hegel's philosophy has to be taken whole. It has an internal coherence and 'tightness' which means that the problems which it poses and the solutions which it offers are merely different reflections of the same thought. This fact has the very important implication that it is not possible to pick up Hegelian problems without at the same time becoming embroiled in Hegelian solutions. We shall see that Churchman considers Hegelian problems to be <u>the</u> crucial 'design' problems in any attempt to set about adequate inquiry, and although he expresses some 'worries' about the comprehensibility of Hegel's solutions (going so far at one point to subject Hegel's dialectic to a 'critique') it is the latter's solutions which he is thereby forced to adopt.

In this chapter I shall show that Churchman's proposals originate in Hegelian philosophy. In the following chapter I shall show that this makes his scheme incapable of realising OR's ideals. This claim may sound surprising because not only is Churchman one of the most vigorous exponents of OR's ideals, but he is also very critical of positivsm.

Churchman continually argues, as Hegel does, that we should understand the progressive development of thought as a movement away from the reality of sense perception (see particularly, 1968a, passim). We have also seen Kuhn argue that the same feature characterises the revolutionary periods of scientific activity. In this sense, Churchman can be seen as openly advocating some form of revolutionary inquiry. However, in the context of Hegelian philosophy, this advocacy amounts to very little because Hegel, as we shall see, replaces the given reality of the senses with a much larger (in fact infinite) given reality which it is thought's task to make <u>manifest</u>. In the Hegelian scheme the problem of knowledge is reduced to a problem of the mechanics of revelation, and is not at all concerned with the process of creative discovery. In Hegel's philosophy it is impossible to distinguish the real object from the object of knowledge (because, we shall see, they are the same).

It legitimates and encourages a kind of speculative idealism, and makes illegitimate and discourages attempts to understand real-world structures and processes. For Hegel the underlying reality is the Idea.

The Role and Nature of Reason:

Churchman shares with Hegel a profound confidence in the power of reason - rational thought - in human affairs. Hegel, as Lauer puts it, "... saw rational thought as that which in the highest degree characterizes man as man and which should, therefore, characterize man when he is concerned with that which is of highest interest to him" (op. cit., p. 11). Given that Churchman considers the highest interest of man to be improvement we can see why he defines "systems design" as "... implementing improvement in social systems by means of the best available method of inquiry" (1974, p. 452), and why he believes that the key terms in this definition are interrelated in the ways in which he suggests they are. Before we turn to these relationships it will, however, be helpful to understand Hegel's and Churchman's views on the nature of reason itself.

Hegel was convinced that "... reality was more concretely present (more real) in thought, in ideas, than in sensation" (Lauer, op. cit., p. 2). We have seen Churchman attack positivism on similar grounds because of the implication that we should base recommendations for improvement in the 'reality' of raw sense data. The identical point - that we grasp reality more completely through our ideas of it than we do from direct 'contact' with reality itself - is of course made by Kuhn. Reason for all three is the reason that is achieved through the development

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"If thought is free and undetermined by the changing world in which it lives, its validity would seem to be unconditioned, or conditioned only by itself. But this would mean that it is self-contained, identical with itself, always precisely what it is, and, therefore, not susceptible of development" (op. cit. p. 18).

And further:

"... since thinking is the activity of spirit, not spirit's being-acted-upon - by a non-verifiable causality in things - it must produce its own objects; there is no other way for them to come into being as objects. At the same time, if thought is to be true, its objects cannot be other than the real; the problem, then, is the problem of any idealism: how to identify the products of thinking activity with reality" (op. cit., p. 35).

We have seen this problem loom large in Kuhn's work where he becomes faced with the problem that "... if paradigms are closed systems of epistemological premises, which succeed each other by processes of revolutionary change, how is anyone to be able rationally to adjudge one paradigm against another?" (Giddens, 1976, p. 142). Churchman runs up against the same problem for the same reasons. We have seen him argue that:

"...scientific method must include a philosophy of the whole system, however vague, however inadequate, however difficult to defent. It is what the Germans call a Weltanschauung, a perception of what reality is like. It becomes an integral part of the applied scientist's behaviour, This, above all, is why the applied scientist is not merely applying the results of pure research; he is also applying his Weltanschauung" (1968a, p. 133).

Elsewhere, speaking in very much the same way as we have seen Kuhn, Churchman refers to a Weltanschauung as "systemic assumptions" and he notes that they refer to an "... understand(ing)... (of).... the purposes and structure of... reality" (1970b, p. B - 43) which tells the possessor "... how the whole system should work or inevitably does work" (op. cit., P. B - 42). Churchman realises that setting things up in these terms means that "systemic judgements... are certainly not verifiable, either by observation or clear reasoning" (op. cit., p. B - 47) because "... one's "world view" (Weltanschauung) shapes the information he uses to reach his conclusions... (therefore)... no data can ever fatally destroy a Weltanschauung, though it may produce modifications in its basic story" (op. cit., p. B -47). That thought is self-determining is the corollary of reality being determined by thought, but Churchman makes it an explicit principle of design. Thus he argues that

"It can be seen that design, properly viewed, is an enormous liberation of the intellectual spirit, for it challenges this spirit to an unbounded speculation about possibilities... The

liberated designer of inquiring systems
... will look at present practice as a
point of departure at best" (1971, p. 13).

And he goes on to talk of "the intellectual freedom that belongs to the designer..." (ibid). He hammers the same message home in the lesson which he draws from Spinoza. He asks:

"Could an inquiring system be designed with a completely free executive? If so, we would have an example of Spinoza's <u>fourth</u> inquirer, whose characteristics are described as follows: "There is the knowledge that arises when a thing is perceived through its essence alone ... and a thing is perceived through its essence alone when from the fact that the inquirer knows something, its executive understands why it knows it" That is, the executive has a valid theory to explain why knowledge occurs ...Intuition in the fourth type must above all be conscious, because as it grasps the truth it immediately reflects on this action and simultaneously verifies that it knows" (op. cit., p. 27-8).

It is clear that Churchman sees the self-determining property of thought as an absolutely essential property of adequate inquiry and it is for this reason that he expends much effort in the "... establishment of a design base... (where)... an inquiring system... is open to its beginnings and... has control over all the material it processes" (op. cit., p. 20). Combined with these features is his argument that the material must not be 'given' from outside: this for him is the essence of 'control' (ibid).

However, Churchman recognises that these strong demands create a nasty problem - the so-called 'hermeneutic problem', in fact, which is the claim that all understanding requires some pre-understanding of the very thing which is to be understood. This problem, in fact is a presupposition of conventionalism (see: Kolakowski, op. cit., pp. 160-1). Churchman poses the problem this way:

> "... what is wrong with the claim that large models can sweep in all reality... (is that)... the models don't mean anything unless they use information. But we can't determine what information is correct unless we understand the whole system. But this is what our realistic models are supposed to tell us. In other words, we need realistic information to start with in order to build our models, but we need the model to get the information... The point seems to be that we can't get facts about systems without making very strong assumptions about systems. Thus it looks as though we're involved in a vicious circle. We must get information to make our models real-

istic, but we must have general models to get our information" (1968a, pp. 161-2).

Churchman is too much of a conventionalist to dilute the role of his Weltanschauungen, and he calls at several points in his various works for 'optimal Weltanschauungen' ("... the critical issue is to decide which systemic assumptions can legitimately be made" (1970b, p. B - 42)) a problem which, like Hegel, he sees Kant's work having implicitly posed (1971: Chapter 6): Churchman's argument that Kant's scheme is in the end incoherent because the 'inputs' have to be taken as given (pp. 144-7) mirrors that of Hegel who points out that Kant's critique of reason must have either been based on something other than reason or was relying on reason to determine its own validity. Given Churchman's commitment to some form of idealism to resolve the hermeneutic circle, the problem turns into finding some basis for comparing Weltanschauungen. This, of course, was precisely Kuhn's incommensurability problem.

Hegel's Solution to the Problem of Progress:

Having wedged himself in the conventionalist corner Churchman turns to Hegel's idealism for a solution:

> "It is really not until the time of Hegel that it occurred to philosophers that external vs. internal only makes sense to a third mind observing and/or controlling the process of learning. The three minds then become parts of the total inquiring system" (1971, p. 35).

Because a solution within the framework requires that "external" resolution of Weltanschauungen be avoided at all costs Churchman is forced to define objectivity in terms of the relationships between Weltanschauungen. Thus, "... a necessary condition for objectivity is that the behaviour of an inquirer be capable of being observed" (op. cit., p. 149).

Kuhn, as we have seen, does locate the origins of the 'potentially-successful-insight' which starts off a programme of research in one great man (though his descriptions often come close to this); he locates the essential creativity of science in the community. And so with Hegel and Churchman. As Lauer says, Hegel differs from the simple idealist tradition partly in"... his refusal to accept the individual's rational insight as the ultimate criterion of truth" (op. cit., p. 36). And Churchman argues for an "appropriate organization of science" and the "... design... (of)... a society in which the possibility of progress, i.e., the ideal seeking, is made secure" (1974, p. 460), again on the basis that the "objectivity" of experience is to be based on some kind of interconnection of observers" (1971, p. 149). For Hegel the philosophy which summarizes all that is best in an age, is, likewise, not to be thought of as a single philosophy but, rather, "... the complementarity of diverse simultaneous philosophies..." (Lauer, op. cit., p. 47). Progress is, then, seen in terms of the development-through-combination of ideas.

Kant's failing was that his solution to the development of ideas presupposed the very rationality that the development was intended to secure. Hegel attempts to avoid this problem by starting the developmental process from an "initial minimal awareness" (Lauer, op. cit., p. 2) so that the rational is not presupposed. This is also Churchman's solution: we have, he says, to "... begin with unclear material which is not an input" (1971, p. 20). Similarly, Kuhn starts scientific development from the 'crude' statement of an idea. The initial idea cannot be justified as reality and yet it is the only place to start, so reality must be achieved by the development of the idea. We are now, logically, forced to worry about justifying the developmental process. As development could not take place within any one thought it must of necessity take place by the conflict between thoughts and, particularly, through the overthrow or negation of existing thoughts. This is the only logic possible, and the logic is called by Hegel 'dialectics'. Thus, the first level of guaranteeing the development of ideas is a logical justification. This is a logic which

> "... follow(s) consciousness from its first act of minimal awareness which had to be true, because it affirmed no more than that it was awareness, through all the successive purifications which were forced on it by its own realization that it was not adequate to the implications of its very awareness, until it reached a form of awareness which was adequate to its content..." (Lauer, op. cit., p. 12).

But what is it that guarantees this logic? This is a question which Churchman raises and answers only implicitly.

<u>The Already Given Object as Guarantor</u>:

Hegel insisted that no thought can be said to be a true thought - a fully developed thought - until <u>all</u> the implications of it had been worked out. There has, that is, to be a development of the 'content of consciousness' as minimal awareness "... to the (ideal) totality of awareness in complete rational knowledge" (op. cit. p. 2). Stopping the process at any point short of the 'absolute' means inevitably that the thought is 'abstract' and not fully 'concrete'. This insistence of the fullest possible development of the idea to the absolute is a distinctive feature of his philosophy. It is more usual to argue that knowledge can be developed to different degrees and still be knowledge. Not so for Hegel because he is not concerned with what is usually taken to be knowledge. He is interested in "... follow(ing) human consciousness until it reaches a level beyond which it cannot and need not go" (op. cit., p. 13). That is, the totality of knowledge. Hegel is concerned with the logic whereby consciousness catches up with its incipient content (just as Kuhn is concerned with the fulfillment of the promise of a paradigm). True knowledge is the state when the fully developed idea (Absolute Spirit) is adequate to the whole of reality (Absolute Idea).

We have here an Hegelian statement of OR's ideal of inquiry into the whole system.

In implementing this ideal Hegel, as Lauer puts it, "... remains within the tradition of transcendental thinking, whose concern is to establish the necessary conditions for thought to be true" (op. cit., p. 35). This is also a tradition which Churchman follows. He ends his (1971) book with the question "What kind of a world must it be in which inquiry becomes possible?" (p. 277). For Hegel the guarantor of thought becoming absolutely true - as an integrated totality of thought - is the pre-existence of absolute reality as an integrated totality. Hegel's dialectic, as the movement from the in-itself through the for-itself to the in-and-for-itself, does not, as Lauer points out, "... make any sense... as an identification of thought and reality... unless reality, too, consists in an interconnected totality" (op. cit., p. 37).

This reality is a thought-reality - that is, it is only real in thought - but the products of the development of thought are seen as revelations or manifestations of what was always (Hegel says eternally) given even in the most abstract of thinking. A well-known student of Hegel describes the dialectic in the following way:

> "What does "dialectical" mean? ... Fundamentally, a dialectical movement is one in which a positing - an initially unmediated, immediate position, a thesis - is negated or "sublated" (tollere) in its immediacy (in the sense of being removed), in that the sense-contents already implicit in the thesis are uncovered in their contradictoriness and posited as antitheses, and the contradictions, "pushed to extremes", negate themselves as such and are sublated to a result, a synthesis, "come about" from these sense contents" (Werner Marx, 1975, p. 78, my emphases).

Again, as Lauer puts it:

> "It is true enough to say that reality is truly real only when it is thought-reality; but it is equally true that thought is truly thought only as a process of coming to terms with the real" (op. cit., p. 3).

For Hegel, then, the development of knowledge is nothing more than progressive 'awareness' of all that is already given - to-be-discovered:

"A truth may be discovered today which was not known yesterday, but it does not become true in being discovered" (Lauer, op. cit., p. 19). Recognising that "the progressive awareness of itself that Spirit achieves through history will be a gradual revelation of all that was there from eternity..." (op. cit., p. 4) is the cornerstone of the Hegelian scheme is the reason why his dialectical logic is guaranteed to be progressive.

Churchman recognises the need for a guarantor of process, that is to say, a guarantee that process will be progressive, and he wonders whether the Hegelian synthesis can in fact be designed (1971, p. 177). He also says that he finds Hegel's free use of the concept Absolute Mind "uncritical" (op. cit., p. 178). In short, he thinks that Hegel's is difficult to operationalize. Nevertheless, he advocates Singer's "... theory of inquiry ... (which)... says that when all is going well, and the data and hypothesis are mutually compatible, then is the time to rock the boat, upset the apple cart, encourage revolution and dissent... (because he thinks that this is the)... only pathway to reality..." (1971, p. 199). Thus, he gives the "observer-of-the-subject" a grant of objectivity because of his attempt to reach agreement through disagreement (in an earlier work (1961a, p. 151) he had called this process "judgement"). He currently advocates it as "dialectical".. He says that

> "... the process is dialectical, which means that two opposing processes are at work in the inquiring system. One is the process of defending the status quo, the existing "paradigm" of inquiry, with its established methods, data, and theory. The other is the process of attacking the status quo, proposing radical but forceful

paradigms, questioning the quality of
the status quo" (ibid).

As we have seen, the only basis on which dialectical processes are obviously guaranteed to be progressive is by presupposing the very thing which Churchman finds problematic about Hegel's scheme, namely, Absolute Idea. It is only by presupposing reality that the straightforward (i.e., unqualified) advocacy of criticism alone as the source of progress. Churchman offers us only the Hegelian "... idea of an "object" as a collection of interconnected observations" (1971, p. 149). The basic problem of objectivity, as he sees it, is to find a way of organising "... a set of representations of an object... (so as to)... capture the essence of the object" (ibid). In the next chapter I shall go into the necessary presuppositions for these views. We shall find that they negate OR's ideals of free inquiry into complex-totalities.

Churchman attempts to support his suggestion that dialectical processes are progressive by emphasising that in such a process "... the most important decision makers are the heroes, those inspired by the heroic mood to depart from the safe lands of the status quo" (op. cit., pp. 200-1). This again is an expression of the revolutionary ideal of OR, and Churchman sees its implementation in terms of "... intuition... (which)... is the force that breaks through the apparently insuperable conflict of ideas to create a new position which can observe the conflict and pass beyond it" (op. cit., p.262). Very much the same role seems to be envisaged by Kuhn when he claims that he has provided, through his category of 'construct paradigm', a philosophical justification of the origins of scienc as an "insight" which resolves the conflict of warring factions in a crisis period. This concept again presupposes Absolute Idea. The hero for Hegel was the philosopher whose role was, by participating in the totality of thought, to reveal that totality more fully. For Hegel "... the history of philosophy reveals... that each thinker thinking his thoughts is simply the overriding Spirit thinking itself" (Lauer, op. cit., p. 41). Philosophers may not intend this but by their revolutionary stance towards current thinking they invariably carry out their role as history determines it:

> "(Great men)... may be called heroes, inasumuch as they have derived their purposes and their vocation, not from the calm, regular course of things, sanctioned by the existing order; <u>but from a concealed fount</u> - one which has not attained to phenomenal, present existence - from that inner Spirit, still hidden beneath the surface, which, impinging on the outer world as on a shell, bursts it in pieces, because it is another kernal than that which belonged to the shell in question" (Hegel, 1956; quoted by Althusser, 1969, p. 91, my emphasis).

Conclusion:

From his acceptance of both Hegelian-type problems and Hegelian-type solutions, we can see that Churchman is fully implicated in the Hegelian scheme. The major feature of the Hegelian scheme for our purposes is the necessity of presupposing the whole system (Absolute Idea) and the related suggestion that the focus of inquiry should be the development of ideas. We have seen Churchman designate this as the first challenge to reason (1968a), and from his involvement in the Hegelian scheme we can understand why he sees inquiry in this way. The most important problem in the Hegelian scheme is the development of ideas so that they 'catch up' with the content which, even in the early stages of their development, they implicitly are assumed to have. It is on the basis of this assumption that Hegel (and Churchman) encourages daring speculation and intuition so as to reveal this implicit content in all its complexity. The bias of this scheme is that the object of knowledge only enters in as a presupposition; it is not taken seriously as a crucial feature of the acquisition of knowledge because it is assumed to already be present in our ideas. Put in this straightforward way, of course, the scheme looks truly fantastic! Yet there must be some basis (or, more accurately, bias) for the persistent neglect of real-world structures and processes which Churchman has shown. How could there possibly be a science of improvement which is not founded on an understanding of the systems which it is intended to improve? This neglect is not confined to Churchman. We have seen how other operations researchers have been biased away from a study of the nature of the systems with which they are concerned by their involvement with philosophical positivism. A similar bias is evident in Kuhn's conventionalism, a philosophy to which some operations researchers are already leaning, and towards which others may be expected to turn if the lead given by prominent OR academics is followed. Hegelian philosophy provides another bias away from concern for subject-matter by its emphasis on intuitions. As Piaget says of this strategy:

> "... if I observe in myself "intuitions" that I experience, from the first I only observe the <u>already elaborated</u>, instead of

> observing the process of formation; and then what I see is so bound up with my conception of it and so dependent on my intentions of finding this or that, that it becomes utterly impossible to trace with certainty the boundary between the "intuitions" of the introspector and that of the introspected... If an "essence" is both a concept of the subject and the phenomenal nucleus of the object... how are we to know if the essence is "true" without examining separately the experience of the object... and also the logic used by the subject to elaborate his concepts" (1971b, pp. 111-114).

Taking the reality of the whole system as a taken-for-granted backdrop which is to be progressively revealed by the development of ideas implies that whatever conception of the subject-matter is implicit in our ideas has only to be developed. It need not be questioned, and this injects a strong bias towards accepting dominant versions of the nature of the subject-matter. This, I think, is the Hegelian justification for the acceptance of tradition which we have seen both Popper and Kuhn advocate on different grounds. We will see this anti-revolutionary bias in action in Part Two. There we shall find operations researchers arguing (in Chapter 6) that the role of OR is to discover and refine the traditional wisdom which is to be found in social systems. This is OR's theory of management. In Chapter 7 we shall find operations researchers accepting the dominant tradition in sociology (structural-functionalism) as a way of providing support for its theory of management. This is OR's theory of social systems, and we shall see that the key feature of it is the injunction to take the nature of the social for granted.

Before we turn to these matters I wish to conclude, in the final chapter of this part, with an analysis of the impact of the neglect of subject-matter on OR's ideals-particularly the ideals of grasping complex totalities. Churchman argues against a strategy of inquiry which suggests starting from the 'simple' and progressing to the 'complex'. He suggests that, in some way, we have to start from the complex. I agree, but the Hegelian starting point is Absolute Idea. The question thus arises as to whether the Hegelian notion of Absolute Idea, even supposing that Churchman's desire for its operationalisation was fulfilled, is truly the presupposition of a complex totality.

CHAPTER FIVE

CONCEPTS OF TOTALITY

Introduction:

In this chapter I shall briefly examine some notions of totality and assess their viability. From Chapter One it is clear that the most significant of all OR's ideals is that of dealing with the whole system. Churchman and others have seen the necessity of developing the idea of totality rather than merely reiterate it as an ideal. To the extent that positivism has a notion of totality (and we have seen that there are doubts about this) it is completely opaque: it is the totality of 'phenomena'. Conventionalists attempt to think of totalities in terms of ideas. To the extent to which they try to make ideas-as-totality objective (as Kuhn does) they revert to either a positivist notion or a metaphysical notion. Kuhn's is a metaphysics of the social system, as we shall see in Chapter 7, because the model which both he and OR adopts is incapable of investigation in terms of generative structures and processes. Churchman opts for the Hegelian notion of totality whereby a totality is intuited in the attempt to capture the underlying whole system. A similar notion of totality informs the systems approach, of which a major exponent is Ackoff. Let us look first at the implications of this approach before turning explicitly to Churchman's notion.

The Adequacy of the Concept of Totality Employed by the Systems Approach:

Ackoff argues strongly for the idea of a "... system... (as)... a set of two or more interrelated elements of any kind..." which have the following properties:

"1. The properties of behaviour of each element of the set has an effect on the properties or behaviour of the set taken as a whole...

2. The properties and behaviour of each element, and the way they affect

the whole, depend on the properties and behaviour of at least one other element in the set...

3. Every possible subgroup of elements in the set has the first two properties... A system cannot be subdivided into independent subsets " (1974, p. 13).

The central message of this view of totalities is that "a system is more than the sum of its parts" (ibid). There are certain logical consequences and action imperatives which can be drawn from this definition and Ackoff does this ably both here and elsewhere. These are not my concern. Ackoff means this to be a perfectly general definition of wholeness, and the question which is of concern is the nature of the strategy of inquiry which such a notion involves.

Ackoff's view of wholeness is that it is the emergent property which is more than the mere interaction of the parts. This view, however, is but one of two possible alternatives to the atomism which he roundly rejects, as the only other competitor to his preferred alternative. The alternative which Ackoff does not discuss (nor anyone else in the OR and related literature of which I am aware) is that termed by Piaget (1971a) 'Operational Structuralism'. In Ackoff's view, the whole, by being more than the sum of the parts, is primary to the parts. Referring to his definition, there are no independent subsets because of their relation to the whole. This emphasis on the primacy of the whole is shown again by the role of the researcher whose interest is focussed on the total or overall behaviour of the whole. This emphasis is misplaced from an epistemological point of view as it takes the whole's existence for granted. This can be shown by contrasting the 'emergent totality' view of Ackoff with the operational structuralism described by Piaget:

"Over and beyond the schemes of atomist association on the one hand and emergent totalities on the other, there is, however, a third, that of operational structuralism. It adopts from the start a relational perspective, according to which it is neither the elements nor a whole that comes about in a manner one knows not how, but the relations among elements that count. In other words the logical procedures or natural processes by which the whole is formed are primary, not the whole, which is consequent on the system's laws of composition, or the elements" (Piaget, 1971b, pp. 8-9, my emphasis).

If we ignore a system's 'laws of composition' and pass straight on to emphasise merely its wholeness, we actually bypass the 'wholeness' of a totality. As Piaget says: "As a first approximation, we may say that a structure is a system of transformations..." (p. 5). "(T)he elements of a structure are subordinated to laws, and it is in terms of these laws that the structure qua whole or system is defined" (p. 7, my emphasis).

Although this perspective on structure is not at odds with Ackoff's inasmuch as"... the whole... (has)... overall properties distinct from the properties of its elements", these properties of the whole are conferred on it by the "... laws governing a structure's composition..." (ibid). The emphasis on the laws of transformation (construction) is at odds with Ackoff's notion of wholeness because it places complete emphasis on the outcome of such processes. This bias is highlighted by Harvey (1973) who emphasises the epistemological imperatives which flow from Piaget's notion. He says that "this concept of totality... leads us on to ask how totalities are structured and how those structures change" (p. 289, my emphasis). These are questions which Ackoff never feels compelled to ask. I shall give an explicit illustration of this neglect in Chapter 7.

The same questions follow from the realist philosophy which we briefly encountered at the end of Chapter 2. Keat and Urry (op. cit.) summarize the realist view of explanation

> "... in the claim that answers to why-questions (that is, to requests for causal explanations) require answers to how- and what- questions. Thus, if asked why something occurs, we must show how some event or change brings about a new state of affairs, by describing the way in which the structures and mechanisms that are present respond to the initial change. To do this, it is necessary to discover what the entities involved are: to discover their natures or essences" (p. 31).

The reasons why we must ask such questions is clear. Unless we are to give up our commitment to totalities we must ask how 'structure' comes about and how it is preserved and how it changes. How could we possibly do this without an investigation into its laws of construction? Unless we ask these questions about construction we are faced with the alternative of totality without origin and structural stability without effort (and we also completely rule out change except by external influence - perhaps a comforting thought for operational researchers!) As Piaget points out: "Were it not for the idea of transformation, structures would lose all explanatory import, since they would collapse into static forms" (p. 12). See also, Allen, 1975, on this point. We shall see in Chapter 7 that OR does in fact entertain a notion of organizations as static, given wholes.

Given that the very notion of totality carries with it the corrollaries of structure, transformation, construction, and self-regulation, what legitimate relationships are there between this 'object' and the researcher (subject)? If we adopt Ackoff's notion of totality, whereby the pre-given whole determines the parts, we have a justification for seeing the really significant relationship between the researcher and the totality consisting of the researcher's conceptualisation of the whole. We have seen him argue that systems are "... subjective insofar as the particular configuarion of elements that form... (it and its environment) ... is dictated by the interests of the researcher" (1972, p. 84). Emphasising this role of the researcher is justified with Ackoff's notion of totality because, as Piaget says, "... viewing the whole as prior to its elements or contemporaneous with their "contact"... simplifie(s) the problem to such an extent... (that it)... bypass(es) all the central questions - questions about the nature of a whole's laws of composition" (op. cit., p. 8). Seeing totalities in terms of the whole which dominates the parts (a conception which forms the basis of OR's social theory, as we shall see in Chapter 7) legitimates subjective conceptualisation of the totality because, not being concerned with the laws of transformation which structure the whole, the researcher can rest content with a view of the whole which 'works'. That is, he has done all that can be expected of him if he is able to provide an account of the gross properties of the whole. In terms of a strategy of inquiry, this makes OR identical to statistics. The concept of totality as a 'simple-unity' (the whole) reassures the researcher that he need not be concerned with more than phenomenal appearance. It is this concept of totality as simple unity which legitimated that remarkable statement by Beer (1966, p. 119) which we have already quoted, namely, that "... the OR man is a special kind of scientist, for he does not have to bother with determining the laws governing basic natural phenomena..." And he says this after having staked out the claim that OR was scientific on the basis that it attempted to uncover a system's "real processes"! The notion of totality held by the systems approach does not require such an understanding, as Beer comes to realize.

The philosophies with which OR is implicated exert, as we have seen, a persistent bias towards grounding any theory of inquiry in the nature of the subject (as regards positivism, recall Popper's emphasis on bold conjectures), and away from an analysis of the object. This we have now seen is the implication of the notion of totality currently expressed in the systems approach. However, if we accept the idea of structure as being constitutive of wholeness then we have also to accept, as Piaget puts it, that

> "The notion of structure is not at all reducible to a simple formalisation due to the observer's mind: it expresses, on the contrary, through its formalisations ... properties constitutive of the structured "being" (1971a, p. 109).

Compare this statement with one by Beer (and such statements are to be found passim) that "... coherence, and pattern and purpose are, all three, acts of mental recognition rather than characteristics of physical things" (op. cit., p. 243, my emphsis). Beer goes on to say: "... the detection of a system in the world outside ourselves is a subjective matter" (ibid). Is this all? Compare Piaget again: "Structures are not simply convenient theoretical constructs; they exist apart from the... (observer)... for they are the source of the relations he observes a structure would lose all truth value if it did not have this direct connection with the facts" (1971a, p. 112). And again: "... understanding or explaining is not just a matter of applying our operations to the real and finding that "it can be done". Such "application does not break through to causes; it keeps us within the realm of laws. Causal explanation requires that the operators that "fit" the real "belong"to it, that reality itself be constituted of operators" (p. 40). Beer, as we would expect, reverts to Hume on causes and argues that they only exist in the mind (see: p. 121). For Beer being 'in the mind' is good enough, and so long as totalities are thought of in terms of simple unities it is good enough. However, as soon as we understand that totalities are constructed and that neither the whole (nor the elements) are primary but the system's laws of composition, the system itself must be brought into account. It is only with a concept of totality which emphasises this that problem of knowledge becomes "... determining how knowledge comes to terms with the real world, and therefore what relationships obtain between subject and object" (piaget, 1972, p. 6). It is not with the concept of totality entertained by the systems approach that "the theory of knowledge is... essentially a theory of adaptation of thought to reality, even if in the last analysis this adaptation (like all adaptations) reveals the existence of an inextricable interaction between the subject and the objects of study" (p. 18).

Emphasising the object should not in any way be thought of as letting positivism slip in through the back door and with it the re-entry of the strategy of inquiry which we dismissed in Chapter 2 as incomprehensible. Structural knowledge must, as Piaget says, have structures as their source but this does not mean that what the researcher gains as knowledge is the real object which could be called 'the' structure. I shall come back to this.

With the notion of totality as a simple, overarching unity there is not pressure to go beyond giving 'plausible' accounts of real processes. Once this notion of totality is discarded, however, the pressure to give theories a special kind of content becomes overwhelming. I believe that this can even be seen from Kuhn's descriptions of the development to 'mature' science. If we ignore his philosophical and sociological interpretations it is possible to understand his descriptions of the development to maturity as the acquisition of models which are closer and closer to the nature of their subject-matter.(see, for example, his description of the developments in explanations of electricity and the composition of the atmosphere, which show this particulrly well). Kuhn himself, as we have noted, argues that maturity has something to do with the type of 'boxes' into which scientists try to force 'nature', but elsewhere he argues that he can see no progress in the 'ontology' of various sciences. However, I think that his reservation, being based on the fact that he can detect no 'linear' progress, does not weaken the case that there has indeed been a 'development': restricting 'progress' in this way does not seem appropriate.

Still, we do not have to rely on either reading between the lines or on Kuhn's opinions. Others have argued on general grounds that a necessary condition for scientificity is a grasp of the nature of the subject-matter being dealt with. Rom Harre, a realist philosopher and social scientist, has argued against what he sees as "the neo-positivist idea of science... (which)... was that the compilation of a catalogue of laws describing... regularities completely exhausted the content of a science... The sole virtue of a theory, on this view", he goes on, "is to bring order into the mass of observational and experimental data. Provided that order is produced, neo-positivists are not much concerned with the meaning of those concepts which appear... in the theory..." (1974, p. 241). As our previous discussion has shown, Harre's description of 'neo-positivism' accords well with the prevailing view in OR. Against this he argues that " a real scientist tries to discover what produces patterns which he observes; that is, what mechanisms are responsible form them. He requires that his theories have a certain kind of content" (ibid). Arguing in very similar terms to Piaget - who points out that "... structures are not observable as such, being located at levels which can be reached only by abstracting forms of forms or systems of the n^{th} degree; that is, the detection of structure calls for a special effort of reflective abastraction" (1971a, p. 136) - Harre goes on to say that at some "... point in most sciences... observations temporarily comes to a halt, and for the next step we are forced to resort to the scientific imagination" (ibid), and imagination which is disciplined. And by what? Well, he says "it is not good just imagining any kind of process that might produce observed patterns. To be scientific one must imagine processes and generative mechanisms which are proper and appropriate to the problem in question ... The model and the unknown real mechanism must be functionally equivalent" (pp. 241-2). In Harre's view, then, "scientific models are iconic; that is, they are imagined things, processes or structures" (ibid).

Compare this view with that of Beer's. He argues that "a scientific model is a homomorphism..." (1966, p. 113, my emphasis) which means, literally, merely a similarity of form or a superficial likeness (Collins New English Dictionary). An isomorphism is a model which is similar in structure. Beer only allows models to be isomorphic with each other (ibid), and he chooses the homomorphism because it reduces complexity to manageable levels. Complexity must be 'managed', but it matters greatly in which way this is done, and the way Beer chooses negates the scientific conception of totality.

If knowledge of totalities is knowledge of laws of composition, then the adaptation of thought to reality is contingent on the activity of attempting to understand the totality in question. It is activity which produces scientific understanding, and not merely reflection. As Piaget puts it, "each scientific "fact" is: (a) an answer to a question; (b) a verification of "reading off"; (c) a sequence of interpretations, already implicit in the very manner of asking the question, as well (unfortunately or fortunately) as in the verification as such, or the "reading off " of experience, and explicit in the manner of understanding the answer given by reality to the question asked" (1972, p. 125). The kind of questions that we need to ask are those directed towards uncovering the structure and functioning of complex totalities. Let us turn now to Hegel's concept of totality to see if it permits this type of question.

Hegel's Concept of Totality:

As we have seen, Churchman is a confirmed Hegelian, and this remains true notwithstanding his 'critique' of Hegel. The concern of his critique is to make Hegel's dialectic explicit, and not to question its fundamentals. Churchman argues for a vision of the whole system to guarantee progress through Hegelian dialectics. What kind of totality could this be? If we interpret Churchman as calling for an operationalisation of the Hegelian concept of totality we get some answers which he should not, and to some extent, does not like. On the other hand we have little choice. We either say that Churchman shares Hegel's concept of totality (although he finds it somewhat vague), or we say that Churchman has no concept of totality. The latter seems out of the question so perhaps the truth is that Churchman is undecided. If this is the case perhaps he will forgive us making up his mind for him by burdening him with the Hegelian concept of totality with which he is most strongly associated.

At first sight Hegel's dialectic seems to offer the prospect of progression to an ever more complex vision of the whole. Althusser (1969) says that it appears that with the Hegelian dialectic "the further we progress in the dialectic of its production, the richer consciousness becomes, the more complex is its contradiction" (p. 101). This is a result which Churchman would like to achieve as is shown by his advocacy of the concept of the "maximum loop" (1968a, Chapter 7). This concept (which is closely related to his notion of "exoteric" inquiry) comes from the belief that "... reason is the process by which man is able to look at himself... (and) ... that the way anything can look at itself is through a series of frameworks". The final conclusion is that "... a social institution becomes rational to the extent that it can be considered to function like some other institution". That is, "... the rationality of reflection comes from using as much of the world, or the "whole system", as one possibly can to understand oneself" (p. 105). However, employing the Hegelian concept of totality Althusser argues that this is not, in fact, a movement towards increased complexity and richness at all. Discussing Hegel's conception of the dialectic as the movement of the Idea he notes that Hegel "... projects this movement on to the reality of scientific labour, ultimately conceiving the unity of the process from the abstract to the concrete as the auto-genesis of the concept, that is, as a simple development via the very forms of alienation of the original in-itself in the emergence of its end-result, an end-result which is no more than its beginning" (1969, p. 189; cf. Piaget, 1972, pp. 115-116).

Churchman, in his 'critique' of Hegel (1971, pp. 176-7) queries the necessity of having only a contradiction composed of two opposites. But, as Althusser points out, "... if we take the rigorous essence rather than the metaphysical sense of the Hegelian model, we can see that the latter does require this 'simple oroginal unity with two opposites"..." (op. cit., p. 197, cf. also, pp. 194-5). As he earlier puts it, "... this ' simple process with two opposites' in which the Whole is split into two contradictory parts is precisely the very womb of Hegelian contradiction" (op. cit., p. 195). He goes on:

> "this is the original unity that constitutes the fragmented unity of the two opposites in which it is alienated, changing even as it stays the same; these two opposites are the same unity, but in duality, the same interiority, but in exteriority - and that is why each is for its own part the contradictory and abstraction of the other, since each is merely the abstraction of the other without knowing it, as in-itself - before restoring their original unity, but enriched by its fragmentation,

> by its alienation, in the negation of the abstraction which negated their previous unity; then they will be a single whole once again, they will have reconstituted a new simple 'unity', enriched by the past labour of their negation, the new simple unity of the totality produced by the negation of the negation" (op. cit., p. 197).

And what of the concept of totality which progress by simple contradiction presupposes? The Hegelian dialectic, Althusser concludes, "... is completely dependent on the radical presupposition of a simple original unity which develops within itself by virtue of its negativity, and throughout its development only ever restores the original simplicity and unity in an ever more 'concrete' totality" (ibid). "The Hegelian totality is the alienated development of a simple unity, of a simple principle, itself a moment in the development of the Idea..." (op. cit., p. 203). This is the concept of totality which is demanded by the notion of progress through simple contradiction, and as we have seen in our discussion of operational structuralism it cannot be taken as an adequate concept of totality at all. As Althusser says, "... negativity can only contain the motor principle of the dialectic, the negation of the negation, as a strict reflection of the Hegelian theoretical presuppositions of simplicity and origin" (op. cit., p. 214). And Althusser speaks almost directly to Churchman's notion of the "maximum loop" when he goes on to say that "... contradiction is a motive force for Hegel as negativity, that is, as a pure reflection of the 'being-in-itself even in being-other-than-itself', therefore as a pure reflection of the principle of alienation itself: the simplicity of the Idea" (ibid). Referring back to our previous discussion of the systems approach, this similar notion of totalities as simple, presumably justifies Churchman's emphasis on the 'intuiting' role of the designer.

Thus, he says that "... the selection of a definition of "system" is a design choice, because throughout this essay it is the designer who is the chief figure. In other words, whether or not something is a system is regarded as a specific choice of the designer" (1971, p. 42). And as far as the designer is concerned "the differentiating feature of systems is that they can be separated into parts and that the parts work together for the sake of the whole" (op. cit., p. 49, my emphasis); this view is again, Churchman emphasises, a 'design choice'. "What is of chief interest to the designer is the relationship of the parts to the whole system" (op. cit., p. 50); "what matters most to the designer is that he can conceptualise how a decision maker can change (design) a part and the change makes a difference in the performance of the whole" (ibid). That is, the whole is an emergent property prior to its elements, and the interest is in how the parts 'coincide' with the whole. This interest of the designer, Churchman notes, can be manifest in different ways (he can, for example, treat the parts separately or simultaneously) all of which merely "... express the attitude of the designer" (op. cit., p. 51). The nearest we get to recognising that the system may have a structure of its own is a note to the effect that there is a reality 'underlying' the designer's conceptualisations, and that the designer's conceptualisations may be wrong. Thus: "In any choice the designer makes, he may identify the components incorrectly and/or measure their effectiveness incorrectly. Thus he may believe that he understands how M (the whole) is related to the m_i (the parts), ... but he may be wrong. This rather obvious point merely states that there is a real relationship between both the true M and the true M_i, and the designer's estimates of these" (p. 51). We never hear of 'real relationships' again.

The neglect of the real by Churchman would follow from his commitment to the Hegelian notion of totality. In its bypassing of the central question of the laws of construction which make up a totality, the Hegelian notion of totality implies the "... absence of any real structure in the whole... " (Althusser, op. cit., p. 209).

The simple original unity which Churchman's notion of totality implies is therefore in its nature hostile to the discovery of structures. In the Hegelian dialectic each 'movement' is merely an expression of internal unity; each 'part' has but an ephemeral existence and because all have significance only with respect to the given totality, there can never be one dominant part (or contradiction in the dialectic). All parts are 'equal' within the totality because all are merely expressions of the single unified whole. Differences are only posed to be negated. They are phenomena which can always be righted with respect to the dominant unity. We shall see this concept of totality working overtime in OR's social theory.

This hostility to the notion of structure goes, I think, a long way towards explaining why, as Mitchell (1973) has put it, "OR has failed to structure, in a way sufficiently persuasive to have a major impact, any problem of major significance in the world at large" (p. 4): OR neglects, and is in every way hostile towards, the very idea of structure. How can one understand and, more importantly, change a totality without grasping and operating on its structure? With the concept of totality prevalent in OR circles we hae not, with the absence of any concept of real structure, moved beyond the 'anything goes' strategy of inquiry which seemed dominant in Chapter 1.

It should be clear that sweeping in complexity is but a corollary of dealing with structured totalities. Understanding that totalities are complexly structured by their laws of construction and transformation means that whatever complexity there is is given in these relatonships. This means that the 'facts' as they appear to us are merely representative of an underlying complexity, which it is the researcher's task to discover. The complexity of facts does not only lie in the interrelationship which they have with theories. As Althusser puts it,the existence of structured wholes implies that "... every 'simple category' presupposes the existence of the structured whole... (That is,)... simplicity is merely the product of the complex process" (op. cit., p. 196). "Facts", then, are only to be understood in terms of the complex whole, which implies their location in terms of the laws, or as Althusser puts it, the 'conditions of existence' of the system (cf. Allen, 1975). Thus, as regards Churchman's conundrum, facts do depend on a view of the whole system, but the whole system conceived of in terms of its laws of composition.

Revolutionary Inquiry:

Clearly, concern for the nature of the subject-matter is merely the corollary of bringing the object into inquiry. Finding out what is 'revolutionary' about this sweeping in of the object takes us back to the subject-object relationship, and back to the question of what it is to have knowledge of structures. Recalling OR's persistent ideal of being "revolutionary", I suggest in this section that the realist-type approach satisfies this condition which is required for a strategy of inquiry to be acceptable to OR. One possible case for arguing that bringing the object in is revolutionary comes from Piaget's argument (and to some extent demonstration) that knowledge as the product of interaction between the subject and object always involves a transcendence of the object. It will be recalled that this is the complete reverse of Kuhn's view of revolutionary science in which the object was given. That is, in Kuhn's view of revolutionary science the object does not enter in, but always lies beneath the activity of the scientist. We have seen that a similar conclusion holds for Hegel, and hence Churchman. For Piaget, on the other hand, although the real object lies beneath and is not knowable as such, "... perception never operates alone: we only discover the properties of an object by adding something to perception..." (1972, pp. 51-2); that is, "... one does not understand the properties of an object except by acting upon it and by transforming it, in the same way that an organism does not respond to the environment except by assimilating it, in the wides sense of the term" (op. cit., p. 48). The views of both Kuhn and Churchman, therefore, contrary to all appearances, are neither revolutionary nor processual. For both the object is at best a source of stimuli which are interrupted and merely 'interpreted' by the subject: the object is 'given' to the subject, and is not constituted through interaction, as it is for Piaget. As Harvey puts it for Piaget "the subject is thus regarded as both structuring and being structured by the object" (op. cit., p. 298); Israel makes the same point when he notes that for Piaget "a thing is not an object of knowledge until the knowing organism interacts with it and constitutes it as an object" (1972, p. 129). There is, he points out, a truly dialectical relationship between subject and object (ibid) whereas for both Kuhn and Churchman the relationship is subject determined.

Because he is himself a structuralist, Piaget sees the growth of knowledge, in terms of a process of interaction between subject and object, as the development of operational structures or laws of transformation which constitute, in their various stages, points of equilibrium representing for that stage a totality. Knowledge, then, as a product of subject-object interaction, is seen in terms of structured totalities. Structures, then, are the conceptual outcome of subject-object interaction, the products of 'reflective abstraction', which, partly because they are this product of 'inquiry', do not merely passively receive experience but always reconstitute it within that structure while at the same time reconstituting the structure itself: this is 'perpetual revolution'. Piaget's psychological interpretation of this dialectical relationship between subject and object is in terms of 'assimilation' - 'facts' are integrated within a previously built up structure - and 'accommodation' - where a structure is applied to a new situation and through assimilation it is differentiated and changed. The structure is continually being built up and modified through interaction with the 'real', and the latter - the 'given' - is being continually transcended through its integration and interpretation within the structured totality.

At the level of consciousness, when employed as an explicit strategy of inquiry, bringing the subject-matter in is revolutionary on two counts: firstly, we search for a structure, that is, for "... a system of internal relations which is in the process of being structured through the operation of its own transformation rules" (Harvey, op. cit., p. 290); secondly, we take the 'given', the 'facts' and what is 'obvious' and attempt to locate them within the structured whole to find out what they mean. The stance is obviously that facts do not have immutable meanings as transcendental 'objects'; their meaning changes as 'knowledge' changes. It implies that 'facts' can only have meaning and existence as part of a pre-given structured complex whole, and it challenges the researcher to look beneath the given to discover the totality and in so doing to transform the given. As regards its objectives, this strategy of inquiry has much in common with phenomenology, where the task is to get beneath the world of 'common-sense' and, in its sociological usage, understand the production of this world. However, as Piaget says, "... phenomenological problems as much as one wishes, but not the phenomenological method, not as long as it remains confined to the philosopher's consciousness..." (1972, p. 111).

Like Piaget, Althusser understands inquiry as a dialectical 'production', as a transformation of the given ('raw material') into 'knowledge'. He sees (as does Piaget) the production of scientific knowledge as essentially open-ended; it has no final resting place as is envisaged by Hegelians (although the latter see knowledge as continual process without end, this is the endless cycle of becoming its own destiny) genuinely new problems (an impossibility for Hegelians) are possible and therefore genuinely new knowledge is possible; knowledge 'grows' and does not merely 'become' (see: Shotter, 1974, for a discussion of this difference in strategic outlooks with respect to social psychology).

As Althusser points out, we never start from an objective 'given', from "... pure and absolute 'facts'" (p. 184). Instead we always start with the 'abstract', with the pre-constituted, as our 'raw material'. The simple being the product of the complex we cannot take the simple as it stands, as it presents itself to us. Science does not start with the 'facts' but, as Althusser puts it, "... its particular labour consists of <u>elaborating its own scientific facts</u> through a critique of the <u>ideological 'facts'</u> elaborated by an earlier ideological theoretical practice. To elaborate its own specific 'facts' is simultaneously to elaborate its own 'theory', since a scientific fact - and not the self-styled pure phenomenon

- can only be identified in the field of a theoretical practice" (op. cit., p. 184).

The realist-type philosophy, therefore, is compatible with OR's ideal of revolutionary inquiry, whereas the philosophies in which OR is currently interested are incompatible with it. With positivism it is impossible to transcend the given because the given is the guarantor. With conventionalism and idealism the given is implicit and can only be interpreted. But from what basis is the given to be transformed and, in being so transformed to be transformed itself? I have concentrated on the importance of explicitly dealing with the object of inquiry, but what is the nature of the subject which does the inquiry? These questions refer to the very difficult and important problem of the relationship between OR and the scientific disciplines, one specific instance of which (the relationship between OR and the social sciences) I take up in Part Two.

Revolutionary Inquiry and the Scientific Disciplines:

An ideal which derives from OR's overriding ideal of dealing with the whole system is that inquiry should have an interdisciplinary basis. En route, however, we have had occasion to question whether, when set within the philosophical frameworks which inform thinking in OR, this derived ideal was understandable. From a positivist point of view interdisciplinary teams 'cover the ground'. Apart from the fact that this view has its basis in phenomenalism we saw that for its justification it relied on the existence of a mysterious meta-logic. From a conventionalist and idealist point of view interdisciplinary teams engage in process and the supposition is that they progress. We have seen that the idealist claim of progress through contradiction required the pre-existence of a simple totality for this claim to be justified. However, even if we put questions of the validity of the underlying ontological presuppositions to one side, it is hard to understand this claim as a practical proposition. In practice the process has to be a social process, and we cannot understand how this process works (and, therefore, whether it will be progressive) without an understanding of these processes from a social point of view. Our understanding cannot be complete from a purely philosophical point of view, and yet Churchman is reluctant to examine the viability of his strategy from a social point of view (see: 1971, pp. 180-185). Others, including Kuhn of course, have not been so reticent, and in the final chapter I shall assess the social basis which has been put forward by some operational researchers to justify the progressive nature of process. In providing this background operations researchers have extended the interdisciplinary team idea to an ideal which was always incipient within it, namely, to sweep into the process of inquiry all the members of the social system being studied (cf. Churchman's notion of the 'well-informed public', 1968a).

From where we stand at the moment, therefore, the interdisciplinary team ideal is difficult to understand in the ways in which it is currently interpreted. The difficulty with which operations researchers have had with the ideal of interdisciplinary inquiry stems, I think we can see, from an initial hostility towards the subject-matters of science, we have seen (particularly in Chapter 2) that this hostility is the product of commitment to philosophical outlooks which bias attention away from the object of knowledge. OR has been very keen to be something different from just another science, and has recognised the inadequacy of basing attempts at improvement on sciences which were not developed to be anything other than themselves. Because of its bias against subject-matter OR has been unable to see how it could be something different from scientific disciplines without being entirely something else. That is, OR has been unable to find a way of using the scientific disciplines in a way that is consistent with its philosophical commitments. The unfortunate thing is that it has given priority to its philosophical commitments and not to its ideals, because by banishing scientific disciplines its ideals have been forsaken. The use of the scientific disciplines is not, however, inevitably incompatible with either its ideals or its justified desire to be something different. We should bring in scientific disciplines, and with them subject-matters, and use them in ways which are more than merely psychologically suggestive sources for homomorphic models, in order to meet OR's ideals.

Let me briefly indicate how scientific disciplines fit within the philosophical outlook which I have used as a basis for prising out some of the implications of OR's current philosophical interests. For Althusser the means whereby knowledge is produced is called by him Generality 2. That is,

> "... the corpus of concepts whose more or less contradictory unity constitues the 'theory' of the science at the (historical) moment under consideration, the 'theory' that defines the field in which all the problems of the science must necessarily be posed (that is, where the 'difficulties' met by the science in its object, in the confrontation of its 'facts' and its 'theory', of its previous 'knowledges' and its 'theory', or of its 'theory' and its new knowledges, will be posed in the form of a problem by

and in this field)" (Althusser, op. cit., pp. 184-5).

For Piaget it is only through 'application' that structures of knowledge are transformed, and Althusser makes the point that the outcome of inquiry ('Generality 3') is never identical to either the raw material ('Generality 1') or the means of production. Existing structures work on the given to produce new knowledge. For both Piaget and Althusser existing structures of knowledge are a point of departure; for Kuhn a paradigm is simultaneously a beginning <u>and an end</u> - paradigms exist to be <u>fulfilled</u> (as do Hegelian world-views). Althusser proposes the word 'problematic' in an attempt to differentiate between knowledge seen as fully consistent 'world-view' and it considered as a complex totality which is not necessarily internally consistent. In the latter sense knowledge is a <u>way</u> of asking questions (op. cit., p. 229), rather than being the <u>source</u> of both questions and answers. A problematic is, as a paradigm is not, a <u>method</u>; a paradigm is a solution. As Piaget points out, "... structuralism is a method, not a doctrine" (1971a, p. 142).

Let me finally, and very briefly, mention the view of Piaget on the relationships between the sciences which stems from his understanding of the inevitable interrelationship between subject and object in the growth of knowledge. Because, as he says, the relationship between subject and object is 'circular', so is the unity of science a circular unity. He describes what he has in mind in the following way:

> "... the object is never understood except through the individual's thought processes, but the individual does not understand himself except by adapting

himself to the object. Thus man cannot understand the universe except through logic and mathematics, the product of his own mind; but he can only understand how he has constructed mathematics and logic by studying himself psychologically and biologically, or in other words, as a function of the whole universe. This is the true meaning of the circle of the sciences: it leads eventually to the conception of unity through interdependence between the various sciences, such that disciplines on opposite sides of this cyclic order maintain reciprocal relationships with each other" (1972, pp. 82-83).

The unity of the sciences, and the role for OR, is based on a grasp of the complementarity of understandings which the development of the sciences should, according to the ideals of OR, bring us. This sounds like Churchman's notion of the rationality of the maximum loop until we realise that the sciences are complementary by providing an understanding of complementarity of real structures and processes. If there is truly a whole system it will be understood as the structures and processes on the basis of which the seemingly disparate aspects of life are related.

Conclusion:

This chapter really stands as the conclusion to Part One, on the relationship of OR to philosophy. I have attempted to trace through the various philosophical associations which have been built up in OR circles and have shown some of the implications of these for its ideals. The overall conclusion, quite clearly, is that the impact of these associations is by and large to lessen the possibility of OR achieving its ideals. The major reason for this is that the philosophies exert a strong and consistent bias against a serious consideration of the subject-matter of OR. I have tried to show that this bias persistently operates against the ideals of OR. However, we leave this part with some unfinished business which is taken up in Part Two.

The preoccupation of this part has been an examination of the philosophical foundations of OR. Operations researchers have resorted to philosophy (sometimes implicitly) in an attempt to operationalise their ideals. In one sense, the overall conclusion of this part shows that philosophy is not likely to help OR in this task. This is because philosophy, broadly speaking, is usually understood as inquiry without a subject-matter. By this I do not mean that philosophers do not talk about anything. Rather, I mean to suggest that philosophy properly understood does not reveal truths about the world. Its concern (at least as it interests us) is with discussing, criticising, and sometimes suggesting schemes which tell us not so much how to acquire knowledge, or what valid knowledge is, but more fruitfully what knowledge is not. It is inconceivable that philosophy could tell us in advance of inquiry what rue knowledge was or what it will look like. There is only one way to gain knowledge of the world, and that is to investigate it. Philosophies, if they attempt to posit the nature of the world before an attempt is made to find out what it is, may hinder us in our attempts to acquire knowledge. If it is suggested that knowledge exists only in our senses or that knowledge exists only in our ideas, then there is a fruitful role for philosophical investigation, and that is to point out that these suggestions, if taken seriously, would impose arbitrary limitations on our ability to acquire knowledge. This, as I understand it, is the justification for and the role of the realist-type view which I have been counterposing to the strains of positivism, conventionalism, and idealism which are to be found in OR. This is why I have not advocated a specific philosophy and claimed that it would solve OR's need for a theory of inquiry. This is why I have referred to the philosophical viewpoints I have employed as critical tools as realist-type. Realism, (or Structuralism, or Wittgensteinianism) is a term which refers not so much to a philosophy as a criticism of philosophies which arbitrarily limit inquiry. If OR has need of a philosophical foundation it is a need for a set of philosophical tools which will ward off such encroachments into its liberty.

If OR cannot look to philosophy to find secure foundations, we must look for its foundations in its attempts to come to grips with the real world. To do this we must move away from philosophy and attempt to understand OR on the terms in which it perceives its subject-matter. Notwithstanding the philosophical attempts to banish subject-matter with which OR is associated, it is impossible to operate without some theoretical understanding of the nature of the object being investigated. This much at least we can learn from conventionalism. The unfinished business which we take up in Part Two is the nature of this theoretical understanding as it is found in OR circles. Although I have attempted to show that OR cannot achieve its ideals without a grasp of its subject-matter, if this grasp is an inadequate one it will just as surely be prevented from achieving them. The theoretical structures which inform thinking in OR circles which I wish to discuss in Part Two are those which inform their owners about the nature of the social world. I do not claim that these theoretical structures are the only ones held in OR, or even that they are the major ones. I do claim that they are major, and that their impact on the likelihood of OR achieving its ideals is profound, and at the moment that this impact is a profoundly negative one. However, notwithstanding this conclusion, the growing interest in the social sciences within OR circles is a healthy one. It would be intolerable for OR to neglect the social world because to do so would be to deprive itself of its most potently revolutionary ally. This is so because as Piaget has so cogently put it (many others have made the same point):

> "Whereas other animals cannot alter themselves except by changing their species, man can transform himself by transforming the world and can structure himself by constructing structures; and these structures are his own, for they are not entirely predestined either from within or without" (1971a, pp. 118-119).

The real structures with which OR deals and theorises about can be changed by those structures themselves. As Harvey (op. cit.) has pointed out, theories which are perfectly 'true' of the social world (in that they enable us to make reliable predictions, etc.) can nevertheless be falsified by that world. This is why the theories of social process which we shall find are being entertained within OR circles are so encouraging. However, it is also the reason why their failure to grasp the real nature of these processes is all the more depressing.

PART TWO

THE SUBJECT-MATTER OF OPERATIONAL RESEARCH

Introduction:

A writer who has shown great concern over the subject-matter of OR is Ackoff, whose early attempt to define its nature was considered in Part One. In a more recent article (1972) he introduces his readers to concerns which will dominate our attention. Ackoff says that:

> "The concept system has come to play a critical role in contemporary science (Churchman, Emery). This preoccupation of scientists in general is reflected among Management Scientists in particular for whom the systems approach to problems is fundamental and for whom organisations, a special type of system, are the principal subject of study" (p.83).

Ackoff goes on to devote 1½ pages (including the conclusion and the references) out of a nine page article to discussing organisations, and most of what he has to say, apart from minor changes in terminology, is repeated from his 1961 article in a collection on progress in operational research. Churchman (1974) defines all OR as social OR, and he says nothing, either in this article or elsewhere, on the nature of social reality (he has a section on 'Social Reality' which only discusses the nature of goals as counterfactuals). In 1964 a conference was held on the problem of OR and the Social Sciences' (Lawrence (ed), 1969); in 1968 another conference was held on Approaches to the Study of Organizational Behaviour (Heald (ed), 1970) and the subtitle was Operational Research and the Behavioural Sciences. The outcome of both these conferences was that the relationship between OR and the social (or behavioural) sciences was a poor one. There was much mutual recognition of the value and importance of the work done on either side and promises and hopes for joint work and close collaboration were expressed.

Such evidence as there is on the relationship between OR and the social sciences seems to point very strongly to the conclusion that there is a large gap between them. This is not to say that there have not been partially successful efforts in bridging this gap (particularly by Friend and Jessop, 1969, and more recently, Ackoff and Emery, 1972) and I shall give these works close attention in chapter 7. But even these efforts have accepted that they were taking on a task which was made difficult because OR and the social sciences are currently very different animals. Many operations researchers would agree with the views of Bennis who appeared at the 1964 conference as a behavioural scientist sympathetic to the task of forging a tie-up between OR and the social sciences. For Bennis the major difference lay in the fact that OR and behavioural science deal with different sets of 'variables': OR deals with the 'hard' variables such as stocks, money, machines, numbers of workers etc., whereas organization theory deals with the human end of things which includes leadership, resistance to change, satisfactions, and so on. Some operations researchers would like to bridge the gap which exists in our attempts to relate these variables in our problem-solving attempts, but this is all sad evidence of their being a gap to be bridged.

In the face of this evidence it might appear foolhardy to suggest (as I do) that OR and the social sciences share a common theoretical orientation towards the nature of the social world. It should be noted that by the term social sciences I do not wish to suggest that the social sciences in general all share one theoretical outlook. This is not the case. Which particular social sciences I have in mind regarding this shared orientation will become clear as we proceed. I should like in the rest of this introduction to at least weaken any strongly held convictions that OR and the social sciences are obviously different as regards their theoretical orientation towards the nature of the social world.

One way of doing this is to look at the kind of demands which operational researchers typically make of social scientists (these demands are, in fact, complemented by the services typically offered to OR by those social scientists who are interested in bridging the gap).

We have seen earlier how Ackoff has lamented the fact that OR has not been very interested in opening the 'black box' of behavioural variables and his interest in extending the scope of OR models to include them so as to 'cover' the total system - which he sees as being completely defined as a man-machine (or, in more current terminology, 'socio-technical') system. This may seem a completely natural move to make given OR's commitment to the whole system, but what else is Ackoff looking for? Ackoff's view at that time (1960) was that

> "in the operation of a system...the aspects of the system which can be <u>manipulated</u> so as to improve its performance are likely to come from many different disciplines"

(p.25, my emphasis) including, of course, the behavioural sciences. One of Ackoff's interests in the behavioural sciences, therefore, is the enhanced ability to manipulate the system which is provided by the ability to manipulate behavioural variables. As OR is only seen to 'sub-optimise' by not including behavioural variables, what is being asked for is the control of behavioural variables. [1] The concern is to eliminate behaviour which is deviant with respect to the organizational purpose which OR is attempting to promote. That this is the attitude of operations researchers to the behavioural sciences is also revealed by the introductory comments of Sir Charles Goodeve to the 1964 Cambridge conference.

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(1) That this is still his interest is shown in Ackoff and Emery. op cit. p.11.

Sir Charles writes:

> "Operational Research people are very much concerned with change and can deal with the logic, including the economics of it. But attitudes of people - managers, technicians, workpeople, salesmen, customers, etc. - can throw the best of predictions into confusion" (p.12).

He goes on to state what he sees as

> "the main object of social scientists... (namely)...to increase their knowledge and in particular their ability to predict" (p.14).

Cook, in his introduction to the 'Organisation and Control' section continues the theme of predictability. After noting that the social sciences can offer help to the operations researcher in improving organisational effectiveness by providing some knowledge of the "far greater" level of complexity of the system where "human characteristics cannot be ignored" he comes to the main point of contact which he sees between them. He writes:

> "...operational research men see the importance of human factors in at least two areas which are vital to operational research.
>
> First, to design a good management control system which will work as predicted, they need to know much more about the likely behaviour of the human components of the system.
>
> Second, in trying to implement operational research proposals, whether for some sophisticated management

> control system or for much simpler change of method, they often find human resistance to change to be a major obstacle. They need all the help that social science skills or theories can provide"
> (p.24).

Cook was not completely disappointed by the contributions which followed (although Burn's paper, he notes (p.89), was criticized for being "negative"). Bennis, summed up his views on the possible and desirable contribution of the social sciences to 'planned organizational change', the banner under which he saw a nexus between OR and the social sciences. He writes:

> "What we know least about - and what continually vexes those of us who are vitally concerned with the effective utilization of knowledge is <u>implementation</u>. As I use the term, implementation encompasses a process which includes the creation of <u>understanding and commitment</u> in a client system towards a particular change which can solve problems, and devices whereby it can become integral to the client's operations. It bears to organisational theory the same relationship as the term <u>internalisation</u> does to personality theory, i.e. it is a process which leads to <u>automatic</u> self-generation and integral functioning.
>
> When it comes to implementation of organisational changes, most practitioners seem to overemphasise the importance of intellectual <u>understanding</u> or the <u>informational</u> status of the intended change.

Now, as I have said, information and understanding are a necessary, but not a sufficient, component for inducing change. More than that is required if the change affects important human responses. For human changes are bound up in the self-image and its maintenance and the complicated context of the social life and groupings which help to define and give meaning to the individual's existence. If an intended change is perceived to threaten (or enhance) the self-image, then we can expect <u>differential effects</u>. If an intended change is perceived as threatening the social 'life space' of the individual, then safeguards must be undertaken which ensure new forms of gratification and evaluation"

(p.73 - second, fourth and last emphases mine; first, third, fifth and sixth emphases original).

Churchman and Emery likewise advocate "the joint consideration of two research strategies" (1969, p.82) on the basis that:

> "There has been a marked reluctance to recognise that which is obvious to any who has had to directly command a body of men. An aggregate of stones has such extremely weak field properties that we can usually ignore them. An aggregate of human beings readily constitutes a powerful contagious social field, more or less inclined to shared emotions and behaviour of hostility, docility, loyalty, flight, fight, etc."
> (ibid).

Finally, we can refer to a more recent contribution to the problem of the interface between OR and the social sciences in the work of Huysmanns (1970) whose focus of interest was 'implementation'.

Being concerned to "...bridge the gap between social scientist and technical operations researcher that underlies the gap between scientist and manager" (p.15) Huysmanns argues for collaboration around the "previously proposed implementations strategies... (which were)... being mainly the work of social scientists...usually centred around the problem of relaxing the human constraints that inhibit managerial adoption of a research proposal" (p.31 my emphasis) and the OR approach to costing the relaxation of these 'constraints'. Thus he goes on to argue:

> "There is no obvious reason why only technological constraints should be considered in the research proposal, while human constraints should be left as implementation problems, other than the fact that the researcher does not know how to integrate the human constraints in his research" (p.31).

The desire to predict behaviour stems from the widespread acceptance within OR circles that an organisation can be defined in terms of its goal. (1) Thus, for Ackoff (op cit):

> "An organisation is a purposeful system that contains at least two purposeful elements which have a common purpose relative to which the system has a functional division of labour; its functionally distinct subsets can respond

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(1) I have more to say on this in chapter 7.

> to each other's behaviour through observation or communication; and at least one subset has a system-control function"
> (p.90).

See also the definitions given by Churchman, Ackoff, and Arnoff (1957), Stringer (1967), Huysmanns (op cit), Beer (1966). Apart from the fact that this definition is also orthodox in the social sciences (Albrow, 1973), the desire to predict behaviour, with this definition as a background, must be based on the presumed ability to be able to distinguish behaviour which is in accordance with the organization's goal from that which is not. Both operations researchers and social scientists seem confident that they can distinguish these two types of behaviour. If OR was not confident about this we might expect worries about what it would do with its increased predictive powers if it had them. Such worries are not strongly evident. What does the ability to distinguish organizationally relevant from organizationally irrelevant behaviour which is shared by OR and the social sciences depend on? Bittner (1973) provides us with an answer when he shows us "...how sociologists go about distinguishing the facts of formal organization from the facts of informal organization" (p.265). One way in which they could do it, he suggests, is by employing a rule such as the following:

> "In certain presumptively identified fields of action, the observed stable patterns of conduct and relations can be accounted for by invoking some programmatic constructions that define them prospectively. Insofar as the observed stable patterns match the dispositions contained in the programme they are instances of formal organisational structure. Whereas, if it can be shown that the programme did not provide for the

occurence of some other observed patterns which seem to have grown spontaneously, these latter belong to the domain of the informal structures"
(p.265).

Whether or not the formal/informal distinction does really correspond to organisations relevance or irrelevance, the ability to construct and apply a rule of this type is prerequisite for both OR and social science. Without such a rule they could not make the kind of distinction which was evident in the above quotations. We can now ask on what basis does the joint application of such a rule depend?

Rules do not implement themselves, as Bittner recognises. As he puts it, their use depends on common-sense concepts (ibid). Zimmerman (1971) has discussed this problem in greater depth. The problem he tackles is "...the question of <u>what it takes</u> to warrant the application of any rule - formal or informal - in concrete situations..." (p.223). The "...crucial question", as he sees it, is "the judgmental processes that members must use to employ rules on relevant occasions" (ibid). Zimmerman was concerned to understand 'the practicalities of rule use' by members of an organisation. However, we need not restrict his comments to this sphere; as Bittner (p.276) says, we are concerned with the "methodical" use of rules by any socially recognised "competent user". Both operations researchers and social scientists are mutually recognised as competent users of the rule which distinguishes organizationally relevant from organizationally irrelevant behaviour. Zimmerman argues that as such rules cannot implement themselves (no rule, for example, carries with it a complete specification of the situations in which it is to be employed, etc.) there must be "...sanctioned courses of common-sense judgment that members use to recognise, to interpret, and to instruct others..." (p.224) on what they mean. The 'reasonable' use of a rule, so the ethnomethodologists argue, is judged against the background of both tacit and explicit understandings of 'what anyone knows' is 'normal'. Kuhn (1962) makes a similar point when he argues that scientists do not first learn a scientific law and then learn to apply it. The law and the situations where it is 'normally' to be applied are learned together by ostension. To learn how to apply a scientific law or a rule to distinguish behaviours in organizations, in Kuhn's terminology, is to learn a paradigm. The paradigm (or world-view) provides the common-sense background context within which the application of a law or a rule makes 'sense'.

The unproblematic way in which operations researchers and social scientists both imply that they can apply the rule referred to above requires that each should have, and suggests that they share, a paradigm or a set of background assumptions about the nature of social organizations. Much effort has been devoted by some social scientists in unearthing and assessing the dominant paradigm in the social sciences (called by Gouldner, 1970, "domain assumptions", and Friedrichs, following Kuhn, the "paradigm"). I shall show in chapter 7 that OR shares this paradigm and I will show some of the implications on the likelihood of OR achieving its ideals which flow from this fact.

Before we can proceed towards a substantiation of our thesis, however, it is clear that in one sense at least OR has traditionally thought of itself as being distinguished from other management sciences by the level of its interest in organizations. We noted earlier on that it was a prevalent view in OR circles that one of its distinguishing features was a concern for 'the whole'. Thus, OR, by this account, is distinctive in being concerned with those responsible for the whole organization, namely, management. OR is not simply interested in organizations as phenomena to be improved. Rather, it is interested in how organizations are to be improved by improving the way in which those organizations are 'managed'. If this is a vague concept which can easily be (and often is) claimed as a focus of interest by other of the management sciences, this is all the more to the point. Perhaps the story of the basic nature of OR is really to be told in its theory of management. This is a possibility that we must look into, and I turn to this task in the next chapter. From this analysis we shall find a very convenient entry into an exposition of our suggest relationship between OR and the social sciences. OR's 'theory of management' - or, to anticipate our conclusion, OR's lack of a theory of management - presupposes a theory of organizations. That is to say, attempts to develop OR's theory of management show very clearly that it is impossible to understand how to manage an organization without first understanding what an organization is. In fact to the extent to which OR can be said to have a theory of management it is submerged in its theory of organizations.

CHAPTER SIX

OR's THEORY OF MANAGEMENT

Introduction:

OR's theory of management has its origin in the wholly familiar homily in OR circles that 'OR works for a decision-maker'. Even today this idea must be seen as one of the key (if inadequately developed) heuristics in thinking about the practice of OR. An early recognition of the importance of the idea of OR working for a decision-maker is to be found in Churchman, Ackoff, and Arnoff (1957). They state that:

> "Before we can formulate a problem we should have some ideas as to what a problem is. That is, what are the components of a problem? First, and most obvious, is the fact that someone or some group must have the problem. This individual or group is dissatisfied with some aspect of the state of affairs and consequently wants to make a decision with regard to altering it. For this reason we shall refer to this individual or group as the decision-maker. Where the decision-maker controls the operations of an organised system of men and/or machines, he may also be referred to as the policy-maker, or executive. The decision-maker is the first component of the problem"
> (p.107).

Much of the criticism of this notion has centred around the difficulty of finding the decision-maker, yet this attack misses the mark for (as noted by Churchman, Ackoff, and Arnoff, p.108) no-one who formulates this basic proposition of the nature of OR's subject matter should assume that it is straightforward to identify 'the' decision-maker. This assumption does not follow from their proposition.

OR's theory of management is a theory of the decision-maker. It is a theory which explains the nature of his values and his role in the problem-solving process. Little effort has been devoted to the development of this theory, so questions of reviewing the literature do not arise. In the light of this fact I shall adopt the strategy of giving an extended consideration of only two efforts at the construction of a theory of management for OR. Firstly, I shall consider the well-known paper by Stringer on 'Operational Research for Multi-Organizations' (1966) for which he won the Operational Research Society Bronze Medal. Secondly, I shall turn to a further analysis of Beer's work on 'Decision and Control' (1966). These two pieces of work have to represent the views of the OR community. However, as the whole question of OR's theory of management will be seen to depend on OR's social theory (of which there are important elements in the works of the two writers considered in this chapter which will be brought out in chapter 7), exponents of OR's social theory complement the writers considered here.

A several key points in the arguments of the two exemplars of OR's theory of management whom I consider there are very striking gaps. The way in which these gaps appear and the arguments which surround them will suggest ways in which they could be coherently filled. At these key points, therefore, I shall draw upon relevant concepts from elsewhere to highlight the full meaning of the arguments presented.

SECTION A:

OR in Multi-Organizations:

Introduction:

Stringer's writing is of particular interest because he attempts to provide a foundation for doing OR in what he perceives is an abnormal social context. We shall be particularly interested in the way in which he deals with this abnormality.

Historically, Stringer notes, the normal context for the practice of OR is industrial. There is, he says, an "...air of confidence which nowadays surrounds a great deal of industrual operational research" (p.106). His paper, however, is concerned with "...a less comfortable point of view" (ibid) which comes from contemplating the practice of OR in the public sector. He argues that as compared with the prviate sector, the services (and goods) provided by the public sector have "...a special structure to the problems of managing them" (ibid). His argument, essentially, is that the 'special character of public problems' means that:

> "The concept of a "decision-maker" with his "problem" which is the formal basis of most writings on operational research, no longer applies and current operational research methodology, centred on the idea of the "optimum decision", may thus prove inadequate" (ibid).

His argument rests on what he sees to be the distinct social contexts in the private and public sectors. In the private sector there can be sensible talk of 'optimum' decisions because the social context of 'a' decision-maker allows for it. In the public sector, on the other hand, such talk is meaningless because the social context of "...a multitude of "decision-makers" who influence the allocation of resources" (ibid) will not allow for it. In the private sector OR can work with easy confidence, on this argument, because the social backdrop provides a reality against which OR can work. One would have expected that this being so Stringer would have argued strenously for an attempt to come to grips with this new social reality in its own terms. This move, of course, rests on the assumption that OR had accepted as a fact of its existence that it depended on social realities and that, in the nature of things, these could be different, and in so being they required different orientations. This is what Stringer appears to be arguing. However, the appearance is deceptive. As his argument unfolds it will become clear that the apparent recognition of different social realities characterising the public and private sectors is overshadowed by his treatment of the social reality of the public sector as an epiphenomenon of a basic reality - a reality which is typified in the private sector. As he sees it, the first task of OR when it is faced with the novelty typified by the public sector is not to tackle this on its own terms, but to transform an apparent reality into a basic one. The basic reality which Stringer takes as typical of the private sector, is summed up under the concept of 'organization'; the epiphenomenon is the 'multi-organization'.

Organisations and Multi-Organisations:

As Stringer sees it, an organization

> "...may be defined for the present as comprising a set of connected interests involving people, resources and channels of communication, and established in such a way as to be recognisable as an entity. An organisation (an industrial firm,

a hospital board, or a partnership of architects, for example) has unifying characteristics, e.g.:

(i) It has imposed upon it, or is capable of defining for itself, a set of goals ultimately applicable to all its parts.

(ii) It has established means for pursuing these goals.

(iii) There is some ultimate expression of the organization's authority as an entity (Its internal structure is usually hierarchical, and there is a "boss" or a "board of directors".)

(iv) It has a permanance which transcends particular tasks."
(p.107)

"A multi-organization, on the other hand, is the union of parts of several organizations, each part being a subset of the interests of its own organization. It is defined by the performance of a particular task (which may be a continuing one) through the interaction between individuals. It therefore constitutes a "socio-technical system"; that is to say, a system in which social and interpersonal relationships are partly conditioned by the task - and vise versa" (p.107).

The crucial distinctions which Stringer appears to be drawing are that: (i) whereas an organization has a clearly defined overall goal structure a multi-organization is a "...situation where parts of several organizations - each with its own affiliations, its own goals and its own values - are all involved in the achievement of a plan or an end-result" (p.106). This difference implies for Stringer that it is possible to unambiguously define 'improvement' and 'optimum' in the case of an organization, but that it is not possible in the case of a multi-organization. (ii) Whereas an organisation has, following its overall goal structure, a matching authority structure which will "sanction" the research and "overcome difficulties", the corrollary of the multi-organization is that no such 'sponsorship structure' exists. Instead of there being the (presumed) harmony of interests, in the multi-organisation case there is "...conflict between the objectives of the component..." (p.119). It might appear as though Stringer intends these distinctions to depict different social realities. This, however, is not the case as has been pointed out by Bevan (1975). Bevan argues that if (as seems reasonable) Stringer is attempting to contrast the 'unitarist' with the 'pluralist' frame of reference - which are nothing if they are not attempted depictions of different social realities (see: Fox, 1969 and 1973) - his attempt fails because if these differences are taken as real they are because of that fact incommensurate and so, without drastic modification, it is not possible to apply concepts such as improvement and optimum to both. Bevan argues that Stringer's usage of terms such as improvement, optimum, interests and conflict all betray a basic commitment to the 'unitarist' frame of reference. In short, Stringer's concept of organization is dominant throughout. Bevan's arguments will be verified here. We shall see in the following sections how Stringer collapses the two, apparently distinct, realities into one. In doing this he draws on a particular 'theory of management'.

We can sum up the distinction which Stringer is making between organizations and multi-organizations by the concept of 'unity': the organization has it; the multi-organization does not. This being so, Stringer's subsequent arguments amount to the proposition that OR is impossible unless carried out within the context of a cohesively unified social entity. This proposition we shall find in chapter 7 is dominant in most of the thinking about the nature of OR's subject-matter. Stringer's reaction to the multi-organization is to transform it into a cohesive unity; only then can OR begin.

Stringer declares as a first principle of OR the necessity of unity:

> "In its absolute need of sponsorship by those who are affected, operational research may be distinguished from many other, descriptive, forms of research"
> (p.108).

Clarifying what is meant 'by those who are affected', he argues that even "scientific objectivity", which he takes to be unproblematically available when we can rely on the <u>pre-given</u> existence of an organization's goal strucutre, may have to be

> "...sacrificed to enable a satisfactory sponsorship structure to be built up. This is essential if there is to be the opportunity to undertake research which is <u>beneficial to the purpose which defines the multi-organisation</u>"
> (p.109, my emphasis).

The problem which Stringer foresees here is that in building a sponsorship structure the operations researcher has explicitly to take sides. Not being able to rely on the pregiven sponsorship structure (and the concommitment goal structure) OR has, as it were, to step back one pace to create one. This, as Stringer's comments imply, can be a messy business, but he draws courage from the altogether remarkable assertion that there is a purpose which defines a multi-organization. But how could he possibly assert this in the light of his definition of a multi-organization?

We can see how he attempts to justify it by recalling that a multi-organization, according to Stringer is "...defined by the performance of a particular task" (ibid). Where does this 'task' come from and how is it recognised? It cannot be from the goals of the member organizations. It is likely, for example, that the various 'organizations' in the Health Service would define 'the task' very differently. On what basis, therefore, can Stringer assert that the task exists and that it is unifying? The clue which Stringer offers us is a reference to Trist et al (1963) as the originators of the concept of socio-technical system. It appears that Stringer views a multi-organization as a socio-technical system. One of the major assumptions of systems theorists is that systems have identifiable boundaries. The socio-technical systems theorists argue that these boundaries have two dimensions - a technical and a social dimension. A major development of the socio-technical systems idea was made by Miller and Rice (1967) when they suggested that a social system had a "primary task". The concept of 'the task' goes unexplained in Stringer's paper, and yet it is clearly the crucial concept if multi-organisations are defined in terms of it. Stringer must have some justification for taking it for granted that such a task exists. As a former director of the Institute for Operational Research he would be well aware of the work of Miller and Rice (members of the parent Tavistock Institute) which provides what amounts to a theoretical justification for his indifference. Let us look briefly at their work. Stringer is preoccupied with making proposals for 'organizational' design. I will show how these proposals rest on the background provided by the work of Miller and Rice.[1] If a multi-organization is defined by its task, a preoccupation with organizational design questions alone suggests that the existence of the task (which the organization is designed to achieve) is taken for granted.

The Given Task:

Miller and Rice reveal the universe of discourse in which they are operating when they assert that "any enterprise may be seen as an open system which has characteristics in common with a biological organism" (p.3). Clearly, the implication is that enterprises can be treated from the analyst's point of view as if they were part of the natural world. That is to say, the analyst is entitled to conceptualise organizations as autonomously occuring and functioning phenomena to which the generalisations which apply to biological arganisms may, by analogy, be applied. Organizations are taken, in this view, to obey the laws to which biological organisms must conform with the convenient theoretical result that properties which may, by virtue of the dominant (Darwinian) paradigm, be taken for granted in the biological sphere, may also be take for granted in the organizational sphere. Thus, the starting assumption of biology that organisms struggle for survival, where 'survival' "...occurs when a line of behaviour takes no essential variable outside given limits" (Ashby, 1952, p.43), can be taken over into the analysis of organisations, so that

> "We ...(can)...postulate that at any given time an enterprise has a <u>primary task</u> - <u>the task that it must perform if it is to survive</u>"
>
> (p.25).

(1) Miller and Rice are, of course, part of a much longer tradition in the social sciences (of. Silverman, 1970). This tradition and its relation to OR are discussed in chapter 7.

The primary task of Miller and Rice corresponds to the cluster of essential variables which an organism has to protect. In the biological sciences the existence and maintenance of a primary task can, literally, be taken as a fact of life. Surviving organisms by definition have, and are continuing to fulfill, a primary task. Also, more than this, the biological sciences can take it that the organism's primary task has been arrived at through processes of interaction with the environment such that it could be nothing other than it is. The primary task is <u>not</u> arrived at by <u>choice</u>. It is determined by the 'nature' of the organism (which may have evolved) and the 'nature' of the environment (which may also have 'evolved'). The biological sciences can, then, start their analysis of the behaviour of the organism with the secure knowledge that in studying the influence of the primary task on behaviour they can assume its existence. It is determined in nature. The biologist is often interested in explaining the origin of the current primary task, but he will do this by explaining the changing nature of previous primary tasks. At each point he can take a primary task for granted: at each point in time he can take it that the primary task has been determined by the organism's history.

The biologist's strategy for explaining the behaviour of an organism can justifiably, therefore, start from the given primary task. Miller and Rice also adopt this strategy of explanation as regards organizations, but it is not easy to see the justification for this (oc. Child, 1972; Silverman 1970). They merely <u>assume</u> that the primary task arises from interactions between the nature of the organization and the environment. Thus:

> "We have said that any enterprise any be considered as an open system, that exists, and can only exist, by exchanging materials with its environment. It imports materials, converts them, and exports some of the results.

Its outputs enable it to acquire more intakes, and the import-conversion-export process is the work that the enterprise has to do to live. The task of any enterprise can be defined in the most general way, therefore, as to secure a payoff by converting intakes into outputs - the minimum payoff being the postponement of death....

The primary task...makes it possible to construct and compare different organizational models of an enterprise based on different definitions of its primary task; and to compare the organisations of different enterprises with the same or different primary tasks" (p.25).

The primary task, then, is the product of the nature of the organisation and its environment. Although they call the primary task an "essentially heuristic concept" it is seen to "...determine the dominant import-conversion-export system, and the operating, as distinct from the maintenance and regulatory, activities. It specifies the resources required and hence determines the priorities of the constituent systems" (pp.25-6). Also, they go on to say that:

"The primary task is not a normative concept. We do not say that every enterprise _must_ have a primary task...(and then either go out to check whether they have one or try to install one)...or even that it must define its primary task; we put forward the proposition that every enterprise, or part of it, _has_, at any given moment, one task which is primary" (pp.27-8, original emphasis).

Their argument is that although there may be conflicts over the definition of the task an underlying primary task exists. Thus they say:

> "In the analysis of organization, the primary task often has to be inferred from the behaviour of the various systems of activity, and from the criteria by which their performance is regulated. One may then be able to make make such statements as: 'This enterprise is behaving as if its primary task were...'; or: 'This part of the enterprise is behaving as if the primary task of the whole were...'. Such formulations may be compared with explicit statements by the leaders of the enterprise and of its parts about their definition of the primary task"
> (p.27).

The resort to a behavioural definition of the primary task - the primary task is what organisations do regardless of what they or others might say - is unsatisfactory. That an organization behaves in a certain way may be explained, perhaps, by citing the primary task, but not if primary task means only behaving in a certain way. If we observe the behaviour of an organization and say that this behaviour is indicative of a primary task then surely we are not entitled to say that the primary task is this behaviour. This resort to a behavioural definition, then, reveals the fundamental nature of the role of the concept of primary task. It has the definitional circularity that one would expect of a truly basic and undefined starting point.

We can just note in passing that no such troubles beset the biologist; he does not require a behavioural definition of the primary task. It is defined independently of behaviour in terms of physiological variables, a move which rests on a developed understanding of the nature of the subject-matter being dealt with.

For Miller and Rice the primary tasks exists on its own, oblivious of its recognition. Awareness of it is necessary only because an "inappropriate" definition or conflict can "jeopardize" survival (p.28); also, on the other side of the coin, awareness of it can further its promotion by guiding a facilitating organization design. An organization that is not cognizant of its primary task is, by this approach, disorganised. Thus:

> "...(I) organization is regarded primarily as an instrument for task performance, we can add that, without adequate task definition, disorganization must occur" (p.28).

<u>Organization and the Primary Task: Stringer's Design Proposals:</u>

The giveness of the primary task provides a touchstone for organizational design. This background explains, I feel, why Stringer devotes most of his paper to resolving what he sees as the disorganization of the multi-organization. In the normal case the organization is in tune with the primary task. This being so OR can get straight on with promoting the primary task with no worries that its efforts will be thwarted by (in the terminology of Miller and Rice) 'human constraints'. In the multi-organization case the 'organization' is not necessarily in tune with the primary task, and this must be put right before normal OR can get to work. In concluding his paper Stringer writes:

"The problems which arises in such a crucial form in attempting to do operational research in the public sector could become the growing point for a new methodology concerned with the problems of designing organizations; with perfecting techniques of co-ordination; with decentralised decision-making; and with the practical resolution of inconsistent objectives. It is the author's belief that methods for handling such problems are a prerequisite for the effective use of operational research in systems where there are multiple decision-makers" (op cit, p.119).

Stringer gives us the first indication that he finds the multi-organization situation disorganized as compared with the organization when he remarks that it is only by using the concept of organization is it possible to understand what a multi-organization is! Thus, he says, although I can see no pressing reason why we should agree with him,

"A multi-organization is <u>necessarily</u> a rather vague concept but some of the ways in which it differs from an organization can be stated" (p.107, my emphasis).

Stringer continues this theme of the disorganization of the multi-organization situation in the examples which he quotes. Of a building team as a multi-organization he writes that the situation was so chaotic that improvement was impossible to define - even theoretically; that the chaos was manifest not only in the absence of an overall goal structure but also in the high level of uncertainty which the lack of organization promoted. I quote him in full:

"The diversity of interests - architects, surveyors, engineers, builders, sub-contractors, suppliers, labour, clients, local authorities, building users, planners, the public, the government - emphasize there is not likely to be a single generally acceptable definition of "improvement". Thus, even at a theoretical level - quite apart from a practical one - the question "What is improvement?" was difficult....

"Apart from the diverse "values" which prevail amongst them, all members of the building team have become conditioned to working in an atmosphere of uncertainty. In addition to uncertainties due to outside factors such as shortage of labour and materials, government policy, economic and social climate, weather and so on, uncertainties are to a considerable extent self-induced, i.e. they are basically organizational. The very actions which one party takes to reduce his uncertainty, or to cope with it, has the effect of increasing the uncertainty for others.... This atmosphere of uncertainty is all-pervasive and is responsible for the style of "crisis management" which is prevalent in the industry"

"The basic problem, therefore, was how to advocate orderly change where the various parties concerned were conditioned to disorder and had different concepts of what would constitute improvement"

(p.108, my emphases).

Notice the quotation makes around the world values - perhaps this indicates that Stringer does not quite take them as real. I shall return to this later. If Stringer were to take them as real he should abandon his concept of improvement and, perhaps, engage in moral criticism of some or all of the values.

Regardless of whether this last option is taken it is clear that Stringer should not set about attempting to mold the behaviour which the values are taken to produce (or the values themselves) to be consistent with some imposed 'higher-order' values. On what basis could this possibly be justified? In the nature of the case such values are said not to exist. This inconsistency in Stringer's position is the central thrust of Bevan's criticism. But we have seen Stringer argue that there is a "...purpose which defines the multi-organization" (p.109). Given this commitment to the idea that there must be an overall purpose it is not surprising, to continue the quotation regarding the building industry example, that:

> "In the circumstances at least as much effort was devoted to setting up an appropriate structure of sponsorship for research as to the conduct of the research itself" (ibid).

Setting up this structure is regarded as "essential" if the overall purpose is to be pursued:

> "For a planning process to exist at all in a multi-organization there must be some recognition of common purpose for which the "planners" provide a focus. This central

focus is often associated with the exercise of central influence. It is situations of this sort which are discussed here"
(p.110).

Stringer identifies this 'central influence' with "top management" (he notes the relevance of his work to the "practice and problems of "top management", in industry, i.e. the level above that at which well-structured problems exist" (p.109) and his reference to "influence" and his call for help from political science in setting up sponsorship structures, implies that he is identifying the overall purpose with the most powerful. We have here, then, both of those features which were supposed by him to distinguish the organization from the multi-organization, namely, an overall purpose and a hierarchy of authority.

Stringer also concludes that to deal with a multi-organization is going to require a model of decision processes because:

"By definition, a multi-organization is not being "managed" in the ordinary overall sense, nevertheless, processes are going on within it which have managerial effects"
(p.109).

What Stringer means by the 'ordinary sense' of management is the detailed individual control which he sees as lacking from the multi-organization situation:

"That some actions are taken, and others not, implies the existence of a "decision process", which need not be an explicit procedure. It may be a haphazard system of some complexity, as the effect of a decision depends on the actions of others" (p.110).

Apart from drawing the obvious corrollary to the notion of organization, namely, that co-ordination is required, Stringer wants to see this insight employed to further the "...objectives of the... system as they are seen by the central planning authority" (p.111). This is on the principle that "...for any one decision-maker to choose the actions he should take to achieve his objectives requires an effective understanding of the behaviour of the other parts of the decision-making system" (p.110). Because 'top management' "...does not have complete control of the system...(I)t can, therefore, pursue its own objectives only by the interventions it makes in the decision-making process" (ibid). Designing these 'optimum interventions' becomes the role for OR (p.111).

Stringer's Theory of Management:

Why should Stringer so clearly and unambiguously align the efforts of OR in the multi-organization situation with the 'central planning authority'? Surely it cannot be solely on the grounds that they are the most influential or powerful (these words are used interchangeably although they could mean quite different things) because most of the effort of the OR team will be directed to increasing the influence (power?) of the 'focus' which is chosen. Stringer gives us no idea as to how this power or influence is to be measured except in terms of the self-same 'interventions' which the OR team will devise for the 'centre'. In fact, it makes as much sense to say that the 'centre' could become any group which the OR team decided to help as to say that the centre is the client who will be helped. If it is not the pre-existing power of the centre which determines its choice, perhaps it lies in the fact that the centre has goals of such a breadth (they are 'overall' goals) as are not to be found elsewhere in the multi-organization? If this is the basis for the choice, then it is surely arbitary. Any one of the groups which comprise the multi-organisation could provide such goals. Why, then, choose the goals of the 'centre' as the only ones worthy of pursuit? Stringer appears to recognise the instability of power/influence relationships and the possibility of many competing definitions of the overall aims of the multi-organization. Why, then, given the argument as he presents, it and the logical conclusions which can be drawn from it, does he persist in his argument that "for a planning process to exist at all in a multi-organization there must be some recognition of common purpose for which the "planners" provide a focus" (p.110)? Although "this central focus is often associated with the exercise of central influence" (ibid) this, by Stringer's argument, is a variable as far as OR is concerned, and the values which this central influence holds are, again by his argument, only one possible set out of many.

Far from Stringer's arguments convincing us that his proposals will lead to the ability to give "...operational meaning to such terms as "planning", "design" and "co-ordination"...(which)...will contribute to the "science" of management in multi-organizations" (p.119) (and, presumably, to the term 'improvement' as well) his arguments seem, if anything to encourage the abandonment of any such attempt. Given the background theory the notion of task looks objective and secure, but we are given no reasons for supposing that the values of any of the groups of a multi-organization coincide with it. Values could be imposed, to be sure, but how would one square this with a 'science' of management? One could choose a set of values to pursue, but no metric is given by which this choice could be made 'operational'. Stringer tells us, indeed, that he wants to "avoid injecting the operational research analysts' own set of values" (p.119), and yet it is clear that unless the questions of the choice of values is ignored altogether. OR has to be something more than a 'science' with purely 'operational' terms.

On what basis could Stringer on the one hand recognise the diversity of values in the multi-organization situation and on the other advocate the promotion of 'higher values'. The only possible basis, I suggest, is a theory (such as that developed by Herbert Simon) which justifies 'higher values' as more rational. This is Stringer's theory of management. I shall now examine Simon's theory to show how it both fills the gap in what Stringer does not way, and fits in exactly with what he does.

Recall his view that multi-organizations in the 'raw state' are disorganized and even confused. The implication of his discussion could be that each individual organization is by itself incapable of rationality. Presumably Stringer found the concept of improvement hard to understand because each individual concept of improvement with which Stringer was presented was in some way not quite adequate: recall the shy use of the term "values" applied to individual groups. What is unreal (?) or inadequate about the expressions of improvement at the individual group level is not argued. If he is assuming that a Pareto optimum could be reached whereby some could benefit but none could lose even this notion is firmly rooted in the adequacy of the <u>individual</u> values (none shall lose), and with it there is absolutely no warrant for deeming any or all of these values inadequate. Yet Stringer is not satisfied with individual values. This dissatisfaction must, therefore, be based on the grounds that the values <u>are</u> individual and not 'comprehensive'.

This is precisely the view of Simon.

There are infact many similarities between the views of Simon and Stringer. Simon was, in the words of Storing (1962), (whose account of Simon's work I follow in several places), "...one of the first to popularize the vocabulary of decision-making..." (p.65). Simon's intention was precisely to provide the "...operationally useful theories of the decision-making behaviour of the several decision-makers forming a multi-organization..." (Stringer, op cit, p.119) which Stringer calls for.

Simon, like Stringer in the case of multi-organizations, claims to start his analysis from the individual and his values (or 'definition of the situation' as it appears in Organizations, 1958) which may conflict with those of both other individuals and the organisation (Perrow, 1972, pp. 148-9). The difference here is that whereas Simon regards value conflicts as normal (wherever he takes the individual as his starting point, which he does not always do) for all organizations, Stringer takes it to be something of a special case. More importantly for the current line of argument is the parallel which exists between the role Simon sees for his Administrative Science and the role which Stringer advocates for OR. Simon argues that the individual cannot approach anywhere near the 'global rationality' which would be implied by tradition notions of what it is to be rational. The individual cannot consider all alternatives because he is an information processing system of limited capacity and, therefore he cannot be truly rational. However, this raises the problem that he cannot be rational by limiting his search by the use of his values either, as the notion of 'satisficing' behaviour implies (Storing, op cit). This is so because the operational use of values requires that they are something less than 'ultimate' values, and if they are we cannot say that the individual is truly rational. Simon attempts to circumvent this difficulty by arguing, first, that the informational demands on the individual are, in practice, overcome because he 'responds' to 'stimuli' which happen to impinge upon him. The individual does not in practice consider all alternatives but is tied down by current (and accumulated) stimuli. This clearly does not solve the problem of rational behaviour so Simon goes on to argue that instead of the randomly generated stimuli to which the individual would be exposed to in the unorganised state, the individual should be exposed to patterned stimuli which reflect 'higher values'. Simon is prepared to make the assumption that these higher values are to be found in an organization. In fact, what Simon means by the concept of organisation <u>is</u> the patterning of stimuli to influence behaviour which promotes higher values (see March and Simon, 1958, chapter 6). This leads him to develop a theory of decision-making in organizations of the foundation for the role of Administrative Science. Thus, Simon argues:

> "The behaviour patterns which we call organizations are fundamental...to the achievement of human rationality in any broad sense. The rational individual is, and must be, an organized and institutionalised individual. If the severe limits imposed by human psychology upon deliberation are to be relaxed, the individual must in his decisions be subject to the influence of the organized group in which he participates. His decisions must not only be the product of his own mental processes, but also reflect the broader considerations to which it is the function of the organized group to give effect" (1957 - p.102).

The individual makes his decisions, according to Simon's stimuli-response model of human behaviour, on the basis of factual and value premises. In the organizational setting these premises are provided by various forms of information which can be controlled. Here, I think, we have the missing raison d'etre of Stringer's intervention strategy. Stringer instances as examples of interventions "...not only the control of funds, and the exercise of statutory powers, but also the publication of plans and estimates and advice together with exhortations of various kinds" (p.110-11). And the objective he sets for OR is the "...choice of those interventions which appear to have the best change of meeting the objectives of the...system as they are seen by the central planning authority" (p.111). Stringer seems convinced that it is both possible and desirable to "...design...a system of interventions sufficient in variety to provide control over the significant inputs and outputs of the system of operations" (ibid, my emphasis). The objective is to "...design ...a strategy for guiding the decision-making system in...(the)...direction" (ibid) of the central planning authority's objectives. Compare this with Simon's view of the role of the science of administration:

> "The need for an administrative theory resides in the fact that there are practical limits to human rationality, and that these limits are not static, but depend upon the organizational environment in which the individual's decision takes place. The task of administration is so to design this environment that the individual will approach as close as practicable to rationally (judged in terms of the organization's goals) in his decisions"
> (op cit, p.240-41)....
>
> "The theory...(of administration)...must be a critique of the effect (judged from the point of view of the whole organization) of the organizational structure upon the decisions of its component parts and its individual members"
> (p.241).

The Given Rationality of Management:

The introduction of the 'higher values' to solve the problem of the irrationality of the unorganized individuals, from which follows the role of Administrative Science and OR, has no justification. If it is true that individuals (or individual organisations) cannot attain rationality without their decisions being oriented towards higher goals, the same must be true of the individuals who constitute the central planning authority (management or administrators in Simon's terminology), and also, of course, their advisors. As Storing is at great pains to point out (pp.87-98) there is no basis in Simon's work for the move which he makes from individual values to organizational values to provide a basis for the rationality of individuals. He is forced to make such a move, but no justification of it is attempted. All we seem entitled to say according to his and Stringer's schemes is that one individual's (or group's) goals are being selected as the criterion of rationality in preference to other individual (or group) goals. As a last resort to resolve this dilemma Simon makes two moves which are completely inconsistent with his view of science: he decides the basis of rationality by fiat. First, he says that organization's by definition have common goals - although it is not clear how this resolves the problem if it is said (as he does sometimes) that these are an amalgam of individual goals: the individual would have to be capable of raising himself to contemplate higher goals (otherwise how could they be shared?) and the raison d'etre of control of the environment of choice would be lost. Second, he latches on to the contradiction inherent in the first move to assert that there are some individuals who are capable of a higher form of thought than that envisaged in his basic model. It is possible, apparently, for some people to contemplate "...the framework within which is own mental processes operate..." (op cit, p.101), and this"...very highest level of integration" (ibid) of behaviour is to be found "...in the tastes of the administrator at the highest levels of the hierarchy" (p.219). As Storing notes this seems to open up the possibility that high level values may themselves be open to rational evaluation, but neither Simon nor Stringer provide the wherewithal to engage in such analysis. The goals of "top management" are indispensable and they are, apparently, to be taken as given. We must conclude that where we looked for a theory to explain the origin of management's values and their role in the decision-making process we have found in Stringer's account (and Simon's) nothing beyond unjustified assumptions. Thus, from Stringer's account of it, OR has no acceptable theory of management.

SECTION B:

Beer's Decision and Control:

The Tension in Beer's Thought:

Beer's book is about the relationship between management and science (p.ix). However, in Beer's account of it there are two types of management and two types of problems, and these are not always explicitly separated. Corresponding to the two types of management with their two types of problems are two different roles for science. Thus, for example, in Beer's account there are two different types of 'fresh look' and two different types of 'rigour'. These distinctions have to be made to clarify a central tension in Beer's work.

The tension in Beer's thought appears in the following form: On the one hand Beer often writes as if the 'fresh look' of OR means that any management problem could be completely transformed and overthrown; practices could be radically changed, or made obselete, or the problem itself could be rendered obselete and a new one instituted. This 'no-holds-barred' approach is to be found informing his work in may places. With it management could become obselete through the 'use' of science. Beer (p.91) recognises this possibility but dismisses it as being contrary to his intention. He writes:

> "...it should be made abundantly clear that
>
> the whole purpose of OR...is to aid the manager.

> People sometimes seem to fear that the aims of scientists in the managerial sphere are vainglorious. Some go so far as to imagine that the scientist wishes to establish dominance. Possibly a few scientists feel like this. If so they are on the same footing as the historian or the classicist who feels the same way. Such men are either psychotics or potential managers and entrepreneurs. The scientist _qua_ scientist seeks only to do science".

Yet the fact remains that Beer's claim on behalf of the OR scientist to have "...complete freedom to study whatever facets of a situation it regards as relevant..." (p.67) could really mean that "...the manager might well say that OR is trying to arrogate to itself a managerial function and perogative. If there is no factor known to the manager that was unknown to the scientists, then (he may ask) what decision is there left for the manager to take?" (ibid). It is difficult to see why the manager should be put at ease by the reply that he should be thankful to the scientist; this does not answer the point at all. Beer writes:

> "The answer...(to the manager's worry)...is really quite simple. Operational research exists to try and eliminate, or at least try to reduce, guesswork. Sometimes it succeeds; and when it does so entirely, there _is_ no managerial decision left to take. No-one should be more pleased about this than the manager" (ibid).

Nor will the manager's worry be much attenuated by the example which Beer gives (following the last quotation) where the manager is presented with a choice where he can exercise 'value-judgement'.

The suspicious manager will surely reason that this doesn't answer the point either because if these are the only types of decisions that the OR process leaves him it is not at all necessary that he makes them. Surely value judgements should be systematized within some 'total view', and what of the manager's role then? As Beer goes on to say (and it is again hard to see how management can take much comfort from it):

> "Once it has fully specified its set of values, a unique scientific solution should become possible. Sometimes the values can be quantified in advance and built into the study. In this case, the manager has exercised his prerogatives before the work begins, and can stand by to await a unique solution in the knowledge that he has already done his job. At other times, it takes an OR investigation to isolate clearly the value judgements that have to be made, and in this case the manager is left with a decision at the end" (pp.67-8).

On the other hand, Beer argues that in some sense management's basic role is left untouched by science; its essence remains unimpugned by the radical disposition which science takes towards its function. The way in which Beer gives his account of the distinctiveness of science points very strongly to this conclusion. He writes:

> "Scientists are people who have been trained in ways of examining the world which offer the best known means of making our beliefs about the way that world behaves correspond to the facts.... There is no question of trying to 'reduce' the process of management to a science'. The intention is simply to augment by scientific method the processes of which brains are capable in making

judgements and taking decisions...

> "The first insight to be attained, it seems, is that to say whether management is a science (or could be reduced to it) or an art (in which all true managers glory) is not to the point. What matters is that every relevant skill should be turned to the task of producing good decisions and more effective strategies"
> (p.10, original emphasis).

By arguing in this way, Beer suggests that science is just <u>another</u> skill to be set beside management's skill. In pointing to what he sees as the distinctiveness of science's skill, he emphasises that it lies in the fact that science is <u>rigorous method</u>; he "...equate(s) the method of science with a kind of rigour which our ordinary modes or thinking do not have" (p.31). Adopting this attitude means that "...the method of science is intended to import rigour into the rationality of managers" (p.32).

Although "managers sometimes use irrational procedures for settling opinions that will determine decisions in their problems..." (ibid) by 'irrational procedures' Beer means not that they are "stupid" (p.26). They "...may all have genuine survival value in the evolutionary sense..." (p.31) and "...are intelligent in so far as...they fit in with the way nature in general operates". (p.26). By the statements I think he means to convey that those procedures which OR makes rigorous have <u>in fact</u> got survival value. In Beer's view OR makes rigorous those management procedures which work. Calling these procedures 'irrational' is only his way of distinguishing them from the procedures of science! and arguing that the procedures of science operate upon the rationality of managers is his way of saying that existing organizations are, by his definitions, by and large rational. As he puts it:

> "Such rational arguments as are used to justify these procedures are based on their practical effectiveness. The choice that people make are usually fairly successful, for reasons other than the method of science itself could underwrite"
> (p.31).

This side of the thought on the relationship between science and management, then, expresses clear diffidence towards the underlying rationality of management. The rationality of management - their success in surviving (it would clearly not be possible to work for a management which was irrational in the sense that it did not survive) - is 'augmented' and it is, by its nature, outside of the scope of the 'method of science'.

There are other sources of tension in Beer's thinking on the relationship between science and management, and it will repay the effort to unravel them. In doing so we get to Beer's 'theory of management'. Essentially Beer distinguishes two leves of management, and two scientific attitudes towards them. First, there is what he variously calls 'opertional', 'junior' or 'middle' management. The attitude of science towards this level is decidedly radical. Second, there is what he variously calls 'top management' or the 'highest level' of management. The scientific attitude towards this level is one of diffidence. It is this latter attitude in particular that we wish to understand, for we shall find that when we do understand it we shall have a much fuller version of Stringer's theory of OR in the multi-organizational context. We shall again find that OR works within the given 'context' set by top management. If, as Beer says, the essence of science is method ("If we want to use words carefully", he says, "in fact, the method of science <u>is</u> method") (p.29) then it must, at this level, be working on a given content. This, in a crude form, is one way in which OR perceives its subject-matter.

Resolution of the Tension: Two Realities:

In Beer's world-view these are two different realities. One is mutable. Here science is radically disposed, and the management involved is 'lower' management. The other reality is given. Here service is diffident, and the management involved is 'higher' management.

The mutable level of reality is the one at which Beer's dictum that OR is something that the manager could not do if only he had the time (p.103) applies. It does not, I think we shall see, apply to the higher levels of management. Why does it apply at the lower levels? Beer is not making the rather crass point that managers are not trained as scientists (either natural or social or OR) because, apart from the obvious fact that many managers have been trained as scientists at universities and elsewhere, the plain fact is that if they had the time they could become trained. Even if all managers were trained scientists, Beer is arguing, they still could not do OR, nor would OR necessarily be done. The reason that managers could never do OR with either training or time is for Beer a fact of social organization. The model of social organization which, for Beer, justifies his dictum lies at the heart of his vision of the nature of OR.

There is a level at which OR adopts a truly radical disposition towards the given. This attitude may be aroused, for example, when an organization is faced with a 'major' technological change.

Beer writes:

> "...(I)f there is a major breakthrough in any technology that is relevant to the enterprise, this changes the character of the whole system. It does not merely have 'far-reaching effects' but actually turns the old enterprise into a new one in which the thinking, the old costings, the old methods of control may well have no place whatever. The scientific description of the whole situation has to be rewritten, which is a job for operational research" (p.41).

It is hard to attribute any meaning to this statement, which tends to imply that there are objective reasons for the radical disposition which is taken to the system. The 'possible' impact of technological change - "...may well have..." later on becomes a certainty. Commenting on the tendency to react to technological change by "dressing up the old system" Beer remarks that "...the old system is no longer required, in the dress of any period" (p.42). If this is the meaning intended to be conveyed in the statement, the attempt has failed. With the key terms in the first sentence - 'major' and 'relevant' - undefined, the rest of the statement becomes tautologous. Clearly, 'major' breakthroughs in 'relevant' technology have radical effects. The meaning of this statement, therefore, turns on the question of how these key terms are defined, and in Beer's account this actually turns on who defines them. Beer's argument, I think, amounts to this: Certain managers who would typically be responsible for thinking through the implications of changes in technology are, by the very nature of their social existence, incapable of taking a radical view of these implications. Operations researchers, on the other hand, are capable, and will take a radical look. This I think is the correct meaning of his statement.

The incapacity of 'ordinary' managers to engage in a radical look at their operations and procedures is the justification which Beer offers for OR doing so. Beer writes:

> "It is worth recalling that many of the problems that most need to be solved, which are (as has been seen) those very problems in which the solution has not only eluded management but has not even been envisaged, arise precisely because of the way in which a company is organized. For practical managerial reasons, a company is divided into clear-cut areas of responsibility; and the manager, whose authority derives from his position in one of these areas, is conditioned by the whole of his experience to seek solutions in which he can exert personal authority. But problems are not respecters of the company organization, nor of the talents of the company servant who first meets a symptom of the problem"
> (p.51, my emphasis).

By the phrase 'conditioned by the whole of his experience' Beer means a good deal more than that decisions are made in terms of what people know. Even when the company tries to breakdown the organizational barriers by means of various forms of liaison and committees he argues that not much good will come of it. If it were just knowledge and information which were lacking (or even other forms of constraint, such as political intrique, pressures of work) then surely something could be done about it, even though there may be no guarantees of success. Beer does not think so. He erects the Simonian model of man whereby man, in being prey to external stimuli, and attempting to act rationally by forming simplified models, he becomes thoroughly irrational. He is 'intendedly rational' (Simon's phrase) in forming simplified models but irrational in doing so because he misses the big picture. Such a man obviously, by definition, needs outside help. No matter what he does, alone or in concert with others, cannot change the fundamental nature of his predicament. This is Beer's view:

> "...(T)hought block...is the mechanism by which the brain learns to find patterns in the world outside: the brain becomes conditioned. This process of conditioning...is vital to survival, for in its absence the world appears entirely incoherent and unpredictable"
> (p.58).

Beer uses the notion of 'phase-space' to describe the 'framework' which the individual of necessity employs to cut down to size the "...infinite number of possible reactions that he could make..." (ibid). He argues that although there is some commonality of phase spaces this is so only for 'simple social situations' (p.59). In more complex social situations specialised phase spaces emerge from specialised experience. This for him is the inevitable consequence of organization, he argues, and although phase spaces may be modified at the margin, they are still phase spaces and they can in no way measure up to the true complexity of real situations. He writes:

> "The phase space for the solution of a managerial problem exists in the head of the man who comtemplates it. It is therefore unique to that man. For a given problem, the phase spaces of the solutions envisaged by the production manager, the accountant, the engineer and the salesman are far less likely to coincide exactly than they are in the case of the simple social situation. The experiences, and therefore the learning and adaption of these men is different. So what is the best solution

for the company? The solution normally adopted quite obviously lies in the common phase space - that is, in the area of overlap between the phase spaces of the individuals who comprise the management team - but there is no guarantee that this gives the most appropriate answer. The company itself has no brain. It has a phase space within which it always operates, but this can only be interpreted by human beings who inevitably get the company's phase space confused with their own. But suppose the company had its own brain and was aware of its own phase space. The answer to a particular problem might then lie on the fringes of this space, in an area not belonging to the phase space of any one manager in the organization. The best answer to the problem would therefore be missed, because it would be strictly inaccessible to the management. The thoughts of a man concerned with this particular problem cannot break through the barriers of his own facilitated pathways to reach the goal, This situation is called 'thought block" (p.59, last emphasis original; early emphases mine).

However, like Simon, Beer will not easily contemplate the possibility of at least some levels of management tearing themselves outside of their institutionalised ways of thought. It is by his absolute insistence on the necessity of ordinary mortals falling prey to the exigencies of conditioning that provides, for him.

"...the reason why it takes an objective, scientific, interdisciplinary investigation to arrive at a solution outside the phase space - a study undertaken by men who are by definition

> not prone to this particular thought block.... The existing organization is likely to come very close to a good answer which does lie within its own phase space, because there will be no thought block to prevent its so doing. Then the reason for employing operational research at all is to see whether there is a solution outside"
>
> (p.61).

The Simonian background again raises Simon's problem of why, if it is true that lower level managers are prone to thought blocks, the same does not apply to scientists and to higher level managers? It is certainly true in the terms of Beer's argument that neither scientists nor higher management will be subject to the particular thought blocks which beset lower level managers. They are not subject to the same experience. However, to the extent to which they are human, in the terms of Beer's argument, they cannot avoid having of necessity to operate with simplified models. Therefore, it cannot seriously be argued without further explanation that the thought blocks of scientists or of top management are any more rational than those of the lower levels of management while they are still thought blocks. They are as much subject to environmental conditioning as any others - at least on Beer's argument so far. Recall, also, from chapter 2, Beer's arguments on the subjectivity of science, where we saw him make this point very forcibly.

We saw there how Beer attempts to guarantee the objectivity of science by reference to the facts. However he also looks for objectivity in his distinction between levels of management. His line of thought seems to run; if the irrationality of lower management is turned into rationality through organization, so the irrationality of science can be turned into rationality by the originators of organization, namely, top management.

Although lower management are, in themselves, irrational their activities are made rational by reference to the totality of which they are part. Scientific models are not in themselves rational because they suffer exactly the same subjective limitations as do the models of lower management; by analogy, however, scientific models can be made rational by their orientation to a larger reality. Top management provides this larger reality; OR's job is to make this reality more 'rigorous'. In short, accepting that the facts and models of science are subjective, Beer's argument turns to a defence of their subjectivity.

Management as the Guarantor of objectivity:

The line of argument we shall trace through Beer's work is that top management provides the 'overall values' which turns the irrationality of both lower management and science into rationality and in so doing it provides for the 'underlying reality' with which OR works (Note that this provides a kind of Kuhnian idealist base to the objectivity of facts). The role of OR is to make this reality more 'rigorous' by adopting a 'professional' attitude towards it. It is the attitude of diffidence mentioned earlier which Beer must be adopting when he remarks that the job of OR is to "...enrich the managerial model by the scientific one..." (p.105), and when he later on says:

> "It is not the substitution of science for management that is advocated; it is the appeal for a professional rather than an amateur approach to the managerial task"
> (p.425).

This diffidence eventually finds its expression in the call for the explicit design of an organization as an organic adaptive system. This is the relevance of cybernetics. The diffidence itself, however, stems from the belief that top management displays qualities of thought which are not to be found elsewhere. Carrying on his line of thought which suggested that lower management inevitably could not see the system whole and could not, therefore, adopt a radical attitude towards their own activities, Beer observes that:

> "(Although)...it is not a manager's job to change his own conceptual framework, but to get on with the responsibilities he exercises within it. (T)he onus on <u>general</u> management to consider such changes is, however, much heavier, Perhaps this kind of decision is their most important task"
> (p.84, original emphasis).

Adopting the analogy of a rugby team (which carries with it the implication that there is an organic unity of purpose: the implications of this view will be explored in the next chapter) Beer argues that "to consider each part of a large system separately, to find a perfect solution to the problems of each part, and then to add the solution together and call the result a strategy, does not necessarily work" (p.40). He carries on:

> "This assertion can be examined in a small way on the rugby field. The individual trying to play a superb game from his own point of view, on the theory that if each player does this the side will win, may be a spectacular wing three-quarter, but is dropped from the team because he is selfish.. The same is true on the battlefield and in industry. But is is the function of the football captain, the military commander and the managing director to see that these things do not happen. These are the leaders who must see in the whole an entity greater than the sum of the parts" (ibid).

The only problem that Beer finds with this anology is that "... the leader has no mechanism for doing this once the system he controls grows to larger diminsions that can be comprehended in one man's brain" (ibid). Enter OR, which"...does this, essentially, as a service for the leader who is responsible for that whole" (p. 41).

Later on Beer argues that given the existence and stable functioning of an organisation "... the 'owners' of the system seek to discuss its future in a metalanguage" (p.372), because the lower level managers are incapable of so doing:

> "The operational managers speak the language of the system: they are content that it should survive in a stable condition, that it should make a profit which is a guarantee of its future, that it should learn from its own experience, evolve and adapt. But at a higher echelon there are the policy-makers: those for whom this state of affairs is necessary but not sufficient; those who would re-direct the motives of the enterprise as distinct from its aims" (ibid, original emphasis).

Let us continue this quotation to see what Beer sees as the special capabilities of this 'higher echelon'.

> "These are the gods of the system. To the operational managers of the firm, they are the directors who represent the shareholders; to the civil service they are (or ought to be) the political masters who represent the electorate.

> Thus it is perfectly possible for a self-regulating engineering company, for example, to evolve satisfactorily as a supplier of markets - even to diversify its products, to re-design them and to change the entire technology of production and distribution. But it is as inconceivable that such an engineering company should suddenly become a publishing company or a shipyard as it is to suppose that a dog should suddenly evolve into a rabbit. With a sufficiently strong notion of the organic wholeness of nature and of history, it is not impossible to envisage such a change <u>eventually</u> - because one can contemplate the infinitely large number of tiny yet directed mutations which might carry the former state into the latter. <u>But the heads of enterprise are distinguished by their ability to take decisions of this kind overnight</u>" (p.372, latter emphasis mine).

Top management, in Beer's view, is the fulcrum on which the long-term adaptiveness of the organization rests. In Ashby's terms, it is the source of adaptiveness which comes from <u>parameter</u> change (op cit). We shall see in a moment that this amounts to Stringer's role for management (and their OR advisors); that is the designing of a strategy of 'interventions'.

Arguing that organizations are, in the sense discussed by Ashby, 'self-organizing', in his view:

> "It is the pathology of self-organizing systems to take a self-defeating turn. Some evolutionary adaptions, such as the increasing size and weight of prehistoric animals, which were viable learning systems in one language, turned out to be metalinguistically defeating...A self-defeating system, in short, is one which succeeds in its self-appraising language and fails in the metalanguage of its top direction" (pp.372-3).

All this is but another way of saying that lower managers are not by themselves fully rational. This is why although

> "(I)t is the function of executive management to make the effort to respond when interrogated by the market, and to cut subsidiary companies out of its will if this seems sensible. It is the function of the board to observe, metalinguistically, when these executive adaptions will prove self defeating in larger terms" (p.373).

Beer, then, distinguishes two levels of adaption: the organisation is self-organizing, and top management set the conditions under which this self-organization takes place. Beer argues that organizations are capable by their nature of "...structural adjustment to a set of disturbances within the context of a set of overriding goals" (p.355). Presumably Beer has these 'overriding goals' in mind when he goes on to say that organizations as"...natural systems organise themselves over a period of time to become what they immanently are" (p.359). Top management should not become involved in the detailed direction of the organization: first, because it is impossibly complex, and second because there is no need. Organizations are <u>self-organising</u>, and all that is necessary is for top management to "...interfere with the alleged randomness with which the...(organization)

...poses solutions, as well as vetoing further attempts to pose the same unsatisfactory solution as before" (p.368). This, I think, is Stringer's strategy of interventions.

The justification for this is the by now familiar model of rationality. The organizational participants can, we have seen (p.372) only have 'aims' whereas the top management policy makers have 'motives'. As with Simon, the notion of rationality becomes one which can only be applied to the goals of the 'organization'. Beer writes:

> "Any viable system is evolutionary, a learning and (in part) an optimizing creature. <u>But its teleology is an artefact: it is something understood post hoc</u>. It is...driven by a 'purpose' which is defined by the 'goals' it did in fact hit. <u>It is only the forecasting, forward planning, intending human mind which chooses goals on the basis of deliberately formulated ideals</u>. These <u>ante-hoc</u> goals constitute the metalinguistic policies of the enterprise.
>
> "Now it is obvious that if we seek to have an enterprise which can attain a goal formulated in advance, but whose structure and mechanism is that appropriate to an organism whose goals are formulated only with hindsight, then our managerial task is one of organizational design. The job is to modify the structure, without destroying, the self-organizing properties of the system, so that the goals it 'just happens' to achieve (the ones recognisable only after the event) turn out to be the goals which the

managers wished to attain all the time"
(p.380), my emphases).

Top management, then, are truly the 'gods' of the system; they are constrained and fettered not in the least by the tought blocks and subjective models with which lower level managers are 'inevitably' encumbered. As Beer says of the latter group:

> "Those employees whose lives are dedicated to the operation of the company as they know it will never understand these overriding intentions and success criteria, because they do not speak the metalanguage"
> (p.397).

I think that a more pertinent question than whether the lower level managers are capable of speaking "...a metalanguage...(which)...the enterprise as a whole talks...(and)...in which the language of the operations can be disputed" (p.388) is whether anyone is <u>ever</u> capable of speaking such a language. Beer gives us no guidance whatsoever on the question of how such a language is constructed or, more importantly, whether the 'metalanguage' of top management is itself rational. To judge it one would, presumably, require an even higher metalanguage...and to judge that?

Note that Beer does not offer us cybernetics as the foundation for such a metalanguage. The language of cybernetics merely <u>clarifies</u> what is already there. The language of cybernetics is merely, in Beer's words, "...a descriptive account of how enterprises actually work" (p.397, cf.1970, p.45). As he goes on to say, use of the cybernetic "...model would radically change the <u>appearance</u> of an enterprise controlled system. For although it was just said that controls are actually exercised rather like this, no-one acknowledges the fact; nor is the machinery <u>explicitly</u> designed - <u>and therefore it must be inefficient</u>" (pp.397-8, latter emphasis mine). This seems to be Beer's major worry - cybernetics merely makes manifest "what is really the core and central nervous system...(of control which)...is locked within the communicating minds of a managerial elite" (ibid). Cybernetics, then, is the science of organization which enables us to "...think in a scientific way about the constitution of management itself - since this is the control structure of the enterprise" (p.425, original emphasis). It may be true, as Beer says, that "this task is more fundamental than any other; more fundamental even than the construction of a global model of the firm" (ibid). However, it does not in any way provide us with a 'theory of management' (although it may provide us with a theory of management control (see: p.398), for when it comes to the fundamentals of which Beer has been talking in reference to management, specifically, the idea 'purpose' or 'higher goals' or 'ideals' which distinctively get top management apart from other mortals, Beer tells us it is a purely subjective matter. Thus he writes:

> "Let it be noted please that these explanations of teleological mechanisms do not explain 'purpose' away. They do not assure us that self-organising systems are not purposive, only that there is a natural mechanism to which the name of purpose is given. How anyone should interpret this is a subjective matter and must depend on the connotation he accords to the word 'purpose'" (p.362, my emphasis).

This reluctance to discuss the propriety of using the word 'purpose' is a refusal to discuss, by the terms of his own argument, top management. It is a conclusion which was foreshadowed by his earlier one that 'purpose' was a property of the 'observer' of a system. Thus, "what the observer does is to project his own notion of purpose on to the system" (p.357) and "when the observer has defined a set of goals as appropriate to his purposes, and has spotlighted an entropic drfit which conforms to those needs, he will dub the system self-organising" (ibid). We have seen Ackoff and Churchman in a similar position. How the observer decides his purposes is left as an ineffable mystery.

If cybernetics depends on the giveness of top management, then OR certainly does. Beer, because of his inclination towards cybernetics perhaps, is very clear on this. Beer's argument is, essentially, that OR should follow from the cybernetician's attack on the problem of organization. However, we shall see in chapter 7 that the reality backdrop which Beer attempts to provide through his cybernetically slanted discussion is no idiosyncratic move on his part. The ideas he dresses in cybernetic language could for example as well be dressed in the vernacular of organization theory, as our references to Simon have indicated.

The Relevance of Cybernetics to OR:

Beer's arguments on the relevance of cybernetics to OR help us greatly in our quest to understand his account of the nature of OR. Put very bluntly, his argument amounts to the thought that we can improve situations by making explicit the given reality which OR takes for granted, namely, top management. Cybernetics improves the structure of top management while taking its content for granted. The content is guaranteed because top management is viewed as the adaptive leading edge of a self-organizing system. The relevance of cybernetics to OR, then, is that it makes these adaptive mechanisms 'more like what they really are'. That is,

> "the enterprise...is an organic whole. It is a homeostat built of homeostats. It is an ultrastable machine. It is, very largely, a self organizing system...(p.371)...(Added to this is a)...hierarchic system which is superimposed upon it, or rather by which it is

> implicitly informed...which speeds up the operation of homeostasis, and which can evoke quick responses when danger is threatened" (p.380).

And the cybernetic solution to what the "...structure of a control system for the enterprise should be like..."? (p.383). It should be

> "an essentially self-organizing system, with hierarchical overtones. It is a control system which operates itself, but which can be monitored from on high, and given new directions towards predetermined goals which it does not itself recognise and of which it cannot indeed be made aware"

(p.383).

If, as Beer seems to be saying, the role of cybernetics is to improve on what is already a reality, then it becomes clear why "...the very first thing to do is to think in a scientific way about the constitution of the management itself..." (p.423). One should do one's cybernetic thinking first because, as Beer immediately goes on to say,

> "...once OR begins to model the totality spread before it, it makes the fundamental acceptance of the structure which controls it. This is because that structure can be discussed only in a metalanguage" (ibid).

Later on Beer clarifies for us what he means by the term 'structure'. Arguing that "if we once have access through science to the very nature of viable organization, then we begin to formulate OR models which can be used to achieve improvements" (pp.436-7), he points out that he does not have in mind what is normally understood by organizational change, namely, "...to make clearer what has to be done in the way of redefining responsibiliti reorganising information flow, reconstituting the committee system..." (ibid) etc. Beer distinguihsed four levels of organizational 'category' as he calls them.

At the very highest level one can understand the organization in terms of 'structure', beneath this is the level of policies, and so we go down to problems, and then to practices. These levels do not represent for Beer levels of activity, but, rather, categories of understanding. That is, from the viewpoint of top management and OR there are questions of practice, problems, policies and structure which are to be considered in organizational improvement. Beer seems to be arguing that if top management can be persuaded to consider the level of structure by reflecting on what it takes for its organization to be a viable organization, then OR is put on a surer footing, Beer argues:

> "If a firm is ready to reconsider the very structures of organization, then the outcome for its policies may be a considerable change" (ibid).

Not only can top management think more coherently about policies, it can also think more coherently about the other levels as well. Providing top management with insight into structure gives rise, that is, to an "...exceptionally well endowed management; a management, that is, which really understands how...(for example)...problems arise, because it understands the structures which provoke them. And if top management can think coherently then, apparently, so can OR. "A managerial situation of this kind is thoroughly amenable to OR attack" (ibid).

This is so because the activities of top management - conscious or otherwise - provides the necessary context for doing OR (as we have seen is also the case for cybernetics).

This interpretation of Beer's thinking is also one which will make sense of some other statements which he is prone to make about the practice of OR. He frequently makes statements which point to OR's realiance on a given reality. These statements would make his overall scheme incoherent were it not for the fact that top management, in their adaptive role, provided this reality. Otherwise it would conflict directly with is view that OR takes a fresh look. The statements which follow reveal that OR has to take a reality for granted. Top management, in Beer's argument, justifies it doing so.

An early indication that Beer sees OR working with an underlying reality is when he remarks that although managerial phase spaces are irrelevant to OR there exists something which he calls "the company's phase space" (p.59). Although a solution could lie outside the managerial phase spaces he does not suggest that it could lie outside the company's phase space. A bit later on, after changing the terminology from phase space to model, he argues that the activity of OR is a process of finding isomorphic correspondences between the 'managerial siutation' (he had earlier talked of the "actual management problem" p.66) and the scientific situation. The origin of his managerial situation, or the correctness of attempting to model 'it', are not discussed. This is seen as a fundamental reality for while he is at pains to point out the subjectivity of facts (e.g. "measurements are themselves based on theory" p.96) we are shortly afterwards told that the 'utility' of the scientific model of the managerial situation "may be accomplished by straightforward testing of the predictive value of the scientific model" (p.113)!

Returning again to the justification of OR being provided by the arbitariness of lower management's phase spaces, Beer writes:

> "The manager who appreciates the sense of this
> description, and the limitation of his whole
> organization as a machine devoted to handling
> an arbitary account of the way things are, will

> use OR in a correct way. He will deliberately send his OR team into orbit around the... (specialised)...phase space...He will enjoin the team to explore the outer space to find out what is going on beyond the confines of his own managerial universe; to think afresh about the nature of his problems in the context of the whole...In short; what is the enterprise all about?"
> (p.404).

'The whole', 'outer space', 'what the enterprise is all about' are the givens which OR attempts to model. Some justification for this attitude and why, as Beer at one point says in introducing a case study report, "...as usual, we start from the premise that we are confronted with a system which works, and is working..." (p.418), seems to be required. All the signs in Beer's writing point to one conclusion: top management is taken for granted.

Conclusion:

Both of these widely respected accounts of the relationship between OR and management fail to provide as adequate justification for the obviously very important role played by management. Management it seems, takes care of OR's 'reality problem'. That is to say, management, in these accounts, provides the context or framework (weltanschauung) within which OR works. From our analysis of conventionalism and positivism in Part One perhaps it was only to be expected that some form of metaphysical response to the subjectivity of individuals and the pressing demand for objectivity would be made. Kuhn's response took the form of the claimed objectivity of science as an adaptive social system; Churchman's response took the form of the claimed objectivity of dialectical processes. The response of Stringer and Beer is to claim the objectivity of management. Making such a claim certainly solves the twin problems of limiting the scope of OR while at the same time claiming that this limitation is objective. These problems only remain solved so long as we avoid asking on what basis it is claimed that management can and should perform the functions which OR asks it to perform. As it stands at this point, OR ought to have a theory of management which explains the origin and use of values in decision-making in organizations. We have found instead, a metaphysical solution which asserts properties of 'management' (or 'top management') without explaining them.

This is merely a metaphysical solution to pressing philosophical problems. It is possible, however, to go further and, as it were, to 'unpack' OR's theory of management to reveal its foundations. At several key points I have referred to the work of Miller and Rice and of Simon to illuminate the conceptual framework within which both Stringer and Beer were working. These social scientists are, however, representatives of a much wider and older tradition in the social sciences. In the next chapter I shall push deeper into the foundations of OR by showing how this tradition in the social sciences in fact provides the supports upon which OR's theory of management rests, and some of the implications which this foundation has for the likelihood of OR achieving its ideals.

CHAPTER SEVEN

OPERATIONAL RESEARCH AND SOCIAL THEORY

Introduction:

The conclusion from the last chapter is that, of necessity, when OR talks about management (and itself) it is talking about something other than management (and itself). Talk of the process of OR and management of necessity entails a notion of the reality with which both deal. One way to discover the nature of this reality is to analyse the language in which the discussion is carried on. It is my contention that this language is a language infused with social concepts. The task of this chapter is to show that these concepts add up to a recognisable social theory. That OR cannot just talk about itself is a point made philosophically by Churchman (1974). However, he does not go on to ask what OR typically does talk about and what it should talk about.

Finding out what OR is referring to in its accounts of itself will go some way towards plugging the gaps that we have found to exist in several accounts which have been given of its nature. There have been gaps between theory and fact, the subject and the object. Also, in the last chapter we found a gap between management and the real world. That is, in this last case, we could not understand how management understood the real world. It needed this understanding, we saw, because it could then provide OR with a guarantor of objectivity. In this chapter we will begin to understand how these gaps are filled by understanding an important aspect of OR's grasp of its subject-matter, namely, the nature of the social world.

The development of this understanding of OR's grasp of its subject-matter to its practical implications for problems in the practice of

OR (e.g. data-gathering, problem formulation and solution, implementation) is, however, no part of this thesis. These developments, if they came at all, would rest upon the development of an adequate understanding of the nature of the social world and how this understanding affected the practice of OR. There is much work that needs to be done in this area. This part is confined to bringing to consciousness the fact that OR does have a coherent grasp of the nature of its subject-matter, and that this grasp is inadequate.

Although popular accounts of the role of the social science in OR argue that the social sciences may provide extra operationalizable variables (for a recent example of this in the area of implementation see: Schultz and Slevin, 1975), in fact the indebtedness of OR to certain schools within the social sciences is greater, as I indicated in the introduction to this part. Borrowing some terminology from Gouldner (1970), my claim is that OR shares the "domain assumptions" of selected parts of the social sciences (which parts will become clear). Writing about sociology Gouldner argues that "...whether or not an empirical study of social life develops, and the kind of study it is, depends upon certain prior assumptions about society and men..." (p. 28). He goes on to argue that domain assumptions are something less than "world hypotheses" by being "...applied to members of a single domain" (p. 31). However, when applied to "man and society" he argues that they imply a disposition to believe that "...men are rational or irrational; that society is precarious or fundamentally stable; that social problems will correct themselves without planned intervention..." (ibid) etc. These are illustrative of the forms which domain assumptions may assume. My argument will be that OR shares some key domain assumptions with the social sciences; particularly those which relate to the rationality of the social world.

Gouldner was preoccupied with a concern which is not one directly shared by this work. Gouldner's ultimate objective was to develop a 'sociology of sociology'. That is, his task was to explain sociological theories sociologically (see also: Friedrichs, 1970). It is not my intention to attempt to explain the theories of OR sociologically. Such an enterprise would seem to be urgently required for at least three reasons. First, OR is concerned to improve social systems and the extent to which it is influenced by those social systems will partially determine the criteria of improvement. Second, OR is an interdiscipline in a more profound sense than being only some kind of combination of sciences. OR works with and for social systems and some means for achieving a full 'understanding' on a mutual basis (cf. Churchman and Schainblatt, 1965) is necessary both as regards the problem of implementation and to be able to claim that 'real' improvement has been secured. Third, it is one thing to understand from the outside (from the position of a sociologist, for example) what the theories of OR imply for the achievement of their ideals, and quite another to engage in fruitful dialogue about those theories. To engage in this dialogue about OR's social theories requires that we understand how those theories come about.

Given that this is not a sociology of OR our route to the domain assumptions of OR is a different one from that taken by Gouldner with respect to sociology. I shall be concerned with what Krupp (1961) has called the "...informal logic" (p. xi) of theories. Krupp's concern, as is mine, was to "...distinguish between "formal" and "informal logic"; that is, between the logic of the logician and the logic of argument in actual theories...(That is)...to sift, from the systematic elements which structure...theory, those guiding themes and premises that express the values, determine the descriptions, orient vision, and pattern policies" (pp. x-xi). It is just such an informal logic that I shall be looking for in OR's account of itself. In this informal logic we shall find OR's conception of its subject-matter.

Clues from the Origins of OR:

A convenient place to start our search for OR's conception of its subject-matter is with the origin of OR itself. What is of interest here is not the details of the early days of OR but the context within which OR is perceived to be a rational response. The context in question is perceived to be the state of development of the social organization of task fulfillment. In the accepted view, OR was a rational response to developments in the form of social organization.

In their discussion of the 'Essential Characteristics of OR' Churchman, Ackoff and Arnoff (op cit) note that "although the activity called Operations Research began in a military context, its evolution or emergence can be described in terms of the well-known development of industrial organization" (p. 3). They go on:

"Before the industrial revolution most business and industry consisted of small enterprises, each directed by a single boss who did the purchasing, planned and supervised production, sold the product, hired and fired personnel, etc. The mechanisation of production led to such rapid growth of industrial enterprises that it became impossible for one man to perform all these managerial functions. Consequently, a division of the managerial function took place. Managers of production, marketing, finance, personnel, and the like appeared. Continued mechanisation, supplemented in part by automation, resulted in still further industrial growth which manifested itself in decentralisation of operations and still further division of the management function...During this period of differentiation and segmentation of the management function a new class of managerial problems began to appear and assert themselves, problems which can be called exective-type problems. These problems are a direct consequence of the functional division of labour in an enterprise, a division which results in organized activity. In an organization each functional unit (division, department, or section) has a part of the whole job to perform. Each part is necessary for the accomplishment of the overall objectives of the organization. A result of this division of labour, however, is that each functional unit develops objectives of its own...These objectives are not always consistent; in fact they frequently come into direct conflict with one another...(Thus)...an executive-type problem...(a)...involves the effectiveness of the organization as a whole, and (b)...involves a conflict of interests of the functional units of the organization...It is important to note that this division of organizational objectives is not "bad". If a large group of persons attempts to accomplish some task, it may not be possible for them to act as a single person would.

Thus it is pointless to develop a plan for a large industrial organisation which assumes that everyone knows and can evaluate what everyone else does. Division of function seems to be the only solution in this situation. The executive problem thus arises out of the need to subdivide functions. Its solution is rarely of the form: "Let's all try to understand the other fellow's problem". Rather, the solution demands a highly refined balance of departmental objectives and over-all objectives; departments need to be motivated to pursue their own goals and excessive interest in the good of the whole may lead to stagnation of effort. Therefore, when we talk of an "over-all optimum" we mean a policy that takes account of the necessity of a split function in the organization" (pp. 3-5).

The conclusion which they reach from this change in the form of social organization is that:

> "an objective of OR, as it has emerged from this evolution of industrial organization, is to provide managers of the organization with a scientific basis for solving problems involving the interaction of components of the organization in the best interest of the organization as a whole" (p. 6).

Ideally, they argue, OR deals with the whole organization, although sometimes it is forced to deal with only a part of it. This is sub-optimization. The clue which this description of the origins (and hence the task) of OR provides for gaining an understanding of OR's subject-matter is that in it we find the simultaneous statement of OR's ideals and a basis on which they could be implemented. At least this is a possibility which I wish to explore. OR's aspiration to optimize implies a radical disposition towards existing departmental responsibilities and functions. This is the radical disposition of the systems approach towards the definition of systems boundaries (cf. Churchman, 1968b) directed towards a specific content. The boundaries in question are the boundaries of social organization, i.e., tasks an relationships. Perhaps it is worth mentioning here, in anticipation of the analysis to come, the types of theoretical entities which it could be argued subsist in accounts such as that of Churchman et al. This may at least encourage us in our task. In their account we are committed to, amongst other 'things': the existence of an entity called an "organization", which has as one of its major properties "objectives"; that there are people who can in some sense be said to be "in" an organization and who, because of their "position" in the organization are "given" and "develop" subobjectives; that it is possible and even likely that these organizational members "recognize" and "pursue"; and that the subobjective which they pursue is controllable. There are no <u>a priori</u> grounds for saying that these terms 'obviously' have no relationship to a background social theory which gives them meaning.

My argument will be that OR's aspiration to optimize the level of integration in organizations requires a prior commitment to the bases on which integration is 'normally' achieved in the first place. That is to say, this aspiration involves the very strong assumption (in the terms of social science) that integration is achievable, and is normally achieved by some means. In making this argument I am in fundamental disagreement with Selznick who argues that there are two "analytically distinct" standpoints from which to view organizations, namely, that "...on the one hand, any concrete organizational system is an <u>economy</u>; at the same time it is an <u>adaptive social structure</u> (1963,

p. 358). Selznick's views neatly capture the prevailing view in OR about the status of the social sciences, but it fails to explain why the 'economy' of an organization is a realistic topic for inquiry in the first place. If Churchman et al's views are representative, OR certainly views the study of an organization's economy as realistic. OR does not usually inquire into the basis of integration upon which study of the economy of an organization becomes realistic, but I shall argue that it can only take its task as given by presupposing that a social basis of integration already exists. Stringer's endeavours in the last chapter can be interpreted as an attempt to re-establish the social basis of integration so that work on the economy of an organization could proceed. We shall see that Selznick's distinction is outdated even for OR. Increasingly OR is blurring the distinction between the economy and the social structure of an organization. We shall see, however, that this (e.g. Ackoff, 1970) is merely an elaboration of a basic model of the social basis of integration which has been employed by OR in both its developed and undeveloped forms.

It might be thought that OR could straightforwardly rely on the activity of top management as a basis for the integration of the organization and, of course, its own activity. However, we saw in the last chapter that management's role was in need of further explanation. This further explanation is provided by OR's social theory, which is in fact a theory of the social basis of integration in organizations.

Our way for proceeding is to look for homomorphisms between the language of OR and the language of the social sciences and to ask whether they amount to isomorphisms. The conclusion will be that OR can justifiably be understood within the context of a particular type of social theory. An implication of this conclusion is that the development of OR has necessarily (though not exclusively) to be sought for within the context of social theory as a whole. This is not a possibility which has been seriously discussed within OR.

OR and Durkheim's Problem of Order:

Emile Durkheim, generally recognised as one of the founding fathers of sociology, has had a profound effect on certain schools in social science. His work has provided a context for these schools by determining the nature of their problems and the terms within which solutions are to be sought. It is in the context of these problems and their modern solutions that we shall find illuminating isomorphisms between OR and social science. But, first, the homomorphisms. I shall engage in liberal, though not I trust distorting, interpretations of some of Durkheim's thought.

In one sense selecting Durkheim's thought as a source of potentially fruitful homomorphisms to give us insight into OR's social theory seems straightforwardly justified. Durkheim was particularly concerned with the very phenomenon which OR sees as being the conditions which were responsible for its emergence, namely, the division of labour. Just as OR is concerned to distinguish a primitive era from a modern one, so too is Durkheim. In both cases the constrast turns on the distinction between the simple and the complex. This distinction will occupy much of the following. For Durkheim the peculiar complexity entailed by a highly differentiated system is not to be found merely in the fact of differentiation. So too for OR. Rather, the peculiarly complex nature of modern societies for Durkheim, as it is for OR with respect to organizations, is the basis on which 'order' or integration is to be achieved, where the criterion of order is the collective standpoint (1). The basic problem faced by Durkheim, as it is by OR, was how was order possible in the face of the diversity engendered by the division of labour?

The peculiar nature of the problem faced by OR is not merely how to gain some kind of order between diverse subobjectives. One possible kind of order might be the now infamous 'minimize costs'. As Churchman wisely points out, there is only one way to minimize costs, "...and that is suicide" (1968a, p. 74). The order that OR has in mind is an optimum, and this is defined in terms of overall goals. The distinctive difficulty in achieving this when there is a complex division of labour is the perceived inability of detailed control over the units responsible for the subobjectives. Not only is detailed control considered to be impossible for all practical purposes, it is not even seen as being desirable. And by desirable I do not mean that it is necessarily seen as morally undesirable (although it often is). It is seen as being undesirable because it is seen to be dysfunctional.
Churchman et al distinguish the different order of problem involved in optimization with man-machine systems compared to the problem involved with mechanical systems. They comment:

> "The "division of labour" aspect of human organizations is different from the component functioning of machine systems... because in human organizations there is the critical problem of motivating the divisions to perform their respective functions" (op cit, p. 7).

(1) Note that OR aspires to solve problems--achieve higher levels of integration--on a wider scope than those posed by organizations: OR is concerned with social systems of any size.

To single out motivation as a separate control problem implies that they see something other than simple direction being required. This is a point made very clearly by Beer (1966) when he comments:

> "The cybernetic commentary on world situations is that controls for them cannot be designed in the sense in which most people would understand that term, because there is nowhere near sufficient understanding about the detailed structure of the organism itself, nor of the environment to which it has to adapt, nor of the interaction between these two" (p. 301).

And he goes on to argue that "...because the human head cannot contain all the variety of which the enterprise is such a potent generator" (p. 371) "...there is no doubt whatever that the enterprise has to be very largely a self-organizing system" (p. 378).

No similar control problem is perceived by operations researchers to exist in the pre-divisionalized state, presumably on the basis that no freedom is required for the members because, with the boss absorbing all the variety that there is, each member does essentially the same task in a repetitious and, hence, programmable way.

Durkheim concurs with OR in its judgements. In a debate which would be instructive for the management student of today, Durkheim established his position on the solution to the problem of order in modern specialised industrial society compared to the solution which was appropriate in pre-industrial societies. If we ignore for the moment Durkheim's obvious and self-conscious use of social concepts, it seems as though we have a meeting of minds between him and OR on how we should at least describe the problem of order. Lukes (1973) sums up Durkheim's views in the following way: any attempt to deal with the problem of control had to be

> "...an explanation which did not, like Comte, exaggerate the role of consensus and conformity, of shared beliefs and sentiments, and of uniform patterns imposed on individual behaviour, and which allowed for increasing differentiation of occupation, beliefs and behaviour; an explanation, secondly, which did not, like Spencer, assume a harmony of interests, but postulated a complex social regulation of behaviour; and an explanation, finally, which, unlike Tonnies and also Comte, detached such regulation from the State, linking it rather to the 'internal' functioning of society, and to the processes of social differentiation" (p. 147).

All of these points are to be found in the descriptions of the operations researchers which we have considered so far, although the latter use different language.

To conceptualise the distinctive differences in the control problems in the simple and complex situations, Durkheim coined the terms 'Mechanical Solidarity' and 'Organic Solidarity'. As Lukes (op cit) puts it, this distinction was, for Durkheim, a "...way of stressing the social differentiation of 'organized' societies, involving interdependence and multiplying specialised roles, beliefs and sentiments as opposed to the undifferentiated unity of uniform activities, beliefs and sentiments and rigid social control found in 'segmental' societies" (p. 148). The parallel between Durkheim's thought and thinking in OR can be taken further if we allow the substitution of OR's notion of management which we found in the last chapter for Durkheim's notion of the conscience collective, which for Durkheim is the repository of collective overall goals (etc). I shall say more on this notion later. If we do substitute the notion of management for conscience collective in the following quotation from Rex (1963), there is a striking homomorphism. Rex says:

> "...whereas the conscience collective in simple social conditions lays down immediate ends and detailed means for achieving them, in more complex conditions it sets only more generalised means and leaves the individual free to choose the intermediate ends" (p. 99).

The difference between Durkheim's and OR's account of the nature of the problem of order in simple and complex situations is obviously that Durkheim sees the problem in social terms whereas OR does not. For OR it would appear that the division of labour is explicable in terms of rationality alone: it is more rational to divide responsibility and therefore it occurs. It would also appear that OR reasons in the same way about its own role: the division of labour rationally produced is susceptible to rational control. However, this way of conceiving of the problem of order, characteristic as well of economists, was seen by Durkheim to be fundamentally misconceived. For Durkheim, to conceive the problem of order in this way was to conceive it in a theoretical vacuum. The fundamental sociological insight which Durkheim applied to the problem of order (an insight which earned him the title of one of the founders of sociology) was that we are only able to think of the problem as a problem of rationality because order was an already accomplished social fact. For Durkheim there is no gap between his version of the problem of order and that presented by OR--except that OR's is not explicit. Durkheim argued that the rational problem of order would not exist were it not for the fact that it was already posed in the existing social organization which, as a social fact, defines the essence of the division of labour. As Aron (1967) puts it:

> "...explanation...(of the division of labour)...in terms of the rationality of individual conduct strikes Durkheim as a reversal of the true order. To say that men divided the work among themselves, and assigned everyone to his own job, in order to increase the efficacy of the collective output is to assume that individuals are different from one another and aware of their difference before social differentiation...Therefore, the the rational pursuit of an increased output cannot explain social differentiation, since this pursuit presupposes that very social differentiation which it should explain" (p. 26).

On the face of it, Durkheim is concerned with the very thing that OR is not. The "...problem on which...(Durkheim)...embarked... was nothing less than the nature of social solidarity itself" (Lukes, op cit, p. 139). The task before us, then, is to show that Durkheim is correct in his (implicit) assessment of OR: that it is impossible for OR to attempt to hermetically seal itself off from conceptions of the social world. Let us see intially whether OR accepts the terms of Durkheim's version of the problem of order.

The Individual and the Collective:

The terms in which Durkheim sets up the problem of order are spelled out by Aron (op cit) as follows:

> "The theme of Durkheimian thought...is the relation between the individual and the collectivity. The problem might be stated thus: How can a multiplicity of individuals make up a society? How can individuals achieve what is the condition of social existence, namely, a consensus?" (p. 21).

On what basis is this relationship resolved?

As an initial move, Durkheim extends his sociological insight in a direction which some have found objectionable. He extends the idea that the economic level of reality depends upon and presupposes a social reality to the much more specific and stronger proposition that the social is a reality sui generis. As Lukes (op cit) puts it, as regards the individual and the collectivity, Durkheim made "...an ontological distinction between levels of reality..." (p. 19). This distinction immediately raises the problem of the relationship between the two levels. It is the criteria by which this relationship is resolved which creates (and solves) the problem of order. I shall return to the question of the criteria actually employed by Durkheim. Does OR see its problem of order as a problem of the relationship between different levels of reality?

A problem noted by Churchman et al (op cit) is precisely this: "if a large group of persons attempts to accomplish some task, it may be impossible for them to act as a single person would" (p. 5). Also, Ackoff (1977) argues that "...objectivity is the social product of an open interaction of a wide variety of individual subjective judgments"; further, "objectivity is obtained...when all possible values have been taken into account...". As OR works for organizations which have "...three basic types of responsibility: to themselves, to their parts and to the whole of which they are part..." it is the "...responsibility of manager and management scientist to try to dissolve or resolve these conflicts..." (all quotations, p. 6). This states the individual/collectivity relationship problem in very clear terms. From our analysis in the previous chapter we can also assert that this problem is much in evidence in the works of Stringer and Beer. It is the central theme of a paper by Churchman (1968c) which presents his "Case Against Planning". Churchman presents two alternative forms of resolving the relationship, resolutions which define distinctive managerial philosophies. First, there is the current situation of "reflective management" (itself a development from an earlier "forceful" management). This philosophy has five axioms. These say, essentially, that (i) everyone should contribute to the plan, (ii) each person contributes in a specialized way, (iii) the object of the exercise is to promote the interests of a relevant public, (iv) there is a hierarchy of contributors, ranked in order of the importance of their contribution, (v) management reflects so as to choose the best plan to serve the public. In this philosophy the public is merely 'represented' in the planning process. The individual's interests are taken care of only insofar as the individual is "part" of the collectivity. Churchman's reaction to this this philosophy is to dismantle the last four of the planning axioms: "Expertise cannot be segmented and separated"; "No one's contribution can be represented"; "Contributions to the plan of social systems cannot be ranked"; "The role of reflection in planning is to maximize contributions to the plan". Churchman summarizes the difference between the two philosophies in the following way:

"...the whole 1968 planning process is predicated on the false and untenable position of serving the public's interest by means of expert representation. This old-fashioned assumption of 1968 is false because in reality the public's "interest" is to plan, to mean something in the planning process, to be a master, not a servant, in planning the system" (p. 76).

All of these views by operations researchers beg the question of the criteria which are to be applied to resolve the relationship between the individual and the collectivity (although answers are latent in them as well).

The Boundaries of the Problem of Order:

Durkehim claimed to have found a basis for the selection of criteria on which the individual/collectivity relationship could be resolved in his specif interpretation of the role of the social reality. His interpretation of this role (for an elaboration of this line of thinking see: Berger and Luckmann, 1966) sets conceptual boundaries to the problem of order. These boundary constraints have been the subject of much discussion in social science. I turn to this later. Let us now extend Durkheim's vision of the problem of order (or control, which for him is synonymous with order).

In simple situations characterised by 'mechanical solidarity' control is the direct correlate of social organization. In the complex situation the connection between social organization and control is attenuated. The problem which exercised Durkheim (and has subsequently exercised his followers) was how was control still possible and actually achieved in the complex situation. In a simple situation no real problem exists. In this case society, being a reality *sui generis* dominates the individual/collectivity relationship. As Lukes (op cit) puts it: Durkheim "...wished to explore how individuals are attached to and controlled by societies, how collective beliefs and sentiments are inculcated, how they change, how they are affected by and how they affect other features of social life, how they are maintained and how they are reinforced" (p. 6). To do this one of his essential ideas was the "...necessity of explaining individual phenomena by the state of the collectivity, and not the state of the collectivity by individual phenomena" (Aron, op cit, p. 27). In the simple situation Durkheim conceived of the collectivity as the *conscience collective*. It was seen as being comprised of the beliefs and sentiments which were average in a society and it had the distinctive property of forming a level of reality over and above individual *consciences*. The *conscience collective*, Durkheim thought, had a 'life of its own', even though it was only capable of realisation through individuals. With the use of the notion of *conscience collective* Durkheim quickly disposed of the problem of order in the simple situation: the *conscience collective* takes complete control. Mechanical solidarity exists (the problem of order is solved in the simple situation) where the *conscience collective* is "...extensive and strong and 'harmonizes men's movements in detail'...; in these conditions, 'the individual *conscience* is scarcely distinguishable from the *collective conscience*' and 'collective authority is absolute', whether it is diffused throughout the community or incarnated in it chiefs" (Lukes, op cit, p. 152).

Detailed direction was seen as the solution in the simple case (cf. Aron, op cit, p. 25). The concern of most of Durkheim's work was how to overcome the fact that this solution would not work in the case where there was a complex division of labour. The simple solution was unworkable because, as Aron puts it:

> "...in societies of which differentiation of individuals is characteristic, everyone is free to believe, to desire, and to act according to his own preferences in a large number of circumstances...Durkheim believes he sees in organic solidarity a reduction of the sphere of existence embraced by the collective consciousness, a weakening of collective reactions against violations of prohibitions, and above all a greater margin for the individual interpretation of social imperatives" (op cit, p. 25).

The difficulties of control with a complex division of labour have also been highlighted by Selznick. He argues that "...delegation is the primordial organisational act, a precarious venture which requires the continuous elaboration of formal mechanisms of co-ordination and control" (1963, p. 358). Both Selznick and Durkheim recognise that formal controls are inadequate. Selznick points out that human beings are "recalcitrant tools of action" and Durkheim sees "...man...(as)... a being with potentially limitless and insatiable desires..." (Lukes, 1967, p. 143). Apart from the fact that this makes detailed control impossible, it is undesirable because "...the conscience collective must leave free a part of the individual conscience, so that special functions may be established there which it cannot control..." (Durkheim, quoted in Lukes, 1973, p. 153). In the complex situation the freedom of individuals was, then, both inevitable and desireable. How is this commitment to 'individualism' to be reconciled with his still firm commitment to the collectivity setting the criterion whereby the 'order' in a society was to be judged? In other words, by what means can it be insured that 'free' individuals are oriented (at least in their actions) towards the collective?

The means whereby Durkheim saw this being achieved was in a change of role for the collective. Rather than having the general role which it did in the simple situation, in the complex situation it was to become specialised. Durkheim emphasized this role change by changing his terminology: in the simple situation the conscience collective represented the functions of the collective; in the complex situation its functions are represented by representations collective. In that the conscience collective referred to all forms of beliefs and sentiments, representations collectives referred to "the way in which the group conceives of itself in its relations with the objects which affect it" (Durkheim, quoted in Lukes, 1973, p. 6). As Lukes puts it: "Durkheim made much of what he called the independent reality of representations collectives" (p. 7); they are, in a sense, 'above' the conscience collective which is common to a population. Lukes goes on:

> "He used the analogy of the individual's mental states, or individual representations, which, though intimately related to their 'substratum', brain cells, from whose combined activity they result, cannot be reduced to and wholly explained by them, but have their own characteristics, and are relatively autonomous, and can directly influece one another and combine according to their own laws" (ibid).

This analogy is put to full use by Durkheim in his development of a role for the collective which would resolve the problem of order. Thus, of the State, a particular example of representations collectives, Durkheim says it is "...the seat of a special and delimited consciousness, but a consciousness higher and clearer, having a more lively feeling of itself" (Durkheim, quoted in Coser, 1960, p. 221). And in distinguishing administrative action from State activity, Coser argues that Durkheim sees them "...differ as the muscular system does from the central nervous system" (ibid). Durkheim argues that "'The state is, rigorously speaking, the organ of social thought...Its essential function is to think'" (Durkheim, quoted in Coser, ibid). And of society (which for Durkheim was to replace God as the object of Religious worship) he says:

> "Society is not at all the illogical or a-logical, incoherent and fantastic being which it has too often been considered. Quite the contrary, the collective consciousness is the highest form of psychic life, since it is the consciousness of consciousness. Being placed outside of and above individual and local contingencies, it sees things only in their permanent and essential aspects, which it crystallises into communicable ideas. At the same time that it sees from above, it sees farther; at every moment of time it embraces all known reality; that is why it alone can furnish the minds with the moulds which are applicable to the totality of things and which make it possible to think them" (quoted in Coser, 1971, p. 138).

Durkheim's elaboration of the initial ontological distinction between the collective and the individual in this way is clearly mirrored by the views of Beer and Stringer which we considered in the previous chapter. Like them, Durkheim sees the collective as the embodiment of all that is rational, global, and thoughtful; the individual is represented as the determined, narrow, and habitual/automatic. For Durkheim, "...man is a bundle of desires, which need to be regulated, tamed, repressed, manipulated and given direction for the sake of social order..." (Lukes, 1967, p. 145). A strikingly similar view was expressed by both Stringer and Beer. For the latter it provided the raison d'etre of OR.

In a weaker form, but one which nonetheless captures the essence of Durkheim's distinction and which also describes his view of the role of the collective in the complex situation, Churchman et al (op cit) come to a similar conclusion. They come, finally, after having considered several "simple" control processes to the highest--the "reflective goal-changing unit". They note that "the mechanism that considers various goals and courses of action can be called the consciousness of the organization" (p. 82). After having analysed its various properties they conclude that the consciousness of the organization can be identified with what the executives do or do not do, and that the function of OR is to enhance the capabilities of it (pp. 84-5).

A similar distinction is to be found in the work of Friend and Jessop (1969) in their concept of "Strategic Planning". They introduce their work with the comment that "it addresses itself in particular to those more strategic levels of choice which tend to be linked with the terms 'planning' and 'policy-making'..." (p. xix). Closely parallelling Simon's distinction between 'programmed' and 'non-programmed' decisions, they demarcate the "strategic" from the "operational" by arguing that "...any process of choice will become a process of planning (or strategic choice) if the selection of current actions is made only after the formulation and comparison over a wider field of decision relating to certain anticipated as well as current situations" (p. 110). Strategic choice is "...the process of visualizing possible designs for the long-term future of a community..." (p. 111) involving a "...capacity to anticipate the future and yet also to adapt to the unforeseen" (p. xxiv). Operational choice, on the other hand, is "...essentially <u>determined</u> by a generic viewpoint, whether this is made explicit through the laying down of formal regulations, or is implied in the growth of conventions which reflect the perspectives and experience of those concerned" (p. 105, my emphasis). At the operational level, they say, little "discretion" is involved; at the strategic level much discretion (which they equate with "thought and discussion" (ibid)) is involved and, in the last instance, the yardstick by which strategic planning is judged is, they go on to say, by the capacity which it provides to "...make <u>intelligent</u> decisions..." (p. 113, their emphasis).

More recently, Chacko (1976) uses the analogy of an orchestra to determine what it is that constitutes management ("the substance of the OR/SA...(Systems Analysis)...." idea (p. 81)). The difference between an orchestra with and an orchestra without a conductor is that "without the conductor, the best of orchestras would only produce a collection of <u>individual</u> sounds..." (p. 86). The conductor sees the "whole"; understands the "intent" of the composer and can "evaluate" the performance (ibid).

All of these operations researchers appear to accept the boundaries which Durkheim lays down for the solution to the problem of order in the complex situation. The boundaries are, fundamentally, that we are to take it for granted that order is to be found by a reconciliation of two givens: the intelligent collective and the determined individual. These descriptions by operations researchers would clearly fall into Churchman's 'old-fashioned' category. Later on I shall consider some more modern schemes. For the present it should not be hard to anticipate both Durkheim's and OR's solution to the problem of order. Let us go on to state both solutions to the problem of order before encountering it in its modern form and then going on to ask on what assumptions both solutions rest.

Durkheim's and OR's Solution to The Problem of Order:

Durkheim is often portrayed as a crude social determinist. This is both true and not true. He is a social determinist, but he is not crude, at least when he is dealing with organic solidarity. Durkheim wished to distinquish 'social facts' from other sorts of facts. He defined a social fact as "'every way of acting, fixed or not, capable of exercising over the individual an external constraint' and further as 'every way of acting) which is general throughout a given society, while existing in its own right, independent of individual manifestations'" (Durkheim, quoted in Lukes, 1973, pp. 10-11). Lukes comments that "Durkheim's definition embodies three distinguishing criteria: externality, constraint and generality-plus-independence" (ibid). I would also stress that the definition implies that a social fact is behaviour which is deemed 'social' by its correspondence, intentional or not, to collective external standards. Durkheim is not a crude social determinist because he does not insist that external constraints cause behaviour in the strong sense of that term. This would require the constraint to be both a necessary and a sufficient condition of the social behaviour. Durkheim only argues that external constraints be necessary conditions of the behaviour for it to be called social. No other interpretation is consistent with his insistence on the 'freedom' of individuals. Also this interpretation is consistent with his insistence on the collective setting the standard by which order is to be judged.

Social facts for Durkheim, then, are 'external' to individuals in that they exist both 'outside' and 'prior' to the individual. They are constraining in the sense (which Lukes feels is "ambiguous") "...social and cultural factors influence, indeed largely constitute, individuals" (Lukes, op cit, p. 13); they are 'general-and-independent' in that they emanate from the collective: "...the social fact is not social because it is general but rather 'general because it is collective (that is, more or less obligatory)...it is a state of the group which is repeated among individuals because it is imposed on them'" (Lukes, op cit, p. 15). The problem of order is solved, therefore, in the face of individual diversity, by the collective issuing general constraints to which individuals conform. Individuality is maintained, in this formulation of the solution, because individuals are 'free' within the general constraints. Order is achieved because action within these constraints 'calculated' to achieve the collective purpose. Order is achieved, in short, by the collectivity setting the 'context' of the behaviour and individuals (in some unexplained way) taking note of it in their actions.

It is unclear exactly how Durkheim's solution works, and how robust a solution it actually is. Of particular concern are the questions of how, exactly, individuals are 'constituted' as social beings, and at what level of complexity does the solution of external constitution hold good? Durkheim does not give specific answers to these questions, but they have been of considerable interest to some modern social scientists. I shall turn to some of their work later. I shall do this because, as we shall see, some operations researchers have expressed a dissatisfaction with the simple Durkheimian solution. Nevertheless, the reaction to the simple Durkheimian solution has been to modify it rather than to dismantle its basis, which is that an unexplained collectivity solves the problem of order. Let us, then, set the scene for what is to come by comparing Durkheim's simple solution to the problem of order with solutions to be found in the OR literature.

Support for the argument that OR adopts the Durkheimian framework can be found from a slight extension of our analyses of Stringer and Beer presented in the previous chapter. Stringer, it will be recalled, envisages a situation where there are several organizations each pursuing their own goals. The task for OR, it being unable to rely on what Stringer presumes is the detailed control available in a normal organization, is to provide advice to a "...central planning authority...(on)...determining the optimum interventions to make in a decision process which is decentralised" (1967, p. 111). The individuals, pursuing their own goals, have to be constrained by the interventions of the collective (by, for example, "...the control of funds,...the exercise of statutory powers,...(and)...the publication of plans and estimates and advice together with exhortations of various kinds" pp. 110-111). The interventions are calculated, through the study of the decision-making system, to influence the decisions of the individual organizations so that they are "...beneficial to the purpose which defines the multi-organization" (p. 109). For Durkheim behaviour is social if it is productive of the goals of the collective, and anything which constrains behaviour in this direction is a social fact. Again, we can fruitfully draw a strong parallel between Stringer's views and those of Simon. This parallel will provide support for the suggested connection between Stringer and Durkheim.

A key idea in Simon's work is that in order to control an individual's behaviour, one should control the "premises" upon which he is assumed to base his decisions. One does not have to change the individual as such. At various points in his work it seems that Simon feels that individuals are so complex that this latter option is not a viable one in any case (cf. Perrow, 1972, who argues that Simon should be seen as a sociologist rather than as a psychologist). The premises of decisions are such things as the communication system, the 'vocabulary' of the organization, the 'programs', the rules and regulations, etc. All these things are to some extent open to control by management. Thus, they can 'guide' the individual to make decisions which are "...not only the product of his own mental processes, but also reflect the broader considerations to which it is the function of the organized group to give effect" (Simon, 1957, p. 102). It is this socialization of the individual "...that enable(s) him to make decisions, by himself, as the organization would like him to decide" (p. 103), because "the rational individual is, and must be, an organized and institutionalized individual" (op cit, p. 102). These remarks of Simon seem to sum up Stringer's argument very well. They are also strictly in line with Durkheim's views (1).

Let us now turn to Beer's cybernetic solution to the problem of order. Beer, too, entertains a Durkheimian concept of social facts as the constraints which emanate from the collectivity to guide (but not control in detail) the behaviour of the individual so that he, of his own 'freewill', produces behaviour (his own 'action') which meets the goals of the collectivity. Beer accept wholeheartedly the necessity for the parts of the organization to exercise freewill, because it is the only way in which the organization can meet the requirements of the law of requisite variety. How does he conceive of control in this situation?

Arguing that systems of <u>all</u> types conform to the "natural law", which says that their inner principle of organization will direct them to their maximum likelihood state with respect to their environment, he moves directly to the solution of the problem of control by suggesting control of the environment. Thus, he says, "the basic answer of cybernetics to the question of how the system should be organized is that it ought to organize itself" (1966, p. 346). He realizes, however, that "to exercise control...and to meet the appropriate set of goals,

(1) I think that Perrow is slightly misleading when he says that Simon's work fills out the "Weberian Skeleton"--Weber's ideal type of bureaucracy. Simon is not a Weberian because he pays no heed to Weber's concept of social fact which requires recognition in one's action of the intention of another (cf. Weber, 1964).

something must be done" (p. 354). His solution, building on his belief that "natural systems...(of which organizations are an example)...organize themselves over a period of time to be what they immanently are" (p. 359), is to claim that what organizations immanently are can be controlled.

> "The answer is not to design the control, but to constrain the system. Let us so uncouple a sub-system that the natural movement of increasing entropy within it will tend towards one of the goals. The design or management task is to set the structure of the system to determine sub-systems in which the process of entropy is so defined that it will find a way (which we may neither detect nor understand) to cope with disturbances from outside" (p. 354).

> "The management of high-variety systems is always concerned with a definition of entropy which serves particular goals, and with defining success as the maturation of a system in its maximal entropy" (p. 355).

The manager is only interested in the outcomes produced by the sub-systems from his own point of view: "...if a system is moving towards what he regards as desirable ends, it is 'under control'" (p. 357). He controls the system by injecting "bias" into the "alleged randomness" of the interacting sub-systems (by means of punishments and rewards--the "algedonic feedback loop") so that the desired outcome is produced. Thus:

> "Cybernetic insights show, in particular that the totality of the organization ought to be made up of building blocks that

will be called _quasi-independent domains_. This is a compromise notion lying between actually independent domains (decentralization) and no domains at all (centralization). These domains have a certain local autonomy and may (in their own language) claim to be altogether autonomous. But they are not autonomous _in the metalanguage of the whole system_, which monitors their activity according to the laws of cybernetics...The extent to which domains are (metalinguistically) independent derives from the need for local fluctuations without which local homeostasis, still more learning, is impossible...(latter emphasis mine).

"On the other hand, it is vital that all these local controls be _mediated_ centrally; otherwise they will sub-optimize and destroy the total system by (as it were) internecine strife" (pp. 381-2).

The image is "...a control system which operates itself, but which can be monitored from on high, and given new directions towards predetermined goals which it does not itself recognize and of which it cannot indeed be made aware" (p. 383). We have in Beer's work a clear Durkheimian solution to the problem of order. The individual is 'constituted' by the collectivity which operates as an autonomous domain. As Beer puts it, we have a control system for the enterprise which is "...essentially. ..(a)...self organizing system, with hierarchical overtones...(p. 383)...by which it is implicitly informed..." (p. 379).

That Friend and Jessop view the problem of order in the same way can be seen by again employing Simon's work as a basis for comparison.

They argue that we can understand decision-making by the individual "agencies" in local government (akin to departments in an industrial organization) by reference to what they call "the context of operations". By this they mean "...the general view of the way in which appropriate actions should be selected" (p. 105). The factors which "contribute" to it are "...the operational policies themselves, the local objectives, and the appreciation of external constraints..." (ibid). This concept seems equivalent to March and Simon's (1958) "definition of the situation". This describes for the decision-maker the "...set of "given" characteristics of the situation...(which)...include knowledge or assumptions about future events, knowledge of alternatives available for action, knowledge of consequences attached to alternatives...and rules or principles for ordering consequences or alternatives according to preferences" (pp. 150-1). March and Simon claim that knowledge of the definition of the situation will enable us to predict behaviour (p. 151). Their's is a more elaborate description of the influences on decision-making behaviour than that provided by Friend and Jessop, but the latter claim, nonetheless, that "the context influenc(es) all...stages of the ongoing decision process..." (ibid).

Friend and Jessop are concerned with modification of the context of operations "...as situations arise...which cannot easily be dealt with within the existing context" (ibid). Processes of adaption are also fundamental to March and Simon's view of organizational functioning. They distinguish between "short-run adaptiveness", which they call "problem-solving", and "long-run adaptiveness", which they call "learning" (p. 170). In describing processes of problem-solving which account for short-run adaptivity they emphasise the "boundaries of rationality"--the fact, in their view, that "...human beings as organisms...(are)...capable of evoking and executing relatively well-defined programs but....(are)...able to handle programs only of limited complexity" (p. 171). Thus, they emphasise how, in the face of novel situations, efforts are made to reduce complexity. This is achieved by (amongst other things) identification with a sub-goal and role. This reduces the number of "stimuli" to which "attention" is "directed". Complexity is also reduced by factoring tasks into semi-independent programs, by the development of specialized vocabularies which screen out much of the information potentially available, by the "absorbtion" of uncertainty by the "reification" of organizational categories and sources of information, by use of specialized communication channels, etc.

Friend and Jessop argue in the same way about short-term adaptivity. In their description of the decision-making activities of agaencies they see it dealing with three basic types of information. These are: information relating to values; information relating to related decision fields, and information relating to the environment. Their description of decision-making processes can be said to follow that of March and Simon in that they concentrate on how uncertainty relating to these three categories of information is handled. An agency may perceive uncertainty in any one or all of these areas of information. Although efforts are made by the agencies to change the context of operations to meet uncertainty, they see this as very attenuated. In gathering information about the environment, for example, they note that "...each agency tends to build its own internal system for gathering and interpreting of information; and it is a common experience for this tendency to operate in such a way that any potential value of information to other agencies becomes very difficult to realize" (p. 123). Later on (p. 272 ff.) they speak of their worries about "...breaking down barriers of communication between existing professions and of promoting more meaningful dialogues with lay representatives" (p. 273). These comments suggest that such information as each agency collects will be restricted to that which is meaningful to itself. As regards information relating to values, they speak of the role of "...any conventions as to matters on which the section head should seek the guidance of his departmental head, because of uncertainty about any political implications or about the 'right' system of values to apply in putting forward a recommendation" (p. 123). On gathering information on related decision fields they argue that "...any agency will tend to adjust the number and capacities of its external channels of communication only to such a level as does not result in any serious instability within the agency itself" (p. 124). There is, they suggest, "...some kind of 'law of conservation of stress'..." (ibid) which restricts the extension of communication channels.

They are particularly worried about this last feature of the decision-making processes of agencies because they "...develop a picture of inter-agency planning activities which suggests the desirability of flexible patterns of organization for planning, in which planning frameworks are not necessarily constrained either by hierarchical relationships or by continued existence over time" (p. 126). However,

> "Because of the limited perspectives of agencies, and the protective forces within them, it is by no means certain that local initiatives alone will be sufficient to generate a relevant pattern of connections, and to adapt to changing

circumstances..." (ibid).

And yet they insist that "the extent to which the context of operations changes will depend on the...system's capacity to learn..." (p. 105). How, then, can things be arranged so that the system as a whole can learn when the parts cannot?

If the agencies themselves cannot, because of their inherent limitations and biases, be relied upon to generate appropriate patterns of connections we have, they say, to rely on a "third party": "it may perhaps only be some third party who can see with clarity the need for a connection..." (ibid). They perceive the need for a "centralized strategic control function" or a "nucleus for strategic control" (p. 129). It is only this, they say, which can solve the "...problems of mobilization, regulation, and scheduling of planning activities involving several different combinations of agencies..." (p. 127). The function of this nucleus of control is with "how the planning process itself should be organized..." (ibid); its job is to "regulate" and ensure consistency between the agencies. Having provided us with this much of the Durkheimian solution to the problem of order they shrink from setting it out in full. They refer to the strategic control function "...regulating in some undefined way..." (ibid). However, the only type of solution which is consistent with the model of the agency which they provide is a Durkheimian one of external constitution. Given their starting points, they seem forced to accept the model put forward by March and Simon for long-run adaptivity (learning) whereby the "...programs of the members of higher levels of the organization have as their major output the modification or initiation of programs for individuals at lower levels" (op cit, p. 150). What other role for the strategic control function can be envisaged within their scheme other than that:

> "the attention of high levels...(should)...be directed principally to those proposed innovations that have significance for the maintenance of organization structure, for the survival of the organization, or for activities in more than one organization sub-division" (op cit, p. 199)?

Also, given their image of the limitations of the individual agencies, they could not possibly suggest that this function could be carried out by full participation with the agencies. The need for the regulatory function arose in the first place because the agencies by their very nature could not be relied upon to "generate a relevant pattern of connections" (op cit, p. 126). If the agencies were not by their very nature in need of external regulation and control the solution might be to give them more information. This is not suggested and with this comes the implication that the agencies are to be constituted as part of the collectivity by external determination.

This implicit feature of the work of Friend and Jessop is, of course, made explicit by their colleague, as we have seen. I refer, of course, to Stringer (1967). It is also made explicit by Ackoff in his development of a new philosophy of planning ("adaptavizing"). This new philosophy is contrasted with conventional OR ("optimizing"). Arguing that normally the "...optimizing planner...takes the system structure for granted...", he suggests that a better way to proceed would be to "...change the "system" in such a way that more efficient behaviour follows "naturally"" (1970, p. 20). Ackoff's "...principle of control...involves motivating participants in the system to act in a way that is compatible with the interests of the organization as a whole, and it does this by providing incentives that make individual and organizational objectives more compatible" (ibid). Later on he makes it clear that he understands that incentives work as external constraints. He argues that "in planning, all existing incentives (even those that are implicit) should be identified and evaluated to make sure that they <u>induce</u> behaviour that is consistent with corporate objectives and goals" (op cit, p. 108, my emphasis). The idea of inducing behaviour to be consistent with collective goals is Durkheim's. It is not consistent with spontaneous and individually produced behaviour. This point has been the focus for a modern revision of Durkheim's solution to the problem of order, especially by Parsons. Ackoff also, notwithstanding his views expressed here, has been concerned with the "purposeful" nature of individual behaviour. Let us postpone a detailed look at Ackoff's views on this and turn to consider the 'modern problem of order' which has arisen as a reaction to the over-emphasis which Durkheim placed on the collective and his under-emphasis of the individual [1].

Difficulties with Durkheim's Solution to the Problem of Order:

Of crucial importance is whether we can understand Durkhiem's notion of the collectivity. If we substituted it for the notion of management which we found in Stringer's and Beer's writings, would we have a clear picture of management's nature? The collective can be seen to play an important role in OR's solution of the problem of order, and yet in its Durkheimian form it is fundamentally opaque. We cannot understand either its rationality or how it functions. It has been a persistent criticism of Durkheim's views that because of

(1) I shall give Ackoff's important work with Emery (1972) detailed consideration later on.

his insistence that social facts can only be explained in terms of other social facts, the nature of the social could not itself be explained. We saw in the last chapter that the nature of management was only explained if one was prepared to accept assertions about it which were themselves unexplained. This, and the implication of the views which we considered in the previous section that the collective constituted individuals in some unexplained manner, comes very close to what Rex (op cit) has called the 'group mind' fallacy to be found in Durkheim's work:

> "Durkheim amongst others has been accused of accepting the 'group mind' fallacy because of his...attempts to give an account of this kind. The point to be made, however, is not that the concept of group mind as such is illegitmate. Provided that its meaning is made clear and statements are made about it in verifiable form, there is no reason why it should not be introduced as a theoretical model. What is illegitimate is the reification of the concept which is inevitable if we follow Durkheim's rule that a social fact must be regarded as existing independently of its individual manifestations" (p. 46).

The central criticism which Rex stresses is that "a quite central fact about Durkheim's sociology is that because of his emphasis upon the externality of social facts he could never really begin to analyse the elementary concept of a social relation" (ibid) and so he could not begin to understand 'society'. The point that Rex is making is that we cannot analyse the collective by taking it as a given starting point. To simplify his argument, Rex is saying (as have many others) that to understand the collectivity we have to understand how it is made up.

A similar worry has been expressed in OR circles. There the worry takes the form that for the development and imposition of plans by management (and OR) to be rational takes the rationality of management (and OR) for granted. In complex situations, this level of rationality cannot be straighforwardly assumed. We cannot, the worry goes, start from the assumption of rationality (in the form of a rational plan)--we must demonstrate it. Thus, Friend and Jessop criticize the "...tendency to distort reality through imposing hierarchical patterns of thought..." (op cit, p. 126). In their view "the set of connections between agencies is always likely to be a changing one; connections will always tend to form in an *ad hoc* way to deal with particular current problems, and these may tend to fade away when these problems have been resolved in an acceptable way" (ibid). It is, therefore, a mistake, they argue, to attempt to treat these flexibly created planning frameworks as though some were subordinate to others (ibid). This line of thought, away from the latent Durkheimian view, has been more fully developed in the subsequent work of Friend, Power and Yewlett (1974), where the emphasis is on the "mutual adjustment" processes of the individual agencies, aided by a "reticulist". A strong shift away from the Durkheimian view where the rationality of the collective is given is also evident in Churchman's views. We have already seen him, for example, attack the role of the representative and argue for the individual as the focus of planning. Thethrust of Churchman's work, as we have seen in earlier chapters, is against the acceptance of a given *weltanschauugn*, and towards the creation of new *weltanschauugen*.

The emphasis in modern thinking is towards process (cf. also, Checkland, 1975; Mason, 1969; Eden, 1976) in planning. An attempt, apparently, to understand the collective from the viewpoint of the individual. A major exponent of this view is Ackoff.

Ackoff criticizes hierarchy and central control: he argues that

> "not everything that can happen can be anticipated. The number of unexpected things that do happen is too large to be handled by a centralized control unit such as are usually designed by optimizing planners. Every part of an organization should be made capable of exercising self-control and of responding effectively to the unexpected even when not controlled from above" (1970, p. 14).

Ackoff provides a very clear statement of the modern problem of order, so let us briefly consider his version of it before we turn to examine the assumptions which underlie it. Then we shall see that although many in OR and the social sciences move towards process they do not thereby move away from the difficulties of the Durkheimian solution with its reliance upon a given collectivity.

<u>The Modern Problem of Order in OR</u>:

The central focus of Ackoff's adaptavizing is "...responsiveness planning...(which)...consists of building responsiveness and flexibility into an organization" (p. 17). The route to the achievement of an integration of the individual and the collective is not, therefore, seen as the imposition of the collective onto the hapless individual. This solution, Ackoff argues, would only make sense if we had a much greater degree of knowledge than we do have (and, he notes, that human motivation was completely programmable). Thus, he argues, "at present the best that can be done is to optimize either complex...(organization)...structures relative to very simple problems or simple structures relative to complex problems. As yet", he goes on. "we cannot optimize complex structures relative to complex problems" (op cit, p. 13). The solution which Ackoff proposes is to modify our conception of the problem of order from one of intellectual resolution and imposition to one of continuous process and building up. Thus he argues that adaptavizing is "...based on the belief that the principal value of planning does not lie in the plans that it produces but in the process of producing them. In effect", he goes on, "the adaptavizing planner's slogan is, "Process is our most important product"...(and)...hence it holds that the value of planning to managers lies primarily in their participation in the process, not in their consumption of its product" (p. 15). It is on this basis that he advocates "multiheaded organizations" in which "...there is no single ultimate authority..." (1974, p. 38). The ideal is a democracy where all are equal and the "parts" are served by the organization rather than the parts serving the organization.

Part oriented and multiheaded organizations are, he says, "humanized". Humanizing organizations is a necessary condition for them to exercise self-control. Humanized organizations allow their members to be "purposeful", and a "...purposeful system is one that can change its goals in constant environmental conditions; it selects goals as well as the means whereby to pursue them...(I)t thus displays <u>will</u>" (Ackoff and Emery, 1972, p. 31). Organizations with purposeful members are "variety increasing". Treating members as "multi-goal-seeking systems", whereby they can "...pursue different goals, but they do not determine the goal to be pursued--the environment does" (ibid), makes the organization "variety decreasing". Decreasing the variety of individuals was a major objective of Durkheim's solution to the problem of order. He would almost certainly have perceived individuals as multi-goal-seeking-systems. Ackoff, on the other hand, wants to promote the "...creation of new courses of action and policies--rather than evaluation of old ones--(because this)...is a key to successful planning" (1970, p. 43). Rather than control the individual as Durkheim enjoins us to do, Ackoff wants to set his imagination free by involving him in the "...design...(of)...an idealized future for the system being planned for" (1974, p. 30). In short, Ackoff wants to use "the human being (even the manager)...(as)...a special kind of resource...(which is)...capable of making decisions" (1970, p. 81). "...(H)uman beings are, in a sense, general purpose computers...(which) ...cannot be programmed like equipment" (op cit, p. 82).

Ackoff wants to solve the problem of order not by imposing a predetermined collective view on the individual but, rather, by organizing such that a collective view can emerge from individuals. Thus,

> "The adaptive planner...sees changes of organizational structure as one of his most effective means of improving system performance. He believes that, if he designs an organization that is foresightful, innovative, and rational, much of the need for planning is removed" (op cit, p. 87).

In this statement of the modern problem of order, the collectivity as a given starting point seems to have disappeared. The collective is is organized individuals. In the place of the rationality of management we have the rationality of adaptive processes. These processes are claimed to be comprehensible through understanding individual motivation, group processes and principles of organization design. This, of course is Kuhn's line of argument: we understand science best as a social process of adaptation. We may be able to understand some organizational and scientific processes in this way, but can we also make their rationality transparent? If we look at the assumptions upon which the modern solution to the problem of order is based, we will find an unexplained guarantor slipping in through th backdoor. Management and Durkheim's collective may be out of sight, but they are not out of mind. We shall find that the suggested connection between OR and Durkheim's solution of the problem of order is a deep one.

<u>The Assumptions Underlying the Solution to the Modern Problem of Order</u>:

The critique of bureacracy which is currently animating some operations researchers has been traced by Mayntz (1964) as a key change which has taken place in the conceptual history of organization theory. She argues that it was the "...neglect of such important processes as goal-setting, decision-making, environmental relations, adaptive change, and in general of self-maintenance as a problem" (p. 98) which prompted a critique of the prevailing classical models of bureaucracy. Mayntz describes the model of organizations sought for by modern organization theorists (cf. also, Hickson, 1966-7 and Lawrence and Lorsch, 1967) as one which should

> "...permit more leeway for improvisation and personal responsibility; minimize the number of fixed rules; de-emphasize the principal of hierarchical authority in favour of de-centralization, team-work, and only conditionally activated

lines of communication; define the individual's area of competence and responsibility less strictly; and emphasize personal authority (expert or functional authority), personal relationships, and personal commitment to the organizational goal" (p. 99).

All of these features are to be found animating Ackoff's work (cf. his proposed design for a "circular organization" (1974)), and they are latent in the work of the other operations researchers I have mentioned. Mayntz makes the interesting point that whether such organizational models are proposed as descriptive (as most organization theorists would see them) or whether they are prescriptive (as most operations researchers would see them) they still articulate one basic underlying model. That is, unlike Gouldner (1959) she argues that there are not two basic models of organizations prevalent in the organization theory literature (the "rational" model and the "natural systems" model). She argues that there is only one model, and that is the social systems model. She argues that

> "both emphasize flexibility, adaptability, and innovative change...(I)n the one case they appear as properties of enduring social systems, and in the other case they are treated as pre-requisites of efficiency in a new and wider sense....(W)e may therefore conclude that modern organization theory does in fact imply one basic system concept, whether as explicit starting point or implicit in the results of analysis" (p. 104, latter emphasis mine).

The chief exponent of the social systems concept, and the person who has borne the brunt of the most pointed attacks on it, is Parsons (1949, 1951).

Parson' work was a reaction against what he saw as the "positivism" in Durkheim's treatment of social facts as 'objects'. As Giddens (1976) puts it:

> "Parsons' early work was directed towards reconciling the 'voluntarism' supposedly inherent in the methodological approach of Weber (and, from a different angle, foreshadowed in Pareto) with the idea of the functional exigency of moral consensus" (p. 95)

which was attributable to Durkheim. As Giddens says, Parsons' "...use of the term 'voluntarism' suggests that...(he)...wished to try to build into his own approach a conception of the actor as a creative, innovative agent, thus seeking to break with schemes in which human conduct is not comceptually differentiated from the explanation of the movements of objects in nature" (ibid). Because of his commitment to volutarism Parsons does not assume (as many popular account suggest) that the problem of order is easily solved. Parsons' view is that "...society represents a veritable powder keg of conflicting forces, pulling and hauling in all ways at once...(S)ociety is not a neatly articulated "organic system" in full control of its own internal processes and mechanisms. It consists instead of a loosely federated congeries of system and sub-systems of many different sorts..." (Devereax, Jr., 1961). Societal equilibrium--the establishment of order--is, for Parsons, something to be explained, and not taken for granted. Perhaps, then, we may find in Parsons' work the background assumptions upon which OR's interest in the modern problem of order is made sensible. There is, in fact a striking correspondence between the problem as it is seen by Parsons and as it is seen by Ackoff. Compare Landsberger's description of Parsons' concerns with Ackoff's view of the responsibility and task of management and management scientist. Landsberger (1961) notes:

> "According to Parsons, the central task of the social sciences is the formulation of a single theory applicable, after appropriate specification, to all kinds of social systems. Such a theory would show (a) how individually motivated units of such systems can attain their private ends while (b) simultaneously furthering the collective (i.e., the system's) end, (c) maintaining stable relationships with other units, and (d) remaining integrated both within themselves and with higher and lower units" (pp. 215-216).

Compare this with Ackoff's recently expressed view of the nature of organizations and the task for OR created thereby:

> "First, they...are purposeful systems that have goals, objectives, and ideals of their own.
>
> "Second, organizations contain as parts other purposeful systems that have goals, objectives, and ideals of their own.
>
> "And third, organizations are parts of larger purposeful systems that also have goals, objectives, and ideals of their own; and that contain other purposeful systems that have goals, objectives, and ideals of their own....
>
> "Goals, objectives, and ideals at these three levels may be in conflict...
>
> "It seems to me that it is the responsibility of manager and management scientists to try to dissolve or to resolve these conflicts and serve all of an organization's stakeholders in a way that reflects the relative importance of the organization to them" (1977, p. 6). It seems as though Parsons and Ackoff

have the same problem: how are the ends of all levels of systems met at once? Let us look first at Parsons' solution to this problem, and the assumptions which underlie it.

The problem is one of establishing an 'equilibrium' between potentially conflicting ends. Are these ends (particularly those of individuals) determined independently of each other, or do they have a systematic dependence upon one another? Parsons' seems to argue that if needs were determined independently then the task of establishing an equilibrium is nigh on impossible. Although he argues that the actor should be seen as an entity which can express free ('voluntary') choice amongst what he calls the "pattern-variables", he argues that for the choices to cohere they must be regulated (1). Parsons calls this problem of choice regulation the "motivational problem of order", and with good reason. He argues that they only way in which the motives, goals, capacities and values of the individual actor can be reliably motivated so that integrative behaviour is produced, is if they are oriented towards and take their cue from the "central value system". "Parsons argues that any particular social system will tend to develop a set of normative patterns which are somehow relevant to its own particular functions" (Devereax, op cit, p. 35). It is the central value system or 'shared orientation to action' which resolves the dilemmas of choice with which the actor as a free agent is faced. With a given central value system the actor can be relied upon to make choices which support these values, and he can confidently expect others to do the same. It can only be by relying on the guidance provided by the central value system that, for example,

(1) Pattern variables are generalised orientations which actors can take towards each other. As Devereax (op cit) puts it: they "...constitute universal and basic dilemmas confronting any actor in any social situation" (p. 39).

Friend, Power and Yewlett (1974, following Braybrooke and Lindblom, 1963) can confidently expect the system to be co-ordinated by processes of "mutual adjustment". Without this assumption we have as much reason for arguing that the system would degenerate into chaos if we relied on the choices of free individuals. This possibility is mentioned in passing by Power (1971). Commenting on Schon's (1971) proposals for decentralized, informal planning, he notes that "...it could create pockets of volatility and unpredictability ..." (p. 48), but then he moves onto other "difficulties". Acceptance of the Parsonian framework within which this difficulty is minimized is a sufficient (but not necessary) condition for indifference towards this as a potentially major problem.

What happens to the individuality of individual ends when the central value system solves the problem of order? Giddens (op cit) reasons in the following way:

> "The notion of 'value', as it is represented in Parsons' writings, plays such a key part in the 'action frame of reference' because it is the basic concept linking the need-dispositions of personality (introjected values) and (via normative role-expectations on the level of the social system) cultural consensus....For Parsons the very same values that compose the <u>consensus universel</u>, as 'introjected' by actors, are the motivating elements of personality" (p. 95).

Parsons' 'volutarism' is lost by his solution to the problem of order, because if the individual's values are, for the purposes of the system, the system's values, the individual can only be creative and free in his pursuit of those values. He can exercise his 'individuality' only in his personal interpretation of values and ends which are given and external to him. With Parsons' solution we have not, therefore, removed the opacity which surrounded Durkheim's notion of the collective. Parsons, himself, notes the shift of levels whereby what are from the actor's viewpoint his own values are, from the 'system's' viewpoint its values. Thus,

> "In the personality system, the pattern variables describe essentially the predispositions or expectations as evaluatively defined in terms of what will...be called ego-organization and super-ego organization. In the case of the social system they are the crucial components in the definition of role-expectations" (Parsons and Shils, 1951, p. 79).

Precisely the same shift of levels from the 'individual' to the collective was found in the last chapter in the work of Simon and, hence, the work of Stringer and Beer (as we saw). What the shift amounts to, in the view of Urry (1970) is an illicit and unsuccessful attempt to overcome the Durkheimian error. Urry reasons as follows:

> "It can be maintained that contemporary sociology has made one of two mistakes. The first alternative has been to operate with a notion of the society-actor relationship in which society is postulated as a superior entity, standing apart, beyond and above the individual actor. The actions of the actor have thus been explained in terms of the superior moral fiction without recourse to the motivations of the individually interacting agents...(Here)...the individual experiences his role as pre-given by the environing society and it is one within which he performs, perhaps non-reflectively, in accordance with its prescriptions. The alternative mistake occurs where this transcendental view of society is replaced

by an immanent one; a view of the man-society relationship in which neither is conceptualised as superior--a view where men make society and society make men...The mistake occurs here...(because of)...the reified conception of role or rule which is an accompaniment of it" (p. 351).

The second type of error brings in society as super-actor implicitly because it "...evad(es) the manner in which such...roles come into existence, are maintained, and are transformed by men's purposive acting within their world" (op cit, p. 356. See also: Turner, 1962; Silverman, 1970). Parsons does not solve the problem of order because if we accept "that the very performance of actors within roles serve(s) to constitute and reconstitute the nature of the framework structuring such norms,...(this)...means that actions may go beyond or depart from the existing organization, since man's actions result also from his interpretation of his experience" (Urry, op cit, p. 360). Dawe makes the same point:

> "The argument here is that subjective meanings are, through the postulate of consensus, ultimately derived from the central value system and are thus, at root, external conditions of the actor's situation; essentially, objects of the environment... (G)iven the view of the relationship between the social and the individual inherent in...(the problem of order)..., meaning can only be conceptualised by postulating social norms as being constitutive, <u>rather than being merely regulative</u> of the self. That is, the problem of order can only be solved by conceiving of the actor as a reflex of the social system and meaning as a reflex of the cultural system. Far from disappearing, constraint becomes total through internalization" (1970, p. 209).

So long as the actor is merely a reflex of the 'social' the social goes unexplained, and hence the problem of order is not adequately solved It is because of the initial definition of the task of social science as solving the problem of order between two independent 'realities' (the individual and society) that Parsons (and others who make the same distinction) experiences the fatal difficulties of explanation which he does (I turn to these in the next section). Conceiving of the task of social science in this way inevitably means that the task is to find a metalanguage to transcend them. This is why Parsons sees his work as providing a "general theory" (Swanson, 1953; cf. also, Mills 1959 and Bottomore, 1975 who points out that Parsons has neither a methodology nor a conception of real-world problems in his work). But with a metalanguage all we have is a way of talking about two separate realities in the same language. We do not have the means of understanding the construction of those realities (cf. Black, 1961). Conceiving of the task of social science within this conceptual universe makes it merely a matter of preference how the problem of order is solved: either society or the individual can be determining (for the latter option, see: Rose, 1962). Parsons preference is to see society as the determining reality, and by taking this course he loses the ability to explain both the construction of the individual and society in their mutual determination. This weakness of many sociological schemes has motivated several attempts to develop a truly 'dialectical' theory of the relationship between 'individual' and 'society' which I have no space to assess here (see, for example, Berger and Luckmann, 1966; Garfinkel, 1967; Giddens, op cit; Coulson and Riddell, 1970). In their various ways these attempts all struggle to avoid the pre-supposition of the subject-matter of social science, namely, the nature of the social.

To be consistent with its ideals OR must similarly avoid the presupposition of the rationality of the collective--which in its terminology is represented by both management and itself. Let us now look at the weaknesses of the Parsonian scheme and, in doing so, draw the homomorphic parallels into an isomorphic net. We will find that OR shares the assumption underlying the Parsonian solution to the modern problem of order. It also shares the weaknesses of explanation which bedevill the Parsonian scheme. An illustration will be given at the end of this chapter of the impact on OR's ideals of involvement in this scheme. In general, however, the thrust of the criticisms made against this erstwhile dominant paradigm is that it fails to grasp (and, for the reasons I have mentioned, is prevented from grasping) the nature of its subject-matter. It is biased in the sense that it systematically excludes many important aspects of reality. We can note here that this debate in the social sciences is matched by the philosophical debate which occupied our attention in Part One. There we saw that the philosophical schemes with which OR could be associated were deficient precisely on the grounds that they could not avoid presupposing the nature of reality, and that it could not retreive this situation because of its persistent bias against serious consideration of the nature of the subject-matter which was being inquired into. OR's involvement with the Parsonian scheme runs up against the same criticism because, as we have seen, Parsons presupposes the very nature of the subject-matter of the social sciences, namely, the nature of the social.

<u>Sociological Assumptions in OR: Criticisms:</u>

Setting up the modern problem of order simply in terms of 'some kind' of relationship between the ends of the collectivity and the ends of individuals, as both OR and Parsonian sociology do, implies, if the formulation is taken literally, that the potentialities for the form which the order takes is open-ended. Literally, all that is required for an equilibrium is compatibility, on whatever terms are available, between the different ends. Because this formulation of the problem of order is not grounded in a conscious attempt to grasp the realities of social relationships, it sets up a conceptual tension which has to be resolved. The same open-endedness which we found in OR's ideals appears here as the open-endedness of possible social relationships. The drive towards closure is met by the adoption of a paradigm. This has, in the social sciences, reduced the potentially large number of possible bases on which order could be judged to one--the goals of the collective (although this has been expressed in different ways: e.g. as "compliance" (Etzioni, 1961) or "prime beneficiary" (Blau and Scott, 1963) as the basis for organizational classification). Given Parsons' commitment to the greater reality of the collective, and his definition of the organization's central value system as its goal, it follows that an organization should be defined by its goal(s). Others follow, defining organizations in the same way(cf. Albrow, 1973). Thus, Selznick (op cit) argues that the "...indivisibility of control and consent makes it necessary to view formal organizations as <u>co-operative</u> systems..." (p. 359; cf. also, Barnard, 1965), i.e., organizations must, by definition, have common goals.

Defining organizations as co-operative systems is as commonplace in OR circles as it is in the social sciences. Consider the views of Rivett (1973). He notes, in discussing one of the problems of doing OR that

> "...we are intimately concerned with the formulation and understanding of organizational objectives. You will notice that it is not a question of asking some benevolent company president to tell us the organization's objectives. It is more a question of looking for a set of purposes and trying to understand the collective will and to see the extent to which the collective will is an amalgam of individual wills" (p. 227, my emphasis).

In his account of the problems of doing OR he does not tell us what he would do (or what anyone else should do) if the collective will was not an amalgam of individual wills. The indifference shown towards this question (after all, he raised the question) suggests that the possibility is considered remote. In fact, the wording that he uses implies that it is an impossibility. The last part of the quotation is a non sequiter. To "collect" is to "bring together; to gather; to assemble; to deduce" (Collins New English Dictionary). How, then, could a collective will be anything other than an amalgam of individual wills?

We have already seen Ackoff (1972) define an organization in the same way. So, too, does Stringer. Defining an organization in this way is, however, much more than a formal exercise. Unless substantial qualifications are made it has profound implications for the way one views social reality. Most fundamentally, these implications can be reduced to the extremely naive view that collective goals determine the behaviour of individuals. Thus, Elger (1975) notes that "for Parsons this view implies that the relevant values, signified by the organizational goal, inform all aspects of organizational structure" (p. 93). Boundary interchanges and internal decision-making processes, for example, are thought to be "structured" by these values embodied in the organizational goal (ibid). An implication of the definition is that behaviour which is not determined by the collective goal cannot properly be thought of as organizational (or social) behaviour at all. This implication is the "curious feature" of Durkhiem's <u>Division of Labour</u> of which Rex speaks when he brings to our attention Durkheim's "...assumption that societies based upon the 'anomic division of labour' are not really societies at all" (op cit, p. 101). This assumption followed from his "...assumption that...norms taken as a whole constitute some sort of social consensus which served to integrate all unit acts into a system" (ibid). And, as Gouldner says of Parsons' views: "The catechism is: no society, no humanness" (1970, p. 420).

An identical view is to be found in the work of Ackoff and Emery (1972). They argue that an organization has the "...properties of self-selection of courses of action in repetitions of the same situation or in varying situations with respect to common objectives" (p. 218). An organization, they say, is the realization of the potentiality of purposeful individuals making "...choices with respect not only to their own purposes but also the purposes of others" (p. 219). If this potentiality is not realized then, they say, we are not talking about an organization at all: "So long as this potentiality is not realized, we are dealing with an <u>unorganized social group</u>, one in which the individual elements, although themselves purposeful, cannot make the purposeful choices of the group" (ibid). At face value this statement could be taken to be purely normative: if you find that a group is unorganized the imperative, as we have seen with Stringer (cf. also Churchman and Emery, 1966), is to find it a common purpose. However, they go further than this. They argue that it is "is the intention to coproduce common objectives...(that)...produces the interactions that lead individuals to cohere as a social group" (p. 213). This implies that unless there are common objectives there will be no social group. I shall come back in the final section to discuss their views on conflict. They reinforce the conclusion drawn here.

Parsons (and Durkheim) is enabled to claim that collective goals determine behaviour because of his treatment of individuals as merely the "reflex" of the collective. In doing this, however, he commits (as is widely known) the error of "reification". As Elger puts it (cf. Silverman):

> "...systems theories...reify the organization as an entity with characteristics independent of the social processes through which organizational members construct and construe social reality. The systems theorist assumes that the organizational "function", generally equated with the organizational goal, provides an unambiguous and virtually unchallenged referent for action within the organization, and thus neglects the ways in which actors' purposes and perspectives intervene in the interpretation and contesting of goal-related prescriptions for action...The theoretical underpinning for this reified conception of the organization is provided by the postulate of an overarching consensus which informs all organizational processes" (op cit, p. 94).

Also, Gouldner, referring to Parsons' definition of the organization, observes:

> "...an organization as such cannot be said to be oriented toward a goal, except in a metaphorical sense, unless it is assumed that its parts possess a much lower degree of functional autonomy than can in fact be observed. The statement that an organization is oriented toward certain goals often means no more than that these are the goals of its top administrators, or that they represent its societal function, which is another matter altogether" (1959, p. 420).

The danger is clearly that unless the connection between the individual's purposeful behaviour and the goals of the organization is made by explicitly showing (or at least attempting to) the basis on which it is achieved, the role of the organization's goals becomes metaphysical.

It might be thought that of all people in OR Ackoff, with his unremitting emphasis on the purposeful nature of human behaviour, would be immune from the criticism of reification. This, however, is not so, and it can be clearly shown because Ackoff is very explicit about his approach to the behavioural foundations of OR. This clarity does not, unfortunately, characterise other major writers in the field. However, before we turn to Ackoff it is appropriate to briefly consider the views of some other operations researchers so as to avoid the impression that Ackoff is alone in his mistakes. These others can be accused of making the sister error to reification, which is the tendency to explain behaviour in social systems in terms of the 'function' which it performs for that system.

Functional explanation is a weak form of explanation because instead of searching for the generative mechanisms which generate or 'bring about' the phenomena in question, reliance is placed on the 'end', 'purpose', or 'function' which the phenomena is 'supposed' to serve. The problem with this form of explanation is that no explicit connection is made between the phenomena to be explained and the function which it is said causes it. Parsons can find functional explanation adequate because of his conviction that collective values are determinative of behaviour. Thus, explanations of social behaviour which refer to the 'needs' of the social system as causal are implicitly assuming the operation of a central value system.

Consider the argument employed by Beer to explain how social systems have organised to meet their'need' for "reliable outputs". Beer argues, from cybernetics, that reliability increases with redundancy. Redundancy, he therefore argues, being a 'need' of all surviving systems, arises naturally to meet that need. Arguing from the employment of redundancy by the brain to fulfill the function of producing reliable outputs he argues that

> "...human societies have adopted the same device. Not only is there a tendency to form boards, committees, conferences and meetings of a formal kind; there is also the tendency to undertake a great deal of unofficial consultatation within any managerial group..." (1966, p. 198).

Beer places much reliance on functional explanation (see also p. 434). From the argument that all systems (including social systems) function 'naturally' to meet their needs, he moves to make his general case for the application of cybernetics to organizations on the grounds that they are like biological organisms. The organismic analogy which

Beer, like Parsons (who also relates his theories to cybernetics), uses extensively, is the paradigm case of functional explanation.

Another example of the use of the organismic analogy in OR is Hertz's attempt to describe what he sees as the basis of "Elegant OR" (1961). Hertz is concerned with the "...innate characteristics" of OR. By this he means "...the methodology used in achieving solutions to operations research problems, and especially methods which seem particularly appropriate to the solution of a significant problem" (p. 34). To help decide when methods are appropriate he adopts a broadly conventionalist perspective. For him elegant work uses an "...intelligible, systematic, conceptual pattern for the observed data which will be capable of uniting phenomena which, without it, are either surprising, anomalous, or wholly unnoticed" (p. 42). "What matters", he says, "is that certain systems of concepts can help us to observe intelligently what there is" (p. 35). What do these systems of concepts refer to? Concluding from an "examin(ation) ...(of)...the published work in the field...that operations research, as practiced, is a science which aims simultaneously at: (a) discovering certain kinds of societal relations, and (b) developing means of deciding on and testing courses of action in a social environment", he suggests "...that the researcher tackle the total problem... recognising that the decision process stems from and returns in its action context to the social environment" (ibid). However, OR must attempt (to be elegant) "...to formulate programs for action with respect to very specific kinds of social phenomena..." (p. 36). Hertz perceives the social base-point of OR to be a "social organism" (p. 37) which thereby displays "...uniformities or laws in nature-- in nature as represented by society...(which has those)...underlying structural characteristics" (p. 39) which guarantee "...the underlying unity of the society and its decisions..." (p. 41). By this he presumably has in mind at least something equivalent to a central value system

Friend and Jessop do not use the organismic analogy, but their account of planning processes nonetheless relies heavily on functional explanation. A blatant example of this is their suggestion, after summarizing what they see as the "five basic operational problems of strategic choice", that

> "each of these problems is...implicit in any process of public planning, and it is inevitable that any governmental body will tend to develop certain methods for dealing with them, even if they are never made explicit" (op cit, p. 119).

This suggestion becomes a principle of analysis:

> "certain organizational changes in Coventry during the period of...(their)...research are interpreted as a response to a growing awareness of the difficulties of coping adequately with the more strategic aspects of the decision-making process" (p. xxiii).

The only warrant for this interpretation is the assumption that however blind, faltering and unconscious these responses were, they were necessary to meet the needs of the system: therefore they occurred. They say that given the complexity and uncertainty of the environment, "in these circumstances planning <u>must</u> become in some degree an <u>adaptive</u> process" (p. 112, former emphasis mine). It 'must' only if it is assumed that collective goals implicitly determine it to be so. To say that planning processes should become an adaptive process is surely an insufficient basis for interpreting responses as an attempt to institute it as such. As with Beer there is a continual confusion of normative with descriptive statements. This confusion is the result of refusing to take seriously the nature of the subject-matter with which they were dealing. Then normative statements can be taken to adequately substitute for factual ones.

The clear implication of functional explanation and the reified conception of the collective with which it is associated, is that an OR which employs them must give up its ideal of freely investigating complex totalities. Within the Parsonian scheme we must give up any hope that operations researchers can do anything but sub-optimize with respect to the social world. Within it operations researchers cannot explain mechanisms (of decision-making and planning etc.) which are of obvious importance to them. The adoption by Friend and Jessop of a framework other than that organizational changes (and also decision-making conventions, see: p. 47) were responses to the 'needs' of the collective, could well have led them to make different recommendations. The responses might, for example, have been interpreted as the outcomes of the strategies of motivated actors to further their own purposes (e.g. political, status or security motives) as these were mediated by both their knowledge and resources, and the motives, knowledge and resources of others (cf. Burns and Stalker, 1961; Silverman, op cit; Elger, op cit). If this were the case, how are we to interpret proposals for organizational change? These comments apply with equal force to the subsequent work of the Institute for Operational Research (particularly, Friend, Power, and Yewlett (1974)).

The institution of organizational forms in this situation may or may not further the interests of certain groups, and may or may not be thwarted in their intention. From their accounts of their work both Stringer (as regards the Building Industry Communication Project - see: Higgin and Jessop, 1965) and Ackoff (as regards a project with General Electric - see: a brief report in Ackoff, 1970, pp. 129 ff.) it would seem that insufficient account of the motives, strategies and knowledge of the actors led to unwelcome results.(1) After discussing the widely diverging interests that characterised the building industry Stringer (1973) merely attributes the failure to resolve them as a "... difficulty... in the communication process" (p. 256). Ackoff, noting that General Electric "... has not been as successful in keeping the... (planning)... efforts, once launched, afloat" (p. 129) concludes that the solution is an organizational one. What are required, he suggests, are smaller, rotating groups. The problem is that we have no way of assessing the efficacy of either of these solutions. My own experience is that formal changes have no raison d'etre outside of an understanding of the purposes, knowledge and resources of the groups involved. With this understanding organisational changes may make sense; without it such changes are, at best, based on intuitive guesswork and, at worst, are positively arbitrary.

I move on now to justify the claim made earlier that Ackoff effectively reifies social systems, and that because of this his concepts can be said to reveal, and in fact rely on, commitments about the nature of the social world identical to those held by Parsons. Ackoff's work is permeated by the fundamental ambiguity inherent in the individual-collective

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(1) See also Young (1973) for an account of the difficulties of manpower planning which ignores sociological analysis of "attitudes" (as he calls them) of the parties involved. Young would get little of the help which he calls for from the sociology I have been discussing here.

dichotomy. The problem in Ackoff's work is the basis on which we reconcile what appears, on the face of it, to be a fundamental incompatibility between the purposeful nature of the individual and the obviously reified conception of the collective.

The collective is reified inthe sense that it is "... attribut(ed) ... (a)... concrete reality, particularly the power of thought and action..." (Silverman, op, cit., p. 9). In fact it is a "social construct" in the sense that it is constructed from the social acts of individuals. Ackoff and Emery are concerned with "social individuals". These are "any collection of psychological individuals that is itself treated as an individual" (op. cit., p. 212). They argue that "... we can observe the properties of a social group without observing the properties of its members... (and therefore)... it follows that we can also observe its behaviour..." (p. 211). They see no difficulty in treating social systems as if they were individuals, and it is on this basis that they speak of an organization 'having' a goal; of co-producing "... the outcomes the system wants" (p. 220); of organizations "surveying" their members (p. 221); of "group personality" (p. 212). It is hard to think of a more reified conception of the collective. And yet, this notwithstanding, Ackoff and Emery are harsh in their criticism of schemes which are not grounded in the fundamental commitment to treat individuals as purposeful. How is this apparent contradiction to be resolved? The resolution is to be found, I think, in the treatment of the individual in the first place. Close examination will show that he is not in fact treated as a purposeful <u>individual</u> in any strong sense at all. He turns out to be purposeful only from the point of view of the collective.

The subtlety of their scheme calls for an equally subtle analysis. If we take their scheme as they intend us to we have, in the first two parts of it the development of the basic methodology and, in the last two parts, an application of the concepts developed earlier. (This is slightly less true of Part 3 (Interactions of Purposeful Systems) than it is of Part 4 (Social Systems and Beyond)). The earlier concepts are seen by the authors to be applied to social systems, rather than these concepts themselves depending on the concept of social systems. Thus, they comment that "a re-examination of the conceptual sytem developed in this book will reveal that it has no properties that restrict its application to persons (to psychological individuals)... (I)t is equally applicable to groups (to social individuals)" (p. 212). This implication is , I will show, incorrect. Ackoff and Emery's scheme depends crucially (and, I will argue, fatally) upon the reified conception of social system which they entertain in the latter part of their work (and elsewhere). I shall show this by pointing out a logical incompleteness in their methodology for measuring purposefulness, and then show how it is filled.

The aim of Ackoff and Emery's work is to "... open to public examination the study of the mind's inner workings" (p. 7) and to do this in such a way that all reference to subjective 'inner' states is avoided. They attempt to do this by the development of what they term an "objective teleology" wherein "... beliefs, attitudes, and traits are attributed to an individual because of what he does" (p. 6). Such subjective concepts, they say, "... do not lie behind behaviour; they lie in behaviour" (ibid).(1) The strength of their claims for this approach to understanding human behaviour amounts to the assertion that the foundation of all 'objective' knowledge is what is directly available to our observation. There are some qualifications made by them on the questions of how we acquire and use this knowledge, but these do not affect their basic point. "Hence", they go on, "in an objective teleology functional characteristics of human

(1) This argument was advanced long ago by the well-known positivists Lundberg (see: Odum 1951, p. 208) and Tolman (see: Taylor, 1964, p. 79) - cf. the discussion of positivism in Chapter 2 of this work.

behaviour are not treated as intervening variables subjectively fabricated to conceal our ignorance: they are derived objectively from what we can observe" (ibid). Thus they would dismiss as "subjectively fabricated" explanations of the sort advocated by, for example, Louch (1966) (and many other similar schemes advocated by social scientists and philosophers, for example, Harre and Secord, 1972). Louch's scheme is summed up by him as follows:

> "when we offer explanations of human behaviour, we are seeing that behaviour as justified by the circumstances in which it occurs. Explanation of human action is moral explanation. In appealing to reasons for acting, motives, purposes, intentions, desires, and their cognates, which occur in both ordinary and technical discussions of human doings, we exhibit an action in the light of circumstances that are taken to warrant a person to act as he does" (p. 4).

Louch's argument (which I do not have the space to reproduce in full here) is that (a) perfectly adequate explanations of human behaviour can be provided by ad hoc 'moral' accounts of human action; that is, there is no universal law of adequate explanation which restricts them to the general case of 'regular' behaviour (a view which is based both on historic myth and an inadequate (Humean) notion of causality) - we can have perfectly adequate explanations of 'special cases', and (b) as far as human action is concerned, precisely because it is 'purposeful', we avoid many gross errors if we employ such ad hoc accounts.

Ackoff and Emery define 'purpose' behaviourally: thus, a system can be said to be purposeful and have a purpose if it behaves regularly in a certain way. There is, however, an immediate difficulty with this as a form of explanation (and note that they argue that "to predict behaviour is not enough; we must explain it" (p. 11)). Ackoff and Emery wish to turn subjective concepts into objective ones by, as Louch puts it, "... 'unpacking' them (as Ryle would say) into descriptions of tendencies or patterns of behaviour..." (p. 61). But, Louch points out, in doing this the concepts lose any explanatory significance whatsoever. For example, "that a man persists in a line of conduct may be explained by citing his strong desire, but not if desire means only the tendency or pattern of behaviour in which he persists" (ibid).(1) Clearly, then, there has to be some warrant, derived <u>externally</u> to the behaviour itself, for designating as displaying a purpose or, more generally, as purposeful. Calling behaviour purposeful has to be nothing more than a 'labelling' exercise (cf. Taylor, 1950) unless we can find some good reason, external to the behaviour itself, for so doing. Louch argues that moral justification - the 'grounds' for acting in such and such a way - provides an adequate external context. Ackoff and Emery argue that "... <u>idealized operational</u> definitions and measures..." provide the only acceptable external context (p. 7). These definitions and measures "... provide <u>standards</u> in the same sense that ateleologically oriented sciences provide standards for structural concepts (for example, length, density, and energy in physics)" (p. 7; cf. Churchman, 1961a).(2) The question

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(1) Ackoff and Emery do not define desire but they do talk of "beliefs in utilities". They argue that to avoid the tautology of taking what an individual says he wants for what he actually wants (p. 97) "... we must find a type of behaviour he displays almost invariably when he is aware of their utlity and that he almost never displays otherwise" (p. 98). This avoids one tautology but commits another.

(2) Again, Lundberg and Tolman anticipate Ackoff and Emery (op. cit.).

thus arises whether these standards can be specified in such a way that they do not rely on background conceptual commitments or, if they cannot, what these background commitments are and what effect they have on the original intention of treating humans as purposeful. Louch argues that discovering the grounds of action requires investigation into the uses of language in a particular society as informed by historical study. It would take me too far afield to fully explain the justification for this, and I mention it here only to avoid the impression given by Ackoff and Emery that no reasonable alternatives to their approach exists. Louch, unlike Ackoff and Emery, is not concerned with developing a methodology which conforms to a preconceived view of objectivity, but wants to avoid confounding the investigation of the social by its presupposition.

The standards which Ackoff and Emery set up to explain human action consits, firstly, of the "parameters of a purposive state" which define the relationships which can possibly exist between the components of a purposive state and, secondly, the purposeful state itself. This defines the subject in his environement and its properties (available courses of action and outcomes possible). The parameters (the probabilities that the subject will choose particular courses of action and how efficient the action is for producing an outcome which the individual values) relate the individual and his environment. The objectives of the exercise are (a) to define state and its parameters in a measurable way and (b) because they are all seen as co-producers of the behaviour, define them such that they are independently manipulable. Ackoff and Emery look upon the producer-product relationship as the "... most critical concept in... (their)...book" (p. 22), and the employ it to define, behaviourally, the "personality" of an individual. "What the individual contributes to a choice situation, then, is a transformation of situational properties into probabilities of choice, efficiencies, and relative values" (p. 40).

They argue, therefore, that "... personality is... an observable function that describes how an individual or system converts a choice situation into an expected relative value for himself or itself" (p. 41). Personality is measured by independently measuring (by treating it as a dependent variable) each of the three parameters of a purposive state (courses of action, efficiency, and relative values). Thus, for example, to measure someone's intention (measure of end preference) we hold 'constant' any preferences the subject may have for certain means and also his knowledge of the efficiences of different means. We can then, they say, measure an individual's preference for certain outcomes by allowing these to vary whilst holding the other parameters constant and allowing him to choose. Over a large number of trials we arrive at a relative frequency score which is taken to be a measure of preference. Thus,

> "in measuring familiarity, knowledge and intention are held constant. In measuring knowledge, familiarity and intention are held constant. In measuring intention, familiarity and knowledge are held constant" (p. 42).

Two questions arise concerning this scheme. Firstly, why is it appropriate to attempt to analyse 'purposeful' behaviour in terms of ends, means and knowledge? Secondly, is it possible to measure any of the properties of a purposeful state (given that it is reasonable to measure these properties in the first place) 'scientifically', that is, by restricting ourselves to the recording and classifying of observable behaviours?

To address the first question: we have an image of the subject who 'transforms' situations by 'generating inputs' (Chapter 4) of 'informational flows' which 'get through' to the subject; these inputs are used by the subject to 'model the situation' as 'simplifications of reality' (Chapter 5): the subject uses his modelto both 'formulate' and 'evaluate' choices (Chapters 6 & 7). Thus,

> "Once a model is accepted, a choice of a course of action can be made... Intuition suggests possible courses of action that can be evaluated by use of the choice model and the process of thought. The model itself is the product of past and present observations or, more generally, perceptions. The consequences predicted are evaluated by feeling. A course of action that is predicted to yield satisfaction is selected" (p. 133).

The subject is treated, as Simon (1957) would put it, as 'intendedly rational' if not fully rational in practice. The model of the subject employed to gain a foothold on understanding his behaviour, by being set up as fully integrated model of rationality (whereby the subject becomes comprehensible as a 'totality' by being a 'system' integrated by rules of rationality), runs the risk of denying the subject the very purposefulness which the scheme is intended to elicit.

This point is brought out forcibly by Garfinkel (1974) when he deliberately exaggerates a distinction between 'common-sense' and 'scientific' rationalities. Scientific rationalities are idealized prescriptions for behaviour. Behaviour viewed within this scheme is deemed rational because of the logic of the scheme itself. The problem for the behavioural scientist employing this scheme is to identify behaviours and compare them with the rationality of the scheme. The rationality of 'everyday life', however, is presumed to have its own logic. What is data for the scheme which employs scientific rationality is taken as a problem in the scheme which looks for the rationality of everyday life, that is, the rationalities which people actually employ. The problem is the use of various rationalities (including the scientific rationalities) in the conduct of purposeful behaviour, and not the assignment of pieces of action as purposeful or non-purposeful by reference to a preconceived scheme which is used as the reference point for the meaningfulness of the action. Thus, Garfinkel's conclusion on the use of the scientific rationalities model of investigation (see: pp. 60-1) is that:

> "it has been the purpose of this paper to recommend the hypothesis that the scientific rationalities can be employed only as ineffective ideals in the actions governed by the presuppositions of everyday life. The scientific rationalities are neither stable features nor sanctionable ideals of daily routines, and any attempt to stabilize these properties or to enforce conformity to them in the conduct of everyday affairs will magnify the senseless character of a person's behavioural environment and multiply the anomic features of the system of interaction" (p. 71).

Thus, for Garfinkel, Ackoff and Emery take for granted the very thing of concern to the sociologist, namely, the rational achievement of rationality, by their reification of the individual in referring the meaning of his action to a preconceived logic of action. No-one would dispute that individuals 'think','feel' and 'evaluate' etc. The question is on what bases are these properties dependent? Garfinkel puts the problem facing the sociologist in this way:

> "... sociological inquiry accepts almost as a truism that the ability of a person to act 'rationally'... depends upon the fact that the person must be able literally to take for granted, to take under trust, a vast array of features of the social order...
>
> "The sociologist refers to these trusted, taken for granted, background features of a person's situation, that is, the routine aspects of the situation that permit 'rational action', as mores and folkways" (p. 72).

The sociologist is concerned to investigate these mores and folkways. In Ackoff and Emery's scheme these prerequisites to rationally purposeful behaviour are ignored. The question thus arises as to whether this defect can be remedied within their scheme (the impression one gets is that they believe that, for example, norms could be derived purely statistically as average behaviour in the classical Durkheimian sense: for a criticism of this approach from a somewhat similar point of view to that of Garfinkel's see: Filmer et al, 1972, Chapter 3).

Consider the problem of actually using their scheme. Suppose we wish, for example, to measure a subject's "beliefs in efficiency" (p. 90). We are told that:

> "As long as there is the possibility that the individual in this environment is pursuing many different ends, we cannot use his behaviour directly as evidence

> of what efficiency he believes a course of action to have with respect to any one outcome, for we do not know with respect to which outcome his behaviour can be taken as an indicator of such belief.
>
> "(Thus)... to determine an individual's belief in the efficiency of a course of action for any outcome, it is necessary for us to isolate the outcome so that his choices cannot be taken to be serving any other objectives" (p. 91).

How would one know, in the first place, whether there was any "possibility" that the individual had different ends? According to their scheme the answer is simple: control for beliefs in efficiency and knowledge and measure the ends that the individual is actually pursuing in this environment. But, it was beliefs in efficiencies that we were concerned with in the first place! To hold them constant we have to know them, but to know them we also have to hold them constant so that we can be sure of the ends to which they are relevant! In Ackoff and Emery's scheme of measurement gaining knowledge of anything presupposes that the very knowledge in question is already at hand. In measuring preferences for ends we have to know that preferences for means are equal. How do we know this? The only way we could know this, according to their scheme, is by measuring means preferences when <u>ends</u> are equally preferred. And we can only know whether ends are equally preferred by measuring them with means held constant. There seems to me no acceptable way within the confines of their scheme of breaking this (vicious) circularity. It might be suggested that we could, by making an initial common-sense evaluation of the relation of ends and means to situations by reference to what would be 'reasonable' to expect in this situation, 'converge' on a correct measurement. Could we check out the reasonableness of our assumptions by attempting to 'falsify' them by observed behaviour (as suggested by Emshoff, 1975). Clearly we could not. We could never be sure that the behaviour was more than 'coincidentally' congruent with our assumptions and, what is more to the point, even if they (eventually) always coincided on what basis would we be able to claim that we had 'explained' the behaviour, as Ackoff and Emery argues is necessary (p. 11)? To be sure, ways of relating the assumptions and the predictions could possibly be found, but what confidence could we have that even as a totality these referred to the purposeful behaviour of individuals? Set against their claims to have provided the basis of an objective science which does not make subjective judgements to conceal its ignorance, the subjectivity of the researcher plays the key role in a completely unexplored way. Nor is this likely to be explored, and for two reasons. Firstly, Ackoff and Emery seem overcommitted to the power of the producer-product scheme of causal relationships as a way of resolving the problem of explanation. Commenting on this approach, Keat and Urry (op. cit.) "... find problematic the manner in which different types of causal conditions are somehow combined or conjoined to constitute the complete cause" (p. 32). As we have seen, the producer-product scheme is essentially open-ended. No guidance is given as to the relevant producers or on how they are related to each other to produce the product. Thus, when, for example, the researcher makes assumptions which are not borne out by experience, where is he to turn to, as Emshoff (op. cit., p. 691) puts it, make his 'theory' more 'general'?

The second reason for believing that the subjectivity of the researcher will not be explored is that, in the view of Emshoff at least, OR cannot (for some unexplained reason) "... afford to rely on other disciplines that have different methodological bases of research to provide it with the needed techniques for modelling behavioural systems" (p. 676). Combining this with the open-endedness of the producer-produce scheme, we can expect needless, wasteful and often erroneous speculation on the reasonableness of assumptions about 'behavioural systems'. Let us return now to the original purpose of this excursion into Ackoff and Emery's scheme for measuring purposeful behaviour, having now demonstrated that it is logically incomplete.

Given the necessity in Ackoff and Emery's scheme for making prior assumptions about the nature of purposefulness, we can appreciate the temptation which they must feel for making general assumptions about the nature of the social world and the relationship of individuals to it. It would be very convenient indeed if it were reasonable to assume that (to repeat) "the intention to coproduce common objectives is what produces the interactions that lead individuals to cohere as a social group" (p. 213). Hence, by theoretical inclination, and this temptation opened up by their approach to the measurement of purposeful behaviour, Ackoff and Emery are led to reify the organization as a social individual and yet, at the same time, maintain that its parts are purposeful. For Ackoff and Emery's scheme to be workable it is only too natural for them to be driven to see the purposefulness of individuals only as an expression of the collective will. It is only by this reification of the individual and organization that it is possible to resolve the logical weakness of their scheme of measurement and at the same time assert, as they do, that "an organization is a social system in which the state of a part can be determined only by reference to the state of the system" (p. 218). That this reification is consistent with their definition of purposefulness says more for their powers of reasoning than it does for the usefulness of their scheme.

There are, then, some good reasons for Ackoff and Emery making broad assumptions about the typical orientations which actors take towards action, just as Parsons argues that we must make assumptions about the "pattern variables" which characterise any social system. The reason that they choose to assume a general consensus over collective ends follows from their desire to deal, in terms of their own version of what it is to be scientific, with social systems. To find purpose in the behaviour of a social system requires that the purpose be intially assumed, as we have seen at the level of individuals. Following this through, it would not be possible to measure the purposeful state of a social system if that system (through its members) were pursuing divergent ends. It it were pursuing divergent ends then the circularity involved in their scheme would prevent measurement of its purposeful state. For Ackoff and Emery's scheme to make sense at the level of social systems the social system has to be pursuing collectively shared ends. We have to assume this as a starting point. The preoccupation which they show with consensus (extending to consensus at the level of ideals, see: p. 246) while at the same time asserting that the essence of purposefulness is the ability to choose ends can, then, be partly explained in terms of its resolution of a gap in their methodology(1).

The consensus assumption, however, also serves another function. It allows us to simultaneously assert the purposefulness of individuals and, at the same time, claim to be able to find the purposeful of the collective by observing the behaviour of the individual. The desire to do this can be found in the early work of Churchman et al (op cit) when

(1) Further evidence of the consensus assumption in Ackoff's work is provided by his opinion that the use of scenarios "...enable an organization's management to reach consensus on the kind of business it would like to be in and the way (style) in which it would like to conduct it" (1970, p. 41).

they advocate the study of an organization as a communication network as the first stage of a study towards "...understand(ing) the organization _and the resulting system_..." (p. 110, my emphasis). Viewing the organization as a communication network which can be studied as such to find out the expression of the collective will in it (which, of course, is then to be improved upon by OR) only makes sense if it is assumed that the purposefulness of individuals is oriented towards collective goals. Similar comments apply to Ackoff's (1970) advocation of the construction of a decision network. Even with the elimination of unnecessary flows, and the institution of needed control, there is no _a priori_ reason, outside of a consensus assumption, why this make sense. Why, for example, it "...makes it possible to identify the decisions required to run the business..." (p. 91). Assuming that individuals are purposeful and are purposefully pursuing their own goals might lead us to the conclusion that the decisions being made were necessary for the pursuit of the objectives of the individual decision makers (as these were mediated by many different factors which are not necessarily adequately catered for by the notion of organizational goals or theories of motivation and personality--cf. Silverman, op cit). It is noteworthy that the najor impetus for viewing an organization as a communication system was Simon (particularly 1957) who explicitly assumes (following Barnard, 1964) that the equivalent of consensus is a necessary feature of organizational life. Simon prefers to achieve the same result as the consensus assumption via his "inducements-contributions" balance theory of organizational equilibrium. That Simon's model rests on the effective non-purposeful character of individuals is shown very clearly by the work of Krupp (1961, especially chapter 8).

An Illustration of the Impact of OR's Social Theory on its Ideals: Conflict Power, and the Problem of Interests:

The consensus assumption seems as essential for the theory of OR as it is for Parsons' theory of social systems. Without it the perceived necessity of formulating OR's task within the modern problem of order would be the source of much anxiety. This anxiety is subdued by the assumption. But what is the cost to OR in attempting to formulate its task within this theoretical framework? We can conclude by pointing out one of the severe limitations which the consensus assumption (and related theoretical framework) has on our ability to come to grips with social reality and the impact which this has on the feasibility of OR achieving its ideals.

The specific limitation which I shall discuss is the inability of the theoretical framework associated with the consensus assumption to allow us to adequately deal with a phenomenon which OR has a declared interest in, namely, conflict. OR explicitly recognises the existence of conflict as a necessary part of the problem of order. This interest should have been heightened by the modern reformulation of the modern problem of order. It has not. Although, I have argued, OR relies on a model of the social world to give a coherent account of itself, nowhere in OR has there been an attempt to develop an understanding of the _social_ nature of conflict (although some interest has been shown in the 'logic' of conflict, e.g., Rappoport, 1961). It is predictable that such interest as is shown in this phenomenon is tailored to fit within the consensus paradigm; precisely the same way that we have seen the notion of purposefulness truncated by being forced to fit within a pre-conceived model of the social world which is dominated by the collective.

The biases introduced by the consensus assumption have been high-lighted in what is probably the most worked out part of the overall critique of Parsonian-type sociology. The general objective of this part of the critique has not been to show a 'conflict model' is generally preferable (although some critics have argued in this way, see: Dahrendorf (1959). Rather, the critics have argued that the major defect of the consensus model is that its selective attention to the phenomenon of normative regulation has rendered it incapable of providing a general theoretical orientation which can deal with the reality of social systems (Lockwood, 1970; Rex, op. cit., Giddens, 1970), and not that the phenomenon with which it does deal is unimportant.

The argument has been advanced by these critics that conflict (and the inextricably related phenomenon of power) is a pervasive feature of social life and that the problem facing sociology is the development of theory adequate to grasp both the nature and existence of conflict, and its relationships to the nature and existence of normative regulation. As Giddens puts it, "... the real problems which have to be tackled, and which lie at the root of much of the debate, concern <u>how legitimation is mediated in its operation in systems of power</u>" (1970, p. 462). The real worry has been not just that the consensus model ignores the reality of conflict but that in doing so it ignores social reality. Focussing on consensus is not merely <u>partial</u>, the argument runs, it is <u>wrong</u>. In fact, of course, the consensus model does not totally ignore conflict; the problem of order is premissed on potential conflict. However, as we have seen, the problem of order is set up as an opposition between the collective and the individual. With this initial commitment, with the collective and the individual already assigned their realities in the conceptual universe, conflict becomes visible only as an "operation" of that reality and cannot be seen as constitutive of it. That this weakness is intrinsic to Durkheim's view of the problem of order has been pointed out by Coser (1960):

> "Durkheim was forced to assume that the major social norms generally express the sentiments of the total society. He never seriously entertained the idea that they might only express the sentiments of a specific stratum within it. If it is affirmed a priori that the major social norms express the sentiments of the total collectivity, then one cannot recognize conflicting norms within a society; one cannot understand... that certain subordinate social strata may accept a norm only because it is imposed upon them by violence or because they passively submit to it, whereas it is the genuine expression of the moral sentiment of only a superordinate stratum" (p. 218).

A similar point is made by Giddens (1970) with respect to the consensus model of Parsons:

> "'Conflict of interest', in this conception, never becomes anything more than a clash between the purposes of individual actors and the 'interests' of the collectivity. In such a perspective, power cannot be treated as a problematic component of divergent group interests embodied in social action, since meshing of interests is treated first and fore-

> most as a problem of the relation between 'the individual' and 'society'" (p. 97).

I shall only deal in detail with Ackoff and Emery's development of the concept of conflict as it is seen to affect OR. Before doing this, however, let us briefly look at the 'problem'of conflict as is to be found in a section of Lawrence (1966), aptly titled "Conflict Resolution and Control" (Part IV). The title is apt because it suggest the overall orientation towards conflict: the 'problems' are to resolve it and/or control it. That this orientation is taken towards conflict is not surprising considering the underlying tendency, expressed by Trist in his introduction, to view conflict as stemming from "our inherent destructive tendencies... "which emerge only under the agency of the exceptional individuals who are not adequately socialized, "... resulting in the extremes of mental illness or anti-social behaviour" (p. 367). When conflict is seen as "regressive behaviour" (p. 368) the only reasonable implication is that consensus is the 'norm' (or that 'society' is collapsing). No attempt was made to assess the causes of conflict in terms of the conflicts of interest of groups of motivated individuals who attempt to skillfully manage their social and economic existence within the context of differential access to resources of power (cf. Burns and Stalker, 1961 and the interpretation by Silverman, op. cit., Chapter 7, of Gouldner's analysis of a 'Wildcat Strike' (1965)). Instead, we have, for example, Walton who stresses the conflict arising from the tensions between organizational roles in lateral interaction (p. 409 ff.); similarly, Ackoff (p. 427 ff.) concentrates on 'structural conflicts within organizations', those conflicts which 'arise' because of flaws in the incentives 'inherent' in the organizational structure. These emphases are indicative of a view of conflict which sees it as a flaw in a given social reality, rather than it being part of that social reality (similar comments are made by Mayntz, op. cit., p. 115, on Beer (1964) and other advocates of viewing organizations as self-regulating systems). It is not merely that such writers as Walton and Ackoff overemphasise specialization as the basis of conflict. Ritti and Goldner (1969) have pointed out that "... specialization is only one dimension of conflict and competition" (p. B-245). The complaint is rather OR has not perceived that the "... important research problem... is the understanding of how organizational actions are affected by the complex identities and expectations of organizational members at all levels... (and)... more fundamentally, the problem... (of)... describ(ing) how members do perceive their identities in the fluid, cross-cutting environment of the modern technology-based organization" (pp. B- 245-6). There is a growing desire in OR circles to model decision-making processes in organizations (as evidenced not only by Ackoff, 1970, but also Stringer op. cit., and Eden, 1976). Ritti and Goldner should be pointing out the obvious when they say that "the explanation of... conflicting interests among organizational factions is a major concern in the systematic study of organizations... for the understanding it brings to the processes by which decisions are reached and the resources of the organization allocated among competing alternatives" (p. B-233). As far as OR is concerned, they are not! The emphasis in OR, because the collective is taken as given, is on the consequences and not the causes of conflict (cf. Silverman, op. cit., p. 58) except insofar as the causes are indicative of the consequence that the collective is not functioning properly! This general orientation towards conflict is to be found in the treatment of it by Ackoff and Emery.

That they see conflict as a somewhat 'unreal' phenomenon is indicated by the following:

> "To say the members of a social group share a common objective is not to say that they do not conflict... with respect to other objectives or even with respect to the courses of action by which the common objective should be pursued. Similarly, we cannot ignore the factors that may lead to relative indifferences or independence... with respect to achieving the common objective. Such conflicts hinder and disrupt cooperation and communication, and undermine the cohesiveness of groups. Consequently the organization of groups must be adapted to manage conflict and assure adequate communication and cooperation with respect to the group's common objective" (p. 214).

Firstly, we can note that 'by definition' there cannot be conflict within a social group over what the common objective should be. Thus, an organization in which such conflict was present (a common enough occurence, see the case studies presented by Perrow 1970, Chapter 5; see also, Perrow, 1961) would presumably not exist! One thing is certain, such an organization has no theoretical existence in their scheme. Secondly, we can note that conflict is by definition a 'bad thing';[1] it must be removed if we are to have a social group at all. One would have thought that the adoption of this normative stance would have encouraged efforts to develop theoretical schemes which would lay bare the causes of conflict so that it could ade-

(1) Precisely the reverse of the way Churchman et. al. claim to view it, as we have seen.

quately be removed. This, however, is not forthcoming. In their chapter on 'Conflict, Cooperation, and Competition' (Chapter 12) there is an almost exclusive preoccupation with the consequences of conflict: we are given a definition of conflict in terms of its affects on the values of the conflicting parties (p. 197) and the rest of the chapter is devoted to "ways of affecting conflict".

Given that conflict is defined by its consequences it follows that to 'affect' it we modify those consequences. As the consequences result (obviously) from behaviour we affect the consequences by affecting the behaviour of the conflicting parties directly (either "resolution" or "solution" depending on the party concerned) or by changing the 'environment' ("dissolution"). Even if we could effectively modify the behaviour of the participants by the use of Ackoff and Emery's scheme of purposeful behaviour, that scheme would be of no use for explaining the conflictual behaviour in the first place. That is, even if Ackoff and Emery's scheme allowed us to modify the purposeful states of the individuals (or groups) concerned, we could not use that scheme to tell us why they had those purposeful states in the first place. The nearest that they get to anything approaching an explanation is cast in terms of the consensus model. They argue that "people tend to cooperate more when they can communicate with each other than when they cannot" (p. 206), the principle being, presumably, that through communication people can find their common interest. As they go on to say: "Social groups are normally held together by cooperative interactions among their members" (ibid), because in a 'social group'"... such contact as there is tends to further the common objective" (p. 214). It should be obvious (cf. Schelling, 1963) that conflict is sometimes reduced by the curtailment of communication, and also that it may increasy with an increase in communication. It is only by swallowing the consensus model hook, line, and sinker that one could so systematically overlook these possibilities.(1)

Clear evidence of the insular circularity of Ackoff's approach to conflict, whereby it is both defined in terms of and is also resolved by the 'system' is to be found in an exchange between him (1974, 1975) and Chesterton et al (1975) on the question of the 'Social Responsibility of Operational Research". We have seen Ackoff argue that the role of the manager and the management scientist is to resolve the conflicts between the three levels of system (the individual, the organization, and the encompassing system (society)). He tells us, more specifically, that the task is "... to learn how to remove the apparent conflict between these lvels of purpose - subsystem, system, and the suprasystem - and to find strategies which serve each of them efficiently" (1974, p. 362, my emphasis). Whereas Ackoff talks of 'apparent' conflict, Ackoff's critics (Chesterton et al) talk of a "... basic conflict between managers (and their scientists) and the managed", this difference being based, as they see it, on "... a difference of understanding about the nature of the dominant structural relationships" (p 91). Although they do not say what they see as the dominant structural relationships, it is clear that conflict for them is 'basic' if it is produced by structural relationshops (which would therefore take structural changes to remove). Ackoff's comment that "they do not define basic conflict" (1975, p. 96) is therefore unwarranted. He, however, interprets their view as a concern for the fact that "... the conflict in question is unresolvable in principle" (p. 96), and using such a vague term as 'principle' it is open to him to say what sort of principle should be applied. For him the principle should be applied. For him the principle to be applied is that "... the only conflicts that are not resolvable in principle are ones involving logically

(1) Ackoff and Emery's comments on the 'ineviable' effects of communication are strongly reminiscent of the much criticized theories of the functionalist Homans (1950). For a trenchant criticism of the naivete of this position see Louch (op. cit. pp. 14-19).

incompatible ends" (ibid - whereas Rosenhead in a further reply reemphasises that his concern is the "the social incompatibility of ends" (1976). We could, perhaps, interpret Ackoff's critics as asking 'what is the reality of conflict, and when is it really resolved?' To this Ackoff in effect replies, 'a conflict is really resolved if it is resolvable in principle and that principle is applied to effect a resolution'. We could, then, understand the reality of conflict and its resolution if we could understand what was meant by Ackoff's use of the term 'logic'. He gives us his answer: the reality of conflicts and their resolution is whether a "... conflict that appears to be unresolvable at one level of means or ends is subject to resolution at a higher level of desirability" (ibid). This is the logic of which Ackoff talks, but, in fact, it is a completely circular logic, because it should not have gone unnoticed that the logic by which the conflict resolution is judged is the systems logic! Saying that conflict can be resolved at a higher level of desirability is merely a reiteration of the systems view from which Ackoff started which says that subsystems are 'parts' of larger systems, and by definition for any two parts there is a system of which both are subsystems. If this is so (and it is an axiom of the systems view) the higher system always provides the basis for the resolution of subsystem conflict. In Ackoff's view, therefore, for conflict to be irreconcilable would be a denial of the systems approach (for an identical view to that of Ackoff see: Churchman and Emery, 1966, p. 81) which we have seen defines "an organization... (as)... a purposeful system that contains at least two purposeful elements which have a common purpose" (Ackoff, 1971, p. 89). As an organization is defined to have this 'higher level' (which provides the basis for conflict resolution), basic conflict would again mean the non-existence of the organization! (1)

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(1) This follows, in fact, from the functionalist view of organizations a co-operative systems. As Allen (1975) puts it: "The assumption that organizations were co-operative systems allowed participants to be accounted for within the formal system of control as individuals or members of small groups and permitted the introduction into the analysis of personal goals which ...... cont.

If OR is prevented because of its sociological commitments from approaching an adequate conceptualisation of conflict, it follows that it could not approach an adequate conceptualisation of power. Ackoff and Emery do not even attempt a definition of power, and I know of no operations researcher who has considered the impact of the phenomenon on their work (although it could be considerable: see, Bevan and Bryer, forthcoming). Ackoff and Emery clearly see no need to attempt an understanding of power when they "note that cooperation and conflict exhaust the ways in which one individual can affect the expected relative values of others" (p. 197). Although Ackoff and Emery do not explicitly define power, a definition is in fact implicitly provided by their definition of conflict. For Ackoff and Emery one individual A is in conflict with another individual B when (say) the subjective interests of A (his "expected value") is less than it would otherwise have been because of the presence of B. In accordance with their scheme the only way in which we could measure the impact of B upon A would be in terms of what they call"... intersystem <u>behaviour</u> - that is, where there is an intent to coproduce a behaviour in the other..." (p. 139). Within this framework one might easily define power as the ability of B to influence by his behaviour the subjective interests of A. To measure Ackoff and Emery's (implicit) concept of power one would look at how the observable behaviour of B influenced the subjective interests of A. Ackoff and Emery's scheme is, in fact, quite

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(1) cont.
participants hoped to achieve within the organization. These personal goals might be consistent or inconsistent with the formal organizational goal. But the assumption that action was co-operative meant that the organization had qualities to survive a predominance of multiple personal goals which conflicted with the formal organizational one" (p. 104). Also, he goes on, with reference to the existence of systems, "if there are systems it <u>must</u> be assumed that in some way they can maintain their identities, otherwise system is not a workable concept" (p. 105).

sophisticated by the prevailing standards of political sociology where most of the attempts to develop a concept of power are to be found. Their scheme easily handles all forms of overt (i.e. behavioural) influences. As they say, "one system can affect the behaviour of another... by producing a change in one or more of the four components of the other system's choice situation, or in one or more of the other's parameters of choice" (p. 142). All the features that for them coproduce behaviour can be manipulated, and hence they form (potentially) part of their definition of power. By Lukes' (1974) classification of power concepts Ackoff and Emery's notion would be "two-dimensional". The features of both the one and two dimensional views of power are (a) the influence of B over A by B's behaviour which (b) affects A's subjective interests in (c) a situation of overt conflict. The two-dimensional view adds to this 'agenda-rigging', whereby power is exercised by the deliberate manipulation of what are taken by A to be 'issues' affecting his interests. For both Ackoff and Emery and the proponents of the one and two-dimensional views power is undetectable except through the influence of observable behaviour on subjective interests. Again, like Ackoff and Emery, subjective interests are only detectable as they are revealed in behaviour, i.e., as 'policy preferences'. It is in these two commitments that Lukes finds the weaknesses of the one and two dimensional views in comparison to a three-dimensional view which he erects. The three-dimensional view argues that the concentration on observable instances of the exercise of power is inadequate because "... the bias of the system is not sustained simply by a series of individually chosen acts, but also, most importantly, by the socially structured and culturally patterned behaviour of groups, and practices of institutions, which may indeed be manifested by individuals' inaction" (Lukes, op. cit., p. 21). The argument is a realist one; we must look for underlying structures and see power as "... a function of collective forces and social arrangements" (p. 22). The three-dimensional view not only takes issue with the view that the form of the power must be sought in observable behaviour, but also with the view which argues that the existence of the exercise of power is provided by the criterion of overt conflict. Just as Ackoff and Emery cannot explain the origin of conflict, they cannot explain its non-existence either. And this is just as important because, as Lukes says in criticism of the one and two-dimensional views, "to assume that the absence of grievance equals genuine consensus is simply to rule out the possibility of false or manipulated consensus by definitional fiat" (p. 24). There are serious issues raised for OR by these defects.

It is, first and foremost, an unacceptable limitation on our ability to investigate the social world. There is no a priori reason for assuming that the social world will be revealed to us solely in terms of observable behaviour, and every reason against such an assumption if we follow the realists and structuralists. Another important consequence of limiting the concept of interest to preferences expressed in conflictual behaviour is the implication, noted by Connolly (1972), that "... there simply cannot be "unarticulated interests"; those without particular policy preferences in certain areas must be viewed as not having an interest in a given policy result" (p. 462). It is possible within the three-dimensional view of power to make the important distinction betwen 'real' and 'subjective' interests; it is not possible to make this distinction within the one and two-dimensional views. The latter are firmly restricted to a consideration of subjective interests only. Since an individual's expressed preference depends crucially on the range of alternatives which are available for him to consciously choose between (as Ackoff and Enery and many other operations researchers accept), our entitlement to claim that an individual is acting in his 'real' or 'best' interests depends on our justified belief that he has chosen from the best alternatives which he really has. It is frequently claimed on behalf of OR that a major function performed by it is the display of realistic alternatives before a decision-maker from which he can choose. It is, of course, fundamental to the view of Ackoff and Emery (and Churchman, 1961) that what an individual's ends really are can only be determined by his choice amongst a range of alternatives. Thus, it would seem that OR embraces a concept of 'real' interests.(1) We have seen, for example, Rivett (op. cit.) telling us not to rely on "... asking some benevolent company president to tell us the organization's objectives" and that, instead, we should "... look at an intricate, social grouping of people working together and try to understand the collective will..." (p. 227). Precisely the same injunction is made by those social scientists (e.g., Katz and Kahn, 1966; Miller and Rice, 1967)who accept the consensus view). Thus, there seems to be clear evidence that OR does make a distinction between 'subjective' and, as Balbus (1971) terms it, 'objective' interests.

Surely Ackoff desires to resolve conflicts between subsystem, system, and suprasystem such that their 'objective' interests are met? Balbus draws the distinction in precisely a way which should be appealing to operations researchers. He says that "... our ordinary language recognises an objective meaning entailed in the concept of "interest", so that when we say an individual has an "interest in" something we mean that he has a stake in it or is "affected by it" (p. 152). Thus, "in this objective sense of the term the existence of the interest is not contingent upon the

(1) The determination of an individual's real interests amounts to the problem of determining valid 'counterfactuals' i.e., the interests an individual 'would have' under certain conditions e.g., 'perfect knowledge'. This idea is central to Churchman's (1961) and Ackoff's (1962) views. For example, Ackoff justifies the maximization of the expected value of a course of action as a criterion whereby a problem can be said to be 'solved' on the grounds that it is the criterion which "... would be preferred under idealized conditions in which the decision maker has perfect knowledge (p. 64). ..... cont.

individual's awareness that he has the interest, i.e., upon any psychological state in the mind of the individual. A person may be affected by something whether or not he realizes it; hence evidence can be marshalled to demonstrate that an individual has an interest even if he is not aware of it or even that what an individual thinks is in his interest is in fact not in his interest" (ibid, my emphasis).

We have here, in fact, a direct conflict between, on the one hand, the aspiration of OR to serve the objective interests of individuals and the systems which they constitute and, on the other, the possibilities which are open to it by reason of its constraining conceptual commitments. In this conflict it is the aspiration which loses out. Also, the ideal of free inquiry crumbles immediately we accept the restriction of confining our inquiries to observable behaviour because, as Lukes puts it in a criticism of the 'pluralist' school in political science who accept it, "... the trouble is that, by doing this, by studying the making of important decisions within the community, they were simply taking over and reproducing the bias of the system they were studying" (p. 36). Without a developed conception of the social world which a developed conception of power and conflict presupposes, the very result OR aspires to avoid is in fact achieved. The only assumption that could save this result is that the social system is unbiased, but then what need is there for OR? It has been pointed out (e.g., by Gouldner, 1970) that this is precisely the assumption lying behind

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(1) Cont.
Also Churchman (1968) argues that "the operations researcher... feels that a very important part of his role is to prevent unsatisfactory suboptimization. He believes that his responsibility is not merely to translate what the client says his needs are, but to study these needs in relation to the broader aims of the client" (p. 13).

Parsonian sociology. Balbus (op. cit.) makes the same point against Bentley, the acknowledged father of pluralism, when he says that "... if the political process is nothing but overt activity, and if interests are manifested solely through overt group activity, then it is logically impossible to say that certain interests are being ignored, distorted, or discriminated against in the policy-making process" (p. 158). Thus, OR's desire to remedy sub-optimization is guaranteed fulfillment: it is logically impossible within its conceptual framework for there to be any suboptimization!

The promotion of objective interests is not achieved merely by the institution of social processes of interaction, even though such innovations have been proposed, partly at least, on the recognition of the distinction between objective and subjective interests (this is particularly true of Churchman). The obvious reason why not is that it is the context within which such interactions are to take place which are, in the three-dimensional view of power, the subject of concern. It is impossible to see how, for example, we could have any faith that Ackoff's 'humanization' and 'democratization' (cf. also. Thorsrud and Emery, in Lawrence, op. cit.) proposals could lead to the furtherance of objective interests,(1) even though he argues that one should never "... force anything on workers or anyone else that they... (do)... not want" or "... pretend to know better than the workers what is in their best interests" (1975, pp. 97-98). Balbus suggests

(1) which are partly based on the equivocal, if not largely discredited, 'job enlargement' thesis for which Ackoff (1974) takes the authority of Blumberg (1969). Human Relationists, from whom this thesis emanated, have according to Perrow (1972) largely abandoned it. Also, Hulin and Blood (1968) after surveying much of the work done on the thesis conclude that the studies which support it are unscientific, and those which do <u>not</u> support it are scientific (p. 50).

that to operationalize the concept of objective interests requires an 'objective' analysis of the whole system of which the individual is a part. Thus, he argues that the objective sense of interest "... is objective because it refers to an effect by something on the individual which can be observed and measured by standards external to the individual's consciousness"; we should be concerned, he says, with the "... complex interdependencies within our society which bind men together and shape their destinies" (ibid). To discover objective interests one has to be concerned with nothing less than the "... structure of social organization as a whole..." (p. 156). Ackoff has not tackled the "... theoretical problem... (of)... understand(ing) the relationship between the way in which individuals interact to objectively affect each other's life chances and the way in which they perceive these interactions, i.e., to understand the origins of consciousness" (ibid). Nor has he shown any inclination in practice to do so.

In Ackoff's book 'Redisigning the Future' (1970) he presents a number of designs to solve real-world, large-scale problems, which he says are inspired by the systems approach. It is, of course, hard to say to what extent the solutions would be accepted by other analysts adopting the systems approach. However, in fact many of the solutions were proposed by other analysts.(1) I believe that most, if not all, of the solutions are built on assumptions about the nature of the social world

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(1) Also, a critical analysis of several instances of systems analysts' attempts to apply the systems approach to large-scale real-world problems has been made by Hoos (1972). She comes to much the same general conclusions as I do.

which would be objectionable to a large number of modern social scientists. I cannot subject all the solutions to the scrutiny which they deserve, and we will have to remain content, in this already overburdened chapter, with two brief illustrations.

In his solution to the ghetto problem the central assumption is that the reason for the persistence of ghettos is that there is a vicious circle of poverty which can be broken by the injection of some minimal amount of resources. Thus: "The blacks cannot go it alone in a white dominated society. They need access to human and material resources controlled by whites. They need whites working <u>for</u> them, not <u>on</u> them. This requires whites placing themselves and some of their resources at the disposal of the blacks, <u>to be used as the blacks see fit</u>" (p. 120). Although Ackoff presents this as a new solution, it is, in fact, the typical one which has been tried and which has repeatedly failed. I think it is fair to say that Ackoff's view is that it is a disequilibrium which has to be corrected. Hardly any other view is possible within the system's approach. However, the systems approach teaches us that what may look like a disequilibrium when the system is viewed one way, may turn out to be an incorrect specification of the system when viewed from another ('higher') level. The comments of a well-known student of planning, Harvey (1973), are relevant to Ackoff's assumption. Noting that the typical assumption made in approaching the ghetto problem is that we have a system in disequilibrium, he goes on to say tha although we might

> "... try to get back into equilibrium by attracting employment back into the city centre by urban renewal projects, support of black capitalism, and the like... (A)ll of these solutions have as their basis the

> tacit assumption that there is a disequilibrium in urban land use and that policy should be directed towards getting urban land use back into balance. These solutions are liberal in that they recognize inequity but seek to cure that inequity within an existing set of social mechanisms ... (p. 136).. (A)lthough all serious analysts concede the seriousness of the ghetto problem, few call into question the forces which rule the very heart of our economic system" (p. 144).

From the viewpoint of political sociology (or 'political economy') Ackoff's solution is the blatantly 'incrementalist' approach of which Churchman (1968, p. 71) complains.

I shall be brief with my comments on Ackoff's views on education. Ackoff has may wise things to say about education. About how it 'processes' pupils in stereotyped ways to produce sterotyped 'products'; how education should be a process of learning rather than teaching which should allow the child to develop his/her individuality; how a major problem is motivation to learn; and how the teacher's role is sometimes redundant etc. Early on in making his case for systems age education which gets around these problems he refers to Ivan Illich (1971) on the necessity for 'deschooling society'. So far so good. Against this background, however, we are suddenly confronted with what appears to be Ackoff's only systemic device for implementing these good ideas (we are otherwise left guessing as to how they could possibly be achieved), namely, the market system. Ackoff, in advocating the "voucher" system advocated by Jencks, a system which provides each child educational 'money' (cashed by the government) which the school uses as its source of income on being selected by the parents, argues that "much of social progress derives from the struggle for survival... (and that)... this is as true for organizations as it is for species" (p. 92). The idea is that the problem with schools at present is that they have no incentive to provide what is 'required'. With this system they would have every incentive because "... if they do not attract and retain applicants they would go out of business" (p. 93). As with the economist's model of perfect (that is 'ideal') competition, all things that are good flow from "... introducing the market mechanism into the educational system" (ibid): there is more choice, more adaptibility and hence more participation. How the introduction of the market system into education will achieve the results which Ackoff earlier specifies as the desirable attributes of education is left to our imagination. By contrasting this vision of Ackoff's with that of Illich we shall see very clearly that Ackoff, once again, takes the wider system for granted in his analysis which, ostensibly, promotes Illich's ideal. Illich comments on the Jenck's scheme:

> "(It)... is like giving a lame man a pair of crutches and stipulating that he use them only if the ends are ties together. As the proposal for tuition grants now stands it plays into the hands not only of the professional educators but or racists, promotors of religious schools, and others whose interests are socially divisive. Above all, educational entitlements restricted to use within schools plays into the hands of all thos who want to continue to live in a society in which social advancement is tied not to proven knowledge but to the

pedigree by which it is supposedly acquired. This discrimination in favour of schools which dominated Jenck's discussion on re-financing education could discredit one of the most critically needed principles for educational reform: the return of initiative and accountability for learning to the learner or his most immediate tutor" (p. 16).

Clearly, one's views on proposals such as those made by Jencks depend on much more than the internal 'sense' of the proposals themselves. Without saying that Illich is correct in his diagnosis, it is clear that the sense of such proposals as advocated by Ackoff depend on a larger world-view than he feels is necessary to investigate.

The reluctance which Ackoff shows towards taking wider social factors into account in his proposals stems from conceptual barriers which he placed in his path. From the point of view of social science Ackoff persistently and systematically 'sub-optimizes'. From the point of view of the ideals of OR he commits the two worst sins; arbitrary limits are imposed on inquiry which have the effect of preventing a considered view of the whole system. The whole system, which from the point of view of modern sociology is social structure, is ignored. The only image of the collective which enters into the picture is the Parsonian one of it being a benign guarantor of his proposals. As with Parson, for example, the 'rightness' of organizational goals is guaranteed by the assumption that organizations are 'adaptive' to society. Ackoff says, for example, that "a corporation, for example, is not evaluated by how well it performs relative to its own objectives but rather relative to the objective of the society of which it is part" (p. 15) and that "for example, universities are <u>explained</u> by their role in the educational system of which they are part rather than by the behaviour of their parts..." (p. 14, my emphasis). It is unclear whether these statements are meant to be explanatory or normative, but even if it is the latter what concepts does the systems approach provide for understanding 'society' or the 'educational system'? As Perrow (1972) has pointed out, the idea that society is the all-powerful is largely a myth, and is certainly not a fruitful orientation to research into the relationships between organizations and their environments. A much more fruitful hypothesis, he suggests, is that organizations to a large extent control their environments. The former orientation is the product of the Parsonian-type view "... toward(s) seeing power as a 'system property'" (Giddens, 1970, p. 457). This seems to be Ackoff's view of his humanization and democratization proposal. His "... circular organization is intended to maximize opportunities for relevant participation by its members, to maximize the extent to which the organization serves the purposes of its own members, and, by doing so, better serves its purposes" (1974, p. 50). Compare the definition of power advanced by Parsons: it is "... defined... as 'generalized capacity to serve the performance of binding obligations by units in the system of collective organization when the obligations are legitimized with reference to their bearing on collective goals" (Giddens, op. ci.t, p. 450). Apparently, for Ackoff, then, as well as Parsons, there "... is no such thing as 'illegitimate power'" (p. 451). The fact that "... collective 'goals', or even the values which lie behind them, may be the outcome of a 'negotiated order' built on conflicts between parties holding differential power is ignored, since for Parsons 'power' assumes the prior existence of collective goals" (p. 457). Once again, we come to the conclusion that the work of a prominent theoretician of OR can only be fully understood within the context of Parsonian social science.

Conclusion:

In this part I have explored the possibility that OR could achieve its ideals by turning, for the objectivity of its inquiries, to an understanding of the nature of its subject-matter. In Chapter 6 I looked into the possibility that OR could, by understanding the real nature of management, meet its ideal of improvement by improving the processes of management. However, we found that OR has no theory of management processes. An examination of the writings of two prominent operations researchers revealed, on the contrary, that the rationality of top management processes was taken as an unexamined starting point from which it was assumed the work of OR could objectively proceed. Our route to reaching this conclusion involved filling out these two accounts of the relationship between OR and management by referring to social theories. In the current chapter I have explored the possibility that the lack of an explicit theory of managerial processes could still be made good because underlying it was an explicit theory of social organization. That is, we have followed through the thought that an adequate theory of managerial processes must be based on an adequate theory of social organization. Thus, rather than 'management' in the abstract constituting OR's subject-matter, it was suggested that one of OR's most important theoretical foundations could be social theory. We have found that this is, in fact, the case. As probably the most important theoretical source on the question of social organization is the work of Durkheim (and later Parsons), I followed through the suggestion by asking whether it was possible to find at least homomorphic parallels between this theory and the views expressed by operations researchers. We have found that this is the case to such an extent that it seems reasonable to say that what we have found is, in fact, an isomorphic relationship between them. On the face of it, this conclusion may seem surprising. However, it is hard to envisage a discipline of inquiry without that discipline having some kind of grasp of the nature of its subject-matter. OR has such a grasp in its social theory. I argued (following Durkheim) that it was incorrect to attempt to sharply distinguish between economic and social realities, and it is to the credit of the theoreticians of OR that they have realized this and have seen that the basic nature of OR is not to be understood outside of its relationship to the social world.

However, an exploration of the social theory with which OR can be seen (in their hands) to be associated, even in its more developed form, led to the conclusion that within the context of it OR's ideals were unattainable. The reason for this was that the social theory in question took the nature of the social world for granted. The questions of how the social world is constructed, sustained and changed were not asked. It was just assumed, for example, that there is a consensus over fundamental norms and values without questioning whether (say) the stability which can be observed in many social systems is a sufficient warrant for this assumption. We saw how this assumption was responsible for plainly inadequate forms of explanation of the social world. Apart from the fact that acceptance of reified or functional forms of explanation (some examples of which were given) destroys any claim to free inquiry, it is clear that within the context of the social theory which produces such forms of explanation claims to be dealing with complex totalities have to be given up as well. I argued in Chapter 5 that dealing with complex totalities involved an attempt to understand structure or generative mechanisms. This is impossible within the social theory which I have been considering. We saw this very clearly in our illustration of the treatment of conflict and power which it allowed. With it we were confined to making untestable assumptions about the social meaning of observable behaviour. The necessity for this approach perhaps explains why Ackoff and Emery adopt it as a deliberate policy. With the social theory entertained by OR we are forbidden from inquiring into the generative structures (which may be unobservable) of social life. This conclusion was foreshadowed by the conclusion to Part One. OR's philosophical attachments militate against a serious study of the nature of the subject-matter which is the object of inquiry. This bias, it seems, has taken its toll in OR's inadequate grasp of the nature of the social world.

C O N C L U S I O N

The problem which has been tackled in this thesis is whether it was possible to find evidence in the writings on the nature of OR that it could approach its ideal of implementing reason in human affairs.

We have found that the works discussed are coherent with each other and with a vast corpus of philosophy and social theory to a surprising degree. The claim that OR is inward looking cannot be substantiated, though the suggestion that it ought as a deliberate policy to be more outward looking can be.

We have found that in every case OR's theoretical foundations conflict with its ideals. They did this either because they neglected subject-matter altogether (Part One) or because when they did consider it the task was not taken seriously enough.(Part Two). In both cases an apparent consistency of OR's theoretical foundations with its ideals was achieved by relying upon uexamined assumptions. When these assumptions were examined they were shown to conflict with OR's ideals.

This conclusion may appear totally negative. It is not. The destruction of the existing theoretical foundations was undertaken in the belief that this was a necessary pre-requisite to the future construction of stronger foundations. A general indication of the way in which OR might attempt to make this kind of progress towards implementing its ideals comes from the criticisms which I have made, particularly those of Part Two. I can suggest an outline of it here.

In Part One I argued that it was inconceivable that OR could meet its ideals without a grasp of the nature of its subject-matter. In Part Two we found that at least one important aspect of OR's grasp of its subject-matter was its social theory. We found there that its social theory was inadequate for implementing its ideals. However, OR has so far considered only a very restricted range of social theory, and its grasp of its subject-matter has thereby been very limited. It is possible that by extending the range of social theories to which it gives serious consideration it could make progress towards implementing its ideals. For example, serious interest in the nature of power and conflict could help throw much light on the processes of goal-setting, problem-formulation, data-collection, alternative evaluation and discovery and implementation. There seems little point in developing normative schemes of social inquiry without first understanding how such processes are influenced by these pervasive aspects of social reality. We have seen how it is impossible to understand the key notion of real interests outside of a serious study of power. It is likely that insight into decision-making processes--including those of OR--could be enhanced and implications for the practice of OR drawn if these realities were seriously considered by operations researchers.

Important though power and conflict are as real features of the social world, they are not the only ones. Power and conflict are best seen as particular manifestations of more general social processes. Other important manifestations are culture, tradtion and convention. Ackoff, throughout his works, for example, gives us many insightful illustrations of the impact of these realities on practical operations research work; of the social construction of reality which had a profound influence on rational problem formulation and solution. However, he does not discuss

is work in these terms and he gives no serious consideration to the
roblems involved in doing so. The same is true of other operations
esearchers who in practice cannot avoid implicitly referring to such
rocesses as they impinge on their work. There are too few case histories
f the impact of culture, tradition and convention and their creation and
onstructive interpretation on the work of operational research. And
et, analyses of the social processes of real-life problem-solving
y (for example) the tools of analysis which are being provided by
thnomethodologists (particularly what is known as "conversational
nalysis") and other schools of modern social science, could go some
ay towards gaining an understanding of the systems which OR desires
o improve. The operations research team is, of course, fully implicated
n this social reality, and attempts to understand both their indebtedness
nd contribution to it must be made. Such objectivity as OR can claim
ust come from its understanding, and not from its aloofness towards,
he construction of that social reality in which it is embedded.

Apart from the relevance of social theory to the small scale
icro-processes of social life which impinge on OR, there are also the
acro-prodesses of history and social structure. I am unaware of any
perations research study which has attempted explicitly to relate its
indings and recommendations to the wider social context in which the
esearch takes place. Ignorance of the history of the social system
n question and its relation to other social systems is an arbitary
imitation on inquiry into the nature of the problems with which the
perations researcher is faced. The 'fresh approach' of OR seems,
n the level of theory at least, to be interpreted as a deliberate neglect
f history (Beer (1966), for example, explicitly rules out the relevance
f history). This seems a pity since the implication of this neglect is

that both OR problems and their solutions falsely appear to be time-independent. Clearly they are not, but the ways in which problems are formulated and solved through time is ignored. Would a 19th Century OR man tackle problems of health care in the same way as he does now? Of what relevance are general principles of problem-formulation and solution without an understanding of the factors which influence their specific use? If it is at all possible that our 19th Century OR man would differ from his 20th Century counterpart, in what would the difference lie? Is it obvious that they would be the same?

Researching into the history of a problem (in its broadest sense) may both avoid a continual 'reinvention of the wheel' (cf. Hoos, 1972) and make a significant contribution towards understanding its structural features (see, for example, the research of Commoner, 1972, on the pollution problem). No doubt, in practice, many operations researchers do their own kind of historical research and have their own implicit grasp of its location in wider social structure. However, they are given little help from theoreticians. Theoretical and empirical research into the life histories of problems (and solutions) should contribute much to our understanding of them as partial reflections of complex totalities.

By attempting to understand the practice of OR in both its micro-social and macro-social aspects, we may progress towards a theory of the practice of OR. The actual practice of OR--both its successes and failures--has too long been neglected as a focus for serious study.

Investigating, both theoretically and empirically, the relationships between OR and social reality will not be easy. The nature of the social world is very elusive, as the current state of the social sciences attests. Also, even if it could be fully understood OR would not thereby be guaranteed an objective base. The social world is constructed and can be reconstructed. Even though we may be unable to say that one construction is absolutely objective while another is not, an understanding of the processes of construction may at least give us an objective basis for understanding what we are dealing with. To move on from there is much more difficult; at present no clear guidelines exist.

Whether the conclusions of this thesis are really negative or not will depend upon whether it is possible to find some form of objective basis for OR in social reality when the theoretical props on which it currently leans are taken away. Looked at in this way, we can rephrase the question with which Churchman (1971) finishes his book as an appropriate conclusion to this work, and as an introduction to future work. "What kind of a social world must it be in which inquiry becomes possible?" (cf. p. 277). The theoreticians of OR have posed this question--it remains to answer it.

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