SOME DANGEROUS MISCONCEPTIONS CONCERNING OPERATIONAL RESEARCH AND APPLIED SYSTEMS ANALYSIS

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FOREWORD

The International Institute for Applied Systems Analysis conducts inquiries in a wide range of fields in which systemic problems occur: energy, food and agriculture, resources and the environment, human settlements and services, management and technology, the theoretical tools needed for such analyses, and others. The problems dealt with cover a spectrum from those of perception and understanding to quite practical recommendations for improved courses of action. And the results of this work are widely communicated to audiences including both specialists and persons with broad interests and responsibilities.

Thus, the Institute is concerned not only with the knowledge and techniques from the many disciplines that enter into applied systems analyses but also with the craft skills needed to conduct a good systems analysis and to communicate its results effectively to its intended audiences.

One result of this latter concern is that the Institute devotes some effort to understanding matters relating to this craft, both to improve its own work, and to communicate this craft knowledge to a wider audience.

This paper, written while its author was Chairman of the Institute's Management and Technology Area, discusses some important craft issues for systems analysis. It was originally given as an invited address at the closing session of EURO III at Amsterdam in the summer of 1979.

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Some dangerous misconceptions concerning operational research and applied systems analysis

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1. Introduction

A colleague of mine once said that there were two kinds of people in the world – those who believed that there were two kinds of people in the world, and those who did not. It is a good point; for the temptation to simplify and divide the world into a small number of distinct categories is very great. It is also very dangerous.

The history of operational research and systems analysis is strong evidence of this tendency. In order to define the subject it has been successively narrowed down, renamed as part of the broadening process, narrowed, renamed, etc., etc. There is no longer even a commonly accepted name that describes what I practice and am now talking about. I shall call it ORASA [1].

Definitions are necessarily of the form 'ORASA is . . .' and various writers and speakers, having failed with single definitions, have tried to produce sets of statements that will define the subject more precisely. Go to any meeting on the subject and listen for the dogmatic statements. They are all inadequate, and are, I believe, doomed to be so. For our subject is unalterably complex, as complex as the reality we study – the reality of decision-making in a social context. Such reality can only be contained within dogmatic statements if those statements are so bland as to be uninformative. I have come to believe that dogmatic axioms about our subject are incorrect, and therefore lies, and dangerous lies at that. If that seems to be an overstatement let me remind you that the most dangerous lies are those that

(a) contain a large element of truth,

(b) say what the listener wants to hear.

Such dogmatic statements are dangerous. They have the persuasive power of truth but they are, at the best, near-truths.

One response to this belief would be to turn away from any serious attempt at definition. 'ORASA is what I do', is one response. A more honest path is to accept the complexity of life for what it is, to replace the axioms by near-truths and to analyse with care what the elements of truth and untruth may be. The result may not be a logical dichotomy – it will lead to a genuine understanding.

I propose, therefore, to describe seven 'near-truths', which would for the most part be strongly accepted by ORASA practitioners. But none of them are absolute truths, and they contain the seeds of disaster if their limitations are not recognised. In truth, they *are* dangerous.

2. Near truth No. 1 – ORASA is problem solving

This is perhaps the most important and therefore dangerous near-truth of all, simply because the truth of it is so important. The truth has to be stated and restated with power and persistence, since it is the very essence of the subject. The most dangerous opponents of ORASA are those who would try to push the subject into some easily labelled pigeon-hole, such as 'mathematical techniques'. [1] It is easy to understand why such attempts are made so repetitively. Life in most organisations, and particularly in universities, is regulated by the concept of functionalism. Everyone has to be defined by his function or his field of knowledge. If only one could 'place' the ORASA expert as, say, the person to call on when there is a computer problem to solve, or a piece of mathematical analysis to be done. This would make it easier to place ORASA staff in the hierarchy, as well as leaving it easy to decide when ORASA should be called in. It becomes a service function, safely remote from policy questions.

This is, of course, *not* what ORASA is about. It

[1] Indeed it is because the phrase 'operations research' seems to have become so closely identified with 'technique' – that I have been forced to coin my own phrase ORASA, which could be interpreted as Operation*al* Research and/or Applied Systems Analysis.

is about *problems,* real situations where people and organisations have to cope with issues they do not fully understand, and where an adequate answer cannot be provided by experience, either of the decision-makers or their technical advisers. ORASA started, and had some of its most spectacular successes, in the period 1937–1945 when there were no techniques available and when the problems facing the military decision-makers were immediate and could not be deferred. From the start the subject was problem oriented and, incidentally, scientific, systemic and non-disciplinary. Problems must be tackled as they are, with the full range of knowledge and techniques obtainable from any source brought to bear in a coordinated approach. That is still the ORASA way.

But, is it *necessarily* problem-solving? It is true that if you look at the work of any in-house team, you will find many tasks, both at strategic and tactical levels, that clearly are problem solving. They may concern an investment proposal, a logistic operation or perhaps some question of organisation or procedure. A clear requirement is formulated and the answer is provided in the form of a proposal, a statistical statement, or a computer program. The problem is 'solved'. Most of the identifiable financial savings in industrial ORASA teams come from such work. Nevertheless, the Operational Research Executive of the National Coal Board discovered that over a period of years only 15% of their tasks came into this category, although the financial returns covered more than twice the costs of the Executive. The remaining 85% of the work could not be so described, and yet management supported it with the greatest enthusiasm. What kind of work was it then?

The answer to that question is contained in the Appendices to a paper by Sir Derek Ezra [2] – the Chairman of the National Coal Board – in which he described the work of the Executive. These Appendices list some of the achievements of the group in 1975/76. Although some of the non-quantifiable achievements are still essentially of a problem solving nature, most of the work can best be described as creating understanding. And, of course, that is often the prime thing that managers required. They certainly do not want decisions to be taken for them; indeed, they will usually make sure that they are not. But the good managers do want to be as sure as they can that they know the likely consequences of any action they can. That is a matter of *understanding.*

Now sometimes understanding can be obtained through the reading of a report, without any real interaction between the analyst and the managers or policy-makers concerned. Such is the case with many of the major systems studies being carried out in the field of energy. Such studies, often of extreme complexity, give an understanding of the energy environment, and identify points of concern where action is likely to be needed. But for effective decision, it is necessary to explore the consequences of alternative courses of action. This will often again call for analysis, but the manager can no longer be divorced from the process. Through comments and criticism as the work proceeds, the analysis is changed and improved. But the process works both ways; in time the analysis will also change the understanding and knowledge of the manager. It is not an unusual experience for an ORASA worker to find that his model, once complete, is no longer necessary. By then the manager has already adapted so that he can out-think the model. He has a new understanding.

The question of understanding arises, however, even in problem solving situations. There are few occasions when one is able to incorporate every relevant factor into a numerical analysis. In most cases, the decision-maker has to weigh the so-called 'imponderables' against the analytical findings. How can he do this? Only through 'understanding', the achievement of which should be part of the research.

The point must not be taken too far; it is the essence of ORASA that problems should be solved and that the decision-maker should be helped. The phrase 'problem solving' contains the spirit of the subject. In practice, however, it can too easily degenerate into an approach that denies the realities of the decision-making process. The achievement of true understanding is often both a higher and more realistic goal.

Near truth No. 2 – models are central to ORASA

It could be argued that the one concept that distinguishes the work of the ORASA analyst from others who presume to study decision problems and advise on their solution is the central position which the ORASA worker gives to the use of models. Most other people who try to help managers depend, to a large extent, on their experience of the subject area concerned, their knowledge and their reputation. The engineer, the economist and the financial analyst come before the manager as professional experts. In the area of their professional competence they claim to have authority. They alone have qualifications and the skills to explore questions that lie within that area of competence. The ORASA man is different. It could be argued indeed, that he is a professional ignoramus. His expertise lies not in his knowledge of the particular subject area under consideration, but rather on his ability to obtain and sift evidence, to analyse, to construct models, to interpret. The validity of what he has to say depends on the accuracy of his data and the reliability and range of validity of his models. It is essential that the model is exposed for public viewing, that its assumptions can be criticised and the results obtained from it tested against reality. The model may be very simple or extremely complex, but it needs always to be there and open for examination.

The importance of models to the ORASA analyst can not, therefore, be understated. Quite apart from the fact that they provide some kind of guarantee of objectivity (although models sometimes conceal an outrageous partiality) they have many other functions which are vital to the analyst. In the first place, they provide an essential basis for a dialogue between the analyst and the manager. Thus, if information is to be obtained from the manager by question and answer, it is more than likely that the rather abstract questions that the analyst asks are only half answered. This is not because of unhelpfulness or laziness on the part of the manager, but from a lack of understanding of the assumptions and implications that lie behind the questions. We all reinterpret what we hear and the manager's frame of reference may be so different from that of the analyst that he is unaware that his personal reinterpretation substantially alters the true meaning of the question. Even if he grasps that question he may be unable to give it a full answer, without realising that the half answer which he gives will be misleading to the analyst. As a consequence, when the information provided by the manager is fed into the model it will give answers that are unacceptable. In many instances, this is the time when the real dialogue can begin – for the attempt to understand why the answers are unacceptable forces questions and answers to be reconsidered. For this reason, the dialogue that the model generates is often a more important output of the analysis than any recommendations that the analyst may make.

The model is also important because it makes experiment possible. It is the curse, but unavoidable curse, of management that decisions have to be made and adhered to without adequate recourse to experiment, i.e., without being able to test the relative efficacy of alternative policies. This happens partly because the complexity of affairs is often such that one could hardly explore adequately the consequences of one potential decision, let alone many, but more particularly because experimentation as it is understood in the natural sciences is usually not possible in social systems. One action whether experimental or not alters the situation in significant ways and automatically rules out many other alternatives for ever. If the situation, or part of it, can be modelled some of the difficulties in the way of experimentation can be overcome.

So, the model ensures objectivity; it makes it possible to have a useful dialogue with management, thus promoting their understanding, and it makes feasible the testing of alternative hypotheses. Can there be any serious question as to its central position?

The answer to this question can best be given anecdotely. A colleague of mine at the Interational Institute for Applied Systems Analysis was recently giving a seminar, one in a series in which we have been trying to explore and discuss the overall methodology of our work. He drew a diagram as in Fig. 1 to explain the very different kinds of problems which we were trying to tackle. At the right-hand side of the diagram he wrote down a number of tasks which were concerned with the technique oriented, more mathematical part of our research. Topics such as non-linear optimization, econometric modelling, etc., etc. At the left-hand side he wrote down a number of *big* problems, Energy, Food, Risk, Environment, etc. These are all topics on which we are engaged on a programme of research. As these are, in one sense, extremes of the work we undertake, it might be implied that they represent extremes of a continuum – that in the middle of the box there were some problems of topics which had an element of bigness but were directly making use of the techniques. This might be through case studies. I rather unkindly suggested that this was a wrong perception of the situation. The things written down were all conceptualiza-

Energy	Optimization
Food	Econometric modelling
Risk	Gaming
Environment	

Fig. 1. The range of problems tackled at I.I.A.S.A.

tions and generalizations. There is, for example, no such thing as a Food problem or an Energy problem. The reality with which we have to deal is people and nations who are hungry and cold, and a decision-making machinery which is either unable to cope or, more horrifying, simply not trying. At the heart of all the conceptualization to which the model building activity of the ORASA worker is devoted, is *reality* – a *reality* which is not even a clearly defined problem but rather what Ackoff [3] has called a 'mess' – signified by distress and unease and by unmade decisions. The 'mess' *is* central, not the model.

Let me give one more example. My own team at I.I.A.S.A., concerned with Management and Technology, are looking at another conceptualization, 'Problems of Scale'. As we have studied the problem the more difficult and the more complex it has become. Indeed, from the analysts point of view there are many distinct problems of scale. It could be argued, therefore, that these different problems should be handled quite separately by different people. In academic terms this argument may be sound, but from the manager's point of view there remains the one problem. Whether he has to build a plant, to reorganise his firm, or build a hospital, he has, at an early stage of his planning, to answer one simple question – "How big?". That is his problem. It does not help to be told that it is ten problems. You must start at the beginning and advise him how

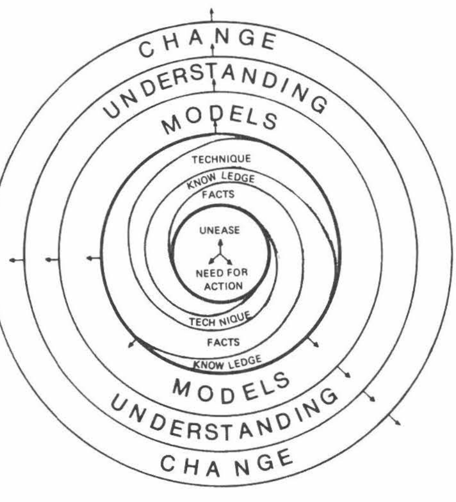

Fig. 2. The ORASA process.

to find out which of the ten he has, or how to combine the elements of five of those problems which are probably relevant to his question.

It is for this reason that I say that the statement that 'models are central to ORASA' is a dangerous misconception, for it places the model and the model builder in a position of precedence which they do not deserve. At the centre of ORASA you do not find the model nor even the problem, simply a 'mess', a situation of unease which may, or may not, call for action on somebody's part. The model is essential but it is not central.

The distinction between centrality and essentiality is not just a question of playing with words, it is fundamental to the methodology of operational research and applied systems analysis. It is so fundamental that it seems worth adding yet one more simplified diagram to the many which have been put forward to try and describe what we are trying to do. A diagram to describe this is set out in Fig. 2. We start with unease, a concern, a need for action. Then, through the acquisition of facts, through knowledge acquired over the years (often in many disciplines, such as economics, technology, social science), and through the use of analytical techniques, we are able to construct a model. This model leads to understanding on the part of those concerned with the taking of decisions, and that understanding will then lead to action and change. *That* is what our subject is all about. We started from a mess in the real world that needs improvement: we end with change, in that same real world.

4. Near truth No. 3 – problems can and must be defined (uniquely and invariantly)

One of the most important warnings that can be given to a young analyst is "Aim at nothing in particular, and that is what you will achieve". Much of the training of an analyst needs to be devoted to such topics as the question of problem definition, the need to define clear objectives, and the necessity of checking that the work remains directed towards the original objectives. You will see this in most work undertaken for organisations where it is necessary to prepare detailed terms of reference at the outset of a study and agree these with management. Even in academic circles, most research grants are only provided after a detailed statement has been made of the problem to be tackled and the methods to be employed in its solution. Problems not only *can* be defined, they *are*.

The need for such care hardly needs explanation. There is nothing more distressing, at the end of a lengthy and exhaustive study, than to find out that the results are of no interest to the management concerned. Alas, it often happens. Perhaps the most common reason for this is that the terms of reference were not adequately defined in the first place, or insufficiently criticised by the management concerned. This may be said to be the fault of the manager but the blame must really be put on to the analyst. The manager may well have little idea of what the analyst proposed to do, and he simply may not have had the time to interpret the language in which the proposals were expressed. He may have taken the analyst on trust and wrongly assumed that he knew what he was doing. In any case, the wrongly defined problem leads to an unusable solution. Another common fault that leads to unusable recommendations arises from the fact that as the analyst gets involved with a problem, he begins to understand it differently. As his understanding changes, the direction of the research starts to change so that in the end he is tackling a different problem from the one described in the terms of reference. It may well be that the problem he ends up studying is a more relevant or more important problem than the original one. But if it is not the problem that the manager wanted solved, the answer, however interesting or valuable, is liable to meet with a refusal or rejection.

But this very example, which shows the need for precise terms of reference and the need periodically to refer back to them, also shows that we are dealing with a near-truth rather than a full truth. We are saying that as an analyst proceeds with his investigation he may understand the situation differently and need to redefine the problem. It is only one step further to realise that if a different analyst were to undertake the study, he – with his different experience and perceptions – would define the problem differently and propose a different solution. If you doubt this, you have only to discuss the matter with any research group that has had to survive the trauma of having the people concerned with a investigation leave half way through. The chance of success for the whole study is reduced simply because the replacement team invariably see the problem in a different way. Thus, in the same situation different people will define the problem differently; even the same person may define the problem in different ways at different times.

The reason for this is, of course, that the starting point for applied research is seldom a single uniquely defined problem. The starting point is more commonly a situation causing unease and requiring some improvement. Thus a firm may be losing money, a manager may have to take action on some staff matter, a government may be facing an energy crisis. The situation demands action, it is not well defined; different people will indeed define it in different ways, and thus suggest different possible ways of studying it so as to reach a conclusion. Moreover, the actual situation may change from week to week, being affected by external conditions, or perhaps by internal decisions made within the organisation. The purpose of the analyst is to understand and to explore the consequences of various actions that could be taken. *But,* there is not a unique problem that could be exactly agreed by all analysts and which will stay invariant with time. All the analyst can do is define the problem, identify its relationship to the overall 'mess', and ensure that the work he is doing continues to be related to the real needs of the situation as time progresses. If we fail in this, our near truth quickly degenerates into a lie.

5. Near truth No. 4 – models are partial representations of reality

The basic concept of modelling derives from the assertion that the model is a statement, expressed in formal terms that can be openly discussed and analysed, of the underlying structure of some part of reality. Thus, a simulation model may reflect the flow pattern of ships unloading at a port, or the behaviour of a queue at a supermarket. Such models are not thought of as complete representations of reality; indeed, the only complete representation of reality is reality itself. Invariably they omit certain parts of the process under consideration. Such an omission may be deliberate for a number of reasons. It may be that a conscious decision is made to confine the analysis to certain aspects of the overall situation; it may be that the analyst simply does not know how to model certain aspects (say, the human interactions); it may arise from the need for simplification. Whatever the reason, if the assumptions are clearly stated and the deficiencies of the results noted and commented on, then the analysis can remain valid. It is accepted, by both analyst and policy-maker, that the model only represents a part of the overall reality. Sometimes, of course, the analyst finds himself in trouble when the simplifications that he has introduced are unacceptable, whether to other analysts or to the people who have to operate in the real world. This may invalidate his whole approach. 'How can we be expected to believe your answers' the managers may say 'when you are looking at average flows in situations where everything is dominated by short term variations, or where there are significant correlations between factors previously assumed to be independent?'. Attempts to cope with decisions like this lead to more and more complex models which try to take account of these points of criticism but in doing so usually introduce very many more parameters, make further assumptions, thus increasing complexity in an exponential manner. Accordingly, the analyst soon finds himself tottering on the edge of a slippery slope. In order to try to be more realistic and improve his credibility he is tempted to produce models that are in fact inherently less credible. This dilemma lies at the heart of applied systems science. Extremely complex situations remain extremely complex, despite our ability to build models. Models do not reduce the complexity of real situations, either in reality or in the perceptions of managers and decision-makers.

One way out of this situation is to cease to strive for the one single model or even the attempt to build a single family of such closely related models that all internal communication can be made by way of the computer. (A disaggregated model remains a single model from this point of view.) For example, most corporate planning departments have a series of separate models exploring different parts of the overall situation. Each model may be an adequate

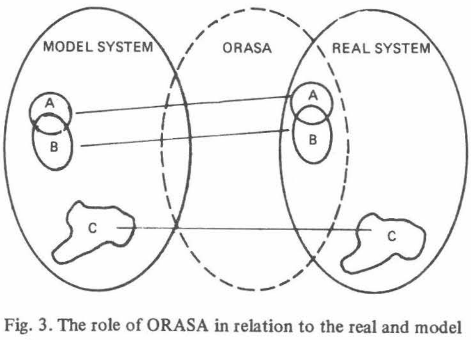

Fig. 3. The role of ORASA in relation to the real and model systems.

representation of some small part of the overall reality, and will have been designed in such a way that information and understanding derived from it may be used to assist in the analysis of some other model. This combination of models greatly facilitates that understanding of the overall situation which makes good planning possible. It is, however, necessary to understand that this collection, or suite, of models is not a representation of that whole reality that constitutes the organisation and its whole environment. It is not so planned, and it in fact, cannot operate in this way. What we have in fact is a 'real' system and a 'model' system whose parts may relate to each other in a clearly defined (though not necessarily 1–1) manner, but where the overall systems do not relate. This relationship can be represented in Fig. 3, which also illustrates the different roles that I see as being undertaken by the ORASA analyst and the model builder. In my view, the ORASA analyst interacts closely with the real world and draws on knowledge and information from as many sources as possible, including the information obtained from the model system, in order to help improve the performance of the real system. Although, in many cases the analyst and model builder may be the same person, it is essential for him to understand and identify his separate functions.

A second method that is used to try and understand the behaviour of very large systems is to replace the real system by some abstraction of it and to model the abstraction. The result can be extremely informative, but it does not provide us with a representation of reality, not even a partial one. I believe this distinction to be extremely important, and some explanation may be needed. I see a 'partial representation', as a good model of part of reality, which attempts to be complete for certain aspects but deliberately ignores others. Thus an analysis that operates in terms of weekly averages – rather than daily variations – or one which explicitly ignores human factors are both genuine partial representations of reality. Many large models which are currently the source of much debate, e.g., most global models, or econometric models which discuss technology as if it were a statistic, are models of abstractions. They are important in assisting our understanding, but they are not representations of part of reality. When an attempt is made to pretend that they are, the potential user of the model is in danger of being seriously misled.

So once again we find ourselves with a dangerous near-truth. All the models I have mentioned can be of major value in the ORASA task of developing understanding and improving decision processes. But their exact meaning and range of validity must be understood by their interpreters – in particular their relationship to reality must be very fully explored. Their relationship with reality may be more distant than you think.

6. Near truth No. 5 – tactics and strategy are entirely separate

The near-truths which follow are more related to the practice of the subject than to its fundamental structure, but they are no less important for that. The fifth near-truth is concerned with the separation of tactics and strategy, and its importance stems from the debate as to whether ORASA should concentrate on being immediately useful (tactics) or should concentrate on the "big" issues (strategy). This debate has raged since I entered the subject, and was in existence long before then. One school of thought claims that the only way to obtain the confidence of decision-makers is to assist them with day-to-day problems. If ORASA can solve the tactical problems, where the effect of the analysis can easily be seen, management will be encouraged to give them the long term strategic problems as well. The other school argues that such studies are dealing with relative trivia. The important matters for an organisation or a country are matters of strategy – not *how* to do something but *what* to do. They believe that the ORASA man who deals with tactical problems may save small sums of money, but will never make a major impact on the organisation for which he works; he will always be the technical back-room expert to be called upon when the manager requires. The strategist laments that the tactician will never get to study the fundamentals, which is what the scientist should primarily be concerned with. The tactician, on the other hand, points out that the strategist all too often stays with his head and feet high in the clouds, with devastating results when organisational gravity returns to normal.

It is interesting to note that this debate is quite different from the traditional debate between academics and practical men, since academics may be equally accused of spending too much time developing over-sophisticated mathematical techniques for the solution of tactical problems, as they are for having their heads in the air and only looking at very general concepts. As usual the academic cannot win, but at least we have an interesting realignment of allies.

In the context of this argument, the truth in our fifth near-truth seems almost self-evident. Tactics are concerned with the short term, with decisions which have to be made as the problems arise. ORASA can only help in such tactical studies if they have already been immersed in the problem in detail, so as to have developed the analytical techniques necessary to work out answers quickly. Strategy, on the other hand, is evolved as a process over a period of time. The evolution depends on policy analysis and understanding of the general background situation. Decisions are seldom taken rapidly or without a good deal of background preparation and widespread discussion. Clearly ORASA has a contribution to make to tactics and strategy, but the commonly held view seems to be that quite different talents are needed for the two kinds of work, different data bases, different peer groups – in fact they are entirely separate. My experience leads me to believe that such a separation is false and dangerous.

We faced this problem in the National Coal Board OR group some years ago, when we set out to establish an Area Systems Group, whose main purpose was to provide a service to the operating Areas of the Coal Board. (Previously the main effort of the team had been directed towards Headquarters problems.) At first sight, therefore, we were proposing to build up a group of tactical experts within a team that considered itself to be largely devoted to strategic problems. It soon became clear that there was a danger that the two groups would develop quite separately, both in terms of temperament and intellect. Inevitably the implication was that the Area Systems staff would be considered as the inferior. This was something we could never accept. Indeed, if one studied the work of the headquarter's teams, much had always been of a tactical nature. Tactical problems had been identified that were common throughout the Board, and we had set out to devise means which would improve the ability of individual managers in the Coal Board areas to handle them. Equally, a number of the problems that were tackled in the Board's production Areas were of an undeniable strategic quality. For example, an Area is responsible for the construction of new collieries and reconstruction of old ones – which means looking ahead for ten to twenty years. We simply could not separate tactics and strategy in terms of level in the organisation. Moreover, we had already found that it was impossible to undertake a sensible analysis at the HQ policy level without a full awareness of the tactical situations that would arise when those policies were put into practice. Conversely, it was not possible to develop good tactics unless one was fully aware of the strategic implications of their decisions. What was true of the analysis was true of the analysts themselves. We often found that the analysts were more aware of the overall situation than the decision-makers at the different levels. We established a deliberate policy of interchanging staff between the various functions. We would not allow one person to say 'I am a headquarters strategy man' and another to say that he was only concerned with Area level problems. Anyone who achieves a senior position within the OR team has first to show that he is good enough to work at both levels, and we believe that much of the team's success has depended on this.

These beliefs have been reinforced by subsequent experience, that the distinction, in kind, between tactical and strategic issues is largely artificial. The tools may be different but no two tactical or strategic problems use the same tools either. The researcher who tells me that he is not interested in tactical problems is in my belief simply saying that he is not interested in application and reality. Equally, the ORASA worker who wishes to stay at the tactical level is showing himself to be unwilling to face up to the real challenges of the systems approach. 'Tactics and strategy' is one of many dichotomies which attempts to divide up the world in which we have to live. It is a useful aid to teaching but a dangerous misconception in practice – it could be the death of ORASA.

7. Near truth No. 6 – all rigorous thought can be expressed in mathematical terms

The ORASA worker is, or should be, proud of the scientific rigor of his work. He likes to think that what he does is logical, based on empirical data, objective, and verifiable. Of course, the experienced analyst is not so naive to believe that the methods and tools that he has acquired in the course of this traditional training are adequate for the solution of problems in social organisations. Nevertheless he finds it distressing that he cannot judge himself by the criteria that, he believes, are applied to the physicist or chemist. (The fact that the methodology of the traditional 'hard' scientist also does not stand up to rigorous criticism based on the above four criteria is no consolation.)

One of the main reasons for this – though certainly not the only one – is that the ORASA analyst finds that he is dealing with social systems which exhibit unpredictable, and even irrational, behaviour. Naturally enough, the first move of the 'hard' analyst has been to turn to the social scientist for understanding, advice and help. Too often he is disappointed. He finds, first of all, that the social scientist has developed his own language which is no less obscure and no less unintelligible to the non-expert than his own more mathematically based jargon. He finds that the social scientist has, of necessity, had to abandon what he has always believed to be the basic methods of science and develop his own methods of developing hypotheses and verifying them. He finds, above all, that the social scientist cannot reduce complexity – as he tries to do – by discarding 'inessentials' and reducing all else to the formal logic of mathematics. In retaliation, or is it self-defence, he rejects the work of the social scientist as immature, unscientific and irrelevant to serious analysis.

To add to his concern he finds that the rejection is mutual. His rationalist approach is anathema to the social scientist since it is quite unable to describe the processes that are at the heart of the problems to be solved. His tendency to revert to mathematics as a substitute for thought, his inability to articulate ideas – all disqualify him from fruitful dialogue with those who take part in political processes and exclude him from serious consideration as a person with a contribution to make in policy formulation. This tendency towards rejection by the social scientist is reinforced by the extreme naivity which is revealed when the typical ORASA worker attempts to discuss such issues as the question of implementation. For twenty years or more I have been attending conferences in operational research and systems analysis and there has always been a group of 'old hands' bemoaning their colleagues' lack of understanding of the process of management and the necessary conditions for successful interaction. Well and good. But they are making the same comments as twenty years ago, and must sound to their younger colleagues – as well as to any social scientists who happen to be near – like discontented have-beens repeating old proverbs. Too often the comments are entirely lacking in serious intellectual content, and reveal that the small but not insignificant literature on the subject has not been read. In fact, for some extraordinary reason, despite the admitted importance of the subject the complainants abandon the intellectual rigor which they believe to be so essential in the 'analytical' part of their work.

This is distressing for a number of reasons. In the first place it implies that the essence of ORASA is not scientific – since the science is confined to the hard analysis – but rather an art dominated by hunch, instinct, and 'je ne sais quoi'. We all know that 'inspiration' or 'hunch' remains an essential element of all true scientific discovery. And as I have already said, no two analysts take a problem in quite the same way. But that does not mean that the process cannot be planned, disciplined and checked. In fact the study of process (both in analysis and in decision-making) is perhaps the key element in moving ORASA towards true intellectual maturity.

We are fortunate that we live in a time when the study of process is just beginning to move forward. In the USA, Churchman and Ackoff have pioneered thinking of this kind – more recently fundamental work has been undertaken elsewhere, e.g., by Boothroyd in the UK. At last, we are seriously trying to understand the process of analysis. But note that the study of process is difficult. The literature is hard to read and understand; its reference points are often philosophy and logic rather than traditional science; mathematics takes a back seat. The serious student finds his powers of rigorous thought stretched to the limit.

Similar studies of the decision process are in relative infancy. Many studies of decision-making are undertaken, of course, but decisions are too often seen as matters of personal or group psychology rather than as questions of process. Yet in major organisational issues, the process is often dominant and the skilled ORASA worker will be trying to identify those points in the process where he can make his impact. He can do so scientifically, but only if he is drawing on the full range of knowledge from all fields and accepting and understanding the basic concepts on which it is processed.

I have talked as if the problem is that of the 'hard' analyst educating himself or being educated by social scientists. I do this because that is the common pattern – though it is exciting nowadays to see that there are an increasing number of 'soft' analysts trying to come the other way. But the traditional ORASA worker will, in general, find that the first step will lie with himself. He must re-educate himself to understand the very great deal that has been learned by social scientists, to appreciate the different and often successful methods of approaching of problems that have been used by psychologists, sociologists, historians and even lawyers. We may believe that we have a vision of how some of the main problems in our society should be approached, but that we cannot make an adequate attack on many of these problems unless we join in with people whose knowledge and expertise lies in the 'soft' sciences. We shall only be able to join with them if, in the first place, we learn to understand their language, and secondly, understand and accept the validity of their approach.

8. Near truth No. 7 – ORASA is a science [2]

I am prepared to start the discussion of my last near-truth by claiming one absolute truth, that ORASA is scientific. (Assuming, of course, that we can reach any agreement as to what the scientific approach may be.) We have already referred to the key features of the scientific method as generally understood: logic, grounding in real data, objectivity and verification. We have also referred to the fact that in practice these four criteria have been found to be far from absolute; there remains a great deal of argument and debate as to what the scientific method is. Perhaps we should refer to the four basic features as ideals rather than laws. It is essential that both managers and peers are satisfied that we accept these ideals and work as closely to them as may be humanly possible. In my experience managers are well able to distinguish between real departures from the ideals and the quibbling deviations that the scientists argue about. Within the limitations imposed by our subject we must be as scientific as we can get.

But the scientific approach does not make a science. A science is based on a distinct body of knowledge, separable from other bodies of knowledge. There have, of course, been those who have tried to convert ORASA into a science within the

[2] When this paper was first presented in Amsterdam, the final near-truth was chosen to fit more closely to the originally announced title of my talk. I believe that 'ORASA is a Science' is a more appropriate choice (it was suggested to me by Roger Collcutt), which still enables me to retain a total of seven. The relationship to Seven Deadly Sins is not coincidental.

body of mathematics; after all in many university departments, operations research is a sub-section of a mathematical school. To those of us who believe that ORASA is concerned with the process of decision-making and policy formulation no such single body of knowledge will suffice. In undertaking any real applied study the ORASA man must be able to draw on all relevant knowledge, whether that lies in the 'science' of physics, engineering, history, economics, psychology or law. On the other hand, his true ORASA skill means that he is usually *not* an 'expert' in the subject fields with which he may be concerned. To be an 'expert' one is normally expected to know everything about a field of knowledge, and also to have a developed skill in applying that knowledge to problems. It is that standardized, highly developed skill, which is so often the death to an ORASA investigation. This is a fundamental difference between the ORASA researcher and the traditional scientist. To the traditional scientist it is inconceivable that anybody should undertake an investigation at the boundaries of knowledge unless he knows all the work and the literature in the field. It is anathema to him to suggest that someone may come in from outside and quickly obtain the necessary parts of such knowledge and incorporate it into a wider study. Yet that is precisely what the analyst has to do. We have thus identified two significant differences between the ORASA analyst and the traditional scientist. He is able to transcend the limitations of approach imposed by a discipline, and he draws on many expert skills not his own. So, of course, does the good scientist when placed in such a situation – but when he does he is in danger of ridicule from his disciplinary peers. It is 'unprofessional'. At the same time ORASA workers are often experts in some particular technique or field of application. It is not *always* an advantage to them.

Where then does the special skill and knowledge lie that enables an analyst to say with pride that what he does is ORASA. It is not simply the area of application – say complex systems or social organisations. Both of these topics can be treated as traditional science. The real skill lies in the approach and the process, in being scientific – not in practicing a science.

Having started this section with a high claim, let me conclude with another. I believe that the problems of management and decision-making in an age dominated by technological change need a new and radical approach. I do not believe that the ideal type of investigator to lead us forward will be found in the image of Isaac Newton or his natural successors of the 19th century. Rather, it is to be found in Leonardo da Vinci – Renaissance Man, artist and scientist – the man who is interested in everything and sees it to be not only permissible but necessary to draw on all possible knowledge to tackle the problem that he has in hand. It is a tragedy of the educational system that has been developed in the Western world that 'Renaissance Man' is more likely to arise again from the ranks of those with training in the sciences, rather than in the arts. For scientists can still show an ability and interest in the arts whereas those trained in the arts seldom have the basic education for the reverse to happen. But it is only a possibility, even for the scientist; our educational systems do their utmost to prevent it.

It may be that I have allowed by personal prejudices to cloud my case in these last few sentences. But the dangers inherent in this near-truth are best perceived in the heart rather than the mind. May we always be scientific – but may we never be confined within the boundaries of a profession or science.

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