Rethinking the Process of Operational Research and Systems Analysis

Edited by

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Preface

Both practitioners and teachers of OR and Applied Systems Analysis have suffered from the fact that, until the present, the subject has lacked a firm methodological base. A simple explanation for this is that the subject has its formal origins in traditional laboratory science, but that its practice lies firmly in the realm of applied social science. Both supporters and opponents of the subject have, therefore, tended to define it to suit their own purposes, and practitioners have, by and large, explained themselves in terms of "this is what I do". There has been increasing dissatisfaction with this state of affairs and in recent years a number of researchers and practitioners in different countries have set out to provide a more rigorous framework for understanding what the subject is really about; based on successful experience, rather than on hypothetical ideas as to what the subject should be. This book is the consequence of a meeting between a distinguished group of such practitioners and methodologists at a seminar at the International Institute of Applied Systems Analysis in August 1980. They found that there was a substantial agreement as to how the subject should be described and went away to write their own personal commentary on this common overview. The papers are diverse in style and intention—some are intensely practical, others are deeply philosophical. Together they provide, perhaps for the first time, a coherent, interlocking, set of ideas which can be considered as the foundations on which we may describe the subject as a science in its own right. Practitioners and teachers of OR and Systems Analysis will find the book directly useful as well as intellectually stimulating, and philosophers of science will find much in it that is relevant to their thinking.

ROLFE TOMLINSON
General Editor

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This is a revised version of one of the papers prepared as a background to the seminar itself. Kindler and Kiss are both practitioners and educators concerned with different systems-based methodologies. This chapter sets out the main issues which, in their experience, as well as in the literature, have caused controversy and which need resolution. They point out some of the hidden assumptions made in connection with Operational Research and Applied Systems Analysis (ORASA), which have limited its value and sometimes caused it to mislead. In doing this they draw extensively on the previous publications of those who attended the seminar.

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1

Future Methodology Based on Past Assumptions?

JÓZSEF KINDLER and ISTVÁN KISS

1. ON THE METHODOLOGY – THE INTERNATIONAL SCENE

One of the most important developments of applied science in the last 20 years has been the increasing attention that has been given to the study of complexity. This approach has been a radical departure from traditional methods of scientific enquiry since it has not been concerned with the reductional approach of segregating a problem into its constituent elements and analysing them separately but with the process of putting related elements together and analysing their integrated effect. In connection with this, then, has been the formal development of systems theory as well as methodological developments associated with systems-based sciences devoted to complex problems, such as systems engineering, systems analysis, action research, evaluation research, etc.

These theories and methodologies demand from us a changed attitude. The essence of this is well characterized by the lecture on the fundamentals of methodology in systems analysis given by Gvishiani (in Obninsk, 1978; Kelle, 1979): "The first methodological starting principle of systems analysis could be characterized as *the idea of the organic unity of the subjective and the objective aspects within a system.* It is typical of systems analysis to modify definitely the traditional, subjective-objective relation. In this respect, systems analysis differs from the traditional acquaintance with nature where the object is rather rigidly demarcated from the subject." (Gvishiani, 1978)

Exactly the same concept was expressed by Cavallo in the Preface of a report dealing with the first 25 years of the international systems movement, which is a moderate and unbiassed survey, prepared in the course of 2 years of international cooperation. (Cavallo, 1979) In his opinion the basic question is: does science accept that man is an inseparable part of the essential problems of today? This idea however entails a new attitude; therefore we have to study how systems approach can affect the treatment of that complexity, of which man is a part, and whether this attitude contributes to the spreading of our

information and to the development of our capacity for collaborating with the world.

Ackoff has called the second half of our century the *Systems Age*. This deviates from analytic thinking, reductionism and the determinism of the Machine Age, and is featured by synthetic thinking, expansionism and objective teleology. Forming this new attitude demands a new paradigm and here a quotation from Martin is relevant: "those people heavily involved in systems endeavors are up to their necks in grappling with the present shift in world-views". (Martin, 1978)

But what kind of means are being employed in developing this new attitude?

Boulding, one of the founders of the systems movement, claims that general systems research is a "level of theoretical model-building which lies somewhat between the highly-generalized constructions of pure mathematics and the specific theories of the specialized disciplines". (Boulding, 1956) This may be misinterpreted in two different ways and the following 25 years have produced examples for both kinds of misunderstanding. On the one hand, it suggests that systems theory only produces theoretical constructions which have no connection with practice; on the other, that on account of its abstract nature it works exclusively with mathematical means. There are series of examples to contradict the former, e.g. the methodologies based on the systems approach like systems engineering and operational research, or the manifold applications of systems analysis. As for the latter misinterpretation, this may have been due to the fact that some representatives of mathematical systems theory regarded their own work as exclusively systems research and they stressed it accordingly. Essentially the "traditional scientific criterion" seems to verify the latter misconception too, namely: only that which is scientific which can be formalized, and formal description can only be given by means of mathematics.

Similar criticism has been expressed by Blauberg–Sadovsky–Yudin:

> "... one of the reasons why several versions of general systems theory are limited is that these conceptions emphasize formal, mathematical problems in describing systems, whereas the content basic in this theory has not yet been sufficiently worked out." (Blauberg Sadovsky Yudin, 1969)

The other extreme viewpoint developed in connection with mathematics is often overemphasized:

> "It is one thing to caricature ... a narrow-minded mathematical approach which would view a particular mathematical and/or quantitative structure as *the reality*. However it is quite another *not* to recognize that mathematics and logic represent high points in the achievement of our collective intellect, and that it is not the use of such achievements but their abuse which is unwarranted and dangerous." (Klir, 1979)

In systems research the choice of the tools has a double role: it helps to point out the limited applicability of tools under given conditions, and it calls attention to new non-mathematical but scientific tools for them to be

developed and applied. The synthesizing feature of systems theory is indicated by the striving for integration of the quantitative and qualitative aspects of descriptions, problem solutions. There are definitions for a complex system to be one which may be analysed more than one way. (Rosen, 1979) According to Klir who represents mathematical system theory: "Within general systems research, the tools for solving the problem are of secondary importance" and "The tools need not be only mathematical in nature, but may consist of a combination of mathematical, computational, heuristic, experimental or any other desirable aspects." (Klir, 1979) The interdisciplinary feature of the systems approach finds expression in the wide range of investigational approaches and of the tools employed.

It is due to the introduction of systems thinking that the systems approach and its methodologies developed in the fifties, that is in the realization; for old problems new methods were developed, even if within a traditional branch of science. This enabled the recognition and definition of newer problems, and a new language for communication belonging to them evolved, which made it possible for representatives of problem areas previously treated as unrelated to think in common, that common thinking which is indispensible when dealing with such complex phenomena of which the recognizer and the resolver of the problem is a part. Thereupon, one of the important functions of systems research has been to remotivate consideration of concepts which, although left out, are widely recognized as needing to be grappled with. (Cavallo, 1979) It is especially interesting, that it was the methodological trends of the systems approach which created the situation in which numerous tools had earlier been employed whose usage began to be questioned. It was this experience in application which induced the heated arguments. Further, reinterpretation of concepts previously regarded as common in scientific practice, the investigation of the relation between theory and practice, and science and its application in a new correlation are being carried out. Why this change? Because instead of the traditional client-analyst relationship, participative problem solving is coming to the forefront; because the resolution of the conflict between the "ideal" concepts of rationality and optimality and the "practical" impossibilities, demands new methods and replenishing the old concepts with new contents.

What therefore is the cause of the debates and what the content? Why is it necessary to redefine certain concepts, and to replace others by new ones? And finally why did the argument over methodologies become more intensive? Let us start with the last question.

Systems engineering, systems analysis and finally, but not least, operational research belong to the group of methodologies which are applying systems approach. These methodologies are used in decision analysis in technical, economic, social, political fields as well as in planning, management etc. where they lead to a sequence of procedures like identifying and weighing the

objectives, generating and comparing alternatives, implementation etc. In the course of using the procedures, the usefulness of the applied tools, and their suitability, becomes obvious sooner or later as in the majority of cases real problems demand concrete solutions. It is just the applications which created a new situation and new problems, and mainly in those countries where these methods were developed and first applied. In this connection we have to form an opinion on the current problems of operational research and systems analysis.

The situation in the field of operational research and management science may be reviewed in terms of the sarcastic lecture by the mathematician R. E. D. Woolsey at the 1978 IFORS Conference. (It should be remarked that the lectures at the plenary session of the conference were devoted to the basic problems of OR/MS and systems analysis experiences of the past, to the present problems and to the possible evolution of the future.)

"OR/MS had a utilitarian beginning, when the primary aim was to 'solve problems'. This was probably the golden age of utility and acceptance of OR/MS. Rapidly . . . the tool makers dominated the profession. There is now a counterrevolution against the tool makers being carried on by the tool users. We tool users have no illusions that we can live without the tool makers. We just realize that we have told them that their tools were unsatisfactory with no result." Today two apparently opposite viewpoints prevail: "The first totally committed to utility and elegance, the second committed to sophistication and the *hope* of utility." The two viewpoints do not always have to be in harmony, but only the two together can provide good solutions for a long time in terms of Woolsey. (Woolsey, 1978)

Other signs also point to the fact that the future of operational research, and at the same time the future of the application of mathematics to management, organizational and social sciences, lies not in the further refinement of the mathematical models but in the transformation of the working style (and this essentially implies a return to the inter-disciplinary team work in the heroic age of operational research).

It is significant, that the papers at the Soviet conference on systems analysis in 1978 in Obninsk were on exactly the same theme. (Obninsk, 1978; Kelle, 1979) They emphasized that complex social and economic problems – different from those of simple technical ones – do not allow optimal solutions, and mathematical models are only approximations which do not so much provide solutions as help to put up good questions. The papers stress those solutions in which there are conflicting objectives or several criteria, when no single individual makes a decision except on the basis of values of the groups.

The work of the All-Union Institute of Systems Research in Moscow is also remarkable from this point of view. They are developing interactive decision procedures or such models as avoid the separation between decision-makers and operational researcher/systems analyst. (Larichev, 1979) With the

methods developed, they wish to ensure that the model analysing decision should serve not the modeller but the decision-maker. It is on account of this that Lapin emphasizes that systems analysis cannot be reduced to certain formalized methods. (Lapin, 1979) Actually over-formalization explains that in the United States, 20% of the decisions on major programs are based on such "scientific" methods of analysis.

As was shown in examples from the USA and USSR, and the list may be extended, there are problems in using scientific methodologies. It is evident, therefore, that there is contradiction between the exact methods of systems analysis and the decision-making in practice. What is the cause of this contradiction?

The significance of the methods in scientific research is well known. It is also known that the approach in certain scientific branches, as well as the uncritical acceptance of methods, has caused many disturbances to this day, in areas where they have been accepted mechanically. (For example the mechanical application of approaching the social phenomenon in a natural science way, like organizational research, has in many respects led to a dead-end.) At the same time, it is also true that certain methods which were worked out in other fields can be successfully applied in phenomena spheres deviating from the original. "Borrowing" methods is absolutely necessary in the research of new areas which do not dispose of appropriate methods to fit the nature of their particular subject. Criticism, checking of the assumptions, and investigation of the validity however are indispensible.

In scientific papers, but also in the daily newspapers, there are an increasing number of reports which regard the traditional method of natural science and technological approach as inadequate – on account of negative experiences – for solving different organizational, management and social problems. The modelling techniques which have become so widespread in the last 20–30 years are being especially criticized. The basic phenomenon is as follows: the scientific proposals for solution are made with a large amount of technical apparatus (formal models, computers) mainly as recommendations of an external body, and the results are never implemented. The phenomenon is repeated much too regularly, and could be explained by the unsuitability of the leaders of the organizations involved.

It can also be established as a fact, that such undesirable consequences can be attributed to the distorting method of approach, in which ideal presuppositions are made referring to man and his society. In conceiving the problem, it is common to exclude – on grounds of "scientific objectivity" – questions of interests, and problems of the value system, although these are fundamental in identifying the problem as well as in its solution.

As it is obvious that scientific information on man and his society belongs to social sciences (according to the traditional classification) it is therefore not surprising that the role and significance of social sciences are growing

throughout the world. With the growth of the role of social sciences, for instance, we need to review our concepts of *rational* behaviour of human beings in organizations.

This is why the investigation of the *presuppositions* have a special role, since every modelling procedure is based on assumptions.

2. ON THE ASSUMPTIONS OF THE PRESENT METHODOLOGY

It may be useful at the start, and perhaps as a frame of reference, to reveal and define the assumptions relating to the problem. In all the material distributed at the seminar, there was some *kind of problem*, as in Churchman, *the systems thinking problem*, with Majone the *problem of pitfalls*, in Tomlinson *misconceptions*, in Blauberg–Sadovsky the *problem of the paradoxes*. However it had not been established just what the *fundamental problem* was, the genus proximum. Without this, as a consequence of the deviating interpretations functioning as concealed assumptions in the course of the discussion, new pitfalls, misconceptions would be generated. Bartee's understanding of the problem seems to be acceptable for us and this assumption of ours will now be made explicit.

> "A *problem* is defined here as an unsatisfied need to change a perceived present situation to a perceived desired situation. A *solution* to a problem is realized when the perceived present and desired situations are perceived to be the same. *Problem solving* is the activity associated with the change of a problem state to a solution state." (Bartee, 1973)

We think that this understanding of the problem and the problem solution is in harmony with, on the one hand, our experiences; on the other, it actually belongs to the Popper-like second world described by Majone:

> "Popper distinguishes three 'worlds' or levels of reality: first the world of physical objects and physical states, second, the world of mental states, of subjective preferences and beliefs; and third, a world of objective structures that are produced by human minds but which, once produced, exist independently of them (theories, artistic creations and styles, norms, institutions, problem situation, critical arguments). This 'World 3' is autonomous from the other two levels of reality, though it is related to them by a number of links and feedbacks." (Majone, 1980)

It is hard to believe therefore that in relation to our topic there are some in the East and the West who do not perceive any problems in connection with systems analysis because they are satisfied with the present situation and so they have no problems, subject preferences, and beliefs: their second world of Popper reflects everything as being in order.

For most of us, however, the situation is problematical or unsatisfactory and our motivation can be expressed by the title of the final chapter in this book, written by Tomlinson: "Doing something about the future" (Tomlinson, 1980).

This is like saying that all of us judge the situation to be problematic.

However it is not certain that we view the differences between the present and the desired situation, or the seriousness of the problem, in the same way. In fact when sketching the desired situation our standpoints may be varied. The fundamental problem may be expressed in the words of Susman and Evered in connection with a crisis in organizational science:

"The principal symptom of this crisis is that our research methods and techniques have become less useful for solving the practical problems that members of organizations face" (Susman–Evered, 1978).

In other words Blauberg–Sadovsky–Yudin also criticize abstract considerations isolated from real system investigation:

> "This gap between the level of abstraction and that of concrete investigation is generally characteristic of the present state of systemic elaboration, and it inhibits the development of this work as a whole." (Blauberg–Sadovsky–Yudin, 1969)

Based on the previous statements, it may be possible that some of us regard the word "crisis" as an overstatement, claiming that the situation is not so serious. However, what we can be sure of, is that we all agree that there is a greater difference between theory and practice than in the acceptable and unavoidable. If we judge this well, *this* implies the essence and focal problem of our discussion. But what procedure is to be followed at the very beginning of inquiry?

Four possible relevant answers can be found in the history of thinking:

Francis Bacon: One must proceed purely inductively, putting all preconceived ideas, or Idols, aside.

René Descartes: One's procedure is purely rationalistic. One intellectually doubts everything which can possibly be doubted and then, from the indubitable minimum which remains, one *deduces* the remainder of one's knowledge.

Morris Cohen: Starting with the problem which initiates inquiry, coupled with scepticism with respect to traditional beliefs, one pursues hypotheses, testing them by the method of trial and error.

John Dewey: Since inquiry begins with a problematic situation, one must first observe the determinate facts, together with the indeterminate uncertainties of the situation, to suggest hypotheses respecting the possible resolution of its problematic character. These hypotheses in turn must be pursued to their deductive consequences and thereby checked operationally. (from Northrop, 1947)

In this way our discussion became a secondary decision problem for the methodology question. However this raises a host of questions. What for example do we regard as "facts", what as uncertainties?

"The potential for chaos in such a system is great, but it does not need to occur because every problem has a storehouse of knowledge and standards by which selection takes place. There is a simple control on the mechanism: the

human mind, unable to tolerate chaos, moves inevitably to decrease the number of options and focus on something from which it can reason and test. Although an infinite number of claims might emerge on a single problem, they will not. Only a limited number will and soon one will be the focus, however temporarily. This characteristic of human thought is a wonderful thing and it shortens our work in finding solutions enormously, but it has its dangers. The mind may be attracted to easy claims that seem to solve problems but which have grave consequences." (Rieke–Sillars, 1975)

This excerpt is from a book on argumentation and decision-making and confirms Bartee's understanding of the nature of the problem whilst, at the same time emphasizing the tremendous significance of argumentation. This and the role of persuasion is accentuated by Majone too. "The question is not whether analysts should use persuasion in proposing new policy ideas, but which forms of persuasion may be used effectively and without violating basic principles of professional ethics." (Majone, 1980) If we accept this then there will be a double consequence of our present discussion. Firstly our present discussion is audience-centred; secondly the results – if there will be such – will have to take this into consideration in oral or written presentations.

Now what are considered to be facts and just what presumptions values and value systems our authors have can only be known from background material and from their other work and may be quite fragmentary. It may be supposed on the basis of this, that in recognizing the problem and diagnosing it, there will be many common features especially with respect to facts. However – and there are subtle causes for this – a diversity of opinion is characteristic. It is even more likely that in relation to the method of the solution – in which not only the deviating assumptions but the conditions anticipated by the participants, as well as the difference in *values* and *value systems*, have a major role a much greater plurality is to be expected than in the former. This however, if we accept the principle of the dialectic approach proposed by Churchman or Mitroff, is no trouble. But while we consider the dialectic approach to be an excellent method for revealing the concealed assumptions, we can, with respect, imagine several solutions to solving the problem. In other words, it is not at all certain that with respect to the method of solution, a uniform standpoint can be worked out. We are therefore inclined to agree with the arguments of Van Gigch who, in his inspiring work published in 1976 (*Planning for Freedom*) propounded that "different kinds of planning spell different kinds of freedom(s). The planning paradigm consists of an 'assumptions-conditions-opportunities' triad by which the types of freedom prevailing in a system are determined. Due to the disparity among the elements of the triad from system to system, it is difficult to make intersystem comparisons of the amount of freedom which obtain."

3. TOWARDS A SOLUTION

The authors of these chapters certainly accept as a *fact* that, for example, the differences between Eastern and Western countries are not negligible. One Marxist principle about using concrete methods appropriate for different objects or systems has often been violated in the West by regarding certain procedures as generally acceptable, and by using pre-fabricated models to treat any object or system; the same approach has often caused difficulties in the socialist countries too. All this applies to concrete methods and procedures (namely to the tactical level of the methods) but does not imply that the methodology policy (and strategy) at greater depth, and built on more abstract assumptions and of much wider validity, could not be worked out. It is clear, however, that at this level we have to accept a more philosophical mode of discussion, although there are quite a few who regard this as empty speculation; in fact the positivists smell in it a brand of new metaphysics. The suspicion of the positivists has not been mentioned accidentally. It is precisely in this area that the more serious methodological pitfalls of the more subtle assumptions are concealed.

Majone mentions the burdens of the heritage of scientific method: "The received view on scientific method, which in one form or another has dominated the philosophy of science from the 1920s to the 1950s, has by now only historical interest for the specialists but it is still accepted by many researchers as a general scientific ideology. In particular, the influence of logical positivism – a key component of the received view – has been felt throughout the social and behavioral sciences, and nowhere more strongly than in the study of decision-making." (Majone, 1980)

As the close connection between systems analysis and decision-making is well known and since "during the past decade, the focus of research on system theory and systems analysis has shifted towards the analysis of large-scale systems in which human judgement, perception and emotions play an important role" therefore either on the basis of the interdisciplinary principle or on the dialectic approach method we have to take into account the large-scale systems inherently containing the human element, or the standpoint of the researchers in their *organizations*. But the positivist approach is not appropriate – from the marxist viewpoint either – for solving organizational or even social problems. We agree with Susman and Evered's claim in that "what appears at first to be a crisis of relevancy or usefulness of organizational science is, we feel, really a crisis of epistemology. This crisis has risen, because organizational researchers have taken the positivist model of science, which has had great heuristic value for the physical and biological sciences and some field of the social sciences, and have adopted it as the ultimate model of what is

best for organizational science. By limiting its methods to what it claims is value-free, logical, and empirical, the positivist model of science, when applied to organizations, produces a knowledge that may only inadvertently serve and sometimes undermine the values of organizational members." (Susman–Evered)

The term *positivist science* is used for all approaches to science that consider scientific knowledge to be obtainable from only sense data that can be directly experienced and verified among independent observers.

We are primarily concerned with the assumptions lying beyond the positivist approach methods. All positivist approaches to science (P.S.) are deficient in their capacity for generating knowledge for use by members of organizations for solving problems they face. The following arguments explain this deficiency.

"P.S. assumes that its methods are value neutral", but "knowledge and human interests are interwoven, as reflected in the choice of methods and the ends towards which such methods are put".

"P.S. treats persons as objects of inquiry, even though they are subjects or initiators of action in their own right."

"P.S. eliminates the role of history in the generation of knowledge. Individuals and organizations are not born in an instant with their present structures and functions intact. Rather, present patterns of behavior can many times only be understood as the product of shared definitions held by organizational members regarding what their common endeavor is about."

"P.S. assumes that a system is defined only to the extent that a denotative language exists to describe it. However, any representational system is always less than the actual system leaving the practising manager to rely on intuition, hunch, interpretation, etc. P.S. generally acknowledges that such methods can be precursors to scientific knowledge, but it does not consider them by themselves to be legitimate scientific methods."

"P.S. is itself a product of the human mind, thus knowledge of the inquirer cannot be excluded from an understanding of how knowledge is generated." (Susman–Evered)

It is not difficult to realize that with this elucidation we have come closer to the roots, although in contrast with this some would acknowledge John Stuart Mill's viewpoint. He claimed that "the relations of the basic principles and science are not like the comparison between the foundations and the house built on it, but that of the roots and the tree. The roots fulfil their task perfectly even if it is not dug down to bring them to the daylight." (Mill, 1863)

In our case, however, we had to dig down to the roots, because as it emerged from the description, the positivist roots in contrast with the physical sciences did not fulfil their role. As Checkland remarks: "The positivist methods of science applied to the physical regularities of the universe are not problematical. However, the question as to whether social phenomena may be