SCIENTIFIC AND SOCIAL ASPECTS OF SYSTEMS ANALYSIS: PROPOSAL OF A CONCEPTUAL FRAMEWORK FOR THE STATE-OF-THE-ART SERIES IN APPLIED SYSTEMS ANALYSIS

Giandomenico Majone

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1. Introduction

Like the legendary phoenix, the question: how scientific is Systems Analysis? (or Operation Research, or Management Science) keeps rising alive from the ashes of past methodological debates and offical definitions. For instance, more than twenty years ago, the Operational Research Society of Britain adopted a definition of OR in which the word "science" or "scientific" recurred three times. Operations Research was proclaimed to be the application of the methods of science to complex problems; a discipline whose distinctive approach is the development of a scientific model of the system being analyzed, and whose purpose is to help management determine its policy, and actions scientifically.

Similarly, Quade[1] observes that "It is easy to find statements in the literature of operations research which imply that analysis to aid any decision maker is really nothing more than the "scientific method" extended to problems outside the realm of pure science"; where "scientific method" is interpreted to mean that analysis advances through the successive steps of formulation, search, explanation, interpretation and, possibly, verification. And according to Olaf Helmer, "in comparing operations research with an exact science, it is with regard to exactness that operations research falls short, but not necessarily with regard to the scientific character for its methods."[2]

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[1] E.S. Quade, "Methods And Procedures" in E.S. Quade, ed. Analysis For Military Decisions, Amsterdam, London: North-Holland Publishing Company, 1970, p. 156.

[2] O. Helmer, The Systematic Use Of Expert Judgment In Operations Research, Santa Monica, Calif.: The Rand Corporation, P-2795, September 1963.

Yet, the apparent agreement conceals doubts and an uneasy feeling that things may not be as simple as that. Thus, in the editorial article of a recent issue of Omega, the international journal of management science, Samuel Eilon comes back once more to the issue of the scientific character of OR.[3]

The reason that so many methodological discussions on the foundations of systems analysis, and closely related disciplines, have achieved such little conceptual clarification is, by now, rather obvious. When the meaning of "scientific method" has not remained implicit, and hence open to a variety of different and often contrasting interpretations, it has been construed in terms which contemporary scientific epistemology finds unacceptable or, at least, in need of substantial revisions. In this respect, the article by Eilon represents progress, since here the scientific character of OR is argued in a framework which is explicitely (if somewhat simplistically) derived from what is probably the most influential of contemporary scientific philosophies. In essence, Eilon's conclusion is that OR may indeed be regarded as a scientific activity, because the OR process can be mapped (up to practically important but conceptually not crucial differences, due to the institutional setting in which the analyst must work) into the process of scientific inquiry as represented, for instance, in the epistemological theories of Popper and Medawar.[4]

The reaffirmation of the scientific nature of analytic work is comforting and, no doubt, fulfills a useful ideological function for the practising systems analyst. But a deeper understanding of the nature and problems of applied systems analysis, and of the conditions for its future growth, requires a more detailed examination of the ways in which analogous issues have been faced and, to some

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[3]S. Eilon, "How Scientific is OR?", Omega, vol. 3, no. 1, 1975, pp. 1-8.

[4]See, in particular, K.R. Popper: The Logic of Scientific Discovery, London: Hutchinson, 1959, and by the same author, Conjectures and Reputations, London: Routledge, 1969; P.B. Medawar, The Art of the Soluble, London: Methuen, 1967.

extent, solved by the scientific community through a process of trial and error that has lasted for several centuries. Indeed, the most important objective of the proposed State-of-the-Art Series as a whole, could be said to be the creation of mechanisms facilitating constructive criticism and the growth to maturity of the discipline, that are similar to those that have evolved, over a much longer span of time, in the field of scientific inquiry.

This is not to say that systems analysis can be simply treated as a particular form of scientific inquiry. The relationship, as I have already remarked, is significantly more complicated. Differences exist at a number of important points; for instance, as it will be shown below, in the role and structure of argumentation. However, the points of contact are sufficiently numerous to make the history and philosophy of science a source of important lessons for the methodology of systems analysis.

The central problem facing the philosopher of science is the explanation of the paradox of objectively valid and practically relevant knowledge emerging from fallible results and logical unjustifiable procedures. There is today fairly general agreement among specialists that the solution of the paradox cannot be found at the level of the research activity of the individual scientist or team of scientists. Rather, it must be sought in the socially determined criteria which give direction and meaning to scientific inquiry, and in the social mechanisms which control the quality of its results. In turn, the social dimension is intimately related to an essential aspect of scientific inquiry which has been overlooked in the traditional views of science: the craft character of the activity of the working scientist.

I shall use these basic insights to argue that the accomplishments, failures, and future prospects of systems analysis can be properly assessed only by taking into consideration the craft characteristics and the social aspects of analytic activity. This broader view of the analytic process will then be used to outline a conceptual framework from which the editorial program of the Survey Project can acquire direction and meaning.

2. Systems Analysis as Problem-Solving

The arguments to be presented in this paper will be developed around four theses:[5]

1. Like scientific research, systems analysis is essentially a craft activity or, as some authors prefer to put it, an "art";[6]

2. However, the objects to which analytic work is applied are not physical things and phenomena, as in the case of the traditional arts and crafts, but intellectual constructs studied through the investigation of policy problems;

3. The work of the systems analyst (and of the scientist as well) is guided and controlled by methods which are mainly informal and tacit, rather than public and explicit. It is the task of a methodology of systems analysis to make these guiding ideas as explicit as possible, as a precondition for a critical discussion of their validity;

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[5]Cf.J.R. Ravetz, Scientific Knowledge and its Social Problems, Harmondsworth: Penguin University Books, 1973, pp.71 and following. The significance of the craft element in scientific work has been pointed out by M. Polanyi, Personal Knowledge, Chicago: The University of Chicago Press, 1958, and further elaborated in Ravetz' important contribution to the philosophy of science. It should be noted that although my discussion owes much to Ravetz' ideas, it differs at a number of important points from his; for instance, in the characterization of policy problems ("practical problems" in his terminology).

[6]Thus the question: Is systems analysis an art or a science? can be seen to rest on a mistaken view of science, since scientific research is also an art; i.e. an activity conducted according to personal and largely tacit rules.

4. The theoretical adequacy and practical effectiveness of systems analysis depends on social conditions and processes, and on the existence of suitable institutional arrangements.

In the discussion of these theses, the central concept is that of "problem". In fact, systems analysis can be described as problem solving on intellectually constructed objects; and the different characteristics of analytic work roughly corresponds to the phases of problem solving, from formulation to proposed solution. Thus, the craft character of the work is seen most clearly in the early phases, where the analyst interacts with the external work (collection of data, assessment of their reliability and transformation into information, modelling of the system under investigation, etc.); the social character is exhibited in the methodological choices and judgments which guide and control the analysts's work; the artificiality of the objects of inquiry is most obvious when we consider what is involved in "solving" a policy problem; while the influence of social processes is evident in the transformation of analytical recommendations into actual decisions and institutional changes.

In the following sections I shall discuss the individual and social aspects of systems analysis. Here I consider the nature of policy problems, and the artificial and abstract nature of the objects on which the analyst operates. On the first point, I maintain that no essential difference exists between scientific problems and policy problems. Consider, for instance, the characterization of scientific problems that has been proposed by Ravetz:[7] a major part of the work is the formulation of the question itself; the question changes as the work progresses; there is no simple rule for distinguishing a "correct" answer from "incorrect" ones; and there is no guarantee that the question, as originally set or later developed, can be answered at all. Only a moment's reflection is needed to see that policy problems

[7] J.R. Ravetz, Scientific Knowledge and its Social Problems, cit., p. 72.

exhibit the same characteristics; and if supporting evidence is wanted, this can be easily found in the literature of systems analysis. Thus, according to Quade "the "typical" systems analysis problem is often first: What is the problem?"; "The problem itself does not remain stationary. Interplay between a growing understanding of the problem and of possible developments will refine the problem itself"; "There is frequently no way to verify the conclusions of the study".[8] Again: "The problems an analyst can be asked to tackle in the public sector are particularly frustrating. Usually they are urgent and ill-defined. Often they are complicated, and sometimes they change radically during the investigation".[9]

Or see what Eilon[10] has to say about solving decision problems under uncertainty (which is, of course, the natural condition in any policy problem): "In all decisions under uncertainty .... actual results often deviate substantially from predicted "expected" results (based on subjective probabilities). To say that the decision is still valid because one should compare the expected results not with the actual results but with their mean value (had the "one-off" reality been repeated many times) is of little help, since the statement is not testable".

That policy problems may have no solution under the economic, political and institutional constraints existing at a given moment in a given country, should be obvious to anyone familiar with its administrative and legislative history. Indeed, it can be argued that the proper role of the analyst consists in establishing the conditions of feasibility of a proposed course of action, rather than in accumulating evidence in favor of a pet solution. As I have written elsewhere, "Too often we take it for granted that any social problem can be solved, if sufficient resources are available.

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[8]E.S. Quade, "Methods and Procedures", in E.S. Quade, ed. Analysis For Military Decisions, cit. pp. 151, 154, 157.

[9]E.S. Quade, Analysis for Public Decisions, New York: American Elsevier, 1975, p. 298. Italics mine.

[10]S. Eilon, "How Scientific is OR?", cit. p.8.

But the manageability of a social task cannot be rationally discussed until we have specified the acceptable means of collective action, as well as the limitations imposed by the availability of resources, knowledge, and organizational skills."[11]

Thus, Ravetz' criteria do not allow to separate sharply policy problems for scientific problems. Such differences as may exist (for instance, the different time constraints facing the scientist and the analyst, or the different possibilities for testing results) are of an extrinsic nature, and do not affect the basic conceptual equivalence.[12]

This equivalence is further emphasized by the shared abstract quality of the objects of both scientific and analytic inquiry. In this respect, systems analysis is actually more "theoretical" than many natural sciences. For, if is is true that even basic concepts like "substance" in chemistry, or "force", "particle" and "field" in physics, are purely intellectual constructs, the more descriptive natural sciences operate largely with concepts

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[11]G. Majone, "The Role Of Constraints In Policy Analysis", Quantity and Quality, 8, 1974, pp. 65-76; see also "The Feasibility of Social Policies", Policy Sciences, 6, 1975, pp. 49-69.

[12]Compare, for instance, the last quotation from Quade with the following passage: "Although the objects of such practical or technical problems are also artificial to some extent, they do not change their nature in the course of the work. One of the things that makes scientific problem solving so uniquely subtle is that the very objects of the work evolve as the work goes on, and in a fashion which is not predictable in advance. For the discovery of new and unexpected properties of the objects of the investigation entails a change in the objects themselves; the objects described in the conclusion of a problem with genuine novelty are not those which existed when work on the problem began".
Ravetz, cit. pp. 130-131. Any experienced analyst would, I believe, reject the first sentence, and accept the last two as a fair description of policy problems.

whose concrete correspondents are fairly obvious.[13] On the other hand, because of the abstract character of social and economic relations, all concepts appearing in the formulation or solution of a policy problem are necessarily the product of convention and definition. This is obvious in the case of terms like "price", "cost", "GNP", "efficiency", "need", "urbanization", "pollution", but it is equally true for concepts like "poverty", "health", "unemployment", "crime" which acquire some kind of operational meaning only when expressed by means of (necessarily arbitrary) statistical indices or in terms of legal definitions. Indeed, as Alan Coddington has observed, "economic statistics are extremely abstract things", the product of "arbitrariness" and "convention".[14]

The same holds true, a fortifori, of the social data (but even of most technical data) which represent such a large part of the numerical input of analytic studies.

Although I have spoken, so far, only of problems, creative analysis usually begins with something less than a problem; we may call it "problem-situation". This in an awareness that things are not as they should be, but there is no clear conception, as yet, of how they might be put right. An important part of the problem situation, is the historical background and the "issue-context" in which the policy debate takes place. It is obviously important for the analyst "to know as much as possible about the background of the problem - where it came from, why it is important, and what decision it is going to assist".[15]

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[13] On the intellectual character of the objects of scientific research, see, for instance, J.R. Ravetz, Scientific Knowledge and its Social Problems, cit., especially ch. 4, and M. Deutsch,"Evidence and Inference in Nuclear Research", in D. Lerner, ed., Evidence and Inference, Glencoe, Ill.: The Free Press, 1959, pp. 96-106. Deutsch gives several examples of the abstract nature of the basic data of high-energy physics,

[14] A. Coddington, "Are Statistics Vital?", The Listener, 11 December 1969, pp. 822-23.

[15] E.S. Quade, Analysis for Public Decisions, cit., p.306.

But notice that, although the problem-situation is in a less specified state than the problem to which it may give rise, it is already a very artificial thing. The very existence of a problem situation presupposes a matrix of technical materials: existing information, tools, and a body of methods including criteria of adequacy and value.

Once a policy problem, after this phase of gestation, comes into being, the cycle of analysis may be described by five distinct phases: formulation; information and argument; conclusion and recommendation; implementation; control.

3. System Analysis As Craft Work

Although craft aspects are evident in every phase of the analyst's work, I shall discuss them here with reference to the categories of data, information, tools, and pitfalls.

Data. Data are the results of the first working-up of the materials relevant to the investigation of a problem. In systems analysis, data are often "found" rather than "manufactured", i.e. they are produced by observation rather than experiment. This requires craft skills that are rather different, and in many respects more difficult to acquire, than those required for the analysis of experimental data. For instance, the sampling process through which the data are obtained is very much influenced by the methods used, the skill of the samplers, and a host of other factors which may lead to result quite unrepresentative of the general situation. Also, data are collected according to categorical descriptions which never fit perfectly the objects of the inquiry at hand.

Data pertaining to preference and probability assessments are notorious for their subjectivity and unreliability.

Even when data can be obtained from experimentation, as in the case of some recent large-scale social experiments, there is no guarantee that even the best experimental design offers sufficient protection against dangers and pitfalls, of which the "Hawthorne effect" is only one of the best known examples.

Since perfection of data is impossible, even in the so-called exact sciences, the standards of acceptance will have to be based on a common judgment of what is good enough for the functions which the data perform in the problem treated by the systems analyst. This judgment depends in turn on the criteria of adequacy generally accepted for the solution of such problems. Thus, the simple judgment of soundness of data is a microcosmos of the personal judgments and accumulated social experience which go into analytic work.

Information. At least in quantitative terms, an excess, rather than a scarcity, of data is the usual situation in systems analysis. Hence the need to reduce the mass of data, to refine them into a more useful and more reliable form. Data transformation involves a new set of craft skills, with the application of new tools (often of a statistical or mathematical nature), and the making of a new set of judgments. This new phase of the analyst's work, the production of information, can be illustrated by a number of examples: the calculation of averages and other statistical parameters, the fitting of a curve to a set of points, the reduction of data through some multivariate statistical technique. The operations performed on the original data may be involved or quite simple, but they always represent a momentous step. Through these operations, the raw data have been transformed into a new sort of material, and from this point on the analysis is carried out only in terms of these new entities.

This transformation of data into information involves three basic judgments, which all present the risk of serious pitfalls. The first is that the advantages achieved through data reduction compensate the probable loss of information; generally speaking, the existence of "sufficient statistics", i.e. of summaries of the data which contain exactly the same amount of information as the original sample, is the exception rather than the rule. The second, is a judgment of the goodness of fit of the model to the original data; the third is that this particular model, among the infinitely many possible ones, is the significant one for the problem under examination. All the operations and judgments involved in data reduction, transformation, and testing are, of course, craft operations.

Tools. Analytic tools may be roughly classified in terms of data production, manipulation, and interpretation.

The category of interpretive tools includes, in particular, "tool-disciplines", i.e. other fields of natural or social science which must be mastered to some extent in order that competent analytic work may be done.

Each set of tools has its characteristic pitfalls, and if major blunders are to be avoided, the user must develop a craftman's knowledge of their properties. For instance, the dangers inherent in the use (and abuse) of statistical tools have been often pointed out, although serious fallacies can still be detected even in standard applications.

These dangers are made particularly acute by the prevailing metaphysic, according to which a field becomes more genuinely "scientific" as it more closely resembles theoretical physics in its mathematical formalization. Thus, in an attempt to give a more scientific appearance to his conclusions, the analyst is often induced to use formal tools that exceed the limits of his mathematical or statistical sophistication, and whose range of meaningful applicability he is therefore unable to assess. The consequences have been well illustrated by the mathematician Jacob Schwartz:

> "Mathematics must deal with well-defined situations. Thus, in its relations with science mathematics depends on an intellectual effort outside of mathematics for the crucial specification of the approximation which mathematics is to take literally. Give a mathematician a situation which is the least bit ill-defined - he will first of all make it well defined. Perhaps appropriately, but perhaps also inappropriately .... The mathematician turns the scientist's theoretical assumptions, i.e. convenient points of analytical emphasis, into axioms, and then takes axioms literally. This brings with it the danger that he may also persuade the scientist to take these axioms literally. The question, central to the scientific investigation but intensely disturbing in the mathematical context - what happens to all

this if the axioms are relaxed? - is thereby put into shadow .... That form of wisdom which is the opposite of single-mindedness, the ability to keep many threads in hand, to draw for an argument from many disparate sources, is quite foreign to mathematics. This inability accounts for much of the difficulty which mathematics experiences in attempting to penetrate the social sciences".[16]

It is important to realize that the influence of tools on a field is more subtle than a mere creation of possibilities. The extensive use of a tool involves shaping the work around its distinctive strengths and limitations; one can rarely apply a new tool to an existing stream of research without modifying it strongly. In the best case, as new tools come into being and are judged appropriate and valuable by people in the field, they alter the direction of work in the field, and the conception of the field itself. In the worst case, we assist to the phenomenon of "new toolism", a disease to which operations researchers and systems analysts seem particularly predisposed.

Those affected by this disease "come possessed of and by new tools (various forms of mathematical programming, vast air-battle simulation machine models, queuing models and the like), and they look earnestly for a problem to which one of these tools might conceivably apply".[17] Of course, if the "paradigm" natural science were to become a discipline like ecology, which uses the whole range of tool-providing sciences, but whose objects cannot be reduced to any of them, then the social relations of tool-providing and tool-using fields (which today reflects the superior intellectual prestige of the former), would be drastically altered.

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[16] J. Schwartz, "The Pernicious Influence Of Mathematics On Science" in P. Suppes ed. Symposium On Logic, Mathematics and Methodology, Palo Alto, Calif.: Stanford University Press, 1960, pp. 356-360.

[17] A. Wohlstetter, "Analysis and Design of Conflict Systems", in E.S. Quade ed. Analysis For Military Decisions, cit. 106. The expression "new toolism" is attributed by Wohlstetter to the late mathematical statistician L.J. Savage.

Pitfalls. The craft character of systems analysis can be seen most clearly in the concept of "pitfall". A pitfall is the sort of error that destroys the solution of a problem and nullifies the validity of a policy recommendation. Perhaps the most reliable way of assessing the maturity of a practical or theoretical discipline, is by the degree to which the ways around its common pitfalls are well charted, and those encountered in the application of the discipline to new fields of inquiry, can be sensed in advance. Hence, the increasing realization of the many pitfalls which can be encountered in the application of systems analysis to policy problems is a sign of increasing maturity, rather than an admission of weakness.

Quade[18] distinguishes two categories of pitfalls in applied systems analysis: Those internal to the analysis itself, and those concerned with getting it used. Internal pitfalls are further subdivided into those that are inherent in all analysis, and those introduced by the analyst himself. Most important among the internal pitfalls of the first type are those associated with misconceptions in the treatment of uncertainty and of the time element; with the selection of inappropriate criteria of choice or measures of cost and effectiveness; with an incomplete analysis of feasibility conditions (e.g. the disregard of political and administrative constraints), and of the distributional consequences of the proposed policy.

Of the pitfalls introduced by the analyst, the most serious is probably that of personal bias, both in the form of preconceived notions concerning the nature of the problem, and of inflexible committments to a given solution. Another common pitfall is a misplaced pragmatism which suggests to "get started" with the analysis, before the problem has been sufficiently understood.

Examples of external pitfalls are many kinds of errors arising in the process of communicating the conclusions of analysis; for instance, the arguments supporting a conclusion may be unsuited to

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[18]E.S. Quade, Analysis for Public Decisions, cit., pp. 300-317.

the type of audience to which the analyst is addressing himself. A particular form of this pitfall is what Quade calls the "myth of a unique decision maker":

> "Analysis are ordinarily designed and carried out, although perhaps not always deliberately, as if they were to assist a solitary decision-maker who had full authority over acceptance and implementation. This may sometimes be the case but it is not the usual situation, even in the military, and almost never when broad social issues are involved. Even when there is a single decision-maker his staff at a minimum supplies the details of any policy that is set .... Influencing organizational behavior can be quite different from influencing the behavior of an individual and, since we understand so little about it, can constitute a pitfall for policy analysis".[19]

In matured disciplines, the avoidance of pitfalls is accomplished primarily in two ways: by the charting of standard paths, through a body of standard techniques which can be safely applied as a routine, which skirt them; and by each researcher becoming sensitive to the clues which indicate the presence of special sorts of pitfalls he is likely to encounter in his work[20]. Methodologists of systems analysis have up to now stressed the second approach, but as experience in the conduct of analytic studies accumulates, we can expect that standard procedures for the avoidance of the most serious pitfalls will be systematically developed.

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[19]E.S. Quade, Analysis for Public Decisions, cit. pp. 314-315.

[20]Cf. J.R. Ravetz, Scientific Knowledge and its Social Problems, cit. p. 97.

4. The Components of Analysis

Having described the activity of the systems analyst as craftman's work applied to the solution of problems involving intellectual constructs, it is now appropriate to examine the constituents making up a solution or policy proposal. As it turns out, the basic categories introduced by Aristotle in his analysis of the craftman's task can be adapted to our present purposes.[21] Aristotle examines a task in terms of four categories or "causes": material, efficient, formal, and final. These four causes correspond, respectively, to the physical substance which is worked on; the activity of the agent in shaping it; the shape which the object finally assumes; and the purpose of the activity, or the functions of the object itself.

In adapting the Aristotelian scheme, the crucial difference to be kept in mind is that the purpose ("final cause") of the analyst's activity is not the production of a material object satisfying certain requirements but the description and analysis of a complex situation. The "form" of the analysis is an argument in which evidence is cited and from which a conclusion is drawn. In turn, the evidence will contain a more or less explicit description of the "efficient cause": the tools, techniques, and models that have been used, auxiliary problems that have been solved, and perhaps, difficulties and pitfalls encountered and overcome. Finally, the intellectually constructed classes of things and events in whose terms the policy problem is formulated, are the "material" component of the analyst's task.

In the preceding section, I have discussed the significance of the abstract character of the objects of analytic inquiry, and the connection between the tools and the personal, craft judgment of the analyst. Here I shall concentrate on the other two constituents of analysis: the argument (with the important related category of evidence), and the conclusion.

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[21]Aristotle, Ethica Nicomachea, Book VI. The Aristotelian scheme has been used by Ravetz to study the activity of scientific inquiry, and by the Polish praxiological school to analyze the general category of "efficient action". On the praxiological approach see, in particular, T. Kotarbinski, Praxiology, London: Pergamon Press, 1965.

<u>The Argument</u>. The argument represents the link between the material and efficient components of the analysis, and the conclusion. In spite of its crucial importance, surprisingly little has been written on this topic by methodologists of systems analysis (with the notable exception represented by the work of Hermann Kahn). In a careful piece of analytic work, the argument will be a structured set of assertions about the objects of the inquiry. Since the "truth" of a conclusion cannot be proved formally (only its plausibility can be established), the structure of the argument must be a subtle and complex blend of factual statements and subjective evaluations. It will include mathematical and logical deductions as well as statistical, empirical, and analogical inferences. The unavoidable complexity of the argument prevents any direct testing of its adequacy as can be done, for instance, in the case of a mathematical proof or a simple sillogism. Rather, the testing is done by applying, often implicitly, the criteria of adequacy that are accepted in a particular field, or by the particular audience to which the argument is directed.

The adequacy of an analytic argument only in part can be judged according to scientific or professional standards; in fact, the nature of the testing process is more social than logical. This can be seen from the fact that the argument is never addressed to an abstract, "universal" audience, as in the case of purely deductive proofs, but to a particular one (client, decision maker, special interest group, etc.) whose characteristics the analyst must keep constantly in mind if his argument is to carry conviction and affect the course of events. In discussing external pitfalls, I have already mentioned the fallacy of assuming a monolithic decision maker, but the relation between the analyst and his audience(s) is more complicated than is suggested by this single consideration. For, while the analyst must adapt his argument to the audience (and this requires a careful selection of data, methods, and techniques of communication), it is also true that the audience is, to some extent, the creation of the analyst:[22] the structure of the argument and the style of presentation will largely determine the type of audience that can be reached and influenced by the conclusion.

It is interesting to note that two rather typical procedures of systems analysis, the so-called a fortiori and break-even analyses, are essentially techniques of argumentation. The argumentative purpose is, in fact, indicated very clearly in the following quotation:

> "More than any other single thing, the skilled use of a fortiori and break-even analyses separates the professionals from the amateurs. Most analyses should (conceptually) be done in two stages: a first stage to find out what one wants to recommend, and a second stage that *makes the recommendations convincing even to a hostile and disbelieving, but intelligent audience*."[23]

In the construction of an argument, evidence occupies a central position. Although the terms "facts" and "evidence" are often treated as synonimous in common parlance and also in some methodological discussions, a useful distinction can be made in terms of the relevant audience. "Facts" are pieces of (supposedly objective) information presented to an abstract audience of persons who are experts in a given field. Arguments, on the other hand, are directed to particular audiences, for the purpose not of *proving* an assertion, but of *convincing* the audience of the reasonableness or convenience of a proposal. The contemporary fashion for using mathematical formalism at every

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[22] On this point, see C. Perelman and L. Olbrechts — Tyteca, *Traité de l'Argumentation. La Nouvelle Rhétorique*, Paris: Presses Universitaires de France, 1958, Part I, sec. 5.

[23] H. Kahn and I. Mann, *Techniques of Systems Analysis*, Santa Monica, Calif.: The Rand Corporation, RM-1829, December 1956. Italics mine.

possible point of an argument, tends to blur this distinction as it induces a tendency to accept statistical information as facts, rather than evidence.

The category of evidence is most easily recognized in fields where problems involve both complex arguments and large masses of information, and where the reliability and relevance of the information cannot be easily assessed by standard methods. This is a common situation in systems analysis but also, for instance, in the law, where there is a highly developed "law of evidence" for the presentation and testing of information offered as evidence in court cases. By way of contrast, in the natural sciences one usually has either a large mass of information with a relatively simple argument, or a complex theoretical argument needing evidence at only a few points. Hence, neither descriptive nor theoretical natural sciences generally require highly developed skills in testing evidence beyond the standard tests for reliability and relevance already involved in producing information.

The assessment of the strength and fit of the evidence is considerably more complicated than judgments about the validity and reliability of data. For this reason, there often arise disputes about the adequacy of a proposed solution of a policy problem, which cannot be settled either by an examination of the data and information, nor by an appeal to accepted criteria of adequacy. Such situations seem to justify a certain skepticism in the ability of systems analysis to provide concrete help to the decision maker. It should be noted, however, that even in the field of "pure" science, this aspect of the objectivity of scientific knowledge, which is really a result of a successful social tradition of producing and testing the materials of that knowledge, breaks down more often than the outside observer usually assumes".[24]

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[24]See, for instance, J.R. Ravetz, Scientific Knowledge and its Social Problems, cit., ch. 4.

The Conclusion. The conclusion of a policy study is not concerned with "things themselves", but with those intellectually constructed concepts and categories which can serve as the objects of an argument. The contact with the external world of economic, social, and political phenomena is always indirect. Of course, the analyst tries to probe as deeply as possible into that sector of social reality with which he is concerned; but the reports of that contact do no more than serve as the basis for evidence which is embedded in an argument whose objective validity can never be formally established. A different conceptualization of that reality, different tools, a few different personal judgments made at crucial points of the analysis, can always lead to radically different conclusions. This is unavoidable in any form of intellectual inquiry, including that of the natural scientist. Moreover, as Quade points out, it is impossible to verify whether or not the decision-maker made a right decision based on an analysis. One cannot be judged by what actually happens, for there are always circumstances beyond his control. Even when social experiments can be carried out, which is seldom, definite conclusions can hardly be expected. Not only because of the possibility that the experiment may not be properly designed or analyzed but, more significantly, because a policy embodies a large number of hypotheses; a negative result will constitute evidence against some of them, and it is usually very difficult to determine exactly which hypotheses are being contradicted by the experience.

Some important consequences follow from the difficulty of verifying the conclusions of an analytic argument. E.g., less reliance should be placed on evaluation of actual outcomes, and more on the critical analysis of the structure of the argument, on the validity and relevance of the underlying theories, methods, and models. I realize that this proposal goes against the behavioristic assumptions prevailing in the burgeoning field of evaluation research.[25]

[25] For a useful survey of the field, see C.H. Weiss, Evaluation Research, Englewood Cliffs, N.J.: Prentice Hall. 1972.

Yet, given the artificial character of the objects of analysis (as, indeed, of any form of disciplined intellectual inquiry), and the enormous complexity of policies, "verification by theory" appears to be a more promising approach to evaluation, than one based primarily on the statistical treatment of doubtful (and costly) reports of policy outcomes. This thesis finds encouraging corroboration in similar views expressed by people actively engaged in scientific research. Thus, in his perceptive discussion of cloud seeding experiments, Myron Tribus writes:

> "The infrequency of "seedable" hurricanes, when taken in conjunction with the very high cost of conducting hurricane modification missions .... limit very seriously our ability to run a large number of blind experiments for the sake of providing a statistical test of a given seeding hypothesis. What then is the alternative? I feel strongly that the reasonable answer is to place primary reliance on theoretical approaches to the hurricane modification problem. It follows that we must develop an improved analytical plan, so that we can make better theoretical use if the information we collect".[26]

An emphasis of the theoretical aspects of evaluation is, probably, more consonant with the training and frame of mind of the systems analyst than a strictly behavioristic approach. It is, therefore, likely that a more active interest of systems analysis in evaluation research requires a prior reorientation of evaluation strategies along the lines suggested here.

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[26] M. Tribus, "Physical View of Cloud Seeding", Science, 168, 10 April 1970, pp. 201-211.

Social Aspects of Systems Analysis

Note. This part of the paper is still in the phase of gestation. In the following pages, I limit myself to an indication of the topics to be treated in connection with the social aspects of systems analysis.

Methods. I have already discussed some of the judgments which are necessarily involved in the analysis of policy problems, beginning with the assessment of a problem situation, and the basic judgment of the soundness of a set of data. These individual acts of judgment do not derive solely from private intuitions of the analyst; rather they are based on a body of principles and precepts, social in their origin and transmission, without which no analytic work can be done. I propose to use the term "methods" for such principles and precepts which (through their interpretation and application in particular situations) guide and control analysis. Methods cannot be established "scientifically", partly because there is no simple test of the correctness of a particular method, and even more because the principles and precepts are incapable of fully explicit statement. Hence, the testing, criticism, and improvement of the methods of systems analysis must proceed by means quite different from those applicable to specific analytic results. In this aspect of the inquiry, the character of the community engaged in analytic work is thus crucial for the nature and quality of its achievement.

Adequacy. A policy problem carries with it no guarantee that there exists a "correct" solution against which the results actually achieved can be tested. The analyst can offer no more than "adequate" solutions; and the criteria of adequacy are set (at present, in a very imperfect and fragmentary form) by the analytic community, and by the audience to which the proposed solution is addressed. An example of the necessity of judgments of adequacy appears in the discussion of the "soundness" of data, and of the reliability and relevance of information. In general, imposed criteria of adequacy are necessary because of the inconclusiveness of the arguments used in analysis.

We can distinguish two sorts of criteria of adequacy: those relating to the argument (e.g. clarity, level of rigor, appropriatness for the intended audience), and those relating to the evidence. The latter are more varied and subtle; for they control not only the conditions of the production of data and information, but also the strength and fit of the evidence in its particular context.

The judgments of adequacy perform the same function in analytic (or scientific) inquiry as the tests of quality control in industry. Thus, in the work of bringing a field toward maturity, an important part lies in the strengthening of the criteria of adequacy.

Quality Control. The problem of introducing suitable social mechanisms of quality control for the results of systems analysis, as far as I know, has never been explicitly discussed. Yet, it is of crucial importance for the growth and general acceptance of the discipline. Much can be learned by examining the nature and effectiveness of such mechanisms in industry, in the professions, and in science. Although the types of control used, differ greatly in these three fields, the nature of the task is essentially the same. Generally speaking, the function of quality control is to ensure that the users of a product can rely on its being of an acceptable standard. The task is divided into several phases: establishment of criteria of quality and setting standards in their terms; testing the set of products for an assessment of their meeting the standards; and enforcing the regular adherence to the standards by a system of penalties and rewards.

The greatest rigidity and formality of quality control procedures, which closely follow the hierarchical structure of the organization, is to be found in industry. In science, by contrast, the situation is (or appears to be) one of "happy anarchy". There is no formal hierarchy of decision and control, and the scientific community has no formal institutions for punishing or expelling a wayward member. There is only one point where formal procedures of quality control in science operate: the assessment of a research paper by the referees of a recognized journal. But for all its informality, the system is (or has been until very recently) extremely effective. Quality control procedures for the professions occupy an intermediate position.

The assessment and enforcement of quality in decision-oriented studies is very complicated and cannot be completely reduced to any of the three cases examined above. Much thought should be devoted to the possible contributions of the State-of-the-Art Series in this direction.

Impact of Institutional Arrangements

It is clear that alternative institutional arrangements can have vastly different consequences for the acceptance and implementation of analysis. Here, too, our knowledge is very scanty. Scattered references can be found in the literature of organization theory, as well as in OR and Management Science papers. These contributions should be examined and compared, even though their quality leaves much to be desired. One general criticism is that these studies lack an adequate understanding of the analytic process, so that it is not always clear whether a given result is really due to the institutional arrangement or, perhaps, to the quality of the analysis.