# WORKING PAPER

THE CRAFT OF APPLIED SYSTEMS ANALYSIS

Giandomenico Majone

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IIASA
International Institute
for Applied Systems Analysis

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## FOREWORD

The central goal of the International Institute for Applied Systems Analysis is to apply the craft of systems analysis to important national and international problems.

To support and improve this work, the Institute explores its philosophical and scientific foundations, as well as the lessons of practice.

This paper focuses its attention on the central conceptual issues of the field: the scientific nature of applied systems analysis, the search for standards of quality for it, its relation to problem solving, the craft aspects of the work, and the relation between argument and conclusion.

The author has contributed significantly to clarifying foundational conceptions of applied systems analysis in other papers as well. Of these contributions, two related to this paper deserve mention here: G. Majone and E.S. Quade, editors, Pitfalls of Analysis (London: Wiley, 1980), a volume in the International Series on Applied Systems Analysis; and G. Majone, "Policies as Theories," issued by IIASA as RR-80-17 (originally published in Omega, 8, 1980, pp. 151-162).

Other papers dealing with the craft of systems analysis are in preparation.

Hugh J. Miser
Survey Project

CONTENTS

1. "How Scientific is Applied Systems Analysis?"

Like the legendary phoenix, the question: How scientific is sytems analysis? (or operations research, or management science) keeps rising alive from the ashes of past methodological debates and official 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" occurred 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, hopefully, 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]

Can anything of value be learned from these methodological discussions, anything, that is, that is useful to applied systems analysis (ASA) as it is practiced today? In this section I shall try to show that questions about the "scientific character" of ASA are, today, rather irrelevant, when not positively misleading, if taken literally; but, also, that they can be reformulated in a way that makes them highly meaningful for the practicing analyst.

One problem with the older methodological discussions about the scientific nature of ASA is that, 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 epistemology finds unacceptable, or at least in need of substantial revisions. Few scientists and philosophers of science still believe that scientific knowledge is, or can be, proven knowledge. If there is one point on which all major schools of thought agree today, it is that scientific knowledge is always tentative and open to refutation. And while the older history of science was little more than a chronicle of the irresistible advance of the different sciences, the contemporary historian tries to understand "how such sciences can succeed in fulfilling their actual explanatory missions, despite the fact that, at any chosen moment in time, their intellectual contents are marked by logical gaps, incoherences, and contradictions."[3]

However, the conceptual revolution that has taken place in the philosophy and history of science in the last three decades--a revolution commonly associated with the names of Popper, Kuhn, and Lakatos--is having its impact on systems analysis and closely-related disciplines, as shown by some recent contributions to the literature.[4] But even these methodologically more sophisticated and updated discussions often fail to explain what lies behind the persistent preoccupation with the scientific status of ASA.

It is, of course, no secret that the claim to scientific status has in the past served an important ideological function by increasing the collective confidence of a group of new disciplines striving for academic and social recognition. But today science (or, rather, folk-science) has lost much of its ideological appeal, and it would be difficult to explain the scientific aspirations of the ASA profession on such grounds. Also, fallibilism--the currently accepted doctrine that scientific arguments are never conclusive and always perfectible--seems to be a poor principle from which to derive mechanical rules of method. Finally, traditional claims to scientific status for ASA have always been faced by what appears to be an insoluble contradiction: if ASA is scientific, its task is not to prescribe or suggest a course of action, but to provide scientific explanations and predictions; if, on the other hand, ASA aspires to guide action, it must be prescriptive (and, I shall argue, persuasive as well), and hence cannot be scientific--not, at any rate, according to the received view of scientific method. Some writers have attempted to solve the dilemma by arguing that ASA offers "scientifically based" advice. But such an argument is logically unsound and runs immediately against the Humean impossibility of deriving "ought" from "is."

So the question about the scientific status of ASA does indeed seem to lead nowhere, except into a thicket of conceptual obscurities and logical dilemmas. But then, why do methodologically conscious analysts keep raising it? The reason, I suggest, is that behind this question loom two issues which people rightly feel to be of crucial importance, even if they are unable to clearly articulate them. First: what is the language of ASA, i.e., what is the logical status of the different propositions which an analyst produces in the course of his work? Second: which standards of quality and rules of methodological criticism are applicable to the different kinds of propositions?

## 2. The Search for Standards of Quality

Even if we interpret "science" in the broadest possible sense of an organized body of knowledge (the sense suggested, for example, by the German term "Wissenschaft"), the existence of generally recognized rules of evaluation and criticism is a necessary precondition for any reasonable claim to scientific status. Only in immature fields of inquiry, as Ravetz has pointed out, criteria of quality or adequacy cannot be taken for granted.

The dilemmas facing the leaders of an immature field of inquiry have been shrewdly analyzed by Ravetz:

> The present social institutions of science, and of learning in general, impose such constraints that the growth and even the survival of an immature field would be endangered by the simple honesty of public announcement of its condition. For these institutions were developed around mature or rapidly maturing fields in the nineteenth century. If the representatives of a discipline announce that they do not fit in with such a system, they can be simply excluded from it, to the benefit of their competitors for the perennially limited

> resources. The field would be relegated to amateur status, and thereby pushed over to the very margin of the world of learning; it would be deprived of funds and prestige.[5]

He continues:

> An immature field, in chaos internally, experiences the additional strains of hypertrophy; its leaders and practitioners are exposed to the temptations of being accepted as consultants and experts for the rapid solution of urgent practical problems. The field can soon become identical in outward appearance to an established physical technology, but in reality be a gigantic confidence-game. . . To thread one's way through these pitfalls, making a genuine contribution both to scientific knowledge and to the welfare of society, requires a combination of knowledge and understanding in so many different areas of experience, that its only correct title is wisdom.[6]

What, then, does the "wisdom" of ASA include (assuming, as I think we must, that it is a still-maturing field)? ASA is concerned with theorizing (at different levels of generality), choosing, and acting. Hence its three-fold character: descriptive (scientific), prescriptive (advisory), and persuasive (argumentative-interactive). In fact, if we look at the fine structure of an analytic argument we usually discover a complex blend of factual statements, methodological choices, evaluations, recommendations, and persuasive definitions and communication acts. An even more complex structure would become apparent if we were to include (as we should, in a complete treatment) the interactions taking place between analysts and their audience of clients, sponsors, policy makers, and interested publics. Moreover, descriptive propositions, prescriptions, and persuasion are intertwined in a way that rules out the possibility of applying a unique set of evaluative criteria, let alone conclusively proving or refuting an argument. Whatever testing can be done must rely on a variety of disciplinary standards, corresponding to the different techniques and methods used in the study, on the plausibility and robustness of the results, on the quality criteria of the clients, and even on such hard-to-formalize qualities as style and persuasiveness. For this reason, the historical pattern of development of ASA can be seen as "the slow business of getting to grips with the problems of devising patterns of criticism, of constructing critical methodologies, for those areas not readily dealt with by the methods built up over so long a period in the natural sciences."[7]

But why has the analytic profession been so slow in recognizing the importance and the necessary complexity of a relevant body of criteria and mechanisms of quality control? The reasons are, to a large extent, historical. The pioneers of systems analysis and operations research were natural scientists, many of them of outstanding ability, with a long experience in the actual conduct of empirical research. Their most important contributions to the new fields of inquiry were not advanced theoretical insights or sophisticated research tools, but active minds, and a set of superb craft skills in recording, analyzing, and evaluating data, in establishing quantitative relationships, and in setting up testable hypotheses. Their main goal, as they saw it, was "to find a scientific explanation of the facts." For, as C.H. Waddington writes, "[o]nly when this is done can the two main objects of operational research be attained. These are the prediction of the effects of new weapons and of new tactics."[8]

Given this paradigm, the relevant standards of criticism were, of course, those of natural science. Indeed, the situations investigated by operations researchers during World War 2 were particularly well suited for this approach. Typically, military operations could be regarded, without distortion, as representative of a class of repetitive situations "where theories built up in response to earlier examples of the situation could be checked out against later examples, monitored while proposals for improved action were in use, and used to detect their own dwindling validity as the situations changed."[9] In the years immediately following the War, industrial applications of OR, exemplified by L. C. Edie's classic study "Traffic delays at toll booths,"[10] still followed the standard pattern, and explicitly appealed to the established criteria of evaluation and criticism.

But soon the situation began to change. While people like Blackett and Waddington were returning to their laboratories and research institutes, a new generation of analysts was entering the scene -- people primarily interested in the more formal aspects of scientific research, and often lacking the craft skills and the maturity of critical judgment of the old masters. At the same time, the problems claiming the attention of the analysts were becoming increasingly abstract and complex. Direct empirical verification of the conclusion was often impossible (as in the case of the strategic studies done at Rand), and the very notion of solution, except in the simplest situations, tended to become a matter of methodological agreement. In sum, as allegiance to the traditional standards of criticism was weakened by changes in the disciplinary background of the profession, the standards themselves were becoming increasingly irrelevant to current professional practice.

3. Systems Analysis and Problem Solving

And yet, the pioneers of OR were correct in asserting the existence of a strong similarity between operational and scientific research. Their mistake, from our present viewpoint, consisted in thinking that the similarity was to be found in the outcome ("scientific explanation of the facts," "prediction of the effects of new weapons and of new tactics"), rather than in the process, that is to say, in the basic craft aspects common to all types of disciplined intellectual inquiry. Shifting our perspective from outcome to process, the following points become almost obvious:[11]

1. Like scientific research, ASA is essentially a craft activity, or, as some authors prefer to put it, an "art;"[11a]
2. However, the objects to which analytic work is applied are not physical things and phenomena, as in the case of 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 criteria which are mainly informal and tacit, rather than public and explicit. It is the task of a methodology of ASA to make these criteria as explicit as possible, as a preconditon for a critical discussion of their validity;
4. The standards and criteria of quality used in evaluating analytic work must reflect the three-fold nature of ASA: descriptive, prescriptive,

and argumentative. These standards are partly technical (reflecting the best practice in the field), and partly social (since their effectiveness depends on the existence of professional organizations and other institutional arrangements).

In the discussion of these theses, the notion of "problem" plays a central role. In fact, ASA can be described as problem solving on intellectually constructed objects; and the different stages of analytic work roughly correspond 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 stages, where the analyst interacts with the external world (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 analyst's work; the abstractness 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. Perhaps the first thing to be noted in our characterization of ASA as problem solving is the difficulty of finding explicit criteria by which scientific problems may be distinguished from policy problems. Consider, for instance, the characterization of scientific problems that has been proposed by Ravetz:[12] 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 for "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 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."[13] 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."[14]

Or see what Eilon 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."[15]

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 only sufficient resources are available. 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."[16]

Thus, Ravetz's criteria do not allow one to separate policy problems sharply from scientific problems. The similarity 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 it 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 whose concrete correspondents are fairly obvious.[17] 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 is equally true for concepts like "poverty," "health," "unemployment," "crime," which acquire operational meaning only when expressed in terms of legal or administrative definitions. Indeed, as Alan Coddington has observed, "economic statistics are extremely abstract things," the product of "arbitrariness" and "convention."[18]

The same holds true, a fortiori, 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 a "problem situation." This is 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."[19]

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.

## 4. Applied Systems 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 ASA data are often "found" rather than "manufactured," i.e., they are produced by observation rather than by experiment. This requires craft skills that are rather different, and in many respects more difficult to acquire, than those needed 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 results 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 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, 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 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.[20]

Information. At least in quantitative terms, an excess, rather than a scarcity, of data is the usual situation in ASA. 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 crucial 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 for 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, which is of special importance here, includes "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 craftsman'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 metaphysics, 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.[21]

It is important to realize that the influence of tools on a field is more subtle than a mere opening up 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 ongoing 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 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."[22]

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 applications 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[23] 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 commitments to a given solution. Another common pitfall is a misplaced pragmatism which suggests "getting 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 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:"

> Analyses 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.[24]

In mature 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.[25] Systems analysts have up to now followed 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.

## 5. The Components of Analysis

Having described the activity of the applied 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 craftsman's task can be adapted to our present purposes.[26] 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 analysis of a complex situation and the presentation of proposals. 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, and perhaps, difficulties and pitfalls encountered and overcome. Finally, the intellectual constructs and the data 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 judgments 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.

The Argument. 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. The three-fold nature of the language of ASA (descriptive, prescriptive, argumentative) is reflected in the complex structure of an analytic argument, which will typically include mathematical and logical deductions, statistical, empirical, and analogical inferences, as well as evaluations and recommendations. This 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 syllogism. 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:[27] 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.[28]

In the construction of an argument, evidence occupies a central position. Although the terms "facts" and "evidence" are often treated as synonymous 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 audience of persons who are experts in a given field. Evidence, on the other hand, is information embedded in an argument, for the purpose not so much of proving an assertion, but rather of convincing the audience of the reasonableness or convenience of a proposal. The contemporary fashion for using mathematical formalism at every possible point of an argument tends to blur this distinction, as it induces a tendency to accept statistical and other kinds of 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 ASA but also in other fields like law, where there is a highly developed "law of evidence" for the presentation and testing of information offered as evidence in court cases. In the natural sciences, on the other hand, 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.[29]

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 of 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.

The Conclusion. The conclusion of a policy study is not concerned with "things themselves," but with the intellectually constructed concepts and categories that 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 the part of social reality with which he is concerned; but his assessment of the problem situation can only 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, it is usually impossible to verify whether or not the decision maker made a right decision based on the 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.

In sum, we are faced here with a situation that arises in many contexts in which some form of evaluation takes place. The natural tendency is to evaluate an activity by the results it produces. This is not only an intuitively appealing, but also a reasonable approach -- provided that reasonably objective criteria of evaluation exist. In such a case knowledge of the process producing the outcomes to be evaluated is largely immaterial -- only results count. A car buyer is not usually concerned about the internal organization of the producing firm. But when the factual and value premises of the evaluation are moot, when no objective criterion for what is a correct decision or a good outcome exists, then the process or procedure by which the results are obtained acquires special significance. This is the basic reason why procedural questions become so important in legislative and judicial decision making.[31]

For the reasons stated above, the conclusions of an analytic study can seldom be validated or refuted unambiguously. Hence, evaluation by results is either impossible or unfair (as when the quality of an analytic study is evaluated exclusively in terms of the actual success or failure of its conclusions and recommendations -- too many factors outside the analyst's control determine the success or failure of a policy). Evaluation by process becomes unavoidable, and in this context the notion of craft and craft skills plays a crucial role.

6. Reprise

Let me recapitulate the preceding discussion, and state my main conclusions in the form of theses.

1. Evaluation and quality control are the main methodological issues facing ASA today. These are crucial questions not only for the users of analysis, but for the producers as well. A profession or craft may be said to exist to the extent that there are generally accepted standards of quality and criteria of criticism.

2. The question: "How scientific is ASA?" can be made more meaningful by reinterpreting it in terms of two other questions: a) What is the language of ASA, i.e., what is the logical status of the different propositions which analysts produce in the course of their work?; and b) which standards of quality and rules of criticism are relevant to the different kinds of propositions?

3. ASA deals with three kinds of activity: theorizing, choosing, and acting. Hence its language includes propositions of three different sorts: descriptive (scientific), prescriptive (advisory), and persuasive (argumentative-interactive). This complex mixture of description, prescription, and persuasion makes it impossible to apply a unique set of evaluative standards.

4. The practice of the pioneers of ASA was better than their theory. They correctly sensed the existence of deep similarities between their activity as operations researchers

and their previous activities as scientists. Their mistake, from our present perspective, was to look for the similarities in the outcomes of that research (explanations, predictions), rather than in its process.

5. The systems analyst as craftsman goes through essentially the same operations that the scientist performs; and both scientist and analyst replicate on an abstract conceptual level what the traditional craftsman or artisan does with material objects and physical tools. The artisan applies his tools to certain materials in order to produce an object fulfilling a given function. The intellectual craftsman (analyst or scientist) works on abstract materials (data, concepts, theories) using different tools and methods (mathematical, logical, "hardware") in order to produce an argument supporting certain conclusions and/or recommendations.

6. This anatomy of the task of the applied systems analyst as craftsman clearly reveals the three features discussed above: descriptive (data, empirical statements), prescriptive (recommendations, proposals, but also methodological choices guided by craft judgments), and persuasive (communication of recommendations, style of presentation, but also problem formulation and definitions).

7. The notion of craft is intimately related to that of quality standards. Indeed, the main function of the master craftsman (and also, to some extent, of the patrons and connoisseurs of the craft) is creating standards of quality for the other practitioners. These standards usually remain inarticulate (they are taught more by example than by preaching), but

they are nonetheless quite effective in guiding and controlling the work of the craftsmen. Scientific leaders fulfill similar functions for their disciplines, with the support of institutional mechanisms like professional organizations, refereed journals, and academies.

8. The search for relevant standards of quality in ASA has been impeded by misconceptions about the nature of the "scientific method" -- by the assumption that analytic work could be judged in terms of results, of "correct" explanations and successful predictions. But the profession is beginning to understand that quality control (in the case of ASA as of all other intellectual activities) is intimately related to a sophisticated understanding of process.

9. ASA is still quite young as an intellectual craft, and quality criteria develop very slowly. We still lack a sufficient body of first-class studies from which relevant criteria could be distilled. But there is enough experience, by now, to suggest at least minimal criteria of adequacy. Such criteria can be derived by studying the most serious conceptual errors into which analysts occasionally fall (in collecting and analyzing data, in choosing tools, in drawing conclusions, and in communicating them). Thus, by identifying the pitfalls of analysis and charting safe paths around them, we are slowly building a solid foundation on which subtler criteria of quality can be based.

## NOTES

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.

3. S. Toulmin, "The Structure of Scientific Theories" in The Structure of Scientific Theories, (ed. F. Suppe), Urbana, Illinois, University of Illinois Press, 1974, p. 605.

4. See, for example, S. Eilon, "How Scientific is O.R.?" Omega, vol. 3, no. 1, 1975. pp. 1-8; R.G. Bevan, "The Language of Operational Research," Operational Research Quarterly, vol. 27, no. 2, 1976, pp. 305-13; T.W. Hutchison, Knowledge and Ignorance in Economics, Oxford: Basil Blackwell, 1977; G. Majone, "Policies as Theories," Omega, vol. 8, 1980, pp. 151-162; and especially H. Boothroyd, Articulate Intervention, London: Taylor and Francis, 1978.

5. J.R. Ravetz, Scientific Knowledge and its Social Problems, Harmondsworth: Penguin Books, 1971, p. 378.

6. Ibid., p. 400.

7. Boothroyd, op.cit., p. 115.

8. C.H. Waddington, O.R. in World War 2, London: ELEK Science Ltd., 1973, p. 26.

9. Boothroyd, cit., p. 113.

10. L. C. Edie, "Traffic Delays at Toll Booths," Operations Research, vol. 2, 1954, pp. 107-138.

11. Cf. J. R. Ravetz, Scientific Knowledge and its Social Problems, cit., 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's important contribution to the philosophy of science. It should be noted that although my discussion owes much to Ravetz's ideas, it differs at a number of important points from his; for instance, in the characterization of policy problems ("practical problems" in his terminology).

11a. 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.

12. J.R. Ravetz, Scientific Knowledge and its Social Problems, cit., p. 72.

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

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

15. S. Eilon, "How Scientific is OR?" cit., p.8.

16. 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.

17. 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.

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

19. E.S. Quade, Analysis for Public Decisions, cit., p. 306.

20. J. R. Ravetz, op.cit., p. 1.

21. 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.

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

23. E.S. Quade, Analysis for Public Decisions, cit., pp. 300-317. For an extensive discussion of this topic, see G. Majone and E.S. Quade, editors, Pitfalls of Analysis, London: Wiley, 1980.

24. E.S. Quade, Analysis for Public Decisions, cit., pp. 314-315.

25. Cf. J.R. Ravetz, Scientific Knowledge and its Social Problems, cit., p. 97.

26. 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.

27. On this point, see C. Perelman and L. Olbrechts-Tyteca, *Traité de l'Argumentation. La Nouvelle Rhetorique*, Paris: Presses Universitaires de France, 1958, Part I, sec. 5.

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

29. J.R. Ravetz, *Scientific Knowledge and its Social Problems*, cit., ch. 4.

30. Ibid.

31. E. Barker: *Reflections on Government*, New York: Oxford University Press, 1958; N. Luhmann, *Legitimation durch Verfahren*, Neuwied: Luchterhand, 1975.