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HANDBOOK OF SYSTEMS ANALYSIS
VOLUME 1. OVERVIEW
CHAPTER 2. THE GENESIS OF APPLIED
SYSTEMS ANALYSIS

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FOREWORD

The International Institute for Applied Systems Analysis is preparing a Handbook of Systems Analysis, which will appear in three volumes:

● Volume 1: Overview is aimed at a widely varied audience of producers and users of systems analysis studies.

● Volume 2: Methods is aimed at systems analysts and other members of systems analysis teams who need basic knowledge of methods in which they are not expert; this volume contains introductory overviews of such methods.

● Volume 3: Cases contains descriptions of actual systems analyses that illustrate the diversity of the contexts and methods of systems analysis.

Drafts of the material for Volume 1 are being widely circulated for comment and suggested improvement. This Working Paper is the current draft of Chapter 2. Correspondence is invited.

Volume 1 will consist of the following ten chapters:

1. The context, nature, and use of systems analysis
2. The genesis of applied systems analysis
3. Examples of applied systems analysis
4. The methods of applied systems analysis: An introduction and overview
5. Formulating problems for systems analysis
6. Objectives, constraints, and alternatives
7. Predicting the consequences: Models and modeling
8. Guidance for decision
9. Implementation
10. The practice of applied systems analysis

To these ten chapters will be added a glossary of systems analysis terms and a bibliography of basic works in the field.

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CHAPTER 2. THE GENESIS OF APPLIED SYSTEMS ANALYSIS

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1. CHANGE AND CONTINUITY

An adequate account of a field of inquiry should be capable of explaining its continuities as well as its changes—possibly in terms of the same underlying process. Considered over a sufficiently long period of time, a discipline like physics changes quite radically in its objects of inquiry, its methods, and its aims. Yet, despite such changes, the discipline maintains a recognizable continuity; less because of a common professional commitment to a central core of principles or key questions, than because the problems on which successive generations of physicists have focused their attention are connected by recognizable lines of descent. These problems form, to use Toulmin's expression, a "genealogy" of issues and of related concepts and tools.¹

Similarly, the development of applied systems analysis (ASA) over the last forty or so years reveals considerable changes in intellectual contents, methods, and aims. The tactical problems that formed the main objects of inquiry of operations research (OR) during World War 2 have been followed by the strategic problems investigated by defense analysts in the 1950s and 1960s. Today's pol-

¹Stephen Toulmin, *Human Understanding*, vol. 1. Princeton, New Jersey: Princeton University Press, 1972, pp.134-144.

icy analysts focus on social and economic problems: regulation and pollution control, energy and education, housing and health care. The accompanying changes in methods have been equally striking: from the relatively simple data analyses and differential equations of the early military applications to the static and dynamic optimization models of contemporary OR, to the econometric models of policy analysis. Aims have also changed. If the goal of the first analysts of military operations was essentially empirical—to give a scientific explanation of the facts, and to make successful predictions of the effectiveness of new weapons and new tactics, that of the systems and policy analysts is primarily prescriptive—to assist the decisionmaker in choosing among alternative courses of action. And we are now beginning to recognize a third legitimate function or aim for ASA, as a vehicle of persuasion and argumentation in the policy debate.

The question immediately facing the historian of ASA is whether an underlying continuity can be detected below these changes in problems, methods, and disciplinary aims. Or should one rather speak of mutations that have altered in fundamental ways the original enterprise? A good argument could be made in favor of the mutation hypothesis; yet the weight of the evidence favors the hypothesis of continuity, as I shall try to show. The difficulties of the proof should not be underestimated, however. In mature disciplines like physics or mathematics, essential continuity is maintained by the joint operation of a dual process of intellectual innovation and critical evaluation and selection. The pool of available theories and methods is continually enriched by intellectual novelties, but only a few of the novelties survive the severe tests to which they are exposed. In this way disciplinary identity can be maintained over considerable periods of time. But in order for this dual process of innovation and selection to work satisfactorily, there must be professional "forums of competition" (Toulmin) within which new ideas can survive long enough to show their merits and defects, but in which they are also criticized and eliminated with enough sever-

ity to maintain the coherence of the discipline.²

By contrast, ASA is still a maturing field in which the rate of intellectual innovation is much greater than the rate of critical selection. Hence a proliferation of approaches and "schools" that seem to have little in common. And because of the fragility of the existing mechanisms of quality control, the survival or rejection of intellectual novelties seems to depend more on academic fashion and external support than on a sober assessment of their potentialities—as shown by the examples of game theory, value theory, or program budgeting.

The example of program budgeting suggests another important reason why the evolutionary model of "conjectures and refutations" is so much more complex in the case of ASA than in the traditional academic disciplines. ASA is a form (indeed, the main form) of articulate intervention into ongoing action programs.³ This means that the conceptual innovations proposed by systems analysts will be evaluated not only by the canons of disciplinary criticism, but also according to criteria of social effectiveness. New proposals must fit into a certain intellectual tradition or research program (like all conceptual novelties), must also be adapted to, and adopted by, an ongoing social process or action program (a problem which theoretical innovations do not have to face). Depending on time and circumstances, one or the other criterion—professional quality or social effectiveness—may prevail; but in the long run, it is doubtful that an analytic proposal can survive without meeting some minimal standards of adequacy along both dimensions.

I have already referred to Toulmin's "genealogy of problems" as the element by which a field of intellectual inquiry preserves its disciplinary identity. We can see now that in the case of ASA we should rather speak of a lattice of descendant problems,⁴ to signify the fact that the problems of systems analysis do

²Stephen Toulmin, *Human Understanding*, cit., ch. 1.

³Hylton Boothroyd, *Articulate Intervention*, London: Taylor and Francis, 1978. See also, Giandomenico Majone, "Policies as Theories," *Omega*, vol. 8, no. 2, pp.151-162.

⁴I borrow this expression from J.R. Ravetz; see his *Scientific Knowledge and its Social Problems*, Harmondsworth, England: Penguin Books, 1973, especially pp.191-199.

not develop along disciplinary (or even interdisciplinary) lines only, but inevitably mix with problems derived from political, social, and institutional sources.

In our reconstruction we shall also have to bear constantly in mind that, like any other historically developing intellectual enterprise, ASA has two aspects. We can think of it as a (composite) discipline comprising, at any given point in time, a stock of theories, conceptual frameworks, and techniques for dealing with theoretical and practical problems; or we can view it as a profession comprising a set of institutions, roles, and people whose business it is to apply and improve these methods and techniques. Hence our account of the evolution of ASA falls into two parts, one dealing with disciplinary developments (Sections 2-4), the other with institutional and professional developments (Sections 5 and 6). Each part, by itself, gives an inadequate and distorted view of the field. A purely intellectual history of methodological developments cannot explain, for example, why OR developed along quite different lines, after the War, in the United States and in Britain or Canada (or why, for that matter, industrial engineering had not developed into something like operations research already in the 1930s). On the other hand, a study of professional organizations, roles (in industry, government, and the universities), and institutional mechanisms of evaluation and control (journals, conferences, policy research institutes) has more than sociological interest only if it is related to the historically developing cognitive basis of ASA.

2. FROM OPERATIONS RESEARCH TO SYSTEMS ANALYSIS

P.M.S. Blackett, the Nobel-prize-winning British physicist who was a leader of the early OR work, wrote two short but influential memoranda toward the end of 1941: "Scientists at the Operational Level" (written in order to inform the Admiralty of developments that had taken place in the Operational Research Sections already established at different Commands of the Royal Air Force), and "A Note on Certain Aspects of the Methodology of Operational Research" ("an attempt to set out, for the benefit of new scientific recruits to the operational

research sections, some of the principles that had been found to underlie the work of the first two years of the war"⁵). Together with another paper written by the same author a few years after the end of the War, "The Scope of Operational Research,"⁶ these notes represent not only some of the earliest, but also some of the clearest and most insightful discussions of the principles of OR as practiced during the 1940s.

The first step in the establishment of a sphere of professional autonomy is a claim to "cognitive exclusiveness" over some portion of reality.⁷ Consequently, Blackett takes great pains to differentiate the functions of the operations analysts from those of their closest potential competitors, technical services on the one hand, and operational staffs, on the other:

The object of having scientists in close touch with operations is to enable operational staffs to obtain scientific advice on those matters which are not handled by the service technical establishments.

Operational staffs provide the scientists with the operational outlook and data. The scientists apply scientific methods of analysis to these data, and are thus able to give useful advice.

The main field of their activity is clearly the analysis of actual operations, using as data the material to be found in an operation room...

It will be noted that these data are not, and on secrecy grounds cannot, in general, be made available to the technical establishments. Thus scientific analysis if done at all, must be done in or near operation rooms.⁸

For example, weapon A is calculated by the technical department of a service to be 50 percent more efficient than weapon B. In actual operations, over a

⁵The two memoranda, the second one reproduced in a text dated from May 1943, can now be found in P.M.S. Blackett, *Studies of War*, New York: Hill and Wang, 1962, pp.169-198.

⁶*Operational Research Quarterly*, vol. 1, no. 1, 1950; now in *Studies of War*, cit., pp.199-204.

⁷Magali Sarfatti Larson, *The Rise of Professionalism*, Berkeley and Los Angeles: University of California Press, 1977.

given period of time, B scores 4 successes, while A scores only 2. Is this sufficient evidence to reject the estimates of the technical department and proceed as if B were the better weapon? Here the role of the operations analyst is similar to that of the statistician facing a standard problem in statistical inference. His task is to try to reject the null hypothesis—represented by the estimates of the technical department.

As another typical example of operations analysis, Blackett considers the problem of discovering the best use, under actual operating conditions, of a new device. Operations researchers can perform a useful function here by interpreting the "operational facts of life" to technical people, and technical possibilities to the operational staff; i.e., by operating in a liaison capacity between the operational staff, the technical department that produced the device, and the development unit.

Particularly in times of war, the demand for new weapons and technical gadgets tends to become overwhelming. But, Blackett points out, relatively too much scientific effort is expended in the production of new devices and too little in the proper use of what is already available. Hence, another important task of operations research consists in providing numerical estimates of the value of changing over from one device to another, by investigation of the actual performance of existing systems, and by analysis of the likely performance of new ones. Incidentally, it will be noted how clearly Blackett prefigures here the future development of cost-benefit and cost-effectiveness analysis which was to play such a large role in systems analysis in the 1950s and 1960s.

Having established a sphere of autonomy for a problem-solving approach that is neither purely technical, nor exclusively operational, but partakes of both functions, Blackett goes on to raise three methodological questions about OR: Is it scientific? Is it new? If so, in what ways?

Now, if one accepts the usual characterization of operations research as the application of the scientific method to the study of operations, then the answer

⁹P.M.S. Blackett, op. cit., p.171.

to the first question must be "yes"—by definition. The trouble with this characterization is that there is no unique "scientific method," least of all in the sense of a set of mechanical rules that would allow one to move safely from data to conclusions. It is true that the pioneers of operations research, men like P.M.S. Blackett, C.H. Waddington, P.M. Morse, G.E. Kimball, and B.O. Koopman were scientists—physicists, biologists, and mathematicians of high caliber. But what they brought to the new field was not a particular "method," or even advanced scientific knowledge, but a new perspective and a set of superb craft skills in examining the available evidence, considering what conclusions could be drawn from it, and deciding what other information was required, and how it could be obtained.⁹

This distinction is important because the view of science as craftsman's work (and it is precisely in this sense that operations research or ASA may be considered scientific, as I have argued at some length elsewhere¹⁰) leads to quite different methodological positions from those suggested by a vulgar-positivistic view of science. A dogmatic interpretation of the nature of scientific method can easily lead to an attitude which John Tukey has recently expressed in the epigram: "We don't want to try to measure anything where we cannot be proud of the measurement process."¹¹ The craftsman, on the other hand, tries to do his best with the materials and tools at his disposal—always keeping in mind Aristotle's dictum that "precision is not to be sought for alike in all discussion, any more than in all the products of the crafts... ." See, for example, what Blackett has to say about the use of rough data in operations research:

No pregnant problem should be left unattended for lack of *exact* numerical data, for often it is found on doing the analysis that *some*

⁹There is an interesting analogy with the take-over in the late 1940s of theoretical biology by men originally trained in physics. The development of molecular biology is essentially due to these "emigré physicists," but as Szilard has emphasized, what these men brought to biology was "not any skills acquired in physics, but rather an attitude: the conviction which few biologists had at the time, that mysteries can be solved"; see S. Toulmin, *Human Understanding*, cit., p.234.

¹⁰Giandomenico Majone, *The Craft of Applied Systems Analysis*, Laxenburg, Austria: IASA, 1980.

¹¹John W. Tukey, "Methodology, and the Statistician's Responsibility for BOTH Accuracy AND Relevance," *Journal of the American Statistical Association*, vol. 74, no. 368, December 1979, pp.786-793, 786.

significant conclusions recommending concrete action can be drawn even with very rough data. In other cases this is, of course, not so. But till the problem is worked out, one cannot tell.

It often happens that when the problem has been worked through in a very rough form, it is found that data which were thought to be important are actually unimportant, and vice versa... . It must always be remembered that the object of the analysis is practical—that is, that it should lead to action. Attempts at undue and unnecessary precision are to be avoided.¹²

Incidentally, the problem of making effective use of rough data is still very much with the policy analyst of today, as shown for example by Frederick Mosteller's insightful paper "Assessing Unknown Numbers: Order of Magnitude Estimation."¹³

Concerning the second and third questions, Blackett argues that operations research has a considerable degree of novelty, but this relative novelty lies "not so much in the material to which the scientific method is applied as in the level at which work is done, in the comparative freedom of the investigators to seek out their own problems, and in the direct relation of the work to the possibilities of executive action."¹⁴

Of these three distinctive features of original OR work, the second one—the comparative freedom of the investigators to seek out their own problems—seems to be the most important. "In fact," Blackett adds, "the most fertile tasks are often found by the [operations research] group themselves rather than given to them. That this is so is only to be expected, since any problem which is clearly recognized by the executives is likely, in an efficient organization, to be already a matter of study."

¹²P.M.S. Blackett, "Operational Research," cit., p.185.

¹³In *Statistics and Public Policy*, W.B. Fairley and F. Mosteller, editors, Reading, Massachusetts: Addison-Wesley Publishing Co., 1977, pp.183-184.

¹⁴P.M.S. Blackett, "Operational Research," cit., p.201.

But if this is so, it is wrong to argue, as A.M. Mood does, that industrial engineers, quality control experts, time-and-motion experts, investment counselors, product packagers, and personnel managers (!) have been doing operations research in industry "for at least a couple of generations."¹⁵

In fact, it seems very doubtful that any of these alleged precursors of OR would meet all the three criteria set down by Blackett. Before the large-scale introduction of operations research methods, most analyses of industrial operations were largely empirical in character. Certainly, they were not carried out in that atmosphere of a "first-class pure scientific research institution" which, according to Blackett, is necessary to the effectiveness of an OR team. And it is also doubtful that the early analysts of industrial operations had the freedom to seek out their own problems, being usually constrained by the specific research tasks assigned to them by management. As already noted, social and institutional factors were probably responsible for the fact that industrial engineering and "scientific management" did not actually evolve into genuine OR work, as the term is understood today—despite some remarkable initial successes and the efforts of people like Frederick Taylor and his favorite disciple, Morris Cooke, to pull the industrial engineer "out of his present status of being a hired servant."¹⁶

Space does not permit going into the details of Blackett's memorandum on the methodology of operations research. I should like, however, to mention briefly two notions that, introduced here for the first time into the OR literature, were to become standard approaches in the subsequent development of systems analysis. My main reason for mentioning them is to point out an interesting strand of continuity in the evolution of ASA. Under the name of "variational method," Blackett introduced a type of analysis closely analogous to the

¹⁵See his critical review of Morse and Kimball's *Methods of Operations Research* in *Journal of the Operations Research Society of America*, vol. 1, no. 5, November 1953, pp.308-308. Probably in response to this criticism, Morse too began to see precursors of OR everywhere: "[T]hough the term is new, this sort of research is not new, of course. Taylor and his followers, with their time and motion studies, investigated a small part of the field; traffic engineers have been struggling with another part; systems engineering is closely related, and so on." Cf. Philip M. Morse, "Statistics and Operations Research," *Operations Research*, vol. 4, no. 1, January 1956, pp.2-19, 5.

¹⁶Quoted by Magali Sarfatti Larson, *The Rise of Professionalism*, cit., p.140.

economist's marginal reasoning. According to the variational method, each new tactical situation is to be treated as a variation of some old one--about which some data are always available. The problem is to find out how a given system would be altered if some of the variables that determine its effectiveness were varied. The practical applicability of the method depends on the fact that technical devices cannot change very rapidly because of the time required by development and production; even tactical operations do not usually change very fast, if for no other reason than the necessary duration of training. Thus, even if a new system B is not very similar to the old system A (so that the differentials of the input variables dX_1, dX_2, \dots by which the effectiveness of B can be derived from that of A, are not very small) the results may be fairly reliable, "provided common sense and judgment are used."¹⁷

A second interesting idea discussed in the memorandum is a method for comparing alternative systems under uncertainty that later came to be known as "*a fortiori* analysis." Sometimes lower or upper bounds on the possible effectiveness of a system are known more accurately than the actual values. Thus, to compare a new system B with an existing system A whose effectiveness Y_A is known, assume upper limits (i.e., most favorable to B) for the relevant input variables. Let Y_B' be the estimated upper bound on the effectiveness of B. If $Y_B' < Y_A$, then system B is certainly inferior (if $Y_B' > Y_A$ no meaningful conclusion can be derived without more calculations). Assuming a lower bound Y_B'' (most unfavorable case for B), if $Y_B'' > Y_A$, B is certainly superior. Some fifteen years after Blackett's original memorandum, two well-known analysts from the Rand Corporation were to write that "[m]ore than any other single thing, the skilled

¹⁷P.M.S. Blackett, "Operational Research," cit., pp.180-182. In more modern language, Blackett is assuming that the effectiveness or yield of a system, denoted by Y, is determined by n inputs X_1, \dots, X_n : $Y = F(X_1, X_2, \dots, X_n)$. dY/dX_i is then the marginal product of X_i . If the marginal products can be estimated (and Blackett discusses some statistical and analytic methods for estimating them), then the operational effect of changes in input variables (weapons, tactics, training, etc.) can be estimated by means of the total differential:

$$Y' = Y + \frac{dY}{dX_1}dX_1 + \frac{dY}{dX_2}dX_2 + \dots + \frac{dY}{dX_n}dX_n$$

where Y' is the effectiveness of the new system. The interested reader should compare Blackett's original memorandum with Alain C. Enthoven's "The Simple Mathematics of Optimization," published as an appendix to Charles J. Hitch and Roland N. McKean, *The Economics of Defense in the Nuclear Age*, Cambridge, Massachusetts: Harvard University Press, 1967, pp.361-406.

use of a *fortiori* and break-even analyses separates the professionals from the amateurs."¹⁸

Thus, before the end of World War 2, operations researchers had already developed a number of concepts and approaches whose usefulness would be fully revealed in subsequent decades. However, it is worth pointing out again that, with the notable exception of search theory developed by B.O. Koopman and others in the US Navy's Operations Research Group, successful wartime applications of operations research were not based on new theories or advanced technical tools, but on a sophisticated use of craft skills, learned in the scientific laboratories, in recording, analyzing, and evaluating data, in establishing quantitative relationships, and in setting up testable hypotheses. The first textbook on operations research, Philip M. Morse and George E. Kimball's *Methods of Operations Research*,¹⁹ contains no more advanced mathematics than multiple integration, differential equations, and continuous probabilities.

The mathematical and statistical theories that form the technical core of OR today—queuing theory, mathematical programming, inventory theory, network flows, applied stochastic processes, control theory—were developed (and sometimes rediscovered) after the War, with the introduction of OR into industry and as a subject for teaching and research in universities. An excellent example of OR as practiced in the early 1950s is Leslie C. Edie's "Traffic Delays at Toll Booths"²⁰—first winner of the Lanchester Prize awarded annually for a book or paper making a significant contribution to the advancement of the state of the art of OR.

Probably the most significant methodological development of the first decade after the War was the creation of a set of efficient techniques for programming several activities sharing limited resources. The general problem is

¹⁸Herman Kahn and Irwin Mann, *Techniques of Systems Analysis*, Santa Monica, California: The Rand Corporation, RM-1829, December 1956.

¹⁹Wiley, New York, 1951. The volume was first published in 1946, as a classified technical report, under the auspices of the US Office of Scientific Research and Development and the National Defense Research Committee.

²⁰Published in the *Journal of the Operations Research Society of America*, vol. 2, no. 2, May 1954, pp.107-138.

to determine the level of each activity that optimizes the output of all activities without violating the given resource constraints. There are several reasons for the practical and conceptual significance of this development, especially the linear programming models developed by George B. Dantzig and other researchers. First, the mathematical problem of maximizing an objective function subject to various constraints covers a very wide range of situations occurring in production and inventory control, in military planning, in agriculture, transportation, financial management, and so on. In the important special case of a linear (or piecewise linear) objective function and linear constraints, the solution algorithm (simplex) developed by Dantzig can be implemented efficiently with the help of a digital computer, thus allowing the explicit solution of quite large programming problems. Second, the programming viewpoint opened up a number of important connections with economic theory—particularly with the neoclassical theory of production and the "new welfare economics." In this respect, great economic significance attaches to the fact that a direct by-product of the solution of a mathematical programming problem is a set of shadow prices, or Lagrange multipliers, representing the effects on the objective function of marginal changes in one or more constraints. Finally, the linear programming approach turned out to be significantly, and often surprisingly, related to other methods of importance for operations research, such as game theory, input-output analysis, and network flow theory. These different connections are discussed at great length in two landmark publications of this period: *Activity Analysis of Production and Allocation*, edited by Tjalling C. Koopmans,²¹ and *Linear Programming and Economic Analysis*, by Robert Dorfman, Paul A. Samuelson, and Robert Solow.²²

As these developments (and others in inventory theory, waiting-time and replacement models, and applied stochastic processes which cannot be discussed here) suggest, important changes were taking place between 1945 and 1955, in personnel, disciplinary aims, and, consequently, also in the implicit

²¹New York: Wiley, 1951.

²²New York: McGraw-Hill, 1958.

standards of evaluation and criticism. While people like Blackett, Waddington, and Morse were returning to their laboratories and university departments, a new generation of analysts was entering the OR scene—people primarily interested in the more formal aspects of scientific methodology and proficient in mathematical manipulations, but often lacking the craft skills and the mature critical judgment of the old masters. The goal of operations research, as the early practitioners saw it, was "to find a scientific explanation of the facts."²³ The phases of investigation followed the pattern prevalent in the science laboratory: "...past operations are studied to determine the facts; theories are elaborated to explain the facts; and finally the facts and theories are used to make predictions about future operations..."²⁴

Given this paradigm, the relevant standards of criticism were those of natural science. In fact, the situations investigated by operations researchers during the War were particularly well suited to such an approach. Typically, military operations could be regarded, without serious distortions, as being 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."²⁵ Works like Edie's "Traffic Delays at Toll Booths," and C.W. Thornthwaite's "Operations Research in Agriculture,"²⁶ still followed the classical pattern, and explicitly appealed to the established criteria of validation.

But by 1955 the focus of professional interests had clearly shifted away from military operations, while the scope and methods of OR work had changed sufficiently to raise serious questions about the relevance of the traditional standards of evaluation and criticism to contemporary professional practice. The increasing popularity of computer-based models (with the attendant serious

²³C.H. Waddington, *OR in World War 2*, London: ELEK Science Ltd., 1973, p.26.

²⁴P.M.S. Blackett, "Operational Research," cit., p.177.

²⁵Hylton Boothroyd, *Articulate Intervention*, cit., p.113.

²⁶Published in the *Journal of the Operations Research Society of America*, vol. 1, no. 2, February 1953, pp.33-36.

problems of validation) made the need for new criteria of criticism even more obvious. A consecutive reading of the recommendations of the Lanchester Prize Committee, starting with the first report in 1954, gives a good indication of the difficulties experienced by the profession in finding agreement on a set of relevant criteria of evaluation.

Let us return to the changes in the disciplinary composition of operations research. In the early stages of development, the part played by the economists in OR activities had been quite modest, compared to that of the natural scientists and the mathematicians. With the expansion of the scope of operations research in the post-War years, particularly in the United States, to include military strategy as well as a growing number of public policy problems in health, education, transportation, housing, and the social services, the role of the economist was bound to become increasingly important—as shown by the election of Rand economist Charles J. Hitch to the presidency of the Operations Research Society of America in 1959. As a group, economists have made two basic contributions to the development of the field: first, a penetrating critique of certain conceptual inadequacies (e.g., in the selection of criteria and in the treatment of time) of early OR applications; second, the proposal of an intellectual framework derived from decision theory and the microeconomic logic of choice as the most appropriate paradigm for operations research.

A good example of the new critical attitude is Hitch's paper on "Sub-optimization in Operations Problems."²⁷ The validity and usefulness of operations research, Hitch argues, depends to a large extent on the ability to choose the correct criterion or objective function for the problem under discussion. "Unless operations research develops methods of evaluating criteria and choosing good ones, its quantitative methods may prove worse than useless to its clients in its new applications in government and industry."²⁸ The main criterion for judging whether the objective function chosen for a given level of analysis is

²⁷Published in the *Journal of the Operations Research Society of America*, vol. 1, no. 3, May 1953, pp.87-99.

²⁸*Ibid.*, p.87.

the correct one is consistency with the relevant objective function at a higher level. Unfortunately, too many OR studies in the past have failed to meet this criterion. For example, in devising a suitable strategy for the defense of naval convoys against attacks by enemy submarines, one should keep in mind that the relevant higher level objective is winning the war. The criterion of effectiveness chosen at the operational level should be consistent with it. But the criterion actually used during the War—which amounted to maximizing the "exchange ratio" of enemy losses to one's own losses—is not necessarily compatible with the higher level goal. As a matter of fact, the decision to increase the size of the convoys so as to improve the exchange ratio disregarded a number of factors (congestion of port facilities, reduced operating efficiency of ships in large convoys, longer turnaround times, redirection of enemy effort) which were obviously important for the general strategy of the War.

The examples of improper suboptimization given by Hitch are mostly of a military nature, but the phenomenon is quite general. Thus, the sales department of a profit-maximizing firm is not supposed to suboptimize, e.g., to maximize the sales minus selling costs, but to choose actions that maximize total profits of the firm. Similarly, the correct goal of the production department (in terms of the profit targets of the entire organization) is not, in general, the minimization of cost per unit of output, nor the maximization of productivity per man/hour but, again, a mode of operation that is conducive to the maximization of total profits.²⁹

Similar criticisms have been voiced by other economists in different contexts. Martin Feldstein, for example, writes that "[q]uantitative methods in government management decisions can be extremely fruitful, but in the absence of an appropriate framework they can be empty algorithms which hide misleading advice in a mass of reassuring calculations."³⁰ He then goes on to argue that

²⁹What came to be known in the literature as "the criterion problem" is discussed at great length in two early classics of systems analysis: Roland N. McKean's *Efficiency in Government Through Systems Analysis*, New York, John Wiley and Sons, Inc., 1958, and Charles J. Hitch and Roland N. McKean's *The Economics of Defense in the Nuclear Age*, cit.

³⁰Martin S. Feldstein, "Economic Analysis, Operational Research, and the National Health Service," *Oxford Economic Papers*, March 1963, pp.19-31, 21.

operations research achieves maximum usefulness only if it is considered in a framework of economic analysis of the appropriate benefits and costs of alternative actions. Feldstein draws his examples from the experience of the British National Health Service. He shows that it is a mistake to approach health-service decisions as problems of meeting specific community "needs." Rather, they should be approached as problems of allocating scarce health resources among competing uses. For example, operations researchers have made elaborate calculations of the number of hospital beds needed to meet doctors' requests in a given region, without raising probing questions about the optimal number of beds, where the benefits of hospitalization and longer stay are weighed against alternative uses of scarce health resources.

In part, these criticisms reflect the traditional opposition between the economic viewpoint, which is concerned with finding the best allocation of given resources among *competing* ends, and the technical viewpoint, which is concerned with finding the best way of using given resources to achieve a *single* end. But in a deeper sense what is at issue here is the appropriate conceptualization of the system under investigation. The economist's recommendation for avoiding the pitfalls of suboptimization is the "golden rule" of allocative efficiency: scarce resources having alternative uses should be allocated so as to make each resource equally scarce (i.e., equally valuable at the margin) in all uses. But allocative efficiency can be achieved only if resources can be freely combined and substituted for each other according to their relative prices or scarcities—fewer hospital beds and more outpatient services, less air support and more ground forces. In this logic, the internal organization of the system is irrelevant if not positively misleading, since it tempts the analyst to make the scope of the analysis coincide with the boundaries of administrative units and decision-making authority.

Thus it is only a slight overstatement to say that the difference between the traditional operations researcher and the economist-turned-systems-analyst is that the traditional operations researcher first establishes what the system to

be studied is, and then inquires about the problems of that system, while the systems analyst first determines what the real problem is, and only then inquires about the appropriate system or systems within which this problem must be considered if it is to be solved fruitfully.³¹ The emphasis on "system design" (as opposed to the static analysis of given alternatives), characteristic of so much early writing on systems analysis, fits quite naturally the new decision-making paradigm, although, paradoxically, it implicitly reintroduces many of the institutional and political factors whose influence the microeconomic paradigm of allocative efficiency had attempted to minimize. As we shall see, policy analysis emerged in the late 1960s as an attempt to reconcile the opposing logics of "economic rationality" and "political rationality"—broadly understood. But in the period we are considering now (from the early 1950s to mid-1960s) the success of the economic paradigm in transforming early-vintage operations research into a more ambitious and intellectually, if not technically, more sophisticated systems analysis is almost complete. Cost-effectiveness analysis, modeling, optimal timing of projects, gaming, grand strategy: everything seems to fall into its proper conceptual place now.

It is true that microeconomic logic does not deal adequately with decision-making under uncertainty. But economists were quick to close the gap by appropriating decision theory—an approach to the problem of choice under uncertainty originally developed by probabilists, but so general in scope that it could claim, with some justification, to include operations research as well as wide areas of economics and statistics. Thus, the new paradigm seemed to have an answer for all problems of choice, at least in principle.³² Systems analysis came to be widely regarded as a decision technology, concerned not with how systems behave, but how they should behave. A prescriptive approach to decisionmaking was the new symbol of rationality, in industry and in government, displacing the earlier emphasis on prediction and the "scientific

³¹Malcolm W. Hoag, "What is a System?", *Operations Research*, vol. 5, no. 3, June 1957, pp.445-447.

³²See, for example, Kenneth J. Arrow, "Decision Theory and Operations Research," *Operations Research*, vol. 5, no. 6, December 1957, pp.765-774.

explanation of the facts."

3. FROM SYSTEMS ANALYSIS TO POLICY ANALYSIS

Cost-benefit analysis (CBA) is simply a method of setting out the factors that have to be taken into account in making economic choices, particularly in the case of investment projects, for the purpose of maximizing the present value of all benefits minus that of all costs, subject to given constraints. This technique of economic calculation had been given special attention in one of the early and most influential discussions of systems analysis, McKean's *Efficiency in Government Through Systems Analysis* (1958), which was primarily concerned with water resources development. Perhaps for this reason, CBA became almost identified with systems analysis in the mind of many people, professionals as well as laymen—despite the warning by two well-known economists that CBA is "only a technique for taking decisions within a framework which has to be decided upon in advance and which involves a wide range of considerations, many of them of a political or social character."³³ Although the claim made by some advocates that CBA is "a natural and logical extension" of systems analysis and operations research, seems in retrospect rather exaggerated, there is some truth in the statement that it is "more ambitious than them in evaluative scope and in technique."³⁴ Hence by examining, however briefly, the underlying purpose of CBA and the type of relation between analyst and decision-maker that it implies, we can gain a better understanding of the strengths and weaknesses of the economist's approach, and its significance for the development of systems analysis.³⁵

Since CBA is used in relation to a decision problem—how to choose between two or more alternative courses of action or "social states"—it assumes a well-

³³A.R. Prest and R. Turvey, "Cost-Benefit Analysis: A Survey," *Economic Journal*, vol. 75, 1965, pp.683-735, 685.

³⁴Alan Williams, "Cost-Benefit Analysis: Bastard Science? And/Or Insidious Poison in the Body Politick?" *Journal of Public Economics*, vol. 1, no. 2, August 1972, pp.199-226, 200.

³⁵For a more complete treatment the reader is referred to *The Principles of Practical Cost-Benefit Analysis* by Robert Sugden and Alan Williams, Oxford, England: Oxford University Press, 1978. The last chapter is particularly relevant to the present discussion.

defined decisionmaker or group of decisionmakers. And since it is typically, though not exclusively, applied to public decisions involving the welfare of the community as a whole, the decisionmaker is supposed to act on behalf of the public interest. Leaving analytic technicalities aside (choice of a discount rate, treatment of uncertainty, estimation of consumers' and producers' surplus, distributional weights, and so on), the distinguishing features of CBA are explicitness and consistency. CBA is explicit in the sense that, in principle, all assumptions are clearly stated, evidence is presented, calculations and conclusions are reproducible. It is explicit also in the sense that it must state clearly not only the decisionmaker's objective function, but also the alternatives that have been examined and the constraints that have been used. In short, the analyst attempts to translate into a well-defined decision problem what was initially, in many cases, only a problem situation—a feeling that things are not as they should be, but without a clear idea of how they might be put right.

The second feature, consistency, is of crucial importance not only for CBA but for the entire prescriptive, or normative, approach to the analysis of decisions. We have already met the problem in our discussion of suboptimization: how does one make sure that lower-level decisions are consistent with higher-level ones? The answer given there—the "golden rule" of allocative efficiency—presupposed a centralized and fully-informed decisionmaker, capable of estimating the marginal utilities of the available resources in all their possible uses. Similarly, the utility-maximization rule of decision theory is a way of making sure that the decisionmaker's choice (under uncertainty) is consistent with his subjective estimates of the probability of different contingencies and with the utilities he attaches to various conditional outcomes. These meanings of consistency are all relevant to the practice of CBA, but in addition to the efficiency and logical aspects there is a political and ethical problem that no serious analyst can evade. To quote Sugden and Williams:³⁶

³⁶*The Principles of Practical Cost-Benefit Analysis*, cit., pp.233-234. Footnote omitted.

If decisionmakers were able to specify a different set of objectives for each decision that they had to make, cost-benefit analysis would be, as opponents of the decision-making approach have alleged, little more than window-dressing. To ensure that a pet project received the sanction of cost-benefit analysis, a decisionmaker would need only to revise his objectives in the appropriate way. If the analyst is to escape the charge of window-dressing he must be prepared, in the report that he makes of his analysis, to discuss the wider implications of the objectives that he has used. If, for example, he has been asked to use in a cost-benefit analysis of a particular medical treatment a valuation of the prolonging of life that is clearly inconsistent with current policy towards medical care in general, he ought to make this inconsistency clear when he reports. Otherwise the result of his work may be to mislead more than to enlighten.

Thus the analyst should practice explicitness and preach consistency. This is a reasonable prescription if we assume a unique decisionmaker, or a group whose members share common objectives and disagree only about questions of fact. But, the political scientist objects, this is not at all the situation prevailing in public policy making. Health, education, or housing policies are not the outcomes of the choices of a unitary decision-making body, however powerful, but of political processes involving different interest groups, a variety of political and bureaucratic institutions, pressure groups, and, in our technological society, the analysts themselves.

The normative approach breaks down, our critic continues, because it rests on the fiction of a "benevolent dictator" with complete information about the preferences and interests of all members of the community, with no preferences of his own, and capable of implementing fully his decisions. Not surprisingly, in the microeconomic paradigm politics and human nature belong to the institutional or behavioral givens and are taken to lie outside the scope of analysis. In fact, normative analysis, being a generalized logic of choice, terminates at the

moment a decision is taken, leaving outside questions of policy implementation, evaluation, and termination (as distinct from *model* evaluation and implementation).

Ironically, the political scientist's critique of the economist's approach to systems analysis is, in a sense, quite similar to the critical stance taken by economists, a decade earlier, with respect to operations research. Both criticisms revolve around the notion of suboptimization—in one case with respect to economic rationality, in the other, with respect to political rationality. The difference is that, while the notion of economic rationality can be explicated precisely in terms of economic efficiency (either in the general Paretian sense, or in the more special sense of allocative efficiency), no generally accepted explication of "political rationality" seems to exist. Consequently, attempts to differentiate policy analysis from systems analysis have moved along different lines. We can distinguish two main directions. According to one school of thought, policy analysis is systems analysis writ large—in the sense that it includes, in addition to the technical and economic aspects of a policy problem, also those political aspects which systems analysis is supposed to have overlooked (whether or to what extent the charge is correct, is an empirical question that cannot be discussed here). Yehezkel Dror's manifesto is typical of this position. In policy analysis:

- 1) Much attention would be paid to the political aspects of decisionmaking and public policy making (instead of ignoring or condescendingly disregarding political aspects)...

- 2) A broad conception of decisionmaking and policy making would be involved (instead of viewing all decisionmaking as mainly a resources allocation)...

- 3) A main emphasis would be on creativity and search for new policy alternatives, with explicit attention to encouragement of innovative thinking...

4) There would be extensive reliance on ... qualitative methods...

5) There would be much more emphasis on futuristic thinking...

6) The approach would be looser and less rigid, but nevertheless systematic, one which would recognize the complexity of means-ends interdependence, the multiplicity of relevant criteria of decision, and the partial and tentative nature of every analysis...³⁷

The immediate practical question is, how can political and institutional considerations be handled with the same professional competence as the more familiar technical and economic factors. One possibility is suggested by the notion of "political feasibility"—a notion that is used frequently, if loosely, in policy discussions. To take political feasibility seriously means to be prepared to list the specific political and institutional constraints that limit the freedom of choice of the policy makers.³⁸ Once these constraints have been made explicit, it will often be possible to estimate the consequences of small variations on the cost of achieving the policy objectives. In this way, a rough estimate of the opportunity costs of a political constraint can be obtained.³⁹ Suppose, for example, that a publicly owned oil company is considering where to locate a new refinery. If government policy forces the company to build the plant in a part of the country in need of special economic assistance, the implied cost of this political constraint can be evaluated by reference to a situation in which the constraint is not present.

As long as policy analysis is conceived as systems analysis writ large, the role of the political analyst is entirely analogous to that of the economist or of

³⁷Yehezkel Dror, "Policy Analysts: A New Professional Role in Government Service," *Public Administration Review*, vol. 27, no. 3, 1967, pp.200-201. Quoted by Aaron Wildavsky, "Rescuing Policy Analysis From PPBS," *Public Administration Review*, vol. 29, no. 2, 1969, pp.189-202. This paper by Wildavsky, and his earlier essay, "The Political Economy of Efficiency: Cost-Benefit Analysis, Systems Analysis, and Program Budgeting," *Public Administration Review*, vol. 26, no. 6, pp.292-310, probably represent the most influential criticism of systems analysis by a political scientist.

³⁸Giandomenico Majone, "On the Notion of Political Feasibility," *European Journal of Political Research*, vol. 3, 1975, pp.259-274; now in *Policy Studies Review Annual*, vol. 1, Stuart S. Nagel, editor, Beverly Hills and London: Sage Publications, 1977.

³⁹Giandomenico Majone, "The Feasibility of Social Policies," *Policy Sciences*, vol. 6, 1975, pp.49-69 and Alan Williams, "Cost-Benefit Analysis: Bastard Science? And/Or Insidious Poison in the Body

the technical expert: he translates his assessment of the political situation into a set of constraints and, together with other specialists, estimates the consequences of those constraints for the expected level of achievement of the policy objectives.

The second direction in which a differentiation between systems analysis and policy analysis has been sought is quite different, since it emphasizes the process rather than the outputs or outcomes of policy making. Here the analyst is viewed less as a problem solver or advisor than as designer of procedures for group decisionmaking, and as a catalyst in the implementation process. The advocates of this process-oriented view of analysis are impressed by the enormous complexity of policy making, and by the cognitive and information-processing limitations of the human mind. This lack of match between intellectual capacity and the complexity of social processes dooms to failure any attempt to find complete and explicit solutions to policy problems. Policy problems are never solved, but only shifted and (sometimes) ameliorated. Or, rather, to the extent that a policy problem is temporarily resolved (i.e., removed from the agenda of issues under current debate), this happens because a consensus has been reached by the participants in the policy process, not because a solution, in the sense of normative analysis, has been found. But if policy problems are resolved by social interactions (bargaining, decentralized markets, voting, persuasion, and so on), what role is left for policy analysts to play?

Charles Lindblom, whose writings represent the most articulate and influential expression of the process-oriented approach, recognizes three distinct forms of adaptation of analysis to interaction:⁴⁰

One is analysis by any participant of how he can play his interactive role better to get what he wants—frankly partisan analysis asking "What shall I buy?" or "How shall I vote?" or (for a businessman) "How

Politick?", cit.

⁴⁰Charles E. Lindblom, *Politics And Markets*, New York: Basic Books, Inc., 1977, p.316. See also

can I increase sales?" or (for a legislator) "How can I get this bill through the House?" The second is analysis of how to enter into existing interactions most successfully to achieve some public purpose which one, as a public official, has a responsibility to pursue. "Should taxes be cut to stimulate employment?" "Should criminal penalties for street crime be increased?" The third is analysis of possible changes in the basic structure of the interaction processes themselves. "Should markets be made more competitive by breaking up big business?" "Should the criminal justice system be revamped?" "What changes are required in parliamentary organizations?"

Notice how the three kinds of adaptation roughly correspond to the historical development of ASA, from the early applications of operations research to specific problems of tactics and logistics, through the broader concerns of systems analysis, to the preoccupation with institutional reform which characterizes contemporary policy analysis. Probably the most important insight to emerge from a serious reflection on this development is the recognition that analysis has a procedural as well as a substantive function. It provides not only evidence and arguments, but also an intellectual structure for the policy process. Even when its conclusions are not accepted, its categories and language, its rational ordering of general ideas affect—even condition—the policy debate. The importance of this procedural function is directly related to the basic lack of certainty of policy determinations. When the correctness of a decision can be established unambiguously, the manner in which it is reached is largely immaterial; only results count. But when the factual and value premises are uncertain and controversial, when objective criteria of success or failure are lacking, the formal characteristics of the decision process—its procedure—become significant. Harvey Brooks draws a revealing analogy between analysis and legal procedures:⁴¹

D. Braybrooke and C.E. Lindblom, *A Strategy of Decision*, New York: Free Press, 1963.

⁴¹Harvey Brooks, "Environmental Decision Making: Analysis and Values," in *When Values Conflict*, L.H. Tribe, C.S. Schelling, and J. Voss, editors, Cambridge, Massachusetts: Ballinger Publishing Company, 1976, pp.115-136, 115.

The usefulness of systems analysis depends on the fact that its conclusions purport to be based on a set of neutral principles that command a wider consensus than those conclusions themselves would be likely to command without a demonstration that they are logically deducible from such principles. In this sense, policy or systems analysis perform a function with respect to political-technological decisions similar to that performed by a judicial process with respect to conflicts between individuals. A court decision is accepted by the disputing parties largely because it is based on a set of rules both parties accept applied through a procedure which both parties are prepared, before knowing its outcome, to accept as unbiased.

One does not have to agree with Brooks that analytical conclusions can be formally deduced, *more geometrico*, from a set of "neutral principles," to recognize the importance of his observations. In our societies the rationality and legitimacy of public policies depend increasingly on procedural, even more than on substantive, considerations. But for analysis to perform a quasi-judicial function with respect to policy decisions, its own rules of evidence and procedure must be spelled out in great detail. As I shall argue in the second part of this chapter, this calls for a determined effort by the ASA profession to develop standards of adequacy and suitable mechanisms of quality control.

4. ... AND BACK TO OPERATIONS RESEARCH

I shall attempt to summarize the preceding discussion by exhibiting in tabular form the distinguishing features and characteristic problems of the three stages of ASA, as shown in Table 2J.⁴²

At this point, two clarifications are necessary to avoid misunderstandings. First, the terminological distinctions among operations research, systems analysis, and policy analysis, while fairly common in English-speaking countries (but not without some ambiguities even there: where, for instance, does

⁴²This table expands an analogous classification proposed by Roger E. Levien in "Outcome Meas-

management science fit in the series?), are by no means universally accepted or used. In many countries a single label like "operations research" applies to all three stages or forms of analysis that have been distinguished here. In such a case, "operations research" assumes exactly the same meaning as "applied systems analysis," as the term has been used in this chapter.

Second, it is important to realize that a classification like the one suggested by Table is only a cross section or time slice of the entire process of disciplinary evolution. To obtain a complete evolutionary representation one would have to combine a cross-sectional description with a longitudinal study. Such a cross-sectional and longitudinal study of operations research, for example, would show, not only the successive changes in the pool of concepts and techniques available at different points in time, but also a continuous evolution in aims, methods, and evaluative criteria (in short, in the self-image of the discipline) reflecting, at least in part, analogous developments in systems and policy analysis. Instead of a linear development, in which systems analysis follows operations research and is followed by policy analysis—a linear order which has been adopted here only for expository reasons—what we have in fact is a sequence ^{dialectical} in which different modes of analysis coexist in more or less close mutual interaction. Thus, in recent issues of journals like *Operations Research* and *Management Science* one finds articles on air-pollution control and water quality management, on majority voting and distributional constraints on public expenditure planning, on evaluation of the quality of social services and implementation of new ideas in bureaucracies, on decision analysis and medical malpractice, even on the design of electoral districts—topics and papers that could have appeared also in *Policy Sciences*, *Policy Analysis*, or some economics journal.

It has already been noted that the most important factor tying the different specializations and approaches of ASA together is the way in which an initial problem develops and mixes with other issues to form a "lattice" of descendant

urement: A U.S. Viewpoint," unpublished manuscript, IIASA, 1980.

	Operations Research (1940s)	Systems Analysis (1950s)	Policy Analysis (1960s-1970s)
Disciplinary aims	Discovery of empirical regularities in operations; operational design; prediction and testing.	Resource allocation; analysis of conflicting systems; system design.	Problem formulation; analysis of distributional consequences and institutional constraints; design of decisionmaking procedures.
Evaluation criteria	Technical efficiency; cost minimization.	Economic (allocative) efficiency.	Political and administrative feasibility; consensus on policy.
Characteristic features and methods	Unitary decisionmaking; system, policy, and goals given; statistical inference; differential equations; search theory; queuing and inventory models; control theory.	Group decisionmaking; policy and goals given; operations embedded in larger sociotechnical systems; microeconomics; constrained optimization; decision and game theory; simulation; econometrics.	Public policy making; ill-defined goals; institutional framework given; public finance and political economy; organization theory; data analysis and large-scale social experimentation.
Typical applications	Tactical operations; logistics; production scheduling; waiting lines; inventory control; programming.	Choice among weapon systems; strategic studies; resource allocation in a national health system; development of water resources.	Policy planning; reform of existing national systems of health, education, or social security; pollution control; program evaluation; program implementation.

Table 2.1. Distinguishing features and characteristic problems at three stages of applied systems analysis.

problems. Energy policy modeling is a good example of this phenomenon. The first energy models developed in the early 1970s dealt largely with technical and economic issues that could be handled by standard OR method. There were short- and medium-term linear programming models of energy supplies, and econometric models of energy demands; quadratic programming models of price-responsive oil demands and supplies, and the resulting international equilibrium; year-by-year simulations of electric utilities pricing and equipment-ordering policies, and so on.⁴³ The omission of social, political, and institutional considerations—health and environmental effects of different modes of energy production, safety problems, the risk of nuclear proliferation, issues of "scale" and of the political implications of alternative energy paths—did not appear to be too serious in the early stages of the policy debate. But as opinions have become polarized and public appreciation of the more remote implications of policy choices has increased, the need to deal explicitly also with the broader social and political issues has been generally accepted by the modeling community. After years of rather fruitless debate, even the most technically-minded analysts have been forced to recognize that technology and economics can play only a limited role in the ongoing energy controversy, and that energy policy is inherently an interdisciplinary field. "It involves economics, law, politics, engineering, resource geology, biomedical impacts, and environmental risk assessment—along with the methodologies that are already familiar to the operations researcher: optimization algorithms, simulations, decision analysis and econometric estimation."⁴⁴

The case of energy policy modeling raises another issue of the utmost importance today for all applied systems analysts: the role and effectiveness of formal analysis in the policy process. But this takes us beyond the strictly disciplinary aspects of the evolution of ASA, and into the professional and socio-institutional dimensions to which we now turn.

⁴³For a very useful recent survey, see Alan S. Manne, Richard G. Richels, and John P. Weyant, "Energy Policy Modeling: A Survey," *Operations Research*, vol. 27, no. 1, January-February 1979, pp.1-36.

⁴⁴*Ibid.*, p.1.

5. THE SOCIAL SIDE OF ASA: PROFESSIONAL AND INSTITUTIONAL DEVELOPMENTS

The preceding sections have largely dealt with the "internal history" of ASA—the development of concepts, methods, and techniques in response to the changing nature of the objects of inquiry, and to intellectual challenges arising within the profession. This intellectual development must now be related to the larger social context in which analysts operate. The question to be investigated now is how the historical development of institutions and roles, publications and incentive systems both reflect and influence the intellectual concerns and aspirations of the ASA profession.

It has already been suggested that neither approach—internal, intellectual history on the one hand, external, social history, on the other—is by itself sufficient to give an adequate account of the entire development of the field. Social or institutional factors do not explain, for example, the cycles of expansion and depression experienced by certain areas of research and application, such as game theory. On the other hand, national differences in style and aims of ASA activity cannot be explained only, or even primarily, on intellectual grounds. Thus, the fact that industrial operations research in the United States adopted quite early a systems approach, has been attributed to the high degree of specialization and professionalization of appliedⁱⁿ industrial research there. At the time the operations researchers arrived on the scene (the first public meeting between operations researchers and industrial managers took place only in 1951, in Cleveland) industrial engineering, statistical quality control, marketing, personnel and financial management, were already recognized fields of professional specialization. Hence, according to this theory, in order to define a field of cognitive exclusiveness, American operations researchers had to focus on the interactions of specific industrial functions and the organization as a whole.⁴⁵ One need not agree fully with the explanation to recognize that the factors involved are institutional rather than disciplinary. Similarly, the relatively late

⁴⁵R.L. Ackoff, "A Comparison of Operational Research in the U.S.A. and in Great Britain," *Operational Research Quarterly*, vol. 8, no. 2, June 1957, pp.88-100.

development of academic operations research in Europe, as well as the difficulty of establishing academic curricula in policy analysis, are largely due to institutional and sociological differences existing between European and American university systems.

At this point some chronology may be helpful. The first OR professional society was formed in the United Kingdom in 1948 as the Operational Research Society (initially, Operational Research Club). The Operations Research Society of America followed in 1952, with Philip M. Morse as its first president. The initial membership of both societies included many scientists who had taken part in the development of military OR during World War 2. However, the focus of professional interest was rapidly shifting to industrial applications. One sign of this redirection of professional interests is the foundation in 1953 of The Institute of Management Sciences—an international society, but with most of its members in the United States. In 1957, the first International Conference on Operational Research was held at Oxford University. It was attended by 250 delegates from 21 countries. One important outcome of this conference was the International Federation of Operational Research Societies (IFORS), formally constituted on January 1, 1959, with three initial members: Operational Research Society, Operations Research Society of America, and Société Française de Recherche Opérationnelle (founded in 1956).⁴⁶ Between 1959 and 1975, twenty-four additional national societies were founded in Western and Eastern Europe, Asia, Latin America, Australia, and South Africa, and soon joined IFORS.

Operations research journals follow closely the developmental pattern of professional societies, starting with the *Operational Research Quarterly*, founded in 1950 and published by the UK Operational Research Society, and *Operations Research* (1952), published by the Operations Research Society of America. In all fields of learning, scholarly periodicals are among the most powerful institutions of science, and ASA is no exception in this respect. In fact,

⁴⁶For additional information and bibliographical references, see the useful article by Hugh J. Miser, "The History, Nature, and Use of Operations Research," in *Handbook of Operations Research*, Joseph J. Moder and Salah E. Elmaghraby, editors, New York: Van Nostrand Reinhold Company, 1978, vol. 1, pp.3-24.

if it is true that even in the older natural sciences "the very raison d'être of many scientific societies lies primarily in the journals they sponsor, only secondarily in their formal meetings,"⁴⁷ in the case of ASA professional journals sometimes take the place of professional societies. For instance, while no professional societies existed until 1980 in the field of policy analysis, policy analysts in government, universities, and research institutes tended to gravitate intellectually toward publications like *Policy Sciences*, founded in 1970, and *Policy Analysis*, founded in 1975. In these cases, the lack of a sponsoring professional organization was compensated, to some extent, by the presence of very large (by usual standards) editorial boards.

Communication between analysts and decisionmakers is one of the crucial practical problems of ASA. Since the increasing specialization of the field creates serious language barriers, publications have begun to appear whose primary goal is encouraging interactions between producers and consumers of analysis, as well as between the various divisions and professional groups within the ASA community. Perhaps the best known exemplar of this literature is *Interfaces*, published jointly since 1974 by The Institute of Management Sciences and the Operations Research society of America (it originated in 1971 as *The Bulletin of TIMS*).

Having stressed the role played by journals in the disciplinary and professional development of ASA, it is important to note also some of their problems and limitations. The first problem, mentioned here only briefly, since it will be discussed in the next section, is that of the critical criteria used in the refereeing process. How to reconcile rigor with relevance is the crucial difficulty. The desire for rigor, especially in the highly specialized sense of formal or axiomatic rigor in which the term is often used, may (and often does) prevail over the requirements of relevance. In the trend toward greater formalization some critics see the possibility that ASA may lose its identity and be assimilated into

⁴⁷Stephen Toulmin, *op. cit.*, p.270.

other fields of inquiry.⁴⁸

A second difficulty in assessing the state of ASA through professional publications is that journals and research reports tend to give a distorted picture of the field—a picture which is strongly biased in the direction of theoretical developments. For security, proprietary, or other reasons actual applications are not published, or may appear in print with a delay of years. Thus, one of the most famous studies in military systems analysis, the Strategic Bases Study conducted by Albert Wohlstetter and other Rand analysts, was initiated in 1951, completed in 1954, declassified in 1962, and discussed in a professional journal only in 1964.⁴⁹ Even when actual case studies are reported, the necessity of concealing the identity of the sponsor and the true nature of the problem investigated often induces a stylized presentation in which many of the details that are so important for understanding the craft aspects of ASA are completely lost.

Next to journals, standard textbooks represent an important locus of scientific authority, and the main channel by which the intellectual advances of a discipline become the collective property of a profession.

Whereas the "micro-evolution" of scientific ideas is manifested in the most up-to-date research discussions ... its "macro-evolution" is embodied in the standard texts accepted as authoritative in each successive generation... . [T]hese standard works define the successive bodies of doctrine that form the accepted starting-points for the next generation. By digesting the specialized literature of the preceding generation, indeed, these comprehensive expositions create a "conceptual platform" on which the next generation of budding scientists can stand firm, in defining and attacking their own disciplinary problems.⁵⁰

The successive stages of development of ASA are clearly marked by a series of distinguished texts, starting with Morse and Kimball's *Methods of Operations*

⁴⁸For a recent expression of this view, see Seth Bonder, "Changing the Future of Operations Research," *Operations Research*, vol. 27, no. 2, March-April 1979, pp.209-224.

⁴⁹See Bruce L.R. Smith, *The RAND Corporation*, Cambridge, Massachusetts: Harvard University Press, 1966, pp.195-240.

⁵⁰Stephen Toulmin, *Human Understanding*, cit., pp.277-278.

Research, issued as a classified technical report in 1946 and published commercially five years later. A comparison of the table of contents of this text with that of the influential *Introduction to Operations Research* by West Churchman, Ackoff, and Arnoff,⁵¹ published in 1957, reveals graphically the shift of professional interests from military to industrial problems, as well as the emergence of new (or rediscovered) analytic methods like queuing and inventory theory, linear programming, and game theory.

This first post-War textbook in operations research has been followed by scores of texts, treatises, and reference works now appearing with increasing frequency in all industrialized countries. The award in 1969 of the Lanchester Prize of the Operations Research Society of America to Harvey Wagner's *Principles of Operations Research* is another indication of the professional significance of an outstanding didactic work. The sheer size of Wagner's book is evidence of the number of ideas and methods that were sufficiently well developed and tested by the end of the 1960s to be expounded in an introductory presentation of basic principles. Yet the differences from earlier works like the Churchman, Ackoff, and Arnoff *Introduction* are not merely quantitative. As an interesting example of the process of conceptual selection referred to previously, I may mention the fact that game theory, which received chapter-length treatment in the *Introduction*, is omitted in Wagner's *Principles*, except for the minimax theorem of two-person, zero-sum games—relegated to an exercise in the chapter on duality.⁵²

Again, while the earlier *Introduction* grew from lecture material prepared for short courses in operations research, Wagner's text reflects a stage of development in which undergraduate and graduate courses in operations

⁵¹C. West Churchman, Russell L. Ackoff, and E. Leonard Arnoff, *Introduction to Operations Research*, New York: John Wiley and Sons, 1957.

⁵²This omission corresponds to the judgment often, if not so caustically, expressed in professional circles that "[i]n practicing operations research, we have found that game theory does not contribute any *managerial insights* to real competitive and cooperative decision-making behavior that are not *already familiar* to church-going poker players who regularly read the Wall Street Journal." See Harvey M. Wagner, *Principles of Operations Research*, Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1975, 2nd edition, p.XI. This should be compared with the opinion expressed some twenty years earlier by Ackoff and co-authors (op. cit., p.519), that game theory "started a new way of thinking about competitive decisions."

research form a well established component of academic curricula in business, economics, engineering, and public administration.

No comparable standard presentations of systems analysis exist, but Hitch and McKean's *The Economics of Defense in the Nuclear Age* (published in 1960) represents nevertheless a milestone in the evolution of ASA. While the title and many of the examples in the text refer to military applications, the underlying philosophy is completely general. As the authors write,⁵³

[i]n this book we will be concerned with economics in its most general sense. Economics is not exclusively concerned ... with certain types of activities (industrial) rather than others (military), or with the traditional points of view of budgeteers and comptrollers. Being truly economical does not mean scrimping—reducing expenditures no matter how important the things to be bought. Nor does it mean implementing some stated doctrine regardless of cost. Rather economics is concerned with allocating resources—choosing doctrines and techniques—so as to get the most out of available resources.

The Economics of Defense is the intellectual product of an institution, the Rand Corporation, whose name stands for one of the most influential "schools" of systems analysis, and whose organization and style of work have been imitated throughout the world. The history of the institution in its most creative period is well documented, and need not be retold here.⁵⁴ However, the history of Rand raises an issue of organizational design for policy research institutes that is too central to our discussion to be ignored. Why was the nongovernmental, non-profit form of organization chosen for Rand and for other policy research institutes like Resources for the Future, the Stanford Research Institute, the Brookings Institution, and, more recently, the International Institute for Applied Systems Analysis? Other institutional solutions were, after all, possible—as part of the government staff, or of a university, or as a (for profit) consulting firm. But

⁵³Charles J. Hitch and Roland N. McKean, *The Economics of Defense in the Nuclear Age*, cit., pp.1-2. Footnote omitted.

each of these alternatives presents serious disadvantages for the kind of work—medium to long range, multidisciplinary, independent, and objective—that a high-level policy research institute is supposed to do.⁵⁵ Research carried out by government agencies tends to be of a narrow and short-run nature because the problems immediately facing such agencies are typically narrow and short-run. Also, the incentive structure of large bureaucracies does not favor independent opinions and serious efforts at deep understanding. Blackett, it may be recalled, had argued that the atmosphere required for an operations research group "is that of a first-class pure scientific research institution, and the caliber of the personnel should match this."⁵⁶ This seems to suggest the university as a suitable environment for ASA activities. Unfortunately, universities are structured largely along disciplinary lines, and the cost of breaking down those lines in order to attack policy problems (which by their very nature cut across disciplinary boundaries) can be prohibitively high. Again, the incentive system of the university, with its emphasis on publication in specialized journals and on peer recognition, is not conducive to policy-relevant research. Finally, an organization operating for profit depends on the financial support of its clients and consequently tends to concentrate on short-run and limited problems, like in-house government policy research. And since, in addition, a consulting firm must show "concrete results" to justify its fee, it will tend to look at the more easily quantifiable aspects of policy problems, where standard tools and techniques can be applied directly.

The nongovernmental, nonprofit form of organization has emerged as a response to the failure of other institutional arrangements to provide a congenial atmosphere for carrying out fundamental, independent, multidisciplinary policy research. This is not to say that the results have been uniformly good. In fact, nonprofit institutions present their own characteristic problems and dangers. To a large extent, these are related to the lack of generally accepted

⁵⁴Bruce L.R. Smith, *The RAND Corporation*, cit.

⁵⁵For a related discussion, see William Gorham, "Why Policy Research Institutes?", IIASA Research Memorandum 75-56, November 1975, and Bruce L.R. Smith, cit., ch. 2.

⁵⁶P.M.S. Blackett, op. cit., p.175.

criteria for evaluating their performance, and to the ever-present possibility of conflict between professional excellence and practical effectiveness. These issues will be discussed in the next section.

6. THE EVOLUTION OF CRITERIA OF QUALITY AND EFFECTIVENESS

The existence of suitable mechanisms of quality control is one of the distinguishing features of a well-established profession. Professional quality controls fulfill a double function: an internal one, to ensure adherence to group expectations about performance by members of the profession; and an external one, to ensure that the users of professional services can rely on their being of an acceptable quality. Ideally, the two functions, and the corresponding criteria, should integrate and support each other. In practice conflicts can and do arise, especially in the case of young professions like ASA, and then it is not clear which function should prevail. General prescriptions are useless, and only a detailed knowledge of the current stage and historical development of the profession can suggest sensible compromises.

Naturally, the importance of quality standards has been recognized since the beginning of ASA. Some of the citations given in previous sections from Blackett's early memoranda show this quite clearly; and the charter of the Operations Research Society of America states as one of the purposes of the society "the establishment and maintenance of professional standards of competence for work known as operations research." But for many years the issue of quality standards remained dormant, only to explode in the early 1970s—in a form for which the analytic profession was intellectually unprepared. A knowledge of these developments is helpful for understanding the nature of ASA as an intellectual craft, and its evolution.

The first practitioners of operations research had little doubt that what they were doing was scientific in character, despite the differences in the objects of inquiry—military operations or, more generally, man-machine systems—from those of traditional scientific research. The main goal of

operations research was "to find a scientific explanation of the facts." For, as C.H. Waddington explains, "[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."⁵⁷ According to Blackett's crisp formulation, "[o]perational staff provide the scientists with the operational outlook and data. The scientists apply scientific methods of analysis to these data, and are thus able to give useful advice."⁵⁸

Similarly, in the definition of OR adopted by the Operational Research Society of Britain, the word "science" or "scientific" occurs three times. Operations research is 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.

Given this paradigm, the relevant standards of quality are those of the natural sciences. In fact, the situations investigated by operations researchers during the War fit the paradigm quite well. Typically, military operations could be regarded 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."⁵⁹ The first industrial applications of the post-War period presented many of the same features and, as in the case of Leslie C. Edie's "Traffic Delays at Toll Booths," explicitly appealed to the same scientific criteria of evaluation and criticism.

Another important characteristic which early industrial OR shared with military OR was a reasonable clarity in the definition of the roles of analysts and decisionmakers. Whether the users of analysis were high-level officers or high-level managers, analysis was done primarily, and often (because of the

⁵⁷C.H. Waddington, *OR in World War 2*, London: ELEK Science Ltd., 1973, p.26.

⁵⁸P.M.S. Blackett, *Studies of War*, cit., p.171.

⁵⁹Hylton Boothroyd, *Articulate Intervention*, cit., p.113.

requirements of military or industrial secrecy) exclusively, for them. The analyst did not have to address himself to any audience other than the decision-maker, or a small group of decisionmakers, who had commissioned the study. Problems of implementation could be safely assumed to be the responsibility of a well-defined hierarchical authority, and the same authority could establish, if not standards of quality, at least criteria of effectiveness.

Already in the early 1950s all this was changing, at an increasingly rapid rate. Changes in personnel were accompanied by changes in the nature of the problems analysts were investigating, and in the institutional context in which analysis was done. As natural scientists like Blackett, Waddington, and Morse were returning to their university departments and laboratories, the new generation of analysts entering the profession—mathematicians, logicians, statisticians, control theorists—was more interested in the formal aspects of scientific research, and often lacked the craft skills and the maturity of critical judgment of the old masters.⁶⁰ At the same time, the problems claiming analytic attention were becoming more abstract and complex. Strategic issues, whether in business or government, loomed increasingly important on the frontier of professional thinking and practice. Subjective uncertainty was seen to be much more crucial than statistical regularities or deterministic models. And the increasing role played by ASA in the public sector meant that analysts—no longer discreet advisors to the prince, but actors in a political process in which advocacy and persuasion could not be neatly separated from objective analysis—had to pay attention to questions of equity, and of institutional feasibility. The high uncertainty surrounding strategic problems and the long times needed to implement a proposed solution also meant that direct empirical verification of analytic conclusions was often impossible. If in 1953, George E. Kimball could still say of

⁶⁰By now a new generation of officers and analysts has come on the scene, many of whom have never had the sobering experience of seeing their optimistic predictions disproved by deaths on the battlefield. They too often are willing to take the assumptions given them by designers and by 'intelligence' as gospel truth, and to base their calculations on them without adding any correction factors for 'the fog of war'. Philip M. Morse, Letter to the Editor, *Operations Research*, vol. 20, no. 1, January-February 1972, pp.239-242, 240.

operations research that:⁶¹

It is based on the conviction that the factors affecting ... operations can be measured quantitatively and that there exist common laws obeyed by the basic variables... . The main problems concerning operations research today are the discovery of such laws and the development of techniques ... for rapid, simple application...

his younger colleagues were increasingly skeptical about the possibility of discovering "laws of operations," and whether, indeed, the discovery of laws was a meaningful professional aim. At the time Kimball was writing, the move away from description (and generalization) to prescription as the hallmark of the systems analyst was already clearly discernible.

Now, the implications of these developments for the search for professional standards of quality are quite far-reaching. If it is no longer possible to believe in the objective validity of the conclusions of an analytic study, and if even the criteria of success of the decision it supports are ambiguous, then evaluation by results becomes meaningless, and must be replaced by such process-oriented criteria as internal consistency and professional (or even political) consensus.

The shift toward process-oriented evaluation is quite visible in the list of 13 criteria worked out by the Lanchester Prize Committee in 1957 in order to supplement the broad, but not very operational, guidelines adopted by the preceding committees.⁶² However, the issue of professional standards for ASA came truly alive only in 1971, with the publication of "Guidelines for the Practice of Operations Research" prepared by an Ad Hoc Committee of the Operations Research Society of America.⁶³ The particular controversy that led to the formation of the Ad Hoc Committee does not concern us here, except for the fact that in that controversy well-known analysts, applying standard technical tools to the same policy issues, had come to opposite conclusions and recommendations.

⁶¹George E. Kimball, "A Philosophy of Operations Research," Abstract, *Operations Research*, vol. 1, 1953, p.145, cited in Harvey M. Wagner, "The ABC's of OR," *Operations Research*, vol. 19, no. 6, October 1971, pp.1259-1281.

⁶²See Report of the Lanchester Prize Committee, *Operations Research*, vol. 5, no. 4, August 1957, pp.575-578.

The primary concern of the Committee was, in the words of the President of the Society, "the professional conduct of the debate, the quality of the argumentation, the adherence to established study procedures in operations research and systems analysis."⁶⁴

Unfortunately, most comments on the report of the Ad Hoc Committee were directed at an Appendix where the behavior of some of the participants in the substantive debate had been severely castigated. With a few notable exceptions,⁶⁵ the Guidelines themselves received little attention, aside from some cursory remarks on their innocuous, if laudable, character. This is indeed a pity, for the ASA profession could have greatly benefited from a critical examination of the specific standards proposed, and of the outdated philosophy of science on which they rested. The philosophy of the report gives great emphasis to two dichotomies: "pure" and "applied" science on the one hand, and "analysis" versus "advocacy" on the other. I shall now briefly indicate why these distinctions are irrelevant, if not positively misleading, in the context of a discussion of professional practice.⁶⁶

To begin with, the most significant similarities between science and ASA are to be found not in the *outcome*, but in the *process* of research, more precisely, in the craft aspects common to all forms of disciplined intellectual inquiry. The actual work of the scientist requires knowledge that is acquired only through practice and precept and which therefore is not scientific in character. This craft knowledge is a repertoire of procedures and judgments that are partly personal, partly social. Thus, when a scientist decides whether a batch of data is of acceptable quality, he applies standards that derive from his own experience, but also reflect the professional norms of his teachers and colleagues, as well as culturally determined criteria of adequacy. Personal and social judgments are

⁶³*Operations Research*, vol. 19, no. 5, September 1971, pp.1123-1148.

⁶⁴*Ibid.*, p.1123.

⁶⁵Two such exceptions are Harvey M. Wagner, "Commentary on ORSA Guidelines," and Ian I. Mitroff, "The Myth of Objectivity or Why Science Needs a New Psychology of Science," both in *Management Science*, vol. 18, no. 10, June 1972, pp. B-609 to B-613, and B-613 to B-618, respectively.

⁶⁶For more detailed arguments, see Giandomenico Majone, "Policies as Theories," *Omega*, vol. 8, no. 2, 1980, pp.151-162, and "The Craft of Applied Systems Analysis," Laxenburg, Austria, forthcoming, as well as the papers by Wagner and Mitroff cited in the preceding footnote.

also involved in data manipulation, in the choice of tools and models, in the selection of evidence, and in the construction of an argument.

The importance of craft knowledge and experience is even greater in ASA. Because the conclusions of a systems study cannot be proved in the sense in which a theorem is proved, or even in the manner in which propositions of natural science are established, they must satisfy generally accepted criteria of adequacy. Such criteria are derived not from abstract logical canons (the rules of the mythical "scientific method") but from craft experience, depending as they do on the special features of the problem, on the quality of the data and limitations of the available tools, on the time constraints imposed on the analysts, and on the requirements of the sponsor and/or decisionmaker.

In short, craft knowledge—less explicit than formalized theoretical knowledge, but more objective than pure intuition—is essential for doing systems analysis as well as for evaluating it. Not artificial distinctions between pure and applied science, between analysis and advocacy, but close attention to the fine structure of the analyst's task is what is required for serious evaluation. This structure can be described in terms of categories like data, information, tools, evidence, and argument that are applicable to any type and style of analysis, retrospective as well as prospective, descriptive as well as prescriptive, argumentative as well as "scientific." Take, for example, the category "evidence." Evidence is not synonymous with data or information; it is information selected from the available stock and introduced at a specific point in the argument in order to persuade a particular audience of the truth or falsity of a statement. Selecting inappropriate data or models, placing them at the wrong point in an argument, or choosing a style of presentation which is not appropriate for the intended audience, can destroy the effectiveness of information used as evidence, regardless of its intrinsic cognitive value. Hence, criteria for assessing evidence must be different from those for assessing "facts." Facts can be evaluated in terms of standard scientific criteria, but evidence must be evaluated in accordance with a number of factors peculiar to a given situation,

such as the specific nature of the case, the type of audience, the prevailing "rules of evidence" (including, of course, all relevant scientific rules), and even the persuasiveness of the analyst. Thus the assessment of the quality of the evidence presented in an analytic study is a microcosm of the complex social process of evaluation in which scientific and extra-scientific, objective and advocacy elements are inextricably intertwined.

Analogous problems arise in evaluating the practical effectiveness of ASA studies. Unlike the analyses of military operations conducted in wartime, and some small-scale industrial applications, it is extremely difficult, as already mentioned, to evaluate the usefulness of large-scale policy studies in terms of actual results produced. This is due to a number of reasons. First, the long time lag between the adoption of a policy recommendation and its actual implementation. Second, the difficulty of sorting out the effects of a particular decision from among a multitude of confounding factors. Third, and most important, the social and institutional context in which systems analysis is done has changed dramatically in the last two decades. In the early days the relationship between decisionmaker and advisor, between producer and user of analysis was much clearer than it is today. This is still reflected in the ORSA "Guidelines for the Practice of Operations Research," though the description given there of the client-analyst relationship was probably already outdated at the time the Guidelines were published. Now it is quite common for policy research to be sponsored by one organization, carried out by another, utilized by a third organization, and perhaps evaluated by yet another agency (which, in turn, may commission the evaluation to an independent research group). Clearly, the criteria of effectiveness of the sponsors are not the same as those of the users, or of the controllers. Thus the analyst must attempt to satisfy a number of different, sometimes conflicting, expectations. The best he can do is to achieve some acceptable level of adequacy in each direction: he must "satisfice," rather than maximize any one particular criterion. Actually, the situation is even more complex than this, for many policy studies in fields like energy, risk assessment,

or education are "designed to influence congressional debates and to affect the climate of public opinion, not to guide decisions within individual corporations."⁸⁷ The effectiveness of such analyses can only be measured in terms of their impact on the ongoing policy debate: their success in clarifying issues, in introducing new concepts and viewpoints, even in modifying people's perceptions of the problem. Here analysis is no longer separable from social interaction as a problem-solving device, but becomes an integral part of the process by which public issues are raised, debated, and resolved.⁸⁸ In fact, the historical development of ASA provides additional evidence for the truth of the statement that "creation of a *thing*, and creation plus full understanding of a *correct idea* of the thing, are very often parts of one and the same indivisible process and cannot be separated without bringing the process to a stop."⁸⁹

In the following, concluding section of this chapter it will be argued that the unavoidable complexity of the language of systems analysis reflects the fundamental difficulty of separating ideas from action.

7. CONCLUSION: THE LANGUAGE OF ASA

As the preceding pages show, the question: How scientific is systems analysis? (or operations research, or management science), keeps recurring throughout the history of ASA. 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 persuasive, and hence it 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 this argument is basically unsound since, as

⁸⁷Alan S. Manne, Richard G. Richels, and John P. Weyant, "Energy Policy Modeling: A Survey," cit., pp.1-2.

⁸⁸On social interaction as a mode of problem solving, see Charles E. Lindblom, *Politics And Markets*, New York: Basic Books, Inc., 1977. Whereas Lindblom treats analysis and social interaction as alternative ways of solving social problems, I stress the difficulty of separating the two in practice.

⁸⁹Paul Feyerabend, *Against Method*, London: NLB, 1975, p.26. Italics in the original.

Hume showed two centuries ago, there is no logical bridge between "ought" and "is."

Why do methodologically conscious systems analysts keep raising the question about the scientific status of ASA, despite repeated failures to answer it satisfactorily? The reason, I suggest, is that behind it loom two issues that analysts rightly feel to be of crucial importance for an understanding of what they are trying to do. First: what is the language of ASA, i.e., what is the logical status of the different propositions an analyst produces in the course of his work? Second: what standards of quality and rules of methodological criticism are applicable to the different kinds of propositions?

The historical evolution of the second issue has been outlined in the preceding section. In discussing the first issue, I shall make use of some concepts introduced there. ASA, I have argued, is a craft. The systems analyst as craftsman is a producer of data, information, and arguments, but also a social change agent. He must influence some people to accept his proposals, and other people to carry them out; he is expected to take some responsibility for implementation. "Experienced practitioners realize that such implementation depends not only on factual analysis, but also on the client's organizational structure, the capabilities and biases of the client's personnel, and the client's management style."⁷⁰ In particular, successful implementation depends on the ability to persuade people that a proposed course of action is not only good for the organization, but also compatible with the self-interest of its members. A well-designed incentive system is a very effective form of persuasive analysis.

Often the analyst must even persuade the decisionmaker. For example, one of the important functions of systematic analysis is to point out what cannot be done, rather than what can; in other words, it is the duty of the analyst to make the decisionmaker aware of constraints that he would rather ignore. But aside from straightforward physical and resource constraints, it is not usually possible to give a logically tight proof that a certain factor is an actual constraint, rather

⁷⁰Harvey M. Wagner, "Commentary on ORSA Guidelines," cit., p. B-611.

than simply a "problem." Hence the decisionmaker must be persuaded to accept some limitations on his freedom of choice on the basis of something less than a full proof.⁷¹

Perhaps we can see now why the long-drawn debate whether ASA is descriptive (like "pure science") or prescriptive (like technology) has been so fruitless. ASA is concerned with theorizing, choosing, and acting. Hence, its character is three-fold: descriptive (scientific), prescriptive (advisory), and persuasive (argumentative-interactive). In fact, if we look at the fine structure of analytic arguments we see a complex blend of factual statements, methodological choices, evaluations, recommendations, and persuasive definitions and communications. An even more complex structure emerges when we look at the interactions taking place between analysts and different audiences of sponsors, policy makers, evaluators, 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 proving or refuting an argument conclusively. As I have tried to show, the historical pattern of development of ASA can be interpreted as the progressive realization of the complexity of the language of policy advice, and the slow evolution of appropriate forms of criticism.

⁷¹For a more detailed argument and some examples, see Giandomenico Majone, "The Feasibility of Social Policies," *Policy Sciences*, vol. 6, 1975, pp.49-69.