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In America in this century we have seen a number of different approaches to the study of organizations. Some of these approaches have relied primarily on descriptive materials garnered by interviews, questionnaires, and other types of observation. Usually the research studies have been done in the style of the different disciplines. Sociology, for example, has traditionally emphasized the questionnaire-interview method; political science has in the past relied very strongly on law and political theory as a basis for its observations. In any event, despite the differences in methodologies, the basic idea has been that the researcher does not make any value judgments about the manner in which the organization carries on its business.

A different approach to organizations has been based on the notion that the researcher wishes to arrive at normative judgments. One such approach was to discover the "principles" of organizational planning. A famous example was the concept of span control, with the associated principle that the manager should have no more than seven persons reporting to him.

In the last two decades, we have seen a quite different approach to the normative study of organizations, which is frankly technique oriented and goes under such labels as operations research, systems analysis, or management science. It is this approach which I want to discuss in this paper in detail, because I have a feeling that in the 1970's we will see a basic change in its philosophy, a change that may mean a great deal in the manner in which organizations are studied in the future by means of models, simulations, and the like.

The following is a brief description of the basic methodology and philosophy underlying the normative technique approach to the study of organizations.

First, the underlying philosophical assumption of systems analysis as applied to organizations is that organizations are goal oriented in a very special sense. The approach goes far beyond the simplistic notion that organizations are purposive. It has specifically assumed that there is a highly centralized goal structure, and that this goal structure can be translated into a "measure of performance": profitability, benefit minus cost, social utility, or whatever.

Second, the approach has assumed that the organization can be subdivided into components which themselves have sub-goals. What systems analysis recognized in the 1950's, which apparently had not been recognized clearly by earlier organization theorists, was that these sub-goals must necessarily be in partial conflict. In the 1950's, many operations researchers considered this to be a great step forward in the analysis of organizations. We believed that earlier students of organizations had assumed that the organization is one vast cooperative effort, whereas realistically it is easy enough to see, say, in an industrial organization, that there is a true conflict between the various divisions of the organization and especially between marketing and production, or between finance and marketing. Also in the public sector there is inevitably a deep conflict between educational programs and defense programs, or between health and education, and so on.

So enthusiastic were we about the notion that one could successfully study organizations in terms of the conflict between divisions, that we were so bold as to suggest that operations research could model the "whole system." We compared this idea to the traditional approach to organizations which have concentrated on one aspect or one division of the total organization. The idea of conflict is well illustrated in inventory models or in linear programming. In inventory we recognize that there is a basic conflict between the production-marketing department's goal of satisfying customer demand and the finance department's requirement to make sure that not too much capital is tied up in inventory. In the mathematical programming the conflict structure is expressed in terms of the constraint equations on the overriding objective function. The constraint equations often represent divisional constraints, either in terms of policies or manpower limitations. These constraints prevent certain activities from becoming too large, although growth may be one of the overriding goals of that particular activity.

Thus the mathematical model (or simulation) expresses the underlying conflicts and provides the basis for resolving them. Thus in mathematical programming, one maximizes a measure of performance, which is expressed as a function of levels of activity of each component of the organization, subject to certain constraint equations.

Third, the systems analyst was required to set the system boundaries. He realized it was not feasible to study the whole world or even a nation in any realistic way. In order to be feasible, it was necessary to set the boundaries of the system so that the analysis could proceed in an orderly fashion. The boundaries were set by the systems analyst by identifying a decision maker, who is usually the chief client of the systems analyst. What lies within the system is what this decision maker can control and change; what lies in the environment of the system are the things which he cannot control but which nevertheless effect the performance of the system. Naturally, in practice there has been a great deal of art in deciding who the decision maker is and what the boundaries of his activities must be.

Hence the complete model represents the measure of performance as a function of controllable variables and uncontrollable variables. The latter are often expressed in terms of some uncertainty calculus (e.g., classical probability theory, Bayesian statistics, or game theory).

Fourth, the systems analyst's task is to identify one or more important problem of the decision maker and to formulate the problems so that they can be expressed in terms of a model. The model may or may not be mathematical in character in the sense that the deductive procedures may or may not be completely precise. But the model must be rich enough to lay out the alternatives available to the decision maker and to enable the systems analyst to estimate optimal solutions. I use the word "estimate" advisedly, because no systems analyst ever believes that the solution he was offering is inevitably correct. Like all scientists, he recognizes that there may be deep errors involved in the assumptions that he has made. Furthermore, a good deal of systems analysis takes place under severe data limitations and gaps. We are lucky to be able to obtain the kinds of information that are needed even to make rough estimates. This situation, as I said, is characteristic of all science; nevertheless, systems analysts in the USA have not been overly humble in offering their "optimal solution" to decision makers.

Fifth, the systems analyst must attempt to implement his "solutions." That is, he should have an active role in trying to persuade or teach the decision maker that his recommendation is the correct one and should be acted upon. It is true that in the early stages of systems analysis this particular step seems often to have been ignored, especially in the 1950's. It is accurate to say that many analysts simply did not have the opportunity or did not wish to try to implement their recommendations.

Instead, the studies were made and reports written with the hope that the manager would read the report and carry through the suggestions into action.

The 1960's, on the other hand, saw a considerable increase of interest in implementation of operations research and systems analysis solutions. Here we began to borrow rather heavily from other disciplines, and especially psychology and sociology which have had a continuing interest in what is called "technological utilization." The spirit of the studies of implementation of the 1960's is interesting to note. In many of them it is clear that the implementation problem is posed as follows: how can one persuade the decision maker that the solution is the correct one? A more plausible but often neglected alternative is to take the failure of implementation to be a sign that the systems analyst's approach has been incorrect. This point is philosophically very important. Traditional science has developed various methods of verification or refutation of theories. These often consist of various designs of experiments. In systems analysis the final test is whether or not the solution will be implemented by the organization. If it is not, then the systems analyst must regard this result to be a refutation which implies that he must revise his model or his interpretation of his data. We have slowly come to realize that managers may have excellent reasons for not implementing what we take to be the best estimates of optimal solutions.

Sixth, whether or not the implementation succeeds, if the systems analyst survives, he goes back to step four again. That is to say, he seeks new problems, new problem formulations, and hopefully keeps the process of solution and new problem formulation going forever.

I want now to turn to a critique of this basic underlying philosophy of systems analysis with respect to organizations.

Strangely enough, the step which has been singled out in the literature critical of the systems analysis in America has been the first. This step, to repeat, states that organizations can be viewed as goal oriented and specifically as having a highly centralized goal structure which can be translated into a measure of performance. The critics have tended to argue that not all organizational goals can be quantified. They have argued that safety, responsibility to personnel, and other welfare considerations cannot be translated into numbers. Thus, they might agree that a highway traffic system can be measured in terms of "throughput" of cars, but they claim that the other goal of the system, the safety of the driver, cannot be so translated. The label that was often used in this criticism is "reductionist," which is almost as bad as being a revisionist. But we weren't reductionists at all, because essentially we were following the philosophical tradition of rationalism. We were arguing that organizations, and specifically the decision makers in organizations, ought to be consistent in their goal-seeking. If they are consistent, and if an economic goal is one part of the goal structure, it is possible in principle to infer that all other goals can also be translated into economic values. The logic is fairly simple to illustrate. Suppose that the measure of performance of a highway is expressed as a function of the economic benefits and costs and the safety record. If a rational decision is made about the design of the highway, then by straightforward mathematics one can infer the imputed economic value of safety. The technique is often used in systems analysis to infer the costs of loss of limb or life, or illness, and so on.

I really doubt that the way our critics aimed their arrows at us was valid, particularly since there was a much deeper criticism that we are gradually beginning to understand as we go into the 1970's. This criticism comes from the realization that at least in the public sector of the USA systems are tending to become "pervasive." To illustrate this point, suppose we consider higher education. Traditionally, we have said that the organizations whose purpose is higher education are composed of three kinds of individual: (a) the students, and specifically those roughly in the age bracket of 17 to 24 with a few stragglers on either side, and (b) the decision makers, whom the systems analysts try to serve; these are of two kinds: the administrators, who decide on the amount of support and its allocation to curricula, and the faculty, who decide on the method and content of curriculum. Hence, in the traditional viewpoint, administrator, teacher, and student are clearly different classes of people, with obviously an occasional overlap here and there. If we use the philosophy of systems analysis stated above to study systems of higher education, we might try to build an input-output model. This has actually been done in several instances in the United States in the past few years. Here the input is measured in terms of funds and the output is measured in terms of student graduations translated, perhaps, into socio-economic benefits.

However, in recent years we've been witnessing what I've called above the pervasive tendency, in which students wish to take on much more emphatically the role of decision makers, both as to the support of curricula and their content.

I said above that the systems analyst in the 1950's tended to regard the decision maker, i.e., the administrator or the faculty, as the chief client of his studies. But now we are beginning to realize that he may have been seriously in error. If there's a strong tendency at work for everyone to be the client, and especially the student, then a model which allocates resources by ignoring this tendency must surely be unrealistic and, indeed, "impractical." In America there does seem to be a strong evidence of the pervasive tendency in education. Education is a womb-to-tomb endeavor. Students wish to decide what they should be taught; faculty wish to decide how much funding is needed, and so on.

In order to make the point about persuasiveness more sharply, consider the often-mentioned need to consider second, third, and nth order effects of social policies. This kind of consideration belongs to the traditional systems analysis discussed at the beginning of this paper. Every engineering designer knows that in designing a machine to move a large mass of dirt, one has to consider the second order effects of the temperature of the motor and the danger of explosion.

But pervasiveness is a different kind of consideration. The engineer can set the boundaries of his machine and talk realistically about the machine and its environment. He is not apt to be concerned with the tendency of the machine to become everybody (although at times the computer enthusiasts in America do get to talking this way). It is not the second and nth order consequences that concern us in the pervasiveness of the system, but rather the spread of the system across a large sector of humanity.

If there is a strong tendency on the part of systems to become pervasive, then I think that there are some important implications with respect to the remaining five points listed above under the description of the basic philosophy of systems analysis.

The second point was that systems can be divided into components. But more important the second point was that the conflict between the components can be resolved in terms of the total system goals. This point is beautifully illustrated by the so-called decomposition algorithm, where there is a sequential resolution of a conflict in terms of the objective function of the central department of the organization.

But I think that the concept of pervasiveness indicates that the value systems of the individual within organizations have become as primary as the organizational goals themselves. Indeed, if one looks at the philosophy underlying the traditional systems analysis, one sees that the analysis could be applied to machines as well as to social organizations. Something of this sort has actually occurred, say, in the works of Jay Forrester (Industrial Dynamics, Cambridge: MIT Press, 1961 and Urban Dynamics, Cambridge: MIT Press, 1969). Forrester's approach is borrowed, in fact, from an engineering approach to the design of machine systems. If the components of the system themselves have their own value, and if these values cannot be resolved in terms of system's goals as decided by an elite decision making body, then we can no longer let the system objective be the overriding consideration.

What I think is required is a dialectical approach. We have to say that, in effect, organizations can be viewed as seeking well structured goals, but any consideration of the attainment of these goals has to be viewed from the point of view of the individuals in the system as well as the total system. We are, in effect, as systems analysts, obliged to give up one traditional concept, namely, that there is such a thing as the best way of looking at the whole organization. In America no one knows clearly how we proceed in our capability of looking at the system from these two points of view—from the total system point of view and from the individual point of view. But it is essential that we explore the two viewpoints, and especially the individual-oriented one, in great depth, else the entire value of systems analysis may be lost. Our aim should be to invent "models" of organizations in which individual values form the basis of decision making; these models, I think, need to be far richer than classical welfare economics suggests.

Next, I said that in the traditional point of view it was necessary for the systems analyst to recognize that the system has boundaries determined by the control capability of the decision maker. But if the pervasive tendency is a correct one, then the decision maker is constantly changing, as is the client of the system, and so also are the boundaries of the system. Systems analysis now becomes far more dynamic than it was under the traditional approach. The analyst has to have a capability of changing the system boundaries even within the context of a specific piece of research that he's doing, because in reality the system boundaries may be changing in such a manner.

The last three points in the traditional approach state that the analyst must seek solutions, attempt to implement them, and then go on to subsequent problems. But the concept of solution becomes quite different under the new philosophy. There will still be solutions, i.e., there will be stages in the life of the systems analyst when he proposes actions on the basis of his research effort. But now solutions are means to a larger design effort. They serve two very important functions. First, the solutions sweep away what we don't want to belabor too long so that we can work on the main exciting, dramatic, and important aspects of social design. For example, inventory control, production, and distribution scheduling have often become this type of solution in America through "packaged" computer programs; in effect they help to clean up the shop in order that we can study more critical problems. But second and more important, solutions enable us to take design into the real world and observe what becomes of it. The key concept here is the process of learning. We regard the solution not to be a solution of the manager's critical problems primarily, but a way in which the manager and the systems analyst together can learn about the organizations in which they live.

It is important to note in this regard that the act of observing whether a suggested solution "works out" cannot be determined in an unbiased manner. All observation in systems analysis is based on an assumption about the nature of reality, and different judgments will occur depending on which assumptions are made. Implementation of design, therefore, provides still another basis for exercising our ability to create imagery, which is the heart of all systems design.

If I am correct in saying that there will be an important shift in the basic underlying philosophy of systems analysis, then I think such a shift must be labeled a "new science." The traditional forms of science in which scientists were obliged temporarily to settle on one model of reality and one way of gathering so-called objective evidence must be relinquished. "Techniques" will continue to be an important part of the future of systems approach, but they will be minor compared to the attempt to see how systems analysts' capability can be used in an era in which individuals will have more and more of a role in the decision making of organizations, and in which problems such as health, education, and production, and so on, become more and more indistinguishable.