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IMPLEMENTING MODELS FOR RESOURCE ALLOCATION AND MANAGEMENT

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INTRODUCTION

It seems that students of economic policy and of logistics systems have at least one common problem and interest: we are both becoming more and more concerned with the implementation question. How does the student -- the research worker -- decide he has a proposal that ought to affect policy and how do he and his sponsor bring it into operation? Or, looking at the negative side, do ideas fail to make an effect because they are irrelevant or faulty; because they did not gain acceptance at the policymaking level; or simply because they could not be put to use in a practical way?

We do not propose to answer these questions in any complete manner here, nor, in particular, are we going to tell anyone how to implement a system or a policy. But it is important that government and other activities make fuller use of economic research results, and we will try to throw some light on the subject by reviewing some highly relevant experience from our particular area of work.

First, let us try to characterize the general area and the kind of problems we have worked on in the RAND Logistics Department over the past thirteen years. The very large, multiplant industrial firm,

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perhaps one in the transportation industry, is the closest economic counterpart to the Air Force logistics system. It is big by almost every standard: one hundred and fifty or so major air bases (and a much larger number of less important stations) to be served, an annual budget of around $10 billion*, with several hundred thousand people engaged in logistics operations. The system has many of the characteristics of a managed economy: there is an underlying objective of economizing on use of inputs, a large number of activities must be coordinated, and the system possesses an important degree of management decentralization. It is obviously also a very limited economy. There is no production for final consumption or exchange of final products, no money system, and so on.

We have a broadly written research contract with the Air Force permitting us to select the topics we work on. However, such selection and research activity involves a continuing interaction with the Air Force. Our points of contact are primarily the logistics sections of the separate operating commands, the Logistics Command, and Air Force Headquarters.

A study of some particular aspect of the overall logistics system typically begins with consultations and empirical work with one Air Force element, chosen because it exemplifies the problem under study. Since the different bases and management areas (such as depots) are similar, with procedures that are becoming increasingly standardized, results can be communicated throughout the organization quite efficiently.

However, although our goal is general policies, affecting overall improvements, success in one situation does not insure success elsewhere. There are differences in military mission, hardware, or other circumstances that affect the appropriateness of the proposals, and, of course, there are differences in the adaptability of the organization involved to new methods.

Further, even though a military organization is a monolithic structure in principle, implementation is not achieved by mere command

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*We are referring here to expenditures we might loosely define as "logistics goods and services."

or fiat. Many staff elements must concur in the wisdom of a proposal, and it may be necessary to develop new technical capability and understanding. Implementation is a complicated process which we understand very imperfectly.

RESOURCE MANAGEMENT AND ALLOCATION STUDIES

In this section, we should like to characterize RAND's efforts and attempt to show what sorts of studies we get into. We like to divide our studies into three categories: policy studies, studies relating to structure and organization, and system improvement studies. Obviously, these separate categories have a great deal in common, and studies in one sector can be used to augment or extend research in another of the areas. But it will help to highlight the implementation activity if we treat the categories as entities in our discussion below.

In policy studies, we are concerned with advising the Air Force on positions relating to specific matters of policy in various fields. These are usually one-time decision problems, and in providing such assistance we rely strongly on what our research has taught us. In one recent important study for the Air Force, we were asked to help establish the efficient mixes of resources required to organize the tactical forces in the 1970 time period. These estimates were required to establish programs that would provide the tactical forces with certain desired capabilities at that time. In providing this assistance, we drew heavily on our research, and as a result the Air Force staff that was responsible for evaluating these programs had to evaluate carefully the validity of the techniques we had helped to introduce. This evaluation, in terms of an explicit solution, helped to increase understanding and appreciation of the usefulness of such techniques.

On another occasion, we were asked to help the Air Force comment on certain proposals being considered by the Department of Defense for reorganizing the large maintenance system of the Department. We drew heavily on our prior research, and could present a systematic analysis of our view as a result of this work. In such policy advice, as well as in all of this assistance, we are acting in the role of advisors; the Air Force is not bound by any of our proposals.

In studies relating to structure or organization, we view the Air Force as a productive unit with certain outputs and inputs, and we may try to determine the desirable level of integration of its facilities, the preferred size of its activities, how it should be organized for efficient operation, and so on. In this effort we have made much use of simulation models. The complexity of the decision process, the variety of information used, and the difficulty of specifying "optimal" policies, all lead to the active use of computers to represent these processes. In addition, the significant interaction of the decisionmaker with his information processing activities also leads to the employment of man-machine simulation techniques for systematically exploring the actions of the decisionmaker in response to his organizational environment. Such tools are valuable for understanding the complexities of real-world decisionmaking, and to developing the supporting technology for the decisionmaker.

In system improvement studies, we are dealing with the more traditional aspects of economic or managerial research. We are studying the recurring types of decisions that help to control operations effectively. Resource allocation is a predominant concern in such continuing management activities, and here we can use the available body of economic thought, which must, however, be adapted to specific situations. Let me discuss briefly two such applications, since they will furnish material for the next section on the Implementation Process.

One of these studies is the so-called Ferguson-Fisher technique [2] for establishing stock-level and ordering policies for low-cost parts. The developers of this technique were two economists, A. R. Ferguson and L. Fisher, who did this work at RAND in 1957-1958. Their research produced a very rational way of looking at the problem, and involved balancing off the gains and costs of having too much or too little stock. It also looked across the mix of resources involved in such an inventory process, and recognized that one can trade off inventory costs for administrative costs, something that was only imperfectly done at that time.

The other study is the so-called base stockage model, originated by George Feeney, Craig Sherbrooke, and James Petersen in 1963 [3]. Here, too, we have a rational approach to the problem of determining the stockage of recoverable spare parts, which, in this case, are expensive items, so that investment cost is the prime cost consideration in the decision.

The Ferguson-Fisher technique, despite its careful analysis, never achieved acceptance, while the base stockage model has been very well received and subjected to comprehensive study, test, and use in the Air Force, and has spawned a host of further models that extend the rational use of theory in inventory management.

We shall try to explore the reasons for this contrasting reception by the Air Force -- and even by the research community -- in order to understand the factors that affect implementation. The two experiences chosen are but a very restricted sample of those encountered in our research for the Air Force. The essence of this overall experience is that we have barely scraped the surface of the implementation problem, and that we have very tentative hypotheses at best for all our experience and analysis.

The Ferguson-Fisher study produced a comprehensive analysis in the form of an optimal inventory policy for low-cost parts. The study provided ways of calculating these policies by means of computers, as well as look-up tables where such computers are not available. To use the procedure, however, it was necessary to estimate certain hard-to-obtain parameters, and to take account of certain difficult dynamic characteristics, some of them subject to great uncertainty. Now, this sounds critical of the procedure, but it would be more appropriate to recognize it as the state-of-the-art of inventory theory applications in 1958. In the intervening eight years, some theoretical insights have been obtained that have both contributed to more feasible approaches to the problem, and provided more satisfactory solutions. One very important advance is the "system approach." This has provided important capability for allocating a given budget over a range, or set, of items so as to maximize their overall effectiveness. The Ferguson-Fisher approach used an item-by-item analysis so that there was no direct way of instituting a specified overall budget or effectiveness target. The effectiveness goal was set for each item without having an overall measure. By contrast, the base stockage model gives a system-wide solution that is consistent with the overall goals and constraints, a capability that was made practical by advances in computer technology between 1958 and 1963.

Now let us comment briefly on how the two methods fared in implementation and use this experience to structure our experience in implementation.

The Implementation Process

It seems appropriate to ask, at this point, what we mean by the notion of "implementation." What separates the implementation phase from the other parts of a program of research? While we believe there is no valid, hard and fast distinction between activities supposedly devoted to problem formulation and model-building, and activities devoted to rendering the system or scheme practical and usable, it is certainly possible to point to several things that an implementation effort typically must achieve.

Certainly, early in a study there must be anticipation, and later the fact, of coping with the numerous details and imperfections that are involved in putting a new idea to use. Many studied compromises will be required. There is also the task of communicating the ideas and theory involved to the many individuals and activities involved. Very possibly technical training is involved, and often an advanced computer program must be transferred to the client and adapted to his computing hardware and exact problem. And possibly most important, something akin to a cultural change must be achieved in the organization that will receive the new system. Managers who are accustomed to viewing their jobs, responsibilities, and capabilities in certain ways must learn to trust new methods and to appreciate the value of more powerful tools.

How do we accomplish this? The eventual usefulness of a new system must certainly be a concern from almost the inception of an idea, but we think we can usefully distinguish three phases or steps upon which implementation depends.

The first of these we call "closing the engineering gap." The theoretical attractiveness of most ideas runs well ahead of our ability to engineer a system that can use them. For example, the base stockage model just referred to incorporated some new theoretical advances, but just as important was the intensive effort to put theory to work in a practical way. The task was to design a practical computer program that could apply a fairly complex type of calculation to a problem of very large scale. The result had to be cheap enough in computer time and simple enough in practice to encourage experimentation by the Air Force recipients as well as RAND.

The Ferguson-Fisher technique similarly passed this hurdle. Its computational demands were simpler, but it made use of the capabilities available at that time.

Another aspect of engineering relates to assessing the performance of the system under realistic conditions. It is important to have a way to assess the impact of these simplified models on real-world behavior. In addition, it may be desirable to have models in which policies can be embedded that permit the researchers to determine the problems of connecting their policies with real-world systems. In recognition of these needs of the researcher, the technique of simulation has been used [4]. Computer simulation is now extensively employed to test the sensitivity of analytic solutions to possible deviations from assumptions about the real world. In the base stockage model, for example, the sensitivity of assumptions about variance to mean ratios of the demand process was studied, as well as variation in resupply times.

Man-machine simulation is also used in this work to test the sensitivity of the solution to those decisions or actions which the man must take in his use of the system. Inevitably, in any of these complex management systems, man is used as a feedback or control device to link the various pieces of the system together, and in exercising these responsibilities, he can affect the system. Part of the engineering process is to give him rules or guidelines of behavior, and such guidance needs to be tested and, in part, developed in some realistic setting. We have found the man-machine simulation laboratory to be very useful and more practical than using the real-world organizations for such development work.

The second phase is the test, usually or preferably a "live" field test. In this phase, two equally important facets are demonstrated: that the engineering gap has been or can be closed, and that the theory is relevant to the problem. In an intricate, detailed system, there may be too many assumptions to allow a simple "pass or fail" test, in the sense that we sometimes talk about testing hypotheses. The failure of the proposed system (if not due to unworkability) may arise from any number of details that really need not discredit the method (e.g., estimation problems, such as the variance to mean ratio in the base stockage model). The test design problem is to emphasize the crucial parts, usually related to the prediction aspects of the proposal. We may also couple the test with an evaluation of previous or competing systems. An important aspect of the test is to gain acceptance from the would-be users.

The Ferguson-Fisher method never passed this research hurdle. The difficulty of getting satisfactory estimates for the parameters, and the unavailability of computers required by the dynamic inventory situation seemed to undermine its possibilities. On the other hand, the base stockage model successfully passed this hurdle twice.*

The third phase is the final system design and implementation. By this stage of the proceedings, the original research staff has receded to the level of technical advisors. The user is now deeply committed to the technical problems of installing <u>his</u> new method in <u>his</u> existing system. The relationship between researcher and user during this period is a very complex one because problems are continually being uncovered, and it becomes very important to attack them in terms of both the theory and the implementing system. Hopefully, the theory will be left unchanged, but the limitations of data, computers, and the theory of system design are such that the theory may also have to make accommodations. All of this communication and understanding during the implementation process is greatly enhanced if the system users are brought in early, probably as phase one is completed and phase two is contemplated. It is through this process of education that the changes of attitude or belief of the future users of the system, which we earlier described as a cultural change, is begun. Their usual way of doing business will be disrupted by the new system, and they must be persuaded by the continual success and advantages of the change that the effort is well worth the cost. This condition is especially important in today's computer-based systems, which are centrally designed and controlled so that the users are much more impersonally related to what they do.

It would be hasty, however, to attribute all seeming inertia on the part of the ultimate system user during this phase to his resistance to innovation; he may be justifiably preoccupied with internal technical or procedural problems that the researcher has not clearly seen or to which he has not given proper weight. The point is that significant changes to large systems have many subtle interfaces with other major components within the system, and, even though capricious judgment is sometimes encountered, the "implementer" must be constantly alert to dimly perceived or totally hidden objections that have nothing to do with the intrinsic worth of his proposed innovation.

The base stockage model now stands on the threshold of this third stage. Information gathered during the second stage is being analyzed and evaluated as a prelude to proceeding to the third stage. Not all the implementation problems have yet been faced, but we have encountered enough of them to make inferences.

MANAGEMENT SYSTEMS VERSUS ECONOMIC POLICY MODELS

In order to provide insights for economic policy models, it might be useful now to summarize the reasons why some management decision models were more successful than others, from the implementation standpoint. All of these considerations have really been developed in what has been said earlier, so that what follows is really a summary of these points.

One rather obvious point in contrasting our experiences with the two inventory systems is that the world changed appreciably between 1958 and 1964. The use and understanding of computer-based systems had become much more widespread. Both proposals challenged the existing technology, but attitudes within the Air Force were perhaps more favorable by 1964. But what are some of the other generalizations that seem justified?

First, implementation is helped if there is a well-developed mechanism in the existing organization so that the proposal modifies the presently used solution instead of replacing it with a complete innovation. This is so because the identification of what the system is trying to do is more readily established, the necessary comparison with current performance is available, and the changes can be tailored better with a greater awareness of their impact on the existing system structure.

Second, it helps if you are proposing a solution to a recognized problem. It is often necessary to con ince the user that he has a problem to begin with, and then persuade him that you have the solution. To secure the user's interest in and support of a solution in such a case is far more difficult. In the base stockage case, we began our work with recoverable items at base level, recognizing that this was only a part of the total supply management system. Yet now, because the base stockage model has been so successful in its tests, we find it much easier to secure acceptance of our other supply inventory research on the lower-cost items, the multi-echelon policies we have developed, and the application of these ideas to mobility kits. And, of course, our efforts at communicating in 1964 may have been aided by our less successful attempts in 1958.

Third, we cannot stress too much the importance of appreciating the detailed problems of system design in proposing implementation. We have tried to do this in the base stockage model, and our staff, we are sure, almost has the feeling that it is performing the system implementation single-handed because of continual concern with the details of the computer program, the data bank, the rules for processing the supply actions, etc. Even so, we are encountering problems we have not previously considered adequately as the implementation proceeds. Computers have their good points, but they also are quite inflexible in that changes to computer-based systems always come hard.

Fourth, the user must develop an early technical understanding of the solution. This imposes a burden on the researcher that he may not be willing to bear. It is one thing to score breakthroughs, and it is another to suffer the frustrations of trying to convey your solution to a user who has a different set of values and frame of reference. But, communication must be achieved, and it gets easier as the broad concepts are mutually accepted, and as evidence of success accumulates. RAND has had a twenty-year history of continuous relationship with the Air Force, and this helps a lot in providing mutual confidence and assurance to the user that we will be around to support him.

Now, what do these lessons learned with management systems mean for economic policy models? At first glance, there seems to be a wide gulf between economic proposals based on models and the area of management systems. However, it may be that there is much more in common than meets the eye. For one thing, economic theory and economists are now active in fields where systems are required to exercise policy leverage. For example, the monetary system traditionally has been viewed in terms of longer-term influences; but now its controls seem to be approaching daily and weekly impacts, and the use of models is increasing. One gets the impression that this area has a strong need both for better policy understanding and improved system management, especially management that can be more discriminating in its effects.

The same is true of the far-reaching social programs now emerging, which have tremendous needs for management systems. The social welfare programs have had their systems, but their really stringent test is yet to come. Medicare will be susceptible to the shorter-term impacts of economic conditions, population mobility, fluctuations in disease patterns, and so on, and will stress the social welfare system far more than we may now anticipate.

The poverty and regional development programs are very dynamic in concept and in utilization. The requirement to sense needs, to respond quickly through resource allocation, and to increase performance are all characteristic of management systems. The growing pains of these programs are significantly caused by lack of such systems. The more basic weakness is understanding how to put economic policy in such a system context.

Efforts in foreign-aid programs suffer from the same defects. They are further complicated by lack of control over the recipient country, its special customs, and the complex of international relations.

There are two related points in this illustration and discussion of our experiences. First, there is obviously some carry-over of implementation "technique" from the area of management models to the area of economic policy models; that is, the approach to communication, design, test, and refinement of complex proposals. This raises questions; for example, who are the agents or activities that must be persuaded or influenced in any given case. These activities are relatively well defined within a management hierarchy, but not necesarily so in broader systems.

Second, successful implementation in economics is related to filling the gulf we observe between policy recommendations and the means for carrying them out. The consequences of economic policy may often imply changes in the behavior of people, instituti ns, and the government itself. What mechanism will be used to effect such changes in a way that is efficient and effective? Economic or program-budget planning is not enough. The operating systems for executing such planning, and for their management, are equally or even more important.

If such is the case, then the lessons presented here on implementing management systems and changes to existing systems may have much relevance to economic policymakers and those doing research on economic policy. At least, that was our thought in composing this paper for the present symposium.

CONCLUSIONS

Implementation, then, is a very complicated process, made all the more so because it entails changing the behavior of people and organizations through changes in policy. Systems have been created to facilitate exercise of policy. The researcher must study such systems as part of his efforts to find better policies. He must consider how the system must be changed if policy is changed, and he must therefore communicate with the users of the system as part of his effort. The range of technique that is relevant to the economic researcher is therefore broader than his traditional econometric models. He may need to employ simulation, and he will have to be interested in the characteristics of computers and data banks, the problems of communicating with computers and making changes to computer-based systems. At least, this has been the experience of the economists at RAND who have been seriously interested in policy research and implementation.

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