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A "Disjointed Incrementalist's" Approach to Measuring Benefits and Costs

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A "DISJOINTED INCREMENTALIST'S" APPROACH TO MEASURING RESEARCH BENEFITS AND COSTS[1]

by

Walter L. Fishel[2]

Introduction

The environment within which the research planning process must function is an extremely complex system of interrelated variables of diverse characteristics. Too frequently, I think, we tend to become easily discouraged and even intimidated by the complexity of the science system and the research process, as well as the social system in which these function. Our response too often is that the less we understand a situation, the more variables we require or attempt to use to explain it or at least to cope with it. But, experience in research teaches us in such circumstances that it usually is more useful to start simply, to explain as much as possible with as little as possible, and only then add more variables until what we gain seems not worth the cost of adding more complexity (see Ackoff, 1967, p. 94). While accepted as a good rule for conducting research, we have seemed to be slow in accepting it for planning research.

In my opinion, our failure to successfully contend with this complexity is the primary reason we have been less than successful in our efforts to develope and implement more formalized planning procedures for research either in the public <u>or</u> the private sector. In economics, probably more than in any other discipline, the problem of having to contend with complex conceptual

[1]Paper presented to a meeting of the Committee on Economics of Natural Resources Development, University of California Extension Center, San Francisco. October 26, 1970.

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relationships and informational requirements has presented substantial barriers to the creation of effective resource allocation techniques and procedures. The framework of economic theory itself is, of course, a highly complex set of interrelationships. To inject into this the evaluation of public expenditures on research could only lead to a situation such as that described in the survey article by Prest and Turvey (1965). Some comfort is provided by the knowledge that other disciplines have fared little better. Nevertheless, it is little wonder, as Baker and Pound (1964) point out, that the seniority attained by scientists devoted to studying these problems of resource allocation has been remarkably short!

Considering the talent that has been committed to this task over the years, "it is difficult not to be impressed with our inability to evaluate the results of R and D" (Horowitz, 1963, p.50). It does lead one to marvel and wonder. But mainly I wonder that if with all this talent we can not really succeed in doing some of these things, is it possible we are trying to do the wrong thing? This rhetorical question has led me to consider several alternative schemes for developing useful resource allocation techniques.

From this brief base, I would like to do three things in this paper. First, I will present a general structure that has been useful to me in evaluating and working on cost-benefit (CBA) methods, one which frees me from the biases in logic of any particular discipline; it is in this context that the rest of the paper is presented. Second, I will describe what is meant by "disjointed incrementalism", which is more precept than mechanics, and point out its principal implications to the resource allocation process. Finally, I will briefly describe one experiment in developing a specific CBA package based on this view of the allocation process.

The Structure of Cost-Benefit Type Models

Any consideration of CBA is doubly complex. First, it is basically a complex topic, as the Prest and Turvey article readily indicates. However, there are many other articles which also stress the complexity of particular facets of CBA, such as Widavsky's (1967) treatment of the alternative interpretations of "efficiency", to name only one. Second, while there are not really many different approaches to CBA, there are quite an array of variations in each of these approaches. Excluding the many variations of _ex post_ studies, the _ex ante_ studies generally boil down to about three approaches, with some intermarriage: economic theory, decision theory, and the management science or operations research approaches (see Baker and Pound). However, it is amazing how many variations in techniques and applications can be generated from these three basic approaches. The literature is vast and growing weekly. Hence, to keep one's perspective in the study of or discussions about such a topic can be a considerable chore.

Over time, a relatively simple framework has more or less evolved for me which permits the logical classification of components for any allocative process, whether formal or informal. The framework greatly facilitates the comparative evaluation of different research planning methods and provides a handy base of reference for arguments that inevitably arise with respect to allocation problems, such as the frequent recriminations relating to the use of formal allocative techniques, often based on wrong reasoning. While presenting this framework to you, I hasten to disclaim originality or uniqueness for it, only usefulness.

Implicit in _every_ structure used either to describe and measure research productivity (whether _ex post_ or _ex ante_) or to predict and plan research (whether a national research policy or a program of research projects) there are five distinct components:

1. Acceptance of a particular conceptualization of reality, at least that part of reality that relates to the research planning process. From education and experience, each of us inherently developes his own belief about what research is and/or should be and how it fits into the scheme of things.

2. Some sort of logical framework is selected for consideration of the problem of research planning, which essentially specifies the information required and the particular relationships assumed to exist among information components.

3. Deciding on the specific application of the structure, that is, what are the objects of consideration and/or analysis to be put through the logical framework.

4. Deciding on the methods of information processing--the collection, filtering, condensation, and analysis of data--that will be used in the consideration and/or analysis.

5. Specifying the decision criteria which denotes whether a phenomenon is accepted or rejected or whether one is better than another.[3]

The particular conceptualization assumed in research planning will affect how one goes about developing methods designed to aid this process or how he interprets and/or evaluates efforts that have been developed. For example, there are three principal conceptualizations of reality that are frequently evident in the literature. First, there is the "public good"

[3] Professor Glenn Johnson pointed out that the decision criteria are, in fact, a component of one's conceptualization of reality. Probably so, but I feel that they are such a special area of reality that they should be considered separately.

view of the economic theorist who mainly views publicly-supported research from the vantage point of what society gets out of it. Second, there is the rather "mechanistic" view of the system analyst who mainly looks at the decision process itself as the central element without any special roles being assigned *a priori* to either the decision making participants or research beneficiaries. Third, there is the "political process" view of the organizational theorist who mainly is interested in interpersonal structures which somehow result in decisions being made. Of course, there are variations to these modes of conceptualization as well as others which could be mentioned. In my opinion, much of the confusion in the study of research planning stems from failure on the part of users and researchers alike to recognize these differences, and to no little degree on a lack of mutual respect among the protagonists. The principal inplication to research planning is that any conceptualization is biased, and conclusions and recommendations must be carefully considered in light of these biases.

The logical framework is something more than an explicit statement of the conceptualization of reality, although it definitely reflects one's conceptualization. Given a particular point of view of reality, there are many ways usually that information can be organized. In economics, for example, cost-benefit equations and equations calculating consumer surplus might be two variations. The logical framework of the systems analyst would likely be some systematic input-process-output network, and that for the organizational theorist possibly a graphical network of bureaucratic structures.

The specific application will effect the logical framework to some degree but will have greatest impact on methods used to proeess the information and the decision criteria that are relevant. The simple case of applying CBA to a project, a program, a firm or department within a firm, a political subdivision, an industry, or to the national R and D effort is sufficient to visualize this difference. Methods useful at one level would be less than appropriate at other levels. In some cases, what is possible at one level may not be at another level.

The actual mechanics of collecting, processing, and analyzing information, in my opinion, are as important as any other component and more important than most. Its principal impact is in the quality of the information specified by the logical framework and is well described by the computer principle of GIGO (Garbage-in-garbage-out). Sophisticated models are little more than a bore unless reasonably high quality information is put into them. Much of the weakness in existing allocation processes result from the communication of poor quality information and less from the lack of formalized procedures.

Little needs to be added to all that has already been written about decision criteria used in selecting among alternatives.

In evaluating any allocation scheme, each of these components become important considerations. Differences among alternative allocation models, which often appear to be of a substantial nature, often turn out to be differences in only one component. Frequently, there has been total indictment of an allocation system, when the statement of charges closely examined pertain only to one component of the scheme. In the case of PPBS, it has sometimes been in the conceptualization of reality (research just simply shouldn't be planned so closely as to interfere with academic freedom) but more often in the information processing (how can one measure incommensurables?). But to condemn the whole ship because of some loose nut down in the engine room is sheer foolishness at best.

In the actual development and implementation of CBA methods, there is too often a lack of congruency between how reality is viewed by the method developer and by the method user. The developer must reflect all the dimensions of the user's conceptualizations in a proposed method and not just those of the decision criteria (usually). Otherwise, he should not be surprised or disappointed if the user seems to ignore the product of his creativity. As has often been said: nothing creates apathy more quickly than irrelevance!

"Disjointed Incrementalism" and it's Implications to Research Planning

I will describe my own particular approach to developing CBA methods for public research within the context of this analytical structure. This section deals with my conceptualization of reality and its implications to methods of solving resource allocation problems in public research. In the next section I will briefly discuss the principal aspects of a CBA technique developed at the Minnesota Agricultural Experiment Station and its experimental application.

At the outset, I would point out that, although trained as an economist, all of my views about reality do not exactly conform to strict economic doctrine.[4] My views are somewhat more mechanistic than would be expected from an economist, tending more toward those of the management

[4]There are some things in economic theory as a logical framework, within which allocation problems are usually described and analyzed, and the impact these seem to have on resulting analytical approaches, that create certain logical problems that are difficult to work with in practical real-world situations. I have often tried to discuss these "shortcomings", from a systems analyst's point of view, with my fellow economists with about as much success as the Englishman had arguing with the Italian, each in his own language, about who in fact truly holds the keys to the Eternal Kingdom! Rather than detailing the arguments here, I refer to the survey article by Simon (1959) in which he compares the various theories of decision making.

scientists described in the last section. This gives me a somewhat different perspective of economic concepts. To the economist economic theories form the fundamental logical framework within which problems are visualized and analyzed, as they should, and accounting, statistics, systems analysis, etc., are tools that might be used in the analysis. Contrarily, I tend to view problems within a fundamental logical framework more resembling systems analysis, and economic concepts, along with accounting, statistics, etc., are used as tools for evaluating certain segments or parts of the system schematic. While not a profound difference, it is a significant one, as will become clear in later discussion.

The concept of disjointed incrementalism originated with Charles E. Lindblom.[5] Although Lindblom's arguments are concerned with policy making, his conclusions for the most part are clearly applicable to the resource allocation problems of public research. While I would differ, at least by degree, with some of his conclusions, his views are sufficiently inclusive of my own that I must acknowledge him as my personal guru, at least for this area of reality.

The foundation of Lindblom's ideas stem initially from his inability to accept two pet beliefs that have special significance to the allocation of resources to research: (1) that public policy [research?] problems can be solved by attempting to understand them, and (2) that there exists sufficient agreement to provide adequate criteria for choosing among possible alternative courses of action. He concludes that conventional synoptic

[5] Vernon Ruttan first pointed out to me the parallel development in the orientation of my attack on the allocation problem with that suggested by Lindblom. The description of Lindblom's ideas presented here are based primarily on a comparative description in Hirschman and Lindblom (1962, pp. 215-216). To obtain a complete description of his views would require reviewing several papers by Lindblom which are listed in this same reference.

attempts at rational decision making--(1) clarifying relevant objectives or values, (2) surveying the alternative means of reaching stated objectives, (3) identifying consequences, including secondary effects, of alternative means, and (4) evaluating each set of alternatives in light of objectives--will not work for several reasons, but principally because of the extent of social or value conflicts over the objectives,[6] the lack of required information which can be made available at reasonable costs, and the overall complexity of the social problems simply being beyond man's capacity to deal with them.

The strategies which Lindblom suggests as most workable within these limitations has been given the name of <u>disjointed incrementalism</u>, which is a most apt description. I only paraphrase the more significant aspects of these. First, Lindblom considers that although we may not know exactly where we want to go in our policy making, or we can't entirely agree on objectives, there is less doubt or conflict about where we <u>don't</u> want to be. Hence, he visualizes planning as a movement away from "social ills" rather than toward a specific state or objective. We repeatedly "attack" problems rather than "solve" them. Second, even with this orientation, attempts at understanding problem situations should be restricted to those areas that differ only <u>incrementally</u> from existing policies. Consequently, the concept of a zero-based budget is considered unworkable, and only a relatively few alternatives are considered, evaluated, and analyzed at any one time. Third, Lindblom adheres to what Wildavsky calls <u>mixed efficiency</u>, that is, alternative means are not necessarily selected

[6]In this view, Lindblom has substantial support in a paper by Crow (1969) in which he maintains that a commonality of values is no longer possible because of the state of "tribalism" that exists in this country. A less pessimistic view is presented by Hardin (1968).

subsequent to a prior specification of objectives, but rather means and objectives are selected simultaneously.[7] The necessity of this stems not from the need for an appraisal of the availability of workable means alone but also from a lack of specificity in objectives over time, requiring that possible ends be continually rediscovered, reappraised, and usually reformulated. Fourth, analysis and policy making are highly disjointed, that is, they are made at a very large number of places by many different individuals. At any one of these points the analysis of the consequences of all possible alternative means is neither complete nor comprehensive. But, while important consequences may be ignored at each point, they become central concerns at some point in the policy making system, which in total reach an acceptable degree of coordination through the workings of a "spook" who undoubtedly is a close relative to the fabled "invisible hand" of marketing system fame, more formally referred to as "mechanisms for partisan mutual adjustments" by Lindblom.

Lindblom's conceptualization is obviously the practical viewpoint of the practitioner who, above all, is faced with making decisions, not studying them. It stands in stark contrast to the idealism of economic theory as well as to the theories and principles of a number of other relevant disciplines. His reasoning is basically that of the pluralist thinkers on political theory, but rather than being interested in the control of power, he stresses the rationality necessary for effective decision making. For the most part it is a very comfortable view of reality. I would disagree

[7]This is not too difficult to accept when it is considered that this is essentially the manner in which individual scientists select their research projects. In Hirshman and Lindblom's (1962, p. 218) terminology: "in an important sense, a rational problem solver wants what he can get and does not try to get what he wants except after identifying what he wants by examining what he can get."

with Lindblom only to the extent that what he refutes as impractical and unworkable can be visualized as entirely possible at some level or point in the allocation decision structure. However, it is not readily evident that this would be inconsistent with Lindblom's conceptualization, since he is really talking about policy making in an aggregate sense.

The implications of the <u>disjointed incrementalism</u> concepts to the development and inplementation of techniques to improve resource allocation decision making are revealed for the most part in the statements of Lindblom's strategies. Nevertheless, there remains a substantial gap between these statements of strategy and the implementation of a workable, formalized, decision making procedure for allocating resources. In order to reach such a stage of development, some agreement must first be established on (1) how allocation decisions are made, (2) where the information is to come from, including the objectives, alternative means for achieving the objectives, and the results from evaluating these, and (3) the source and channel(s) for communicating values to be reflected in the selection criteria. These are largely determined by the influence the implications have both indirectly, through the effects of certain characteristics operable throughout the total resource allocation decision making structure, and directly, through the characteristics of the specific organization for which the improvements are being attempted.

The extreme complexity of the resource allocation system makes the fragmentation of the overall analytical and decision making structure a practical necessity, at least down to a very specific decision making level. One effect of this is a natural tendency for each decision point to seek <u>autonomy</u> in its decision processes, to reflect mostly internal interests in its decisions. However, there is also the necessity of reflecting the will of and responsibility to other decision points in the total system, some of which are higher authority, others not. Hence, the particular decision processes at each research organization or administrative level have evolved their own particular characteristics to form a mutually acceptable (or workable) interface between internal and external interests.

Two considerations of primary concern are implied by this. First, because organizational characteristics and problems are different, the decision processes at each decision point will be different, and any suggested modification in order to improve resource allocation must reflect this fact. Second, the changes suggested for improving decision making at any one decision point always have the potential of disrupting the effectiveness of the total allocation system even while improving it at the one decision point; implementing the Current Research Information System in the USDA is a good example.

A second major consideration is that research planning in most public research settings _is_ incremental, and there would not appear to be much chance to change this pattern, even if there were justification to do so; that is, I am not prejudging the good or bad of this pattern but only accepting it. The essential character of this pattern of planning is comparable to that of a Markov Chain in which planning of future activities always progresses from the current research mix as the zero base, although information about past events do enter into the analysis on which allocation decisions are made. This, then, suggests a Baysian type of analysis in which improvement in resource allocation decision making is brought about by improving the information on which the decisions are based. Hence, efforts to improve resource allocation are mainly directed at the information and not necessarily at the method of making decisions.

These factors have led me to conceptualize the resource allocation decision process in a purely _functional_ framework. This is a generalized model of the process which considers that there must be specific types of activities and information present for decisions affecting the allocation of resources to have occurred, although frequently these are only implicit in the decisions. This model neither prejudges how allocation decisions are to be made, what selection criteria are to be used, what are the relevant objectives to be pursued, or what the alternative means for achieving these objectives must be. It simply considers that these are categories of information that must be supplied but that the specific characteristics of the information as well as the relationships among them is a particular feature of _each_ research organization. Hence, the generalized model can cope with the tremendous diversity in organizational characterisitcs reflected in differences in decision processes and yet its basic structure remains unchanged.

My generalized model of the decision process assumes that the chief administrator of an organization _is_ the decision maker for that organization regardless of where or by whom the decisions are in fact made. Although he may (and does) allocate some of the information generation, analysis, and/or decision making functions to others, it is still _his_ decision to do so. Further, the administrator is the one most concerned with the interface between his organization and other organizations; he is not only responsible to other authorities for the proper functioning of his organization, but also he is responsible to those within his organization (especially the professionals) for actions by outside other authorities that affect them. Hence, in practice, decision criteria reflect a complex composite of organizational objectives (distinct from research objectives) and internal personal goals and aspirations, but always according to the administrator's interpretation of these.

The essence of this "black box" approach to decision making is that while it is necessary to know what information the administrator requires to make decisions (including those he allocates to others), it is not necessary to know specifically <u>how</u> he makes the decisions <u>unless he should want it known</u>. Some of the required information will be direct requests for specific items (research proposals), but some will come about indirectly, by the administrator explicitly specifying that certain decision criteria, which in turn requires additional information to evaluate (rate of return), are to be included in the analysis.

The role of the"analyst", a function that in practice is performed by many rather than a single being, is the formalization and operation of the information system which obtains and processes the information used in the decision making. In the context of the general framework presented in the last section, his responsibility is <u>formulating</u> the logical framework and the data processing stages. Principally, he sets up the information structure, helps to identify specific information requirements (including those which the administrator is not consciously aware that he uses), identifies information sources and procedures for tapping these sources, and processes and prepares information for presentation to the administrator.

The foregoing indicates that any changes in resource allocation procedures must, first of all, be made within and compatible to existing decision making structures, and secondly, that the primary emphasis on improving the effectiveness of resource allocation decisions is through improvement in the information supporting the decision making and not by replacing or altering the decision process itself. Hence, regardless of whether the concern is on improving decision making in a specific research organization or on improving the effectiveness of the interface among decision points, the primary effort should be directed at developing an information structure for efficient handling of required information from source to decision and developing procedures for improving the quality of the information fed into that structure.

There are several somewhat random but related considerations that should be mentioned to round out this generalized model of decision making. First, the analysis of information involving certain of the decision criteria gives rise to a concept of the "preordering" of research alternatives. The decision criteria essentially can be divided into two sets: (1) those that are generally known, generally accepted, and explicitly specified by the administrator to be considered in the evaluations, and (2) those that are known essentially only to the administrator. The analysis can only include the first set, and since these are only a subset of all decision criteria to be applied, the process in effect "preorders" the array of alternative research activities being considered.

Second, the <u>disjointed</u> concept can also be applied to the sources of information used. In the evaluation of each alternative allocation, there are several items of information required which usually doesn't need to come from the same source. If the system is sufficiently structured, the "best" source can be used for each piece of information. Most of these will be scientists, although possibly different ones, because the information we seek is about things that haven't happened yet, and the best sources of this information probably would be scientists who have the best knowledge about the current state-of-the arts in related areas.

Third, the <u>disjointed</u> concept also leads to a "different" point of view concerning the handling of secondary and tertiary effects in the evaluation of alternative research activities. I would not deny the inevitable importance of these factors in planning future public expenditures. I do rebel at a public research organization developing an engrained paranoia about the necessity of reflecting negative effects in their planning configuration to such a degree as to impede its effectiveness as a knowledge discoverer. Again, these effects do have to be examined and they do have to be reflected in decisions at some point, but special groups--other government agencies, citizen ecology groups, the many society protection groups, institutional innovations designed to modify organizational behavior, etc.--should handle the main thrust of such efforts. A research organization should contend itself with only those effects that appear of the most immediate and general concern.

Fourth, my particular view of "value" used in "pricing" research product cannot be blamed on Lindblom or disjointed incrementalism. It is basically a composite of Decision Theory and its forerunner Utility Theory. Hence, market prices are indexes for relative values and, conversely, where relative worth of alternatives can be established, a proxy for a pricing system exists. The basic problem in pricing research products, then, becomes one of selecting those who are to indicate relative worth. To be consistent I should say that since prices are a part of the decision criteria, then it is up to the administrator to specify those individuals who will establish the relative values or prices. However, the problem is not that easy. But the basic problem is not that we can't establish prices, rather we can't agree on who is to set them. At present, pricing is implicit in allocation decisions and is a very disjointed process.

Regarding the value of basic research, I would disagree with those who say that its products should be established by ~~a~~ tracing through a network of subsequent projects in which this product is used to the ultimate applied research project which results in a "commensurable" consumable commodity, whether it be marketable or social. The public is not the primary consumer of basic research products but rather the other scientists who use the new knowledge as inputs in other research. In this view basic research products are considered as research tools in the same way as, say, an electron microscope. Hence, the value of basic research products is based on how useful scientists consider it to be relative to other knowledge in the same, or possibly in other, applications.

One final observation should be made, because it is implicit throughout all of the foregoing discussions. I believe that in the whole, administrators (decision makers) in public research organizations will make better decisions that are in the interest for all concerned if given high quality information on which to base their decisions. While possibly a naive view, I do not think that this conclusion has been proven false. For one thing, we have never really tested it by making available "high quality information" to administrators. Secondly, the considerable disenchantment in the direction public research is progressing, a relatively recent trend, cannot be blamed on research administrators or their decision processes. Both informational and "value" signals <u>are</u> changing and have not settled down to the reasonably uniformity that existed prior to the start of this trend. Hence, at present, I do not see that the existing decision making structure has been proven ineffective and deserving to be replaced.

The Minnesota Experiment[8]

A computer-based generalized information system, which I will refer to as the "System", for collecting and processing information relevant to

[8] This section is taken largely from Fishel(1970b). For a detailed description of the procedures see Fishel (1970a).

resource allocation decisions under situations characterized by a high degree of uncertainty was developed at the Minnesota Experiment Station. The primary aim of the System was to generate relative measurements of benefits and costs of proposed research activities, the use of which would conceivably lead to a more efficient allocation of research resources within the organization as well as to facilitate the process. The System was primarily concerned with the selection from among proposed research activities and the efficient allocation of resources among these activities, not with the identification of possible research topics.

The System was experimentally applied at the Minnesota Station to establish the likelihood of or extent to which ex ante types of cost-benefit analysis and methodologies have a place in the decision-making processes of public research administration and in what manner they might fit into the administrative framework. By means of the experiment, it was intended that some substantive conclusions could be reached regarding the efficacy of the various estimation and analytical techniques developed for the System, in particular the techniques which were designed to elicit scientists' judgements about certain future events. It was also intended that some conclusions could be made regarding whether or not these methods, designed either to reduce or specify the degree of uncertainty in research resource allocation processes, actually represented an improvement in information over that generated by less formal means, and whether or not the information would, in fact, be used by research administrators.

Specific Application

To test the System, nine "model" research project statements were developed from a review of approximately 300 active research project reports, which were provided by the Smithsonian's Science Information Exchange. The nine projects included three that were essentially "basic" (nodulation morphology, radiation genotypes, and fat synthesis), three that were "applied" or "developmental" (storage deterioration, minimum tillage, and breeding genetics), and three that generally fell somewhere in between these two categories (soil effects on root growth, herbicidal selectivity, and rotation effects on diseases). Each model project statement consisted of its title, a statement of purpose, the objectives of the research, and a general statement regarding the method of study. The statements were neither so general as to be meaningless not so specific that scientists would be prevented from injecting some individual interpretation about the scope and method of conducting a project. Also, while most of these projects did have multiple objectives, each still presented a singular type of "benefit" to be achieved.

Decision Criteria

In any case in which benefits of a proposed research activity are evaluated, even on the basis of market values, an implicit preordering process, as described in the last section, occurs. In this case, the "commonly-held" selection criteria are values associated with the market place. For the System analysis, the "commonly held" criteria for comparing the relative worth of alternative research activities were considered to be (1) its benefit to the soybean industry, (2) its contribution to scientific knowledge, (3) its cost, and (4) the availability of required facilities. A fifth, the availability of required manpower (of specific capabilities) is also important, but it was assumed that research proposals would not be submitted unless this requirement was satisfied. The interpretation given to these criteria was that industry benefits would include increases in market value and resources saved (see Kaldor and Paulsen, 1968), knowledge benefits also included the "process values" of graduate student education and improvement in the skills of the scientist, costs included all "real" costs as contained on annual budgets. Estimates of the costs of facility requirements was used as a proxy for its availability.

Logical Framework

The logical framework of the System is based on the premise that the principal problems of research resource allocation are associated with gauging the flows over time of both costs and benefits of alternative research activities, but that other significant factors affecting allocation decisions are also important, including the feasibility of achieving research objectives, the feasibility and cost of implementing new knowledge generated, and the degree of substitutability, complementarity, and synergism among alternative research activities.[9] The System was designed to generate these separate pieces of information as well as an index (maximand) which would provide the basis of comparison required for preordering. The System actually permitted three forms of the maximand to be generated:

(1) $$R_1 = B - C > 0$$

(2) $$R_2 = B/C > 1$$

(3) $$R_3 : (B - C)/R_3 = 0$$

where

B = research benefits over time

C = total cost of conducting the research

[9] All these factors except the interrelationships are included in the System. With respect to the interrelationships, the proposed CBA methodology assumes that if research activity <u>A</u> and research activity <u>B</u> are in some manner related, then activities <u>A</u>, <u>B</u>, and <u>AB</u> would be evaluated.

R_1 = difference maximand

R_2 = ratio maximand

R_3 = internal rate of return

While it is possible to use single values in solving equations (1) to (3), such as the "expected" values, the System considered these values to be probability distributions based on data that also is expressed as probability distributions. In addition, the "present value" of benefits and costs was used in evaluating the research alternatives, that is, all benefit and cost values were discounted back to the present regardless of when they might be incurred or realized. Hence,

(4) $$B = \sum_{t=1}^{\infty} \frac{B_t}{(1+I)^t} \text{ and } C = \sum_{t=1}^{\hat{t}} \frac{C_t}{(1+I)^t}$$

where

B_t = benefits derived in year t

C_t = costs incurred in year t

I = a discount rate expressing the social time preference of consumption

t = number of years to complete the proposed research project or program

For the internal rate of return maximand, equation (3), the above has a slightly different form, namely

(5) $$\frac{B-C}{R_3} = \sum_{t=1}^{\infty} \frac{(B-C)_t}{(1+R_3)^t} = 0 \quad .$$

The model of the flow of benefits and costs of research used in the System required several kinds of information, some of which were estimated and some assumed given, some fixed-point estimates and some distributed estimates. Where b̲ is a single value of the distribution B̲ and c̲ is a single value of the distribution C̲, then b̲ and c̲ were calculated by the following:

(5) $$b = fv \sum_{t=\hat{t}+1}^{\alpha} a(t)k^t + Sk^{\hat{t}} + P_v(t,\overline{C})$$

(6) $$c = \overline{C} \sum_{t=1}^{\hat{t}} k^t + E \sum_{t=\hat{t}+1}^{\alpha} d(t)k^t$$

Where

$k = \frac{1}{1+I}$

f = randomly selected value from P(F), the estimate of technological feasibility, the relative success expected in achieving the research objectives

v = randomly selected value from P(V), the estimate of average annual benefits at 100 percent level of adoption of new knowledge generated

$\hat{t}$ = randomly selected value from P(T), the estimate of time required to complete the research activity

$\overline{C}$ = the average annual expenditure on the research activity

S = residual value of facilities, equipment, and supplies on termination of project

The "product" value of the research was assumed to be adopted over time according to

$$a(t) = 1-\phi^{t-\hat{t}},\ t>\hat{t},\ 0\leq\phi<1$$

where

a(t) = the time rate of adoption of new knowledge

ϕ = an estimated shape parameter specifying the rate.

The "process" values, which included the increased value of the scientists and graduate student training, were calculated by

$$P_v(t,\overline{C}) = w_s\overline{C}\ \{\sum_{t=1}^{\hat{t}+1} Atk^t + \sum_{t=\hat{t}+2}^{\alpha} A\hat{t}k^t\} + w_g\overline{C} \sum_{t=4}^{\alpha} Dk^t$$

where

$P_v(t,\overline{C})$ = process value of project

$w_s\overline{C}$ = estimated number of scientists per project

$w_g\overline{C}$ = estimated number of graduate students per project

A = marginal increase in scientist value per year

D = average difference in income attributable to graduate training

In addition to direct and overhead costs of conducting research, costs included an estimate of dissemination costs, including anciliary development costs, which were estimated by

$$d(t) = \sum_{t=\hat{t}+1}^{t'} (1-\theta^{t-t}) + \sum_{t=t'+1}^{\alpha} 1-b(t-\hat{t}) \qquad 0\leq\theta,b<1$$

where

d(t) = time path of annual dissemination costs as a function of the maximum annual outlay expected

E = maximum annual dissemination expenditure

θ,b = shape parameters

t' = epigy of dissemination expenditure function (year)

Once the required variables had been estimated, solutions for b and c according to equations (5) and (6) were generated by repeated random selection of values from cumulative density functions derived from P(E), P(V), and P(T) and single-valued estimates of the other variables. With repeated solutions for single values of the maximands r_i indicated by equations (1) to (3), approximately 20,000 to 25,000 of them, r_i tended to R_i. Parameters of the resulting distributions were then calculated from R_i and presented in tabular form.

Measurement Methods

The principal distinction of the System lies in the structure of and the procedures for generating the data required by the analysis. The total task is disassembled into information components in a manner which, on the one hand, permits collecting from the best possible sources the individual segments of information independently of other segments and, on the other hand, permits effectively separating the data collection and analysis procedures from the specification and application of desired selection criteria used in determining the relative goodness of alternative research activities. The uncertainty that arises in making estimates about the various pieces of required information are retained throughout the analysis, which results in the benefit-cost estimates being distributed values. Space does not permit a full discussion of the quantitative methods used in the System; hence, only those which might be considered "distinctive" are described.

Probably the most distinctive technique used in the System is that one used to obtain estimates from scientist-experts about events that have not yet happened. The resulting subjective probability distributions generated by the estimation procedures are simply graphical transformations of the mental impressions these experts have about the likelihood of uncertain events. Including estimates of data elements as probability distributions simply reflects an effort to <u>explicitly</u> recognize in the procedures one of the two predominant difficulties of research evaluation, namely, uncertainty. The other predominant difficulty, of course, is obtaining explicit values for the products of research, which is discussed later.

While there are a number of good methods for describing subjective probability distributions of estimates of some event P(x), the one used in the System is based on the beta function and was constructed in a manner such that estimates by the experts alone determined its shape. A prior distribution of a basic random variable <u>x</u> in terms of a beta distribution, with a useful modification by Schlaifer (1959, pp. 673-676) is

(7)
$$P_\beta(x;\rho,\nu) = \frac{(\nu-1)!}{(\rho-1)!(\nu-\rho-1)!} x^{\rho-1}(1 - x)^{\nu-\rho-1}$$

where $o \leq x \leq 1$ and $o < \rho < \nu$. The mean, mode, and variance are functions of only the two beta parameters ρ and ν. A simple transformation normalizes any range of values, say, "L" (lower bound) and "H" (upper bound) to $o \leq x \leq 1$ from $L \leq X \leq H$. Hence, the values L, H, ρ, ν uniquely define a beta function, which may have a retangular, symetric, skewed left or right, or "cubic" shape.

To obtain a subjective probability distribution of an event, experts were asked to predict (1) the values "H" and "L" for an event which they would expect to be exceeded only under very exceptional circumstances; (2) the values "h" and "l" for an event which they would expect to be exceeded only one-third of the time; and (3) the value "m" for an event which they really would expect to occur (the mode). Using techniques in the System, these five values can be used to generate the values "H", "L", ρ and ν and, consequently, a unique subjective probability distribution. It is this process which is implied in the notation P(X).

The estimation procedures developed for the System reflected the fact that there usually is a relationship between total cost, benefits achieved, and time required to complete a research activity. While there are a number of approaches to estimating the basic cost-time-benefit relationships, the approach used in the System was as follows:

1. The average annual expenditure $\bar{C}_{jk}$, where k = 1,...,k are alternative levels of average annual expenditures on research activity "j", may be specified to estimators or discrete values estimated.[10]

[10] In the Minnesota experiment, three levels of resource use were estimated by scientists: (1) the level that would represent a bare minimum of research effort and still hope to get something done, (2) the level that would represent an all-out effort without simply wasting resources, and (3) the level that the estimator expects to be the most likely scale of effort.

2. For each $\bar{C}_{jk}$, estimates of the time in years to complete the project were made in the form $P(T_{jk})$.

3. For each $\bar{C}_{jk}$ and Exp (T_{jk}), estimates of research benefits in the form $P(B_{jk})$ were made.

The estimation of research benefits $P(B_{jk})$ is performed in two stages: (1) the values of research products are estimated on the basis that research objectives would be achieved 100 percent, and (2) the technological feasibility of achieving the stated objectives are estimated given the constraints of each pair of $\bar{C}_{jk}$ and T_{jk}. Hence, for a given $\bar{C}_{jk}$ the resulting benefit would be computed by

$$B_{jk} = F_{jk}B^*_{jk} \text{ or } P(B_{jk}) = P(F_{jk})P(B^*_{jk}) \tag{8}$$

where F_{jk} is the esitmate of technological feasibility of the research activity and B^*_{jk} is the value of research products based on 100 percent attainment of research objectives. This two-step procedure is a logical separation of the two types of information that are inherently contained in any estimate of research benefits. In addition to permitting estimates of F_{jk} and B^*_{jk} to be obtained from the sources best qualified to provide them, it also permits a procedure, described later, that enables B_{jk} from all types of research activities to be incorporated into a single array of relative value.

Because of the degree of variability in the nature of the types of research and resulting research products, no single procedure was considered appropriate for estimating the B^*_{jk} of equation (9). Two methods were used in the Minnesota experiment. For those projects which had research products readily expressible in physical units, such as yield increases, or as a percent increase or improvement over existing conditions, values were obtained by direct evaluation of increased value, resources saved, etc., in the form $P(B^*_{jk})$. Other research required a different approach be employed. This method assumed that both research activities with expressible product values and those without can be ordered in a common array based on a set of preference criteria which reflect subjective estimates about the relative worth of the research products to the achievement of organizational objectives. Further, estimates of "first differences" provide the basis for computing relative indexes of value for the research products that are not measurable in terms of the positive values of the research products which are.

For the nine projects included in the Minnesota experiment, some experimentation was done on the ranking procedures, alternately using the "contribution to the soybean industry" and "contribution to scientific knowledge" criteria. However, whichever criteria was used, the procedure was the same. Estimators first ranked the projects according to specified criteria. Then "first differences" were obtained by asking estimators to specify approximately how valuable (in percentages) they expected the product of a research activity would be relative to the value of the product of the research activity ranked immediately above it, if all research objectives were 100 percent achievable. Hence, where even only one research activity had expressible dollar benefits, implicit benefit values could be imputed to all other research activities. Where more than one research activity had expressible dollar benefits, a weighting scheme based on ranks and first differences was used to impute these values. But, the benefit values B^*_{jk}, whether obtained by direct computation or imputed, were assumed to be the research benefits that would result if research objectives were achieved with 100 percent success and they were fully adopted.

A large number of sources were employed in obtaining all of the information and data required by the System. The estimates on which subjective probability distributions were generated for the various data required were obtained from scientists located throughout the eastern half of the United States. These scientists either conduct or administer research on soybeans in public research organizations. From an initial mailing list of 170 scientists, 55 from outside Minnesota and 14 from within the State actually provided all of the information requested of them. The first survey requested individual professional data which was used to analyze later responses; the second requested various rankings and first differences for the nine model projects; and the third requested detailed estimates of personnel, supplies, equipment, and facility requirements on specific projects, in addition to the time and technical feasibility estimates. In addition to the general mail survey, a number of panels were set up using scientists from within the state to test different configurations for effectiveness in providing estimates.

Sources of cost data also included the University's business office (overhead costs), various laboratory, greenhouse, and farm supervisors (use rates on equipment and facilities), an extension administrator (dissemination costs), and other specialists and technicians for specific costs and rates. The explicit computation of benefit values for one of the projects included several scientists, marketing economists, industry personnel, and secondary sources of data. One of the more successful aspects of the System was the facility with which it permitted data from such diverse sources to be brought together in a single analytical framework.

Analysis Steps

The analysis steps included generating "consensus" estimates from the individual estimates of $\bar{C}_{jk}$, $P(T_{jk})$, and $P(B^*_{jk})$, and carrying out the Monte Carlo solutions of equations (6) and (7) to generate the distributed maximands R_1 and R_2 based on these variables and other estimated information. The final step was the reporting of the analysis in a form useful to research administrators. Several forms were possible, including the form and information shown in Table 1. In addition, the System graphically plotted the three maximands R_1 to R_3 for each research activity and level of support $\bar{C}_{jk}$. Finally, a "consensus" budget for each $\bar{C}_{jk}$ was provided, in dollars for personnel and supply requirements and in physical terms for land, facilities, and major equipment requirements, both by subcategories.

Conclusions

There are two questions raised by the experimental application of the System. The first is a methodological question regarding whether or not and how well information aimed at improving resource allocations could be generated. The second question is concerned with the application of the information generated for research administrators: whether or not the information would be used in the decision-making process and the impact this information would have on the resulting decisions.

An overall evaluation of the estimation techniques would seem to indicate that these do collectively outline an information system that demonstrates potential for facilitating and improving information used in resource allocation decision making in public research organizations. Despite an obvious need for some further refinement of procedures and techniques, the System could be implemented in a form at least similar to that used in this study. However, acceptability of these conclusions hinges on the acceptability of certain key assumptions, discussed in an earlier section of this paper, and the relative precision and efficiency of the estimation techniques employed. A major fault of the System as it is now constructed is in handling of interrelationships among projects, both with respect to effects on uncertainty and with respect to levels of costs and benefits. One effective way of

Table 1. Information Provided by Benefit-Cost Estimation Technique for Two Projects Included in the Experiment.

Project	Cost Level	Estimated Average Annual Expenditure[a]	Planning Period Mode-Mean Stand. Dev.	Maximands: Benefit-Cost Mode-Mean Stand. Dev.	Maximands: Benefit/Cost Mode-Mean Stand. Dev.	TFP Index[b]
		($1,000)	Years	($1,000)		
A	1	28.7	14.0 - 14.9 2.89	709 - 964 560	1.3 - 5.4 7.5	1.31
	2	58.7	9.0 - 8.9 1.30	2023 - 2274 904	3.5 - 7.5 5.1	1.14
	3	114.9	5.8 - 5.8 .77	2942 - 3162 1550	4.5 - 6.5 3.3	1.09
B	1	15.6	5.2 - 5.2 .65	388 - 416 148	3.2 - 6.5 3.4	1.11
	2	28.2	4.7 - 4.8 .66	526 - 563 270	3.8 - 5.1 2.8	1.07
	3	57.2	3.5 - 3.6 .48	624 - 657 279	3.1 - 3.8 1.4	1.05

[a]Includes the allocated dollar cost of all resources used plus an expected desimination cost of the information generated.

[b]Technological Feasibility Predictability Index. A measure of the variability in the overall estimates of technological feasibility, entirely comparable to the coefficient of variation but based on the variability of the plus and minus 1 standard deviation points of the individual about a concensus subjective benefit/cost distributions. The lower the TFPI, the more predictable the proposed project.

handling this problem might be by generating a covariance matrix of research characteristics analagous to that used in comparison of alternative investments, but this must wait for future study.

While the results of the experiments indicated that reasonably acceptable information about research costs and benefits can be generated by such a system as this one, the extent to which the generated information would be applied by research administrators was much less conclusive. There was reason to believe that the information would be used, but determining whether or not resulting decisions would actually have been improved as a result of using this information was well beyond the limit and scope of the experiments.

It can be concluded that we are a long way from such techniques being employed as the allocator of research resources. Neither the current state of development of methodologies nor the temperament of the research establishment in the public sector are up to what would be required of them. I suspect that the nature of the application of such techniques as discussed in this paper, at least for the next few years, will be in the nature of experiments or special studies of the kind described here. This will be applications to rather specific studies of the topics where impending critical policy decisions require substantially more information than is available at present or by traditional methodologies. However, even now as refinements in methods are occurring, there are venturesome administrators willing to gamble on applying advanced management information systems to a wider range of decisions. As so familiar to those of us in agriculture, the rest will follow.

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