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RESOURCE ALLOCATION AND DECISION ANALYSIS FOR AGRICULTURAL RESEARCH

by
Walter L. Fishel

Department of Agricultural and Applied Economics

University of Minnesota
Institute of Agriculture
St. Paul, Minnesota 55101

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In all the computer applications discussed in this volume, a common factor is the types of situations in which they are most effectively used, namely: (1) there are large amounts of data to be processed, (2) the stages of data processing are notably repetitive, and/or (3) the situation being analyzed is logically complex. In one aspect or another, these three characteristics, particularly that of logical complexity, are found in the processes whereby the allocation of limited resources to agricultural research are determined.

Administrators and managers of research organizations and activities, as those of other types of activities, have tried various procedures and techniques to assist them in their decision making processes. Traditionally, these have taken the form of committees, task forces, or other special study groups which were brought together usually to provide information in a specific decision area. More recently, data storage and retrieval information systems, such as CRIS and EMIS, have been implemented to take advantage of the efficiency afforded by high speed EDP equipment in handling voluminous and repetitive data generated in reporting processes. As yet, the more formally structured decision analysis techniques, or Management Information Systems (MIS) as they are usually referred to, have been less commonly applied and, in fact, have largely been limited to experimental applications. These systems go beyond the relatively simple tasks of data collection and retrieval capabilities and include analytical routines of varying degrees of computational sophistication. The informational product that results from an MIS typically has all irrelevant data filtered out and is condensed to those few information components that bear directly on the decision at hand.[2] It is for this reason that research administrators and managers have shown particular interest in the MIS, either as a means of facilitating their decision making processes or simply as a means of improving the quality of information on which allocation decisions are made.

The intent of this paper is to present a brief discussion of the application of these computer-based decision aids to the problem of allocating limited resources to alternative research activities. Unfortunately, even briefly treated the presentation has severe limitations. First, the topic of research evaluation, an activity which must precede allocation decisions, is an extremely complex one. Much more explanation and description of this topic than can be presented here is necessary to adequately understand it. Secondly, even for a single decision making situation, there are a number of alternative approaches or techniques which may be applied, depending on what sort of "ground rules" the MIS developer and user assume about the situation. Third, the nature of the allocation task and the types of techniques used differ both by type and level of organization. For example, consider the differences in allocation decisions between organizations in the private sector and those in the public sector because of differences in what is being "optimized", between evaluating a research program which includes the activities of many organizations and evaluating the many programs of a single organization, or even between decision processes in the USDA where research is closely affiliated with action programs and those in the state agricultural experiment stations where frequently research is closely associated with educational activities. This complexity and variation preclude any detailed treatment of the topic, and no more than a constrained treatment of the limitations and scope of such applications can be presented here.

There are a number of ways in which the topic at hand could be discussed, depending on particular interests. The technically oriented individual would likely prefer emphasizing the discussion of methodology; even here there are two separate structural considerations, regardless of the method discussed, namely: (1) the logical framework of the method--the identification of the factors to be considered and the relationships among those factors, and (2) the data methods used--how the needed data is to be collected and measured. On the other hand, the practitioner would certainly prefer emphasizing the function of decision making itself, in which case there are two equally important considerations, namely: (1) questions of relevancy--selecting the "right" research alternatives from among all possible ones, and (2) questions of efficiency--once the "right" research activities are selected, how these can be pursued most efficiently. The latter dichotomy would seem most appropriate for this discussion.

The Task of Allocating Resources

The function of the research administrator and manager is fundamentally one of guiding the activities of his research organization in such a manner that it effectively contributes to the operation of the total system of which it is a part. The administrator and manager interpret the objectives of their particular organization or agency as established by some higher authorities and, within the context of this interpretation, acquire and allocate resources in a manner they believe will most effectively meet these objectives. Four general functional areas are usually attributed to administrators and managers to achieve the above: (1) planning future activities of the organization; (2) making decisions about the organization and allocation of resources; (3) maintaining accountability for these resources; and, (4) <u>facilitating and coordinating</u> the activities that go on within this organization and between this and other organizations. Although decision processes are not usually so distinct as this in practice, the computer-based decision methods that have been developed to date tend to be specifically designed to aid one or the other of the above functions. The remaining discussion considers each of these areas in turn, first indicating what research administrators and managers are attempting to accomplish under each function and then describing how computer-based MIS methods can assist in this effort.

Planning Future Activities

What is usually referred to as "long range planning" has been one of the more successful areas for MIS applications in research organizations, particularly for those in the public sector. The principal reason for this success probably lies in the fact that such planning efforts are not too demanding of MIS methodology; that is, while taking advantage of its conceptual strengths, they can be quite tolerant of the imprecision and basic crudeness inherent in most of the existing MIS methodology. Since the planning refers to activities at least 3 to 5 years away, the allocation decisions that have to be made at this point in time are usually quite broad in their implications, tentative, and not nearly so specific as required in the case of planning current budgets. There is time to adjust or "fine tune" allocation decisions before these future plans become "next year's budget".

There are many dimensions to planning agricultural research, such as evaluating a single research program area at the national, regional, or local level, or evaluating different activities or programs carried out within a single research organization. While the different problems of evaluation may be more or less important in each of these areas of research planning, the fundamental tasks of evaluation for the most part are nearly the same. Likewise, what research administrators and managers are attempting to accomplish in these planning activities are the same, namely: (1) determining the more important research activities in which their organizations need to be engaged in the future and (2) planning for the availability of resources to carry out these activities. These two goals are not always accomplished in this order and seldom independently of each other. At least in part, research activities will be included in future plans, or excluded, on the basis of the quantity and quality of resources that are expected to be available in future periods. However, in practice the two areas are mostly separated, and they are treated separately here.

Identifying Research Alternatives

Efforts to identify the more important research activities for consideration in future plans always requires four analytical components, regardless of the particular approach employed. In the traditional approaches, these tend to be less distinctly differentiated and are notably more implicit in the evaluation procedures than in the case of the more formal MIS methods. These four components are:

(1) Some procedure for identifying and specifying potential research topics, including the range of activities to be considered, in most cases a more definitive identification of the alternatives, and some specification of the overall characteristics of the alternatives such as their general purposes or objectives and scope.

(2) Methods and sources for acquiring information about each of the alternative research activities, such as time and resource requirements, technological feasibility, etc.

(3) A classification scheme for organizing the information about the proposed alternatives so each can be logically evaluated.

(4) Some method for evaluating the information, including at least a subset of all the criteria that will eventually be used in selecting among the alternatives.

The "task force" or "long range study committee" has been the usual vehicle for generating the information required in long range planning, which is the product of these four components. In one variation or another, groups of specialists are selected to carry out the planning effort for a rather broadly defined area of research activities (beef production, for example). The same scientists identify the alternatives, provide information about them, and by proxy determine the selection criteria by deciding which are more important versus less important and/or more feasible versus less feasible. The essence of this approach is the large degree of subjectivity inherent in it, not only in the information generated but also in the orientation of the whole planning structure and procedures. While reliance on subjectivity is certainly necessary, the degree and manner in which it enters into the evaluation process is not altogether desirable.

The differences between this approach and that of the MIS's is more than simple computer versus manual tabulation and analysis, although the role of the computer is not to be minimized. Its speed in processing large amounts of data makes practical, or even feasible, the handling of the more detailed consideration of the evaluation steps. The signal difference arises at the time the MIS is being implemented. At this stage, many of the steps implicitly performed by the task force or study committee members, separately or as a group, become explicit subjects of study. It is not enough that an evaluation is to be done; how and why each step is carried out is scrutinized, evaluated for effectiveness and efficiency, and more formally structured than in the above. By this careful control of each source of input into the evaluation process, for example, the manner in which subjectivity enters into the product of the MIS is more nearly known, and instead of being a weakness of the system, it can be made a very useful tool for the evaluation process. The benefits resulting from such a methodical approach to the implementation of a MIS are comparably evident at each stage of the evaluation process.

In particular, one product of long range planning that inevitably seems to result is an improved taxonomy (classification) of research activities carried out within an organization or program area. Certainly, an organization benefits in part from simply going through the process of reappraisal, but the major benefits are something more than this. Mainly, it is that a classification developed today is more relevant, or reflects more effectively, how the total population of problems are viewed or considered than one derived yesterday. A striking example of this effect is the increased incidence of classifications reflecting interdisciplinary interests now than in the past.

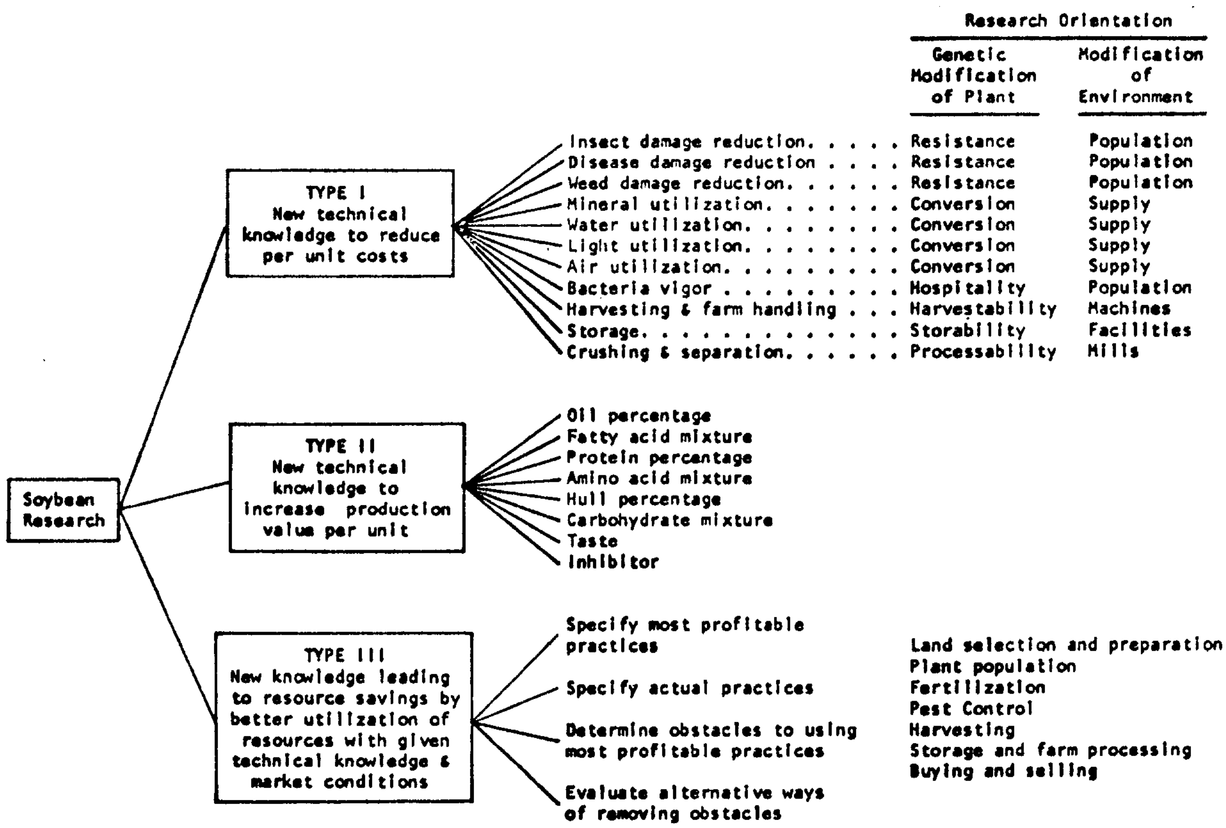
Although such benefits are realized by traditional methods of planning research as well as in MIS applications, there are some characteristics of the latter that highly recommend it. First of all, traditional classifications tend to be oriented to existing scientific disciplines and as such are largely knowledge oriented. Although "down-to-earth" problems may be included in the selection of projects for study, there is always the connotation of adding to the "basic" knowledge required within each discipline as the principal interest. The MIS approach, on the other hand, is fundamentally "end" oriented in the PPB sense, even though not overtly PPB in nature. There is a strong "purpose" orientation and, consequently, a stronger tendency to reflect in the classification scheme both the basic-applied-development chain in research plans as well as an interdisciplinary attack on problems. Second, in the traditional approach only broad categories are initially specified, such as by commodities, and then mainly as a means for delineating areas of study for appointed study committees. These committees separately and independently develop the remainder of the classification usually concurrently with identifying relevant research areas. On the other hand, the use of computers in MIS applications requires that the classification scheme be prespecified; that is, a complete classification of information is developed by a separate effort independent of problem identification or information generation steps. These two points imply that the traditional approach follows an identify-classify type of format while the methods used in MIS require a classify-identify format in the process of developing a research taxonomy for evaluation.

The method of developing a classification for research itself can be a source of strength to the overall MIS application. It is most clearly reflected in how efficiently the alternative research activities to be considered for further evaluation can be identified. In fact, identification of the research alternatives can become almost a residual product of setting up a research classification. Hence, it is important to realize that the research administrator, simply by deciding how the classification is to be established, greatly influences the nature of the research mix that the planning effort will evolve as well as the possibility that the full range of potential research activities will even have a chance of being brought to his attention.

Any number of examples of methods for deriving a classification can be given. Three are very briefly considered here. Probably the most comprehensive effort was that developed in the long range study of research needs which is now used by CRIS.[3] The taxonomy of research reflects what has been referred to as the traditional approach, that is, one derived largely from an inventory of existing activities, even though in this case subsequent effort was made to relate these to national goals. The result of the evaluation stage could then be predicted reasonably well: an appraisal of expanded effort of ongoing research activities, no recommended reductions or curtailment of effort in any activities, and no reorganization of research activities to more effectively attack existing problems.

The remaining two examples utilize an ends-means configuration in the development of a research classification. That is, it is assumed that there are generally applicable goals to which the activities of a research organization or program must contribute if it is to perpetuate its justification for existence and, consequently, support for its activities. Given these, how then are the organization's activities related to the achievement of these goals? An Iowa Agricultural Experiment Station study[4] starts with the general goal of maintaining or improving People's Welfare which is affected by conditions of Security, Growth, and Equity. Growth (economic) is effected by the introduction of new physical capital, knowledge, and human captial. Knowledge in turn is the product of research in several areas including agricultural research, which consists of organization and management research, commodities research, and resources research. Commodity research consists of soybean research which is then further subclassified as shown in Figure 1. A continuation of this process of subcategorization leads to a comprehensive identification of alternative research activities. Obviously, other branches of this network could be comparably subcategorized.

Another approach to developing a classification of research is contained in a study to evaluate human nutrition research in the U.S.[5] This method also starts with policy goals, but limits itself to only those related in some manner to human nutrition. These generally have something to do with the quantity and quality of human life, implicit in the phrases



A Classification Scheme for Soybean Research length of life, productivity, and well-being. Impediments to attaining the policy goals (generally nutritionally-related health problems but including lack of education and certain institutional restrictions) is then identified and classified in two ways, one that is most meaningful to the policy makers (effect of human nutrition problem) and one that is most meaningful to the bench scientist (nature of the solution). This approach has two principal advantages: first, it relates each research topic to every problem for which it is a (partial) solution, and second, it provides an expedient cross-over between policy language and scientist language in communicating resource needs of human nutrition research, traditionally a significant point of conflict.

While it might appear that a great deal is made of the classification of research used in planning research activities, its importance can hardly be overemphasized. First, it is usually the case that the benefits or impact on resource use provided by this step alone is greatly underestimated, until it is in fact undertaken. Probably in the largest number of cases in which public agricultural research organizations have undertaken such long term planning exercises, the formal part of the exercise has stopped with the development of a classification of research and a reclassification of current research projects according to it. But, while it may be that the benefits provided by this exercise alone were greater than expected, it is equally probable that the next step was so much more difficult than expected that administrators and managers considered the added product not worth the effort. Nevertheless, except for very few instances, the steps beyond this stage that have been applied are still largely of an experimental nature. A second consideration is that for the MIS approach in particular, the classification is the foundation for all that follows and the remaining identification and evaluative efforts are diminished to the extent that the classification is inadequate.

The remaining steps in the identification of potential research activities--delineation of specific research areas, collecting information about these areas, and applying decision criteria to reduce the range of alternatives for further consideration--are not so distinctly separate in long range planning as one would expect in planning the budget, say, for next year. Nevertheless, these are three functions that must be performed, whether explicitly or implicitly, for effective planning to occur. Once the classification is developed, and regardless of the method of planning employed, the process of identifying potential research activities often may iterate through the three functions as follows: (1) a rough delineation of research areas based largely on categories of the classification; (2) pulling together useful information about each area; and (3) delineating more closely the potential areas (i.e., starting to specify activities) based on an evaluation of the information. In the last step, decision criteria are being applied in deciding what is to be eliminated from further consideration. This process is then repeated from time to time, each time potential research areas becoming more narrowly defined. Final definition and specification do not usually occur until the research area is to be considered for inclusion in the upcoming budget.

How, then, can an MIS benefit such a process as this, one obviously requiring a great deal of intuitive, subjective input by bench scientists themselves? There are two general considerations and a number of specific ones. First, an MIS is basically a communications device; hence, its worth is gauged primarily by how well it improves the information it is intended to convey between interested parties. It is also gauged by how useful that information is and how much it costs to generate it. Very generally, information is "improved" by identifying more completely those factors relevant to particular decisions, more precisely measuring or estimating factor relationships, and by imbedding greater reliability in the information generated from the standpoint of the user. All three of the steps in identifying alternative research activities are involved in improving the information.

However, since there is no practical way to obtain a "before and after" picture for a proposed MIS, and consequently no way to quantitatively measure the worth of the system, it does not immediately follow that more or improved information results in better decisions. But it is possible to do more than presume that they do. The principal benefit of having more or improved information is the reduction in the level of uncertainty surrounding the decisions being made. One immediate result is that knowing the uncertainty more clearly establishes the true bounds between what constitutes feasible versus infeasible research efforts. Maybe more than this, the administrator should gain more confidence in his decisions, confidence that all has been done that can be done to come up with the "best" decision. While it would be extremely difficult to prove *ex ante* that *any* decision was not the best decision, the administrator is particularly interested in obtaining a closer correspondence between *ex ante* and *ex post* evaluations of worth. Even if better decisions did not result immediately, a well structured MIS does provide the foundation for a good learning device by greatly facilitating historical analysis of specific allocation decisions, a practice that is not too feasible by traditional methods of decision making.

Second, the essence of a MIS is its formalized, standardized, largely "mechanized", and (very importantly) specified procedures. While these are the characteristics usually attributed to bureaucratic processes (reputedly the arch enemy of scientific inquiry), they are nevertheless the means by which information can be improved and certain efficiencies in the planning process achieved. In particular, by reducing the amount of person-to-person contact required, especially for the administrator, and by achieving a degree of uniformity over diverse areas of research in planning procedures, it becomes feasible to have a perpetual long range planning activity rather than the periodic crash efforts usually encountered. In addition to the benefit of having a continual long range planning effort, there is also a substantial advantage in spreading out scientist involvement in planning and avoiding the large commitment of time the crash studies usually involve.

In particular, the delineation step in identifying possible research activities--including eventual specification by purpose, objectives, scope, and possibly even method of study--should be as logically formulated as is practical. However, this is one area of the procedures that hasn't been very well mechanized or even standardized to any great degree. Consequently, the process must rely on the capabilities of the individual scientist's mental computer and his knowledge about an area. In overly simple terms, the initial delineation of possible research areas can start with a specific research category in the classification, determine the nature of the problem to be investigated and what is needed to be known about the problem to provide a solution, inventory what is currently known, and then decide what new knowledge must yet be generated. But while this effort heavily relies on intuition, impression, and other subjective contributions, the product of the delineation process still must be a statement that tightly defines a bounded set of research activities, regardless of how broad in scope these are.

Contrary to traditional long range planning efforts, subsequent procedures in the more formal MIS approaches require specificity, so that estimates about resource requirements, etc., will only vary because of inherent uncertainties in the study area and as little as possible because of differences in interpretation of what the study area itself includes. This is the first requirement for achieving greater precision in the information on which decisions are to be based. This requirement for preciseness also can provide the administrator some basis for sharper control over individual research activities once implemented, because it is more definite both to the administrator and the scientists what the latter are being held accountable for. But more importantly, experience has shown that the scientists themselves provide substantial self control as a result of simply going through the planning effort and being forced to generate precise rather than general results.

The more rigorous approaches to information collection and analysis characteristic of most MIS can be especially fruitful for long range planning efforts. In part, the computer-based routines and analytical methodologies provide a reasonably effective means for coping with the basic difficulties encountered in obtaining information about things that haven't happened yet and about things that are so inherently uncertain. In part, these methods provide a blueprint for the controlled processes by which information needs can be "disected", each component measured or estimated from the best sources, and then the whole reconstructed to provide a higher quality information than is usually available by treating information needs collectively. Again, the principal role of the computer in this is handling the detail that results from breaking up the information into components, as well as the usual benefits of performing tabulations and complicated mathematical computations where necessary.

Based on the information collected and analyzed about each research area or activity, in however much detail this is carried out, decisions then must be reached regarding the relative worth of the different research possibilities based on certain decision criteria. However, making such decisions in long range planning is more like deciding whether or not its even on the target rather than if its in the bullseye, which will be discussed later. Consequently, the criteria are of a more general nature than if an actual allocation were to be made directly as a result of the decision. In fact, scientists are applying decision criteria of sorts when they make very broad subjective judgments regarding even the vaguest degree of relevance or irrelevance in the delineation stage. Regardless, the key to the application of decision criteria in a MIS devoted to long range planning is working toward logical exclusion of possible research areas versus convenient inclusion.

Even for long range planning, there are a number of decision criteria which affect the relative desirability of a research area, such as a number of factors relating to its potential worth to the agricultural industry or to consumers, its contribution to basic knowledge in general, how technologically feasible it is, etc. In a MIS, the administrator specifies which of these criteria he wishes reflected in the evaluation, which include values that are commonly judged to be applicable and are ones which he would normally not be qualified to make the best judgments about. Since there are other criteria that will eventually enter into the actual allocation of resources (and are not commonly held), the resulting evaluation would be considered a "preordering", as compared to the actual allocation of resources. The significance of using the structured approaches of a MIS is that the individuals most qualified to evaluate the proposed research for each criteria are used, whether this involves actual economic prediction or intuitive scientific judgment. Consequently, the application of decision criteria in long range planning is similar to and, in fact, in some cases is part of the information collection and analysis procedures.

The functions described in the preceding are performed in all cases, although they may be carried out with different degrees of numerical sophistication. They could be performed, and frequently are, without the use of a computer. However, in addition to the simple efficiencies afforded, the very rigor required by their use becomes a source of primary benefit to the long range planning effort--not only in the greater precision of the information generated but also in improved self regulation by the scientists themselves.

Planning for Resource Availability

It is a fact of life in research organizations that there is never enough resources to satisfy all of the requests for them. Hence, a principal responsibility of administrators and managers is to plan for the future availability of resources, as well as to allocate the available resources in the current year among selected research activities. To my knowledge there is no MIS which will permit the administrator to generate new sources of funds for his organization. However, there are systems which will assist him in anticipating future allocation patterns and better understanding the effect on these patterns of particular decisions he may make regarding research programs.

A most useful type of MIS for this purpose is an extension of the annual budget to include, say, five year projections of resource requirements by research activities. In the simplest form, these are simple storage and retrieval mechanisms which accepts statements of future resource needs by year, submitted at the same time as the requests for inclusion in next year's budget, and generates formatted reports useful to the administrator in interpreting future allocations. An example of such a system is having projected resource requirements for the next five years attached to the annual CRIS reports. In a less pervasive system, similar reports would be included on proposed research as well as ongoing activities.

Such a system as this one could be carried further by including within it a simulation model of the decision process. Hence, given certain decision criteria (which could itself be varied), the system would provide the administrator with the changes in the patterns of future resource requirements he might expect from his decision, say, to incorporate a new research program in the organization, or from some source of funds suddenly drying up, etc. Obviously, there is substantial advantage in better knowing the effect of a decision before rather than after it is made.

Allocating Resources

Regardless of how much planning is done, and how much "preordering" of alternative research possibilities is carried out, at some point a decision has to be made and resources have to be committed to the conduct of specific activities. This might be facetiously refered to as the administrator's "moment of truth!" He necessarily must reject some requests for research support and often must reduce the level of support requested by others. And, since he is dealing with professionals, he must justify to them these denials as well as justify to those who supply the resources those which were accepted. Hence, the administrator is particularly anxious to receive help in this area.

Regretfully, the relevant methodology of management science has not attained a level of sophistication that makes it a practical reality to perform this function, nor for that matter have research administrators developed the characteristics necessary for the use of those techniques that have been developed. While the final allocative process is immensely complex, it is not so much the numerical complexity that is beyond the scope of an MIS to handle. Rather, it is the complexity in the array of values that make up the decision criteria. Nearly all commensurable values can be readily manipulated, and in many cases the difficulties of handling totally incommensurable values can be adequately reflected in the decision process. But there are many other values of both types which are extremely difficult to represent in these types of systems, such as criteria dealing with humane aspects of an organization's operations (the horses-out-to-pasture idea), or certain politically expedient activities to maintain support for easily justifiable activities, or any number of others. Further, many relevant selection criteria are not linearly related over the full range of their values, and there are frequently interrelationships among research projects that cannot easily be reflected in quantifiable terms. These are just some of the impediments to effective use of numerical processes in this particular task of the effective allocation of resources.

The literature is resplendent with descriptions of decision models that have been developed for this purpose. These include the investment appreach of portfolio analysis, the various simple and complex ranking procedures of decision theory, the straightforward decision tables, various economic and non-economic interpretations of cost-benefit analysis, the various optimizing mathematical programming methods, simulation, heuristics, and the whole spectrum combining these. However, as one survey concludes, these are either so general in effect as to be naive or so detailed and complex as to be impractical in other than the specific applications for which they were developed.[6] A general conclusion would be that these can be extremely useful adjuncts to making decisions about resource allocations, but in no sense can they (or should they) replace the administrator or manager in carrying out this most difficult task.

Accountability and Control

Accountability is a necessary function carried on by research organizations in accepting responsibility for the resources allocated to it from various sources. On the other hand, control, which is the power of administrators and managers to effect change in the activities of subordinate units, is typically a passive activity in professional research organizations and is largely limited to influencing the characteristics of annual budgets. However, aside from the legal requirements to account for resources used, there are some definite benefits to research planning and effectiveness in having both good accountability and good control procedures in an organization. Among these could be included the likelihood of a higher cost-effectiveness ratio as a result of eliminating costly divergent or ineffective activities and better concentration on the primary goals for which funds were budgeted. Also, a more substantive basis is provided for more accurate planning of future activities, providing greater assurance that results will more nearly adhere to plans.

The application of computers to accounting and control systems is far from novel by now, and such applications in research organizations, whether private or public, differ in only minor respects from most other types of organizations. For the most part, the applications are simply doing by computer what has traditionally been done manually, in most cases affording some clear cut economies. But the use of computers does permit the design of more sophisticated accounting and control procedures. A degree of detail and/or a frequency of report generation which may be purely impossible by manual methods can usually be attained with slight increases in effort and cost.

While accounting is being computerized at least gradually in most research organizations, control procedures during intrabudget periods still seem to be restricted only to whatever is necessary to keep from overexpending budgeted resources. Nevertheless, useful control systems are possible at all levels of research activity. For example, at the bench scientist or project level of research activity, a simple control system might take the form of monthly estimates of percent of planned effort completed versus percent of budgeted resources expended to date. For large programs involving a number of affiliated research activities, a PERT type of control system could be initiated.

Facilitating and Coordinating

The making of decisions regarding the allocation of resources and the control over their uses does not end the active role of administrators and managers in the research activities of their organizations. Especially in multidisciplinary research efforts, coordination becomes as important to the effective conduct of research as possibly the initial decision to engage in specific projects in the first place. In addition, there are usually many opportunities to facilitate research within the research organization with the result that the overall efficiency of resource use is improved. These efforts typically exhibit characteristics of specialization or synergism. A few examples of such efforts are given which make use of computer applications.

In agriculture, probably the best known of such devices is the Current Research Information System (CRIS). While primarily intended for administrative use, CRIS also provides a reasonably quick source of information about currently active research for scientists. Generally it would take a substantially larger amount of time and effort in total if each scientist had to search out the information himself, or the search would be incomplete, or not carried out at all. A comparable device is the Minnesota Analysis and Planning System (MAPS) which selectively retrieves and analyzes socio-economic data from selected census, city-county data, and other files for desired political subdivisions and socio-economic characteristics. The obvious potential in savings of time and effort over individual search and tabulation efforts from secondary sources is substantial.

Another type of information system which aims at facilitating research efforts is the "capabilities" file (CF). A CF maintains a record on the particular capabilities of scientists within the research organization, usually based on records of actual experience both in research methodology (linear programming, analysis of variance, gas chromotography, etc.) as well as by topic of study (blood cholesterol, physiology of cow udders, rural taxation, etc.). The primary justification of such systems is based on the fact that even with good library facilities other scientists are still the best source of initial information on unfamiliar areas of study. A comparable information system is the "equipment" file (EF) which identifies and locates research equipment available within or to a research organization. The EF also indicates necessary information to its use, such as its principal characteristics, when and for how long it may be used, charge rates if any, the principal contact, and other relevant information. There are a number of other examples of such systems that could be given.

Summary Comments

Information Systems literally are means of communication that are more objective, direct, explicit, and concise than interpersonal communications that typify the usual information collection procedures. As such, Information Systems that are well tailored to a research organization and that operate effectively can provide at least four kinds of benefits: (1) Better decision will in all likelihood be made, or as a minimum the decision maker will be more confident in his decisions. (2) Improved coordination of activities within the organization usually results by providing a form of communication that can be understood by different disciplines, as for example in comparing benefit-cost ratios of alternative research possibilities. (3) Similarly, control within the organization also can be substantially improved as a result of usually very explicit statements of goals, time, etc. (4) In many cases the organization of the research system itself may improve as a result of the revelation of relationships, or the extent of their importance, that were not obvious before.

So it is with information systems designed to aid administrators and managers of research activities. However, it is important to realize the manner in which information systems can enter into the decision making process. If too much is expected of the MIS, then limitations may lead to interpretations of failure. On the other hand, beliefs (or fears) that an MIS will take over most of the decision making task can create unhealthy resentments. In either case, these will impede the MIS from becoming the effective tool in decision making that it is intended to be.

The decision process can be viewed simply as <u>information</u> being fed to a "black box" (a <u>decision maker</u>) from which <u>actions</u> are produced. Usually there is a fourth element, the <u>feedback</u>, in which the resulting actions are observed, evaluated according to some criteria, and the results incorporated into future decision making. Hence, there are <u>potentially</u> three points at which improvement in the decision making process can be made, the actions themselves being outside the scope of the MIS to effect. But, in addition to this, anyone who has attempted to apply a MIS knows that the "black box" is also far too complex an activity to very effectively duplicate, or even understand. The variety of values that make up the decision criteria applied to the information in the black box, except for a very few simple ones, are simply beyond the capabilities of existing technology to handle adequately. Hence, the role of the MIS is fundamentally one of providing the decision maker with high quality information, to anticipate for him the likely results of his decisions, and to report what the results actually were. Only indirectly can the MIS improve the operations that go on within the black box.

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2/ A brief descriptive survey of various kinds of information systems is given in Walter L. Fishel, "Information Systems for Agricultural Research" Agricultural Science Review, Vol. 7 No. 1, First Quarter 1969. Cooperative State Research Service, U.S. Department of Agriculture. Washington, D. C.

3/ See Table 1 and following discussion in Association of State Universities and Land Grant Colleges and U.S. Department of Agriculture, 1966. A National Program of Research for Agriculture. U.S. Government Printing Office, Washington, D. C.

4/ The problems of planning in an agricultural experiment station as well as the foundations of the method used are discussed in Arnold Paulsen and Donald R. Kaldor, "Evaluation and Planning of Research in the Experiment Station", American Journal of Agricultural Economics 50:1149-1163 (1968). The procedures of the planning effort are discussed in J. P. Mahlstede, "Long Range Planning at the Iowa Agricultural and Home Economic Experiment Station" in Fishel, Resource Allocation..., op. cit.

5/ The method of development of the classification is described in An Evaluation of Research in the United States on Human Nutrition, Report No. 4, Prepared by a Joint Task Group of the U.S. Department of Agriculture and the State Universities and Land Grant Colleges.

6/ N. R. Baker and W. H. Pound, "Research and Development Project Selection: Where we Stand," I.E.E.E. Transactions on Engineering Management. EM-11(4):124-134.