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FINAL REPORT

A GRAPH ORIENTED MODEL FOR RESEARCH MANAGEMENT

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I. THE RESEARCH PROCESS

The second half of the Twentieth Century has been characterized by some as the beginning of the "Research Age". It is indeed true that in the last decade and a half, organized research has undergone an explosive development, and that the welfare and security of our nation is increasingly dependent upon the proper management and planning of our research activities. Management of research remains, however, a task of extraordinary complexity. It is essentially an unstructured problem, dealt with largely on an ad-hoc basis.

This statement is true in spite of the fact that organization of research goes back at least to Aristotle, who received large grants from Alexander the Great, with which he financed and organized expeditions of researchers to provide him with descriptive material which he incorporated into his books. Centuries later, Bacon of Verulam in his Novum Organum (1620) stated that the three most important discoveries up to then -- compass, printing and gunpowder -- had been made by accident and that incredibly more could be produced by systematic procedure. He conceived the idea of truly scientific laboratories; yet it was not until the 19th Century that these arose. Now, almost 100 years later when we have an abundance of such institutions, we still lack a body of accepted rules which would securely guide those whose task

it is to determine what funds to raise for research, how to distribute them among conflicting demands, and how to evaluate the results of the research activities.

The problem of research management is not new. Everyone in control of some funds and facilities for research has encountered it, given thought to the problem, and had to make decisions as best he could. These ad hoc decisions lack the firm foundations of a theory from which rules of behavior could be derived. It is not surprising that such theory does not now exist, since there are still great difficulties in establishing valid theory in those closely related fields where preliminary progress would have to be achieved, especially in the field of public investment. Thus while a workable theory of research management is the ultimate aim, we are far from achieving it at present.

Another way of looking at research processes is to view them as part of a game played by the researcher with nature, where the usual assumption is that nature is merely indifferent about being found out. In addition, the various researchers undoubtedly play a game with one another and with the supporting agencies—clearly a game of very great complexity. This line

of thought shall not be followed explicitly in the subsequent considerations. However, it should be pointed out that one of the main difficulties is that one will have to start with a finite system (whatever its characteristics), but that one is really confronted with an infinite system. Concluding from a finite to an infinite system is one of the worst difficulties encountered in scientific methodology.

With this restriction in mind, it will nevertheless be shown that some fairly rigorous models can be formulated which will at least provide a set of conceptual tools with which further descriptive work can be carried out, all to be done in the expectation that from a variety of such procedures some useful rules may emerge. But before going in that direction, it is necessary to make some qualitative, essentially morphological observations.

What a model is to be in this context is not a simple matter. We shall be satisfied if we can at least describe possible relationships, without necessarily demanding that they be true or empirically completely valid ones. Only after the possibility of a model has been established can the further questions be approached. In the present report, we shall be content if the first question can be answered in the affirmative (as we believe

to be the case). This will, of course, not involve the establishment of an axiomatic system, which would normally be the ideal test of a model; the above remarks concerning the need to conclude to an infinite system make this amply clear.

A. General Characteristics of the Research Process

What then are the characteristics of the research process that make it so difficult to cope with? First, of course, is the fundamental uncertainty that envelopes any effort to make accurate predictions concerning future results, when one is essentially seeking new knowledge. Uncertainty is only one aspect complicating the problem, however, since even after results are obtained from research activities, it is difficult to make ex-post evaluations of them. This is so because scientific ideas and results are intricately intertwined with each other. These interconnections and feedbacks must be assessed in assigning values to particular research projects. Results in fields related or even widely differing as to subject matter often find common use or supplement each other. Thus, a result in chemistry may prompt new work in biology and lead to new medical practices. Pure mathematics supports a great variety of applied mathematical work devoted to quantitative

models in many scientific fields. This diffusion of research results must take time for propagation, but the derived effects are the stuff of scientific progress.

The more specific the research problems tackled and the closer to naval and other specific governmental tasks, the less applicable will be the results for other areas of work, and the derived effects will not be not great, in general. However, the more basic the research, the greater will be the applicability of results to a large number of different problem areas, and the derived effects will be more important. In any event, it does not seem possible to evaluate alternative programs of research without a serious attempt to include probable derived effects. Here, one may direct research support to specific areas in scientific endeavor or tend to scatter the support widely. In either case, the derived effects cannot be ignored, although they may be more significant in the latter mode of support.

How is one then to evaluate research programs? A research-supporting agency such as ONR must consider as one of its primary responsibilities a self-evaluation of its program policies. ONR, as any other similar agency, should possess and develop the classical "Facultas Semipsam Interpretandi"—notoriously difficult to achieve.

For concrete engineering projects, a judgment on results may not be difficult. If a particular mechanism is sought by the support, it is possible to determine whether the output corresponds to expectations and whether the expectations were reasonably justified; but the nearer the supported research area is to basic research, the less precisely can one stipulate the expected outcomes, if at all, and the more important is the measurement of the derived results for other fields of research. But not all engineering projects are narrow in their effects on general science. A device allowing measurements with new degrees of precision may enable the realization of some critical experiments which in turn will lead to widespread new developments. Thus, one cannot diminish the potential derived effects at all levels of specificity of research objectives.

If possible, one should strive to find a model of the structure of research which allows, at first, better qualitative understanding of the consequences of alternative programs of support and ultimately, perhaps, serves to predict outcomes. Such a model is proposed in this paper, though in a still rudimentary form.

B. Desirable Characteristics of Research Organizations

Aside from the study of models of research, one may consider the desirable characteristics of research supporting

organizations. Such characteristics can be specified even before models become available.

1. Staff Requirements

First, the organization must have a staff which is scientifically trained in various fields and remains at a high level of acquaintance with new developments in the fields. With this knowledge, the staff may better judge the worth of requests for support and also challenge the recipient to justify his use of the money received, without upsetting him. In order to be able to inform research workers concerning the needs of the supporting agency, the staff must be versed in the operating and planning problems involved—not overtly to direct work on specific problems, but to assure research people of the importance and challenge of the problems the agency hopes to see solved. A demanding and narrowly controlling research support staff may easily defeat its own ends, because it takes insight derived from study to formulate meaningful problems.

Second, The process of self-evaluation will be greatly helped if recipients of support are challenged by the demand, from time to time, of progress reports, participation in conferences with others who are possibly not supported but would like to be

supported, and presentation of results. Even the occasional cancellation of projects, or the threat of cancellation, should be considered so that some uncertainty may hang over the head of every recipient. Such cancellations or switches among groups is only possible if no significant fixed installations are involved. Obviously, an observatory like Palomar has to be supported continuously, though another crew might be put in charge. If allocations of funds involve very large amounts to be used for fixed installations, say a linear accelerator, there is implied a commitment for the future and an evaluation of probable results to extend over a long period of time. Methods other than threatened cancellation have to be found in order to secure that the best programs be competently executed. Reports both frequent and in great detail are cumbersome, time-consuming and disturb the researcher; they should, therefore, be reduced to a minimum. But they may be asked for at random intervals of time, rather than at specified fixed intervals. This can be achieved by invitations to participate in the above mentioned conferences, by requests for specific information or assistance and the like.

Researchers, as everyone else, have to be put on the spot from time to time, just as show horses have to be scared

ever so lightly in order to put their best foot forward. In business, this function is taken over by competition. There clearly is open competition also in research, and this should especially be encouraged in basic research. An in-house laboratory of the Navy is less exposed to criticism from the outside, because of the specificity of the problems with which it is concerned. This fact becomes the more apparent when the problems are more specifically related to Naval matters and have no duplication in other fields, either in industry or in other parts of the armed services. The more we approach the area of basic research, however, the more likely it is that results that may be obtained will become applicable in other directions, sometimes entirely unexpected. These results are therefore constantly under scrutiny of people even outside the particular field in which they have been obtained. This continuous challenge and scrutiny has to be preserved and use of it has to be made by ONR and similar organizations, as best as possible.

The problems of management encountered in the field of basic research are often quite different from those found in

applied research and actual engineering development work. As has been observed by Dr. Conant, the former President of Harvard University, in the first area the task is to find genius and to leave him alone. In the second area, it is also necessary to find genius or otherwise extremely talented people, but to focus their intention on the concrete application one has in mind. For a research supporting organization, this requires different attitudes, both in respect to the personal relations with these people and also in regard to the type of support which is required. One of the important ways in which ONR has supported its people is not only that ONR has understood this particular dichotomy in attitudes, but it has also, besides offering money, given moral and scientific support. This is indeed extremely important. The moral support lies in continuing good personal relations, the scientific support lies in the fact that it is possible to go to the ONR offices and to discuss scientific problems, to learn about other people's work in the same or related areas, and to receive other such stimuli. If there is a suggestion to be made here, it would be to intensify this kind of work and to think of organizational devices by which this can be done.

2. Information Exchange

ONR can do much more by informing research workers as to what the needs of the Navy are, by relating what other

researchers are doing in regard to these needs, and by inquiring if certain ideas have turned up in basic research which might have a direct application to Navy problems. This would help to cut down the long time intervals which all too often elapse before results in basic fields find even obvious applications. Although we are making great progress in respect to data retrieval by electronic devices, personal contacts are still a superior way of securing this kind of information flow. Even within ONR, this information flow should be improved. It would be desirable to have more frequent visits of people from the head office to the various ONR supported laboratories. In addition, laboratory workers and scientists in the Naval laboratories should spend substantial time with ONR-supported research workers, especially in universities. Such time could be set up as a kind of "working sabbatical". These sabbaticals could be awarded as prizes, if that should be advisable. The researchers in the universities, in turn, would learn how the laboratories are doing, and the flow of information to the laboratories would improve also. This type of information flow should not be on a vast scale, but by means of small occasional visits and by extended summer study groups. Such frequent exchange visits would also be very valuable for students. They would furthermore contribute to the delay of, or even prevent, the almost inevitable ossification to which labor-

atories are always exposed.

These remarks point to another issue of great significance. It is the fact that individual research workers have phases through which they must inevitably go. They are in an ascending phase, they reach a prime, they decline. While they are progressing on this cycle, new people appear. The new ones may be better than their predecessors, although the latter are still in the higher and more influential positions. In order not to restrain the good newcomer, he should be made mobile within the same organization or be advanced by transfer. Too frequent transfers are also a danger, because tradition is undoubtedly a very great and important asset. It is apparently impossible to lay down hard rules of behavior.

It is natural to view the research process as events occurring not only within individuals, but also in organizations composed of individuals. The individual researchers have different capabilities to begin with. Some produce a flash and then stay at a certain level; others show a more gradual rise in inventiveness but also reach a level. Some remain productive for a long time, often throughout their lives. These patterns are different for various sciences. It is well-known that mathe-

maticians do their best original work early as do theoretical physicists (after which periods their teaching activities assume particular importance). In other areas, a great deal of experience has to be gathered by the individual before he can make significant contributions, e.g., in history.

In general, however, it is imperative to let the promising young scientists come to the fore as soon as possible, without getting them involved in too many administrative duties that take time and energy away from their creative efforts. Yet without intelligent and scientifically competent administration, modern science could not sustain itself. So it becomes, clearly, the duty of the more mature man to enter this field. He may face many internal personal conflicts, inasmuch as some newcomers may eventually overshadow him, helped along by him in this very process. If he truly understands the nature of this development, he will, indeed, find gratification rather than frustration. He must be made to realize that in the life span of a scientist there are many phases, all of them valuable and indispensable. The point is for him to realize which phase he is in and to find the optimal employment of his talents, as they shift from one stage to another. Yet higher authorities will have to watch that

no dampening of the evolutionary cycle occurs, and they may have to intervene from time to time.

Since most modern research somehow is a team affair, whether formally organized within laboratories or not, what was said above concerning the individual, also applies, mutatis mutandis, to the laboratory. Some laboratories and centers of research have succeeded in staying "young" and, indeed, in feeding on their previous accomplishments. Others have not done so. This is only in part due to their internal organization structure and to the accident of individuals working in them from time to time. There are deeper reasons: some are engaged in a strongly developing field of science, say communications or biology, others are concerned with fields from which the interest has shifted away. The former fields attract bright young people; the latter therefore have to be taken under special scrutiny to discover whether relative stagnation is a phenomenon of science proper or the consequence of organizational and personal circumstances. Should the latter be the case, there are various remedies. One of them is to introduce competitive efforts by setting up new laboratories or, depending upon the physical demands of the science, by merely forming new groups in universities, etc.

The conditions indicated here most definitely do prevail, but the variance is great and, at the present state of our under-

standing, defies the derivation of firm rules for the best overall administration of research. But awareness of these problems will lead towards observation, careful attendance and occasional intervention, especially when a new national need appears which requires that a particular field of research and development be stressed.

These considerations apply not only to the top administrators of research, but ultimately to the laboratories and the working individuals. All must be induced to engage in self-evaluation, however difficult or painful.

An additional important issue arises specifically in regard to the scientific role of the government scientist. To the extent that he is involved in secret work, he has the great difficulty of establishing his reputation or maintaining it in the open scientific world. If he participates in this kind of government work, he is making a personal sacrifice in the national interest, provided the secret classification requirements are sensible. If secrecy is excessive, as is so often the case, he is wasting his talents and energies. In ONR, fortunately, there is a large area of free unrestricted work, and it is one of the best practices prevailing there that it allows its staff members to take part in scientific life, including publication and partici-

pation at scientific meetings, without thereby committing ONR as an agency. Scientists who have for long periods been engaged in secret work should be taken out of it into non-classified fields, so as to give them the opportunity of greater interaction with others, as far as their work is concerned. This would contribute to widening their interests and would expose them to the kind of public criticism and appraisal which is of the essence in science. When they return to the classified work, the beneficial effects of this "breather" will not fail to materialize. The importance of this practice,therefore, cannot be overstressed; it falls in line with what was said elsewhere about the interchange of scientific ideas between the ONR staff and those supported by ONR.

3. Information Flow on Foreign Efforts

A significant and somewhat neglected or starved field of activity on the part of ONR is supplying contractors with information about events of a scientific nature in their related fields, as far as the Soviet Union, China and other countries are concerned. Here it is not so much a matter of detail, but rather of trying to keep contractors informed of changes in general outlook and in basic philosophy and attitudes. This is much more than a problem of data retrieval; it belongs in a category of research which is in itself an area which should be supported very strongly.

What is important is to find out more about the intangibles which govern research in other countries. An early discovery of changes in orientation and shifts of interest, a sense of what fascinates foreign scientists, what kind of problems they discard, pick up, etc. will give many a clue as to whether we are on top or may suddenly be confronted with unpleasant surprises. For a military organization such as ONR, one of the chief assignments ought to be to minimize the danger of scientific surprises coming from the other side. These surprises are not necessarily restricted to what would naturally be highly classified by other countries, such as an entirely new kind of radar, proximity fuses, or a missile guidance system, important as these are, but may rather be in the preliminary basic work which goes into the making of these devices. It is believed that there exists here an area in which considerable improvement is possible which can be achieved with very small means of financial kind.

C. Recommendation on Multiple Sources of Support

As a final point, we note that since, as was said above, the threat of cancellation is an important though dangerous and unpleasant tool in research management, a further condition must be fulfilled. Should this condition not be fulfilled, the threat of

cancellation as well as the actual cancellation are sure to be misused to the detriment of all. The fundamental condition is that there must be alternative sources for support. In other words, there must not be only a single monolithic agency which supports defense research, at whatever level, in the United States. Fortunately this is not the case, but nevertheless we must emphasize this point and possibly improve conditions in various areas. There are always tendencies working towards concentration, based on the easy and seemingly convincing argument of "rationality" and "economy", though a closer analysis shows that these terms are either inapplicable or indefinable, when used in regard to research.

If there were only a single authority granting support, let us say in mathematics, and it decided that a certain branch should not be supported, then this would be the end of this matter. It would be left to the individual's own resources to continue or to shift his interests to whatever he thinks he could get supported. If, on the other hand, more than one agency exists, he can "shop around" for his own initial proposal until possibly he finds an agency other than the one that has declined the support. This brings a certain competition into being among agencies, which is just as necessary as the competition among research workers.

The nearer we are in the entire hierarchy of research activities to the field of basic research, the more important it is to leave many avenues open to simultaneous support by different agencies for the same kind of activities. When, on the other hand, we are dealing with engineering problems, duplication on a large scale is, as a rule, not advisable. This is as far as government support is concerned. In the engineering area, multiple efforts by industry will appear at any rate, as soon as profitable government or commercial uses can be imagined. Multiplication of effort by multiple support expresses urgency for the hoped-for result and is a measure of importance assigned to the goal. It is, of course, also a sign of uncertainty in choosing a source from which the desired results should flow in a given time.

It is one of the great strengths of ONR that it has built an enviable reputation and is capable of drawing into its orbit some of the best research workers in the country. As a consequence, ONR is likely to receive some of the most interesting and promising applications for funds. The fact that ONR has the dual capability of using its own staff as well as by calling in additional advisors, will tend to keep away the trivial while encouraging the novel, though still untried, ideas.

II. PRESENTATION OF MODEL IN GRAPHICAL FORM

A. Allocation Problem

The basic problem facing an agency that is set up to support research at various levels is clearly the follow.ng: given a certain amount of money, how is this amount to be distributed optimally among different possible uses when there are more of them than can be satisfied with the available funds? This looks like any ordinary economic problem and it has many similarities with one, as will be shown, but there are formidable complications which set it apart.

No rational allocation is possible among fields, unless there exist clear notions about values, preferences and utilities. Alternative uses of resources must exist and be compared with one another; this requires the introduction of a notion of cost which, in conformity with modern economics, has to be viewed in terms of "lost opportunity". That is, the support of one research project is the inability at the same time and with the same funds to support another project. This is the true "cost" of supporting the chosen project. To justify the choice made, the supported project must be more important, promising or valuable than the one discarded. This is, of course, what every support agency tries to establish, even though a formal calculus for determining values may not exist. In that case, rules of thumb are used,

experience is called upon, hunches play a role, etc. The considerations in Part I above are an attempt to indicate how the organization of the support agency may bring about the best approach to the use of these qualitative and subsidiary elements in the decision process.

In ordinary economic allocation, there is a comparison of a known utility with another one. That is to say, we know what an automobile will do, or the consumption of a pound of meat, sugar, etc. We form an expectation of the effects, based on experience or at least on a projection of effects from similar situations in which we have encountered the object of our choice. If there were no experience of this kind, no preferences at all could be formed and rational action, as understood commonly, would be impossible. Not being sure of the effect upon us, if the event is realized, necessitates the introduction of expected outcomes with certain probabilities.

In research the complication is as follows: a decision of allocation of the right amount is to be made when there is sometimes no information as to what effect, if any, can be expected. One can compare, ex post, a phenomenon or result of an allocation with others; but there is no information, in general, about what would have happened if another allocation

had been chosen. This holds ex ante as well as ex post, for the simple reasons that no event has occurred in the unsupported area, precisely because it was not supported; though the suspicion remains that, had support been given, something possible of greater value might have materialized. So we compare the result X^1 (realized) at t_i with our hoped for X at t_{i-n} (when the allocation was made) and with the unknown effects Y at t_i, about which we can only form vague ideas, since they remain at t_i just as inaccessible as they were at t_{i-n}. So the difficulty is that only realized effects can be compared with one another, which allows a partial evaluation of the allocations made. But in research the essence is that an unknown effect was aimed for, which in most cases can only be imperfectly described and in some cases, not at all. The more basic the research, the more is the case. In engineering research, concerned with a novel application in a well defined and well controlled environment, most of these difficulties are diminished.

It follows that the agencies supporting basic research encounter greater uncertainties than those primarily involved in research directly connected with improvements of known devices, methods, etc. Most agencies, certainly ONR, support both basic and applied work and therefore have the problem of allocating funds among areas and then of choosing optimally within each area.

A further complication is the time factor. Expected effects may happen, if they do at all, at moments of time quite different from those for which they were anticipated. This affects the value of the results, one achieved early normally being more valuable than one obtained later. Yet some are interdependent and the exploitation of an early outcome may have to wait for another one, whereby its value is temporarily diminished. This intertwining of research activities, which extends from one to the other and variously over time, is one of the basic characteristics of the field and represents one of the most formidable difficulties of analysis. Furthermore, whatever patterns can be discovered are not likely to remain stable for long periods, because instability is precisely the consequence of new effects produced by the entire research effort. It is natural, however, to look at a stationary state first, in order to see whether any kind of structure can be discovered at all.

In order to approach the problem of allocation of financial funds and human effort, it is necessary to obtain a description of the entire process of research activities as they go on at various levels and in diverse fields. If a model can be constructed at all, it would be the first indispensable

step towards an operationally useful procedure for any research-supporting agency to discover its own position in the entire complex of research activities. Clearly, no agency can support all fields, yet it is fully exposed to the effects of research carried on by otherwise supported activities, on which in turn it also produces some feedbacks.

B. The Graphical Model

1. Classification Schemes.

We turn to a discussion of our model of the structure of research for planning and management. As stated above, the structure of research is similar in form to other activities in the economy; it involves the utilization of scarce resources as inputs in order to obtain desired goods or services as outputs. Research outputs, however, are not measured or evaluated like commodities in the market. Clearly, they are multi-form and multi-facet, qualitatively complex, and not at present measurable or describable in simple terms; they range from mathematical formulations of phenomena to critical experiments and laboratory devices. Published papers are descriptive of work done but, in themselves, as so many pages of printed material, cannot serve to measure outputs of research, which are ideas, conceptions, theories, models, experimental results, devices, new materials, etc.

The value of the outputs from any one research project is intricately intertwined with the outputs of other projects. The accumulation of knowledge is a self-generative process. One idea depends on a host of others and leads to another in an endless chain. In trying to evaluate the outputs of research activities, it is thus

more significant to seek the consequences for further research in the given and related areas of research than to try to measure outputs as values of an exchange economy. When results of research can be used directly to resolve or facilitate a particular social purpose, government responsibility or national objective, a social utility can possibly be identified, but the full social value is not restricted to such direct impacts, which may be purely temporary. A more obscure result may lead indirectly but surely to other research outputs, which have greater social impact. Thus the interrelationships between research activities leading to derived effects are essential for any true model to serve the planning and management of research. It is therefore clear that the worth of any given research support should be determined from both derived and immediate effects. In fact, for basic and applied research not directed to obtain particular social ends, practically all of the social worth comes from derived effects.

While it is an extremely difficult task (in actual practice) to obtain a quantitative measure or index of the expected interrelationships between research activities, one can usually specify at least which activities are interconnected and which are likely to be independent of each other, for all practical purposes, over the next decade. As a first step, one can then utilize this information

to build a graphical model of the type which we suggest below. Such a graph may be used to make qualitative conclusions about questions of research management. As methods and procedures are developed which are capable of assigning meaningful numerical estimates to these interrelationships, one can formulate the graphical model into matrix form and use it to make quantitative conclusions about different research allocations.

In order to construct our graphical model one must first make a reasonably comprehensive taxonomy of basic and applied research fields. For this purpose, the classification system of the National Science Foundation may be used, supplemented by project statements for the more directed research activities carried out in government laboratories. Development activities are another matter, distinct from research, and they should not be described with research in one and the same model, since they have, presumably, specific objectives which can be programmed to yield well defined systems.

The specification of research activities is not something which can be expected to result in a unique classification system. In each field of research, the expert can conceive of finer distinctions which lead to more and more categories of research, and one must proceed in some fashion without resorting to the extreme of taking each researcher as involved in a separate distinct

activity. Thus, we are involved in a procedure of aggregation and the guide lines for the choices involved should be drawn with an eye to the uses contemplated for the classification system.

2. Structure Modeled by Linear Graph

Once we have constructed a classification system, we may now display on a linear graph the interrelationships between research activities and connections to Government activities, as illustrated in Figure 1. The nodes of this graph represent distinctly identified research activities, arranged so as to progress from Pure or Basic to Applied to Direct research, and the directed arcs show the interrelationships.

Each node corresponds to an aggregate of research efforts in some category of the taxonomy of basic and applied research fields, involving groups of people ranging from a single individual to large groups of people scattered among many universities or concentrated in large laboratories. The number and complexity of researchers involved depends upon the degree of aggregation which may vary as one progresses from Basic to Direct research.

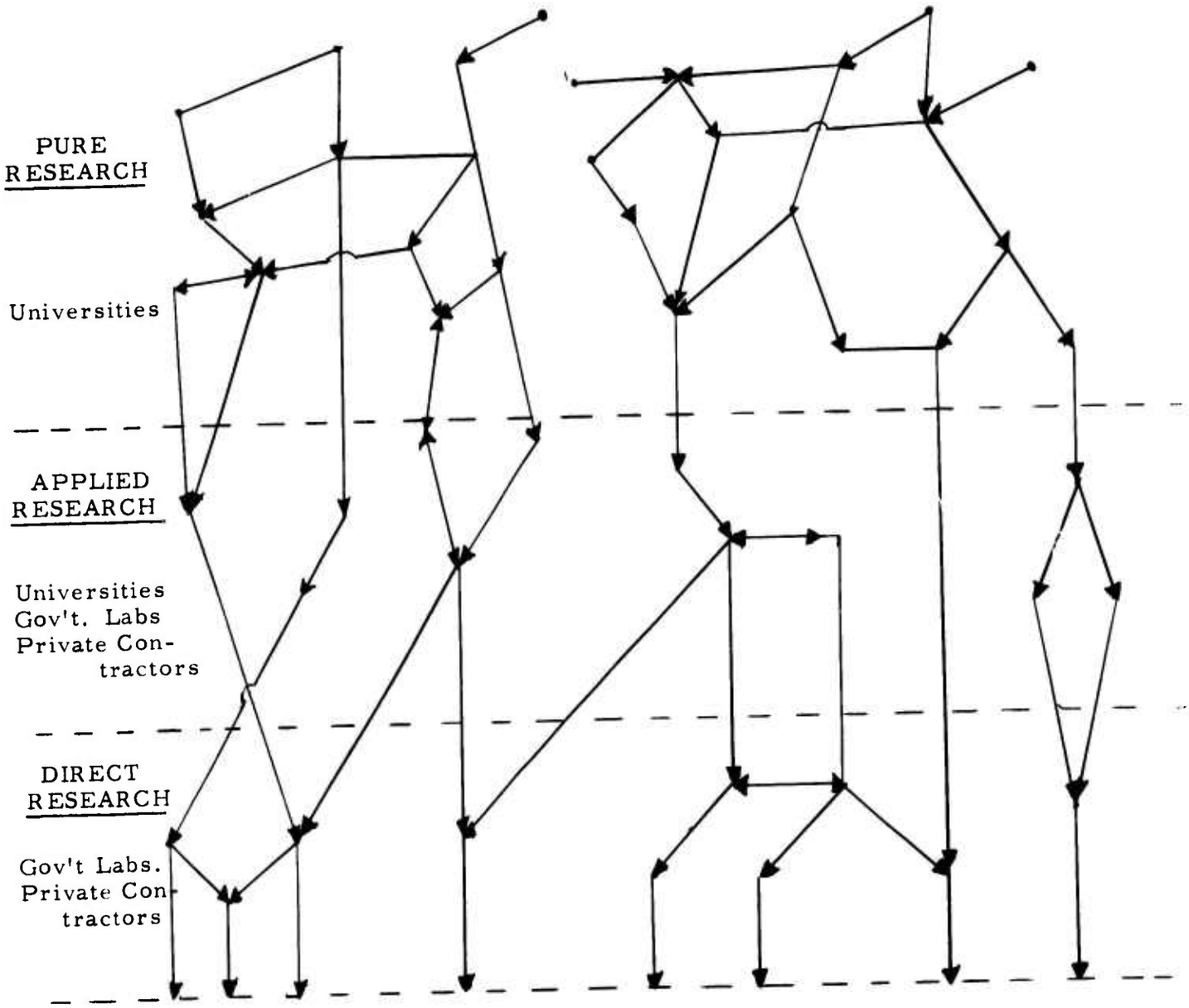
Direct research is distinguished from Applied research as primarily bearing directly upon some specific problem area of government activities, while Applied research generally deals with a larger class of problems having identifiable potential

applications in several Direct research areas and even, at the same time, perhaps, having direct use for the resolution of some governmental problems. The distinction is as much one of motive as it is the result likely to be obtained. A direct research effort may sometimes lead to generally formulated solutions and methods which have application and use in apparently dissimilar areas of other direct research efforts. They thereby have the property of Applied research of being applicable to several distinct Direct research areas. Pure research is undertaken to gain new knowledge in itself and is not consciously concerned with specific practical problem areas, although there may be an awareness that the results may be significant or important for a class of engineering problems, such as purely mathematical studies were for propellor design. Here again, the dist.nction between Pure and Applied research is mainly one of motive, and some work in the latter level of research may develop into a pursuit of knowledge for itself, stimulated by a class of applications to be made.

For all three levels of research effort, the distinctions depend upon the degree and kind of aggregations made in defining research activities. As in all aggregations, there is a certain freedom or arbitrariness regarding the amount of

FIGURE 1

Interrelationships between Research Activities



OPERATING GOVERNMENT ACTIVITIES

lumping together of different activities or processes.

The arcs between nodes may be uni- or bi-directional, indicating independence of one of the two fields from the other or mutual dependence. Those applied research activities which bear directly upon specific problem areas of government responsibility will be primarily uni-directional toward a platform of government responsibility, having the appearance of the trunks of treelike graphs whose upper stems and branches fan out as one proceeds to more pure research activities, with greater interconnections between nodes, i.e., more like a web than a tree.

When a particular research activity is regarded in its relationship to some operating government activity, very often one may find no single, direct arc of connection to it and be inclined to regard the research activity as having no bearing upon the problems confronted. Yet, study of the graph will show relevance through one,or more, other research activities and thus indirectly that it has strong influence. The test for relevance is whether there exist connected paths between the given research activity and the particular government problem studied. Within the accuracy of the graph, one may seek research activities to support which have bearing, direct and indirect,

upon some broad area of government responsibility. It is clear that if a direct arc exists, the connection is strong and intuitively convincing. If there are many nodes through which the connection has to pass, the matter is more complicated. The significance for the government problem of the research activity at the most distant node arises from the derived effects along the totality of connected paths to the problem area in question, and these effects are not simple to perceive, since they interact and are carried along with the effects of the research activities at all of the nodes of the connected paths. More will be said about these derived effects when a quantitative form of the graph model is considered to Part III below of this report.

3. The Three Research Trees

There are three possible variants of the linear graph described above which will be useful to the Navy in making allocations. We shall denote these three graphs as the Fundamental tree, the Navy tree and the ONR tree. We shall now discuss each of these concepts in some detail.

(a) The Fundamental tree describes the "objective" relations among nodes, i.e., those influences of each node on

others as can be established from our understanding of the interconnectedness of science and engineering. It shows necessary and possible connections (at the given level of aggregation), such as that between the mathematical theory of differential equations and the theory of heat transfer and related problems in engine construction, or between any mathematical theory and the various theories of physical phenomena. There is a web or circularity within the field of pure research, a lesser web and more direction in the applied research area, and usually a one-directedness in the final phases of direct research. But given a certain state of knowledge in pure science and technology, and neglecting time, the relationships between the aggregated nodes of the fundamental tree are reasonably objective (i.e., they are reconstructable by different individuals with knowledge of science and engineering). This tree tells us nothing about financial support; it exists whatever the level of support may be.

(b) The second concept of relevance to research management is the "Navy Tree". This is a fully connected sub-graph of the Fundamental tree which delineates, again objectively, the specific influences leading out of the pure research via applied research to the specific Navy purposes

and hardware programs. This is one interpretation of the tree described in Figure 1. If the Navy has a very narrowly defined or limited set of objectives, the tree will be a very small and slender subgraph (though extending into the fundamental research area); if the Navy has very ambitious goals, hoping to utilize the fruits of wide areas of industrial and government research, the subgraph will be broader but it will never coincide with the whole Fundamental tree, which describes all research going on in the country, some of which will never have any perceptible Navy connection. A complete coincidence would be a sign of an absurdly large aggregation.

Similarly, an Air Force and an Army Tree can be distinguished, both having corresponding relations to the Fundamental tree. The three military trees differ from each other because of the desired end-products to which research is supposed to lead; they demand in part different sequences of arcs. The differences may be nil or negligible in the pure research area, but become visible in the engineering part and substantially greater as the distinct final products are approached. From this it follows that all services (and industry) will benefit from any development in the pure research area and to a lesser and lesser degree from each other as the other areas are approached.

As an illustration, the Air Force does not build submarines and consequently is uninterested in all research specifically connected with them, such as pressure gauges, torpedo tubes, optimal bunk size for the crew, etc. But the equations determining the hydrodynamic behavior of the hull in water are reducible to those by which one determines the aerodynamic properties of planes and missiles, in which the services have overlapping interest and which, therefore, both may support. This will show up in that certain parts of the Navy and Air Force trees which have common nodes and arcs.

The Navy tree as such tells us immediately nothing about support, actual or desirable. It merely informs us about the specific relations and subsets of the Fundamental tree which are of concern to the Navy and the other services, shows where they have common interests and at which points they begin to diverge. Once the shape of this tree has been determined, it is possible to begin to evaluate the effect of research interrelationships to Navy goals. The various arcs are necessary arcs, considering our understanding of these relationships in an essentially stationary state. An examination of the graph would show which connections are relevant to the achievement of some Navy objective. If in actuality there is a lack of performance at one or the

other node (a fact to be determined by records, evidence obtained from scientists, etc.), then a reason is given for support of the node at which the arc originates in order to stimulate the desired flow, in the next or at any stated later period of time.

(c) The above considerations lead us to the formulation of our final graph, the ONR tree, which need not be a fully connected subtree of the Navy tree. Rather, it is a picture superimposed on the Navy tree of research activities supported (nodes) and arcs of influence, which ONR thereby hopes to generate or has generated and tries to keep going or to strengthen.

Support of research is reflected at the nodes. If it is un-directed, (i.e., without prescription as to the kind of results sought), the aim of the support is general enhancement of the generation of knowledge, and the benefits to the Navy arise from the diffusion of this knowledge to other related research activities (i.e., undirected derived effects). In such support, new nodes of research activity may be supported by ONR. If the research support is intended to yield results which affect and facilitate other specific research activities, the arcs of intended effects on other nodes, along with the node supported, need to be distinguished. Such directionally effective research support will

typically be attempted in the Applied and Direct research areas, near to the final engineering stages. In these cases, there is strict guidance of the research. Specific instructions are given concerning the direction in which one should look, prescriptions and projects of very definite character are worked out, quite different from the support of a node in the area of basic research, where one will gladly accept any connection to another node that may arise. It may, of course, be that no new arc forms, no additional influence on existing arcs is observed, though the node itself may benefit. The uninstructed support will largely be in the area of fundamental research; the nearer we come to final, practical applications the more definite will the instructions become that the supporting agency designs. Even here there are dangers, since creativity is just as much present in those fields and many ingenious turns cannot be foreseen. Indeed, results in the final technical area may produce, on occasion, even an arc back to the area of fundamental research, for example, by creating the possibility of new measurement tools which are required to provide new inputs, even for some of the most rarefied fields of theory. The history of mathematics is replete with cases where new problems were put by physics which, in turn, could only arise because new experiments had become possible.

The "tree" of actual ONR support will look something like this

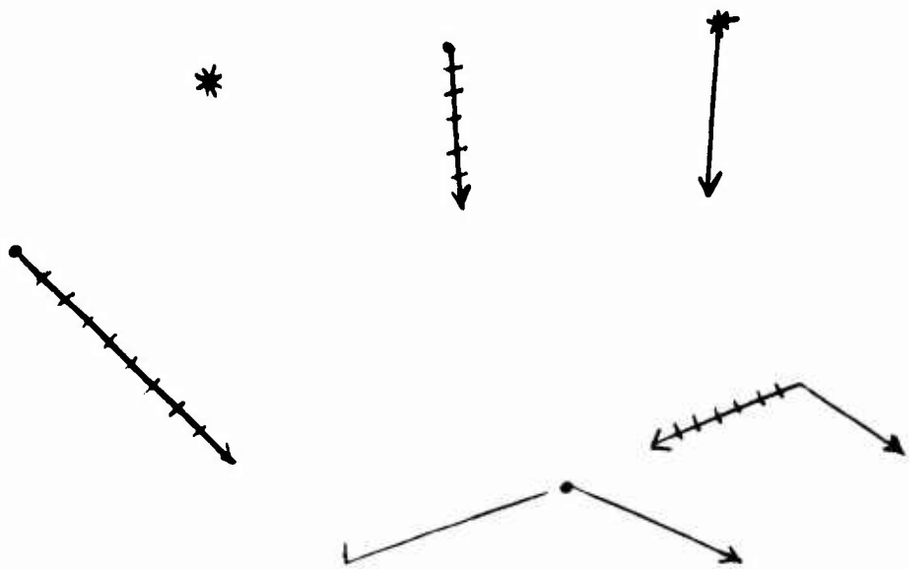


FIGURE 2

The ++++++> indicates general support (in the hoped-for direction), a ———> indicates that the support is tied to a directive, for the node, to try to evolve results that will augment the state of activity at a specific subsequent node, etc. These arrows are superimposed on the Navy tree. A * at a node indicates that it is being supported but that, at best, only an expectation exists that consequences of the support will affect other unspecified nodes. Clearly, these do not have to be supported

everywhere, since there are also other sources of support (e.g., NSF in the pure research area, universities, AEC, . . .), although ONR may very well want to add to those expected streams in order to obtain a larger or speedier effect.

Thus ONR's interest is not primarily with the necessarily rudimentary ONR configuration, but with the underlying Navy tree which ONR must establish, keep up to date and observe continually. ONR will measure its own performance in terms of its effect on the Navy tree, which was, in turn, derived from the Fundamental tree. ONR will also have to compare the flux described by the Navy tree with the simultaneous support given to the Army and Air Force trees by their respective support agencies.

At each node, the dollar value of support may be entered; it will have to be compared with a value index for the ultimate results expected, i.e., payoff for Naval responsibilities.

At present, one is far removed from possessing a generally acceptable method for assigning numerical values for different expected research results. Even ex post values can only be determined roughly by stating orders of magnitude. Nevertheless, it is helpful to describe the research process in

terms of the above analysis, since understanding of a morphological kind has to precede a deeper analysis.

An important lesson to be learned from the history of science is that there is no point in demanding too much (an easy thing to do). Progress has always been connected with limiting objectives in a modest way. In this specific case, one should recall that it has taken generations to clarify the economic concept of utility and preference and that this work is not concluded. Yet economic utility is a far simpler notion than the utility of new scientific or engineering discoveries, especially when they have multiple uses and ramifications.

The next section will show that it is possible to distill from the graphs described so far a considerable amount of the information which should prove to be useful in applications. The procedure has the advantage of making use of some recent advances in economics, combined with proper mathematical techniques.

III. PRESENTATION OF MODEL IN MATRIX FORM

As a model of the structure of research, a linear graph provides several insights which are not easily obtainable from isolated evaluation of research programs. Such a graph makes possible an analysis of the interrelationships between research projects, at least in qualitative fashion. For a more sophisticated treatment of the derived effects of research, however, we need to introduce transfer coefficients which define what fraction of the research in one activity carries over to another with which it is connected. Once we have introduced such coefficients into our analysis, we may use the mathematical methods and techniques associated with matrix algebra to make quantitative inferences concerning the derived effects of research activities. Although the actual coefficients may not be known or determinable with great accuracy, we may for the present, for model purposes, assume their existence.

We now proceed to introduce these coefficients into our analysis. Suppose that for any two research activities A_i and A_j, the transfer coefficient c_{ij} defines the transfer of research results of A_i and A_j. In general, we assume these coefficients to have the following properties:

(1) $0 \leqq c_{ij} \leqq 1$

(2) $c_{ij} = 0$ if no arc connects A_i in the direction of A_j, otherwise $c_{ij} > 0$

(3) c_{ij} is not necessarily equal to c_{ji}

(4) $c_{ii} = 0$

Property (1) expresses that the transfer coefficient represents a fractional or percentage transfer of research effort from one research activity to another, which we assume cannot be negative and can at most have a value corresponding to a transfer of one hundred percent. Property (2) relates to the one to one correspondence between the graph and the coefficients (unitless numbers), which represent arcs on the graph. Property (3) states that the interchange between two activities is not necessarily equal in both directions, and Property (4) states that the derived effect of a research activity for itself is zero.

We may interpret these coefficients in the following manner. If a given level of research effort is being performed at research activity, i, say y_i manhours, (we shall use manhours as an index of effort for the present), then this implies that there will be a "derived effect" or transfer of research effort to the j

activity equal to $y_i c_{ij}$. By a derived effect here, we mean that y_i manhours of work on activity i has an effect on activity y which is equivalent to $y_i c_{ij}$ manhours of work being directly done on project j. The derived effect at research activity j, however, will have its own impacts on other fields of research, i.e., will produce a second stage of derived effect, and these secondary derived effects will be equal to the product of the initial derived effect times the transfer coefficient at the jth activity. For instance, the secondary derived effect of research activity j on activity k, resulting from an initial amount of research done at research activity i, is given by $y_i c_{ij} c_{jk}$.

The process of derived effects will continue through successive stages until the value of these effects become negligible. This model of the research process expresses the explosive growth features which we have all observed in our own experiences.

We may arrange all the transfer coefficients into a matrix as follows:

$$C = \begin{vmatrix} c_{11} & c_{12} & \cdots & c_{1n} \\ c_{21} & c_{22} & \cdots & c_{2n} \\ \vdots & \vdots & & \vdots \\ c_{n1} & c_{n2} & \cdots & c_{nn} \end{vmatrix}$$

where c_{ij} is the transfer coefficient representing the effect of

research activity i on research activity j. A matrix such as C allows for all possible interchanges between research activities. We may further represent the research efforts done on the activities as row vector Y

$$Y = (y_1, y_2, ---y_n)$$

where y_i is the research effort of the ith activity. We may now represent the total derived effects by a series of vector matrix products of Y and C. Let us denote the vector of total derived effects by a row vector, X, which is measured in the same units of research effort as Y. Then we have

$$(1) \quad X = Y(I+C+C^2+C^3 ---)$$

The first term on the right hand side of (1), the product of Y times the identity matrix I, is just the initial levels of the research activity. The second term, YC, is the first round of derived effects resulting from the initial level Y. As described above, the first round of derived effects will have their own derived effects, given by YC^2, which in turn will have derived effects, and the process continues ad infinitum. We would expect, however, that the coefficients of C be such that the successive stages of derived effects steadily diminish. If

this is the case, the series of terms on the right will converge to a finite value of X. The series approaches its limiting value in asymptotic fashion, and the incremental contribution after the first few terms is usually very small.

When the series converges, we may write equation (1) in the form

$$(2) \quad X = Y[(I-C)]^{-1}$$

and

$$(3) \quad Y = X[I-C]$$

by making use of a well known algebraic identity.

The equations set forth in (2) and (3) are identical in form to those used to study the production process in Leontief Input-Output Analysis. In input-output analysis, one investigates the problem of how to produce a particular final bill of goods, given the interdependencies that exist between various goods in the process of production. For any net final bill of goods produced for an economy's consumption, a much greater gross bill of goods must be produced, in order to have the inputs available to produce the final outputs. The situation is reversed in the research process, where we obtain a greater final output of research effort than our initial input, due to interactions between research activities. The structural similarities between the two

situations underlies the mathematical isomorphism between Leontief Analysis and our model.

Implicit in our relationships derived in this section are the following assumptions concerning the input-output structure. The level of research outputs in each field are proportional to the level of inputs in that field, and in addition there may be derived effects between fields which take the form of fractional transfers of research outputs from one field to another. Mathematically these assumptions may be expressed by the formula

$$(4) \qquad x_j = k_j y_j + \sum_{i \neq j} x_i c_{ij} \qquad i, j = 1, \ldots, n$$

where x_j = total output of the jth activity

y_j = total input of the jth activity

k_j = the factor of proportionality between inputs and outputs in the jth activity

c_{ij} is the transfer coefficient expressing the output of the ith activity relevant to the jth activity.

Since we have assumed that research output is proportional to research input in a given field, it is convenient to normalize

the units of (4) so that a unit of input in a research field leads to a unit of output in that field. Doing this, we get

$$(5)\quad x_j^* = y_j + \sum_{i \neq j} x_i^* \, c_{ij}^* \quad \text{where} \quad x_i^* = x_i/k_i$$

$$c_{ij}^* = \frac{c_{ij} k_i}{k_j}$$

The vector-matrix formulation of (5) will be identical with that derived above in equations (2) and (3).

Thus, for an input-output system where there is a strict proportionality between inputs and outputs, the input units may also be used as indices of research outputs. This is implicit throughout our analysis at the beginning of this section, in which the above process is viewed as occurring in successive stages; now we directly consider the final relationships between inputs and outputs.

In the above analysis, we have abstracted from the fact that there must normally be a certain amount of search effort for the derived components of research output, in order for them to become effective in any research field. Otherwise they will remain only as potential research outputs for that field. Since this search effort leaves less time available for direct research, the total out-

put corresponding to any given input will be somewhat less than that given by equation (2).

For various programs of research effort Y, some emphasizing direct and others, more strongly, pure research, the total derived effects X can hypothetically be computed and compared by means of equation (2). In this way, one may seek to evaluate the merits of various balances of effort between pure, applied and direct research, in so far as they yield total effects on research programs directly related to government activities or national objectives.

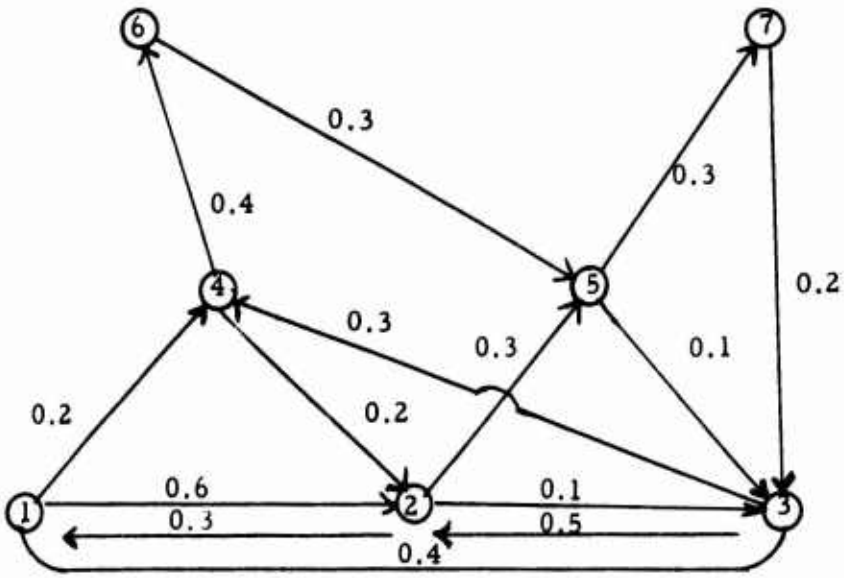
The foregoing model is a linear steady state formulation in which the vector Y and transfer coefficient matrix C are not obviously measurable for practical purposes. Yet it provides a system suggestive of the underlying structure of research for planning and management purposes. In what terms can the components of Y be measured and how are the corresponding transfer coefficients to be estimated ? These are questions requiring some thought. One possibility is to define the level of effort of a research activity in terms of manhours of scientific effort per annum, since ultimately research is a process of applying the trained, imaginative mind of man. Then we may conceive the

transfer coefficients as fractions expressing the manhours translatable to other research activities and seek to estimate them in terms of manhours expended on results which are applicable to other research activities, ignoring the differences between insights of individual researchers. For this approach, it is not clear how expenditures on equipment may be handled. Another possibility is to use dollar value of expenditures on the various research efforts as measures of effort and estimate the transfer coefficients in terms of dollars expended on results which are applicable to other research activities. Here we have to face the fact that comparable research efforts cost different amounts, depending upon whether they are carried out in universities, government laboratories or industry, use heavy equipment etc., and some price adjustment will be required.

We may illustrate the steady state for our model by means of a simple numerical example. In actual practice, we would expect a considerably larger and more intricate a system than the one which will be discussed here, but it will serve the purpose of demonstrating the above type of analysis.

Let us suppose that a graph depicting the interrelationships between research activities has been constructed, corresponding

to that shown in Figure 3. There are seven nodes in the graph



Numerical Example
FIGURE 3.

representing seven research activities arranged in denoting a progression on the basic-direct research spectrum. The directed arrows portray the interrelationships between research activities, with the coefficients above each arrow showing the transfer effects of research activities. For example, we see from the graph that a manhour of work on activity 1 has a derived effect of .6 manhours of work on activity 2, .4 manhours on activity 3, and .2 manhours on activity 4. The matrix of coefficient corresponding to the graph

in figure 3 is given by

$$C^* = \begin{vmatrix} 0 & .6 & .4 & .2 & 0 & 0 & 0 \\ .3 & 0 & .1 & 0 & .3 & 0 & 0 \\ 0 & .5 & 0 & .3 & 0 & 0 & 0 \\ 0 & .2 & 0 & 0 & 0 & .4 & 0 \\ 0 & 0 & .1 & 0 & 0 & 0 & .3 \\ 0 & 0 & 0 & 0 & .3 & 0 & 0 \\ 0 & 0 & .2 & 0 & 0 & 0 & 0 \end{vmatrix}$$

In order to compute the derived effects associated with any initial level of research we must first calculate $(I-C^*)^{-1}$. Subtracting C^* from the identity matrix and taking the inverse of the resulting product, we get

$$[I-C^*]^{-1} = \begin{vmatrix} 1.40 & 1.32 & 0.76 & 0.51 & 0.46 & 0.20 & 0.14 \\ 0.46 & 1.52 & 0.41 & 0.22 & 0.48 & 0.09 & 0.14 \\ 0.26 & 0.86 & 1.24 & 0.42 & 0.31 & 0.17 & 0.09 \\ 0.10 & 0.32 & 0.11 & 1.05 & 0.22 & 0.42 & 0.06 \\ 0.04 & 0.14 & 0.20 & 0.07 & 1.05 & 0.03 & 0.31 \\ 0.01 & 0.04 & 0.06 & 0.02 & 0.31 & 1.00 & 0.09 \\ 0.05 & 0.17 & 0.25 & 0.08 & 0.06 & 0.03 & 1.01 \end{vmatrix}$$

We may now compute the derived effects for the research situation depicted in Figure 3 by use of equation (2):

$$X = Y[I-C^*]^{-1}$$

For an initial level of 1 manhour for each research activity (i.e., $Y = (1, \ldots, 1)$), we have a total effect given by

$$X = [1, \ldots, 1][I-C^*]^{-1}$$
$$= [2.32,\ 4.37,\ 3.03,\ 2.37,\ 2.89,\ 1.94,\ 1.84]\ .$$

One can see that the total effects from an input of 1 manhour to each activity are several times the initial input. Activity 2, which has the largest first stage effects (the sum of the second column of C^*), also has the largest total derived effects. It is a central research activity in this structure, having single or multiple interconnections with most of the other activities. Activities 6 and 7, which are the least interconnected, have the smallest total derived effects. Nevertheless, an examination of the matrix $(I-C^*)$ shows that there will be derived effects from <u>each</u> research activity to <u>all</u> other research activities (although some are negligible). This is true even for a structure of research where most activities have no direct interconnection with each other (14 of 49 possible interconnections). This example thus illustrates the necessity of obtaining a system which provides an "overview" of the entire structure of research rather than the consideration of each activity in isolation.

Our model has thus far been developed in a steady state formulation, ignoring the complications arising when time is

explicitly considered. Since the development of research is undoubtedly a dynamic process, time ought to be introduced into the model so that the derived effects in one period arise from transfer of research in previous periods. If we assume for simplicity of analysis, that each stage of the derived effects takes one period of time to become effective, then the total effect occurring in period t, X(t), is given by

$$X(t) = \sum_{j=o}^{k} C^{j} Y(t-j) \quad ,$$

where a finite interval of k units of time covers all transfers. This is essentially a distributed lag model, with the research occurring in any given period related to that occurring in the immediately preceding period given by the formula

$$X(t) = CX(t-1) + Y(t) \quad .$$

In actual practice, we would expect the time for the transfer effects to work themselves out to be different for different research activities. We might expect that the more direct the research activity, the shorter will be the lagged transfers and vice versa. This introduces an interesting complication but also increases the burden of the problems of estimating the transfer coefficients.

The roundaboutness of derived effects, both in time and field of research activity, appears to be characteristic of the research process. For some considerations, the specific time when research results become available may be important and optimal expansion plans may be sought toward some future goals. Contrariwise, future results may be discounted for current goals.

Throughout the foregoing discussion, we have not tried to make social value judgments concerning the final activities, i.e., the government objectives and the related direct research, and hence no optimizations are suggested for the present model. The social worth of end objectives is a valuation problem, which must be studied in conjunction with the relevant resource limitations over time, such as budgetary constraints of the ONR and scarcity of specific scientific personnel. If one were to expand the model by introducing explicitly various resources as inputs to the research activities, with constraints upon the total availability of such resources, a preference or utility function could be set up to express the relative social worth of the various final activities. Optimal allocation of research efforts might then be attempted. As a simple example of such a model,we consider the problem of optimally allocating the annual budget

B among various research activities assuming one has a utility function $U(y_i, \ldots, y_n)$ representing the relative social work of the final activities. The utility function will in most situations be a non-linear function of the research outputs due to the desire on the part of most research organization for a balanced program of research. We assume as above that each stage of derived effects takes one full period to become effective. Allowance is made for the possible discounting of future returns by the presence of a discount factor r. Scarcity of research personnel in the different research activities is also taken into account.

Our model may be represented by the programming problem

Maximize

$$U(Y[I + rC + r^2C^2 + ---])$$
$$= U(Y[I + rC]^{-1})$$

subject to

$$\sum_{i=1}^{n} p_i y_i \leq B$$

$$y_i \leq A_i [i=1, \ldots, n]$$

where:

U is the organizational utility function

C is the matrix of transfer coefficients

Y is the vector of research allocation in period t

r is the discount factor

B is the total fund available in the budget

p_i is an index of the cost of a manhour of work in research activity i

and

A_i is the maximum amount of scientific manhours available in a given research activity

The programming problem may be interpreted as choosing the allocation of research effort which maximizes the value of the discounted derived effects, subject to the constraints that the total budget is not exceeded and the research effort allocated to any given activity does not exceed the maximum effort available.

The above primitive model may be considered to represent the annual budgeting situation. It encompasses mainly short term considerations in the sense that future budgets and changes in structure are not taken into account. A long run growth model may be constructed which explicitly considers these complications. Many variations and extensions of the model may be made for optimization purposes. However, before doing so, we should try to implement our basic model and empirically ascertain whether our underlying model actually describes the real process of research. But we feel assured

that the primary aim has been achieved, viz. that it is possible to construct a model even at the present preliminary state of conceptualization, and we know that the possibility has to be demonstrated before the further problem of empirical relevance can be attacked.

APPENDIX TO PART III

Since errors in the determination of structural coefficients of an input-output system are inevitable in any empirical application, it is important to analyze the effect of these errors on the estimates of the output vector for various input allocations. Because of the cumulative nature of the research process with its multitude of derived effects, any errors in research transfer coefficients will likewise have a cumulative effect on output estimates. Nevertheless, the structure of this process is such that the errors possess certain desirable properties, such as positive and negative errors in the structural matrix having compensatory effects on the estimate of the output vector. We now proceed to an examination of these properties in some detail.

Suppose C represents a "true" or exact structural matrix of research transfer coefficients and C^* represents the estimated or observed structural matrix, the two being connected as follows:

$$(1) \quad C^* = C + U$$

where U is an $n \times n$ matrix of errors. Then from the above definition

$$(1') \quad (I-C) - U = (I-C^*)$$

which implies

$$(2') \quad (I-C^*)^{-1} = [I-C]^{-1}[I-U(I-C)^{-1}]^{-1}$$

Equation (2') shows the relationship that exists between the inverse of the observed matrix, the true matrix and the error matrix.

To enumerate some of the properties of the inverse of the observed matrix, let us assume that there exists an error in only one transfer coefficient (i.e., that all elements of U are zero except one). We will denote an element of $(I-C^*)^{-1}$ as a^*_{jk}, with the corresponding element of $(I-C^*)^{-1}$ as a_{jk}, and assume $u_{i\ell} \neq 0$. Then eq.(2') implies

$$(3') \quad a^*_{jk} - a_{jk} = a_{ji}\, a_{ik}\, u_{i\ell} / (1-a_{\ell i} d_{i\ell})$$

It follows directly from eq. (3') that for $u_{i\ell} < 0$ we have $a^*_{jk} \leq a_{jk}$. Since a^*_{jk} was any coefficient of the inverse and $u_{i\ell}$ any element in the error matrix, it follows that any negative error in the transfer coefficient will impart a non-positive bias to all elements of the inverse, and thus to any output estimate based on the inverse. It can also be shown that positive errors in the transfer coefficients will induce positive biases in the inverse matrix, although there exists the possibility of explosive behavior and thus singularity of the C^* matrix, if the errors

are sufficiently large. To sum up, an important property of input-output matrices is the following: an error in the structural matrix will leave every element unchanged or introduce an error of the same sign in every element of the corresponding inverse (provided there is non-explosive behavior).

A further property of errors in a Leontief system is that they are additive: the total effect of all the errors in the transfer coefficients is equal to the sum of the effects of the errors considered separately. This property, together with the one described above, means that positive and negative errors in different elements of the structural matrix will have compensating effects on each other in the inverse. It also implies that the error in the estimate of the output vector is bounded above and below by the sum of the individual positive and negative errors from each element of the structural matrix.

Thus errors in input-output analysis have both some welcome and some undesirable features. On the one hand, the cumulative effect of the research process implies a cumulative error effect on any estimate, but there is also the tendency for positive and negative errors in different elements of the structural matrix to cancel each other, because they affect the final estimate in opposite directions. Furthermore, upper and lower bounds on

the size of the error in the vector of research outputs are easily computed for different assumptions on the magnitude of errors in the structural coefficients and various input vectors.

* * * * * *

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IV. CONCLUSIONS AND SUGGESTIONS FOR FUTURE RESEARCH

The problems encountered by support agencies such as ONR in managing large diversified research programs are unquestionably very complex and difficult. Decisions must often be made on what fields of research should be supported and on the level of such support, without any apparent method of assigning comparative value to the research supported. Over the years ONR and other government agencies have demonstrated a great deal of practical experience and intuition in making such decisions. These informal procedures and methods are certainly invaluable to successful research management. However, the tremendous growth in basic knowledge and the increased utilization of this knowledge in the form of new technologies has increased demands for the development of methods and techniques should provide a framework capable of expressing the complex relationships of the research activities to each other and to goals and objectives of the research program.

Of course, any models or other formal devices which are used for the purpose of research management must necessarily be oversimplified. The models that we offer in this paper are no exception. Yet the models must not be oversimplified in such a manner as to abstract from the essential nature of the

problem studied. In the case of research management, we feel that the logical interdependencies that exist between research activities must be a starting point in the construction of any models.

As a first approach, we offer a graphical model which takes these interdependencies into account by means of directed arcs. The nodes in our graphical model each represent an aggregation of individual research efforts into a single research activity by some rule or principle. Ideally, this rule should satisfy properties that guarantee minimum loss of information due to aggregation. The comparison of various alternative rules is a subject for future research.

One can use a graph of the above type along with various of its subgraphs to trace out the direct and indirect impacts of various ONR sponsored research programs, at least in a qualitative fashion. In our future research, we hope actually to construct some of these research "nets" or "trees", to study the particular graphical structure which characterizes current research efforts. We have some preconceived notions about the structure which we are eager to test empirically. Once we have empirically established some of its special characteristics, we can begin to use the mathematics of graph theory to investigate operational rules and general guidelines for research management.

A graphical research tree or net is only a first approach in our attempts to characterize the complex interdependencies between research activities. Such a formulation has some obvious advantages over methods which attempt separate comparisons of each research activity with specific government objectives. If we are to deal adequately with these interactions between activities, however, we need a method which is capable of quantitative as well as qualitative analysis. This kind of formulation is given by our matrix models presented in Section III. In order actually to use a model involving quantitative estimates, one must have a method of measuring the outputs of each activity as well as its effects on other activities. Obtaining a good index of research output has been, of course, a central problem in research management. Various attempts to construct such an index have been suggested in the literature, such as counting the amount of papers issuing forth from a research project, weighing experts' opinions, etc., but these have usually been less than satisfactory.

In our paper, we make the simplifying assumptions that research output is proportional to research input measured in manhours, and that there exists a matrix of numbers between zero and one which expresses the fraction of research spent on any given project applicable to any other project. The latter

assumption is equivalent to assuming that the transfer of research between activities is linear in nature. In some sense, these assumptions allow us temporarily to bypass the problem of measuring the output of research, but the question of how one measures the transfer effects still needs to be answered. Some index will have to be devised which yields an estimate based on the historical evidence of the transfer effects and the current feelings of the people conducting the research. An investigation of some possible alternative indices and the feasibility of using statistical sampling techniques for collecting the necessary data is one of the major areas of future research. Of course, there are also bound to be errors in such estimates, even putting the conceptual difficulties aside, and therefore a knowledge of the consequences of various errors will be necessary. The appendix to Section III discusses this problem in a preliminary manner.

Hopefully, the estimation problems discussed above can ultimately be resolved. For the present, we have traced out some of the implications of our assumptions concerning the structure of research for research management. The total effect of any given initial allocation of research effort may be expressed by a matrix series of initial and derived effects. Various alternative allocations of research effort emphasizing

different mixes of basic and applied research may be investigated and the resulting derived effects of different allocations compared. In addition, the matrix series may be used as a basis for constructing models which incorporate the objectives of the research program and any budgeting or manpower constraints that exist. Examples of some possible models of this kind are given in Section II. A central problem in the construction of such a model is expressing goals and objectives, which are usually couched in the form of broad general statements, in a form applicable to formal normative models. Research on this question is certainly one of the most important areas of further work.

After the various problems associated with our basic model have been resolved, our next task would be to relax our assumptions on the linearity of the interactions between research activities and introduce more complex ones involving various non-linearities. This type of procedure inevitably introduces difficult problems but, we hope, still of mathematical tractability. At any rate, one would hope to obtain from this type of analysis some idea of the biases which result from using the more operational assumption of linearity.

Finally, one must not forget, in the midst of all this

formal analysis and model building, that research is fundamentally people with ideas, and that a good deal of the success of the supporting agency will depend on their relations and ability to communicate with the scientific community. As we have stated earlier, we believe that ONR's behavior in this regard has been exemplary. We feel that this is an important enough area, however, that it is desirable to express our opinions and experiences on these matters, freely and openly. We feel that others on both sides of the research process should also be encouraged to do so,in order to maintain the best possible working relationship between the supporters of research and the scientific community.

Research management poses many interesting and difficult unsolved problems. We believe that some significant results have been obtained during our initial project and that further work will yield fruitful returns. We are confident that our approach will eventually prove to be of practical usefulness to the Office of Naval Research.

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