A STUDY OF THE RATIONALE AND TECHNIQUES FOR LONG-RANGE TECHNOLOGICAL FORECASTING IN THE BIOLOGICAL AND MEDICAL SCIENCES

Life Sciences Research Office
Federation of American Societies for Experimental Biology, Washington, D. C.

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I. INTRODUCTION - THE PROBLEM OF THE PREDETERMINATION OF THE TECHNOLOGICAL ENVIRONMENT

Forecasts are potential aids in the planning of future technological environments. Their usefulness depends on their validity. Accurate deductions are essential regarding anticipated responsiveness to existing and expected opportunities for achievements in selected areas of science and technology. This is a review of the multiple forces that determine the reliability of such deductions or predictions.

The Army Technological Forecast is an attempt to describe the probable technological environment of the future. It is based on the answers to three questions: (1) What is the technological environment today? (2) How does today's environment compare with that of yesterday? (3) On the basis of past and present changes, what are the probabilities for tomorrow?

The first question is concerned with current research and development as estimated from the observation of present practices and from the evaluation of scientific reports and of ongoing research projects. The second question provides a measure of the rate of progress in research and development as this is determined from comprehensive surveys of the technical literature of the past and of the present. The third question requires a conclusion regarding the opportunities for additional accomplishments in research and development and a judgment concerning the likelihood that advantage will be taken of these opportunities for future achievements in specified areas. Answers to the first two questions depend on facts. The answer to the third depends on deductions from the facts and, accordingly, is only as valid as the deductions are reliable.

The technological environment of today and what it will be tomorrow are the result of numerous factors that have operated in the past and, insofar as can be determined, will continue to do so in the future. Conclusions based on an evaluation of these factors today can modify tomorrow's objectives and, if correct, can hasten their realization. For this reason, it is to be expected that deductions, projections, forecasts, judgments, whatever they may be called, that are formulated from such conclusions may influence the planning of new research and development programs and of budgetary support for the programs. Judgments of this type are, in fact, consciously or unconsciously a part of the planning of all except "hit or miss" programs of research and development. The important question is, "To what extent and under which conditions are these judgments valid and useful?"

It is conceivable that precise, accurate conclusions and deductions arising from answers to the three questions could yield as an end result a predetermined future environment with maximally favorable biomedical characteristics. The hazard of inaccurate judgments or deductions about the future is involved, especially for time periods (one to 20 years) significantly beyond the immediate future (under one year). The value of planning that is based on <u>facts</u> is indisputable. But, it is important to ask, "To what extent is there value in planning that is based on judgments or deductions about the probable accomplishments of the future?"

This report deals with the forces that influence progress in science and technology and with their relation to forecasting and to the planned control of the biomedical environment of the future.

II. RELATION OF FORECASTING TO RESEARCH AND DEVELOPMENT

A. Historical Aspects of Science and Technology

The history of science and technology does not provide evidence of consistent predictions of future accomplishments or of a beneficial role of forecasting in planning. Whether or not forecasting was attempted has not been determined.

In any attempt to predict the technological environment of the future, it is important to estimate the influence of previous discoveries of research scientists and the role of past achievements of technologists on the present environment insofar as these effects are evident from historical records. It is recognized, however, that the relation of trends and of rates of progress at any given time in history to a subsequent environment is not easily ascribed to the accomplishments of either scientists or technologists because each group makes its own autonomous contribution and each shares, also, in a mutually interacting effect on the entire area of science and technology.

R. P. Multhauf has discussed the historical aspects of these relationships in a paper entitled "The Scientist and the Improver of Technology."(1) In the 17th century scientists were technologically oriented and were concerned primarily with civil and military engineering, with applied mechanics and with optics. Their activities were closely associated with those of instrument makers although the important gains in automatic instrumentation were not to come for another two centuries. However, the thoughts and works of such men as Galileo, Torricelli, Descartes, Huygens, and Newton brought about marked improvements in the technology of the experimental science of the period. Indeed, the technology of the 17th century was so advanced that the contributions of sciencc *per se* were surprisingly minor. Scientists, for example, had little to do with the Industrial Revolution of the 18th century which was hastened by the technical advances in power and textile machinery and in iron metallurgy. Although many were physicians or clergymen with independent means, scientists generally were dependent on the uncertain support of governments or of wealthy patrons. It was not until the 19th century that the position of the scientist in society was strengthened by the prestige and the resources associated with university appointments. Contributions to scientific knowledge increased at this time. Simultaneously, the position of the technologist became more stable as a result of protective patent laws. In general in the 19th century, the activities of scientists, inventors, and engineers were so similar that it was difficult to distinguish between science and technology. Early American journals of science laid great stress on applications. Silliman's "American Journal of Science" (1819) was intended "to enhance the circle of the physical sciences with their application to the arts and to every useful purpose." The introduction of the first volume of "Science" in 1883 stated that "the leading feature of American science is its utilitarianism," with the addition, "nor is this to be at all regretted."

The success of the research scientist in increasing knowledge of synthetic reactions in organic chemistry during the latter half of the 19th century was responsible for the phenomenal growth of the German chemical industry. This was followed in the 20th century by the rise of science-dominated electronics in the electrical industry and the subsequent extension of science into metallurgy. At this time the improver of technology was becoming increasingly dependent on the scientist and for the first time science and technology showed indications of developing on a broad front consisting of the pure scientist, the applied scientist, the engineering physicist, and the pure improver of technology. Despite the role of research scientists in these developments, the technological achievements resulting from scientific advances in this century have been of such magnitude that science in the minds of many is regarded as an appendage of technology, even though their interdependence is conceded by all. As an unhappy result, scientific research is forced to engage in a continuous program of justification in order to obtain funds for its support.

The slowness with which progress was made through joint contributions of science and technology in the 17th and 18th centuries is illustrated by the history of an automatically recording meteorological instrument. This device was first made by Sir Christopher Wren and Robert Hook in the latter part of the 19th century. It recorded 12 different measurements but there is no evidence of its general use. Similar devices continued to be produced for another two centuries without any single one being accepted in either research or industry. It was not until 1870 that a self-recording instrument was made which immediately became widely used. The reason for the success of this particular recorder is not clear. Some say that such an instrument had to await the invention of the telegraph and it was the principle of the telegraph that made possible efficient automatic recording. Others, however, suggest that it was not until this time that scientists actually demanded such an instrument. Significantly, this demand was related in time to the endowment of a number of astronomical observatories in which research and scientists were supported by private benefactions.

Regardless of the explanation, there is little in the course of events in science and technology in the 17th and 18th centuries which appears useful in the development of a mechanism of successful prediction of specific attainments in science and technology. A similar situation is found in the beginning of the present century. This is not to say that the delays in the application of knowledge in the past are irrefutable proof of the unpredictability of future achievements. It may be that serious forecasting was not attempted in earlier periods or, possibly, that previous technological environments were not favorable for the complete execution of research and development plans based on deliberate predictions.

II. RELATION OF FORECASTING TO RESEARCH AND DEVELOPMENT

B. The Nature of Research and Development

Research and Development (R&D) are separate but mutually interacting activities. Research is concerned with knowledge and with the better ordering of knowledge. It is usually manifest in the form of discoveries, although these may also arise by chance. Development is the practical application of discoveries and is aided by invention. Although the difference between pure research and pure development is sharp, the two merge in a broad spectrum of activities variously called exploratory research or exploratory development. A clear distinction between research and development in this intermediate zone is difficult. Failure to recognize the differences confuses the issue of forecasting progress in the respective areas.

The combined term "research and development" (R&D) now serves as a widely used symbol for the general technological activities associated with scientific research and with the application of the results of research, discovery, and invention. The use of the term is fraught with serious problems, however, because a clear recognition of the separate identities of the two components is lacking in the minds of many. Even for those who recognize a difference between "research" and "development" no general agreement exists regarding either the explicit meanings of the two words or their respective contributions to the technological environment. Some consider that both terms refer to research with "development" representing applied research in contrast to basic research. More commonly, "development" is the label for the utilization of a research finding in some practical endeavor. Certainly, from the military viewpoint, development includes the utilization of knowledge in any aspect of national defense. Needless to say, most of the problems of national defense require scientific research as well as technological development for their solution.

A sharp distinction exists between the activities involved in "research" and in "development" despite the influence that each exerts on the other. These separate but interacting effects on science and technology must be understood and clearly defined before there can be any reasonable consideration of the feasibility of forecasting the future in the two areas. The requirement of a precise terminology is evident from the fact that about 90% of R&D funds and personnel is concerned with "development" rather than with "research." Failure to differentiate between the two processes leads to continuing misconceptions in which the incentives, objectives, successes, and failures of research scientists are confused with those of engineers, and vice versa.

Figure 1, which is modified from that presented by Gibson(2), illustrates the steps that commonly occur in progressing from the actions initiated by the "research" and completed by the "development" of R&D. Some of the influences that serve as driving forces for the actions and some of the results of these actions are also shown. Scientific research is the quest for new knowledge, for a better ordering of knowledge, and for an increased understanding of man and of the universe in which he lives. It results from man's curiosity about that which is unknown. It is experimental research and includes theoretical and exploratory research, two related activities concerned with the new knowledge that is the product of experimentation. As admirably stated by Oppenheimer, the getting of new knowledge is not only ennobling, it is also useful because it affords man larger choices for technical applications(3). Its productivity is measured not only by the knowledge, skill, imagination, incentive, vigor, and dedication of the investigator but also by the input from discoveries resulting from creative insight, intuition(4), chance, and serendipity(5).

Exploratory research is stimulated by the needs of society, including the need of national security, and is aided by the higher level of understanding based on new facts and a better ordering of knowledge. It merges into exploratory development which adds additional new knowledge, new techniques, new substances, and new capabilities or opportunities for freedom of action. Certain of these developments are chosen for public utilization and, after proper test and evaluation, are brought to the stage of final development in the form in which mass production can result. The selection of particular opportunities for new freedom of action and for subsequent "reduction to public practice" is commonly based on a consideration of technological, geopolitical, economic, and social factors in which scientists and engineers may or may not have a voice. Mass production of the product is normally required and this is the field of the engineer. Utilization of the product often requires the support of promotion and of consumer education.

The scheme used in Figure 1 is admittedly arbitrary. The justification for its use is the fact that logical conclusions about the usefulness of forecasts depends on their relation to specific action steps. The influence of "discovery" and "invention," for example, may or may not be limited to the actions indicated in Figure 1. These are powerful factors and are largely unpredictable with respect to the time and place of their occurrence. The need of invention in science and the importance of encouraging and protecting the patent system and technology have recently been emphasized by Sparks, as follows: "To be complete, total science must include invention, the force that develops a mere potential into a valuable and salable asset. Invention transforms knowledge from dusty archives into human service. In my mind, science should be for something, for someone. And unless it fulfills this job the true work of the scientist has not been realized."(6)

Two areas which are frequently controversial in discussions of research and development are those represented in Figure 1 as "exploratory research" and "exploratory development." Where research ends and development begins is difficult to define except in specific instances. This is a judgment that is best arrived at by the examination of the questions asked by the investigator as he undertakes a problem and by the nature of the solution he seeks. It is of considerable importance, however, that the concept of the "research" of R&D should not be limited to the seeking of new knowledge irrespective of its evident application, i.e., new knowledge for its own sake and without

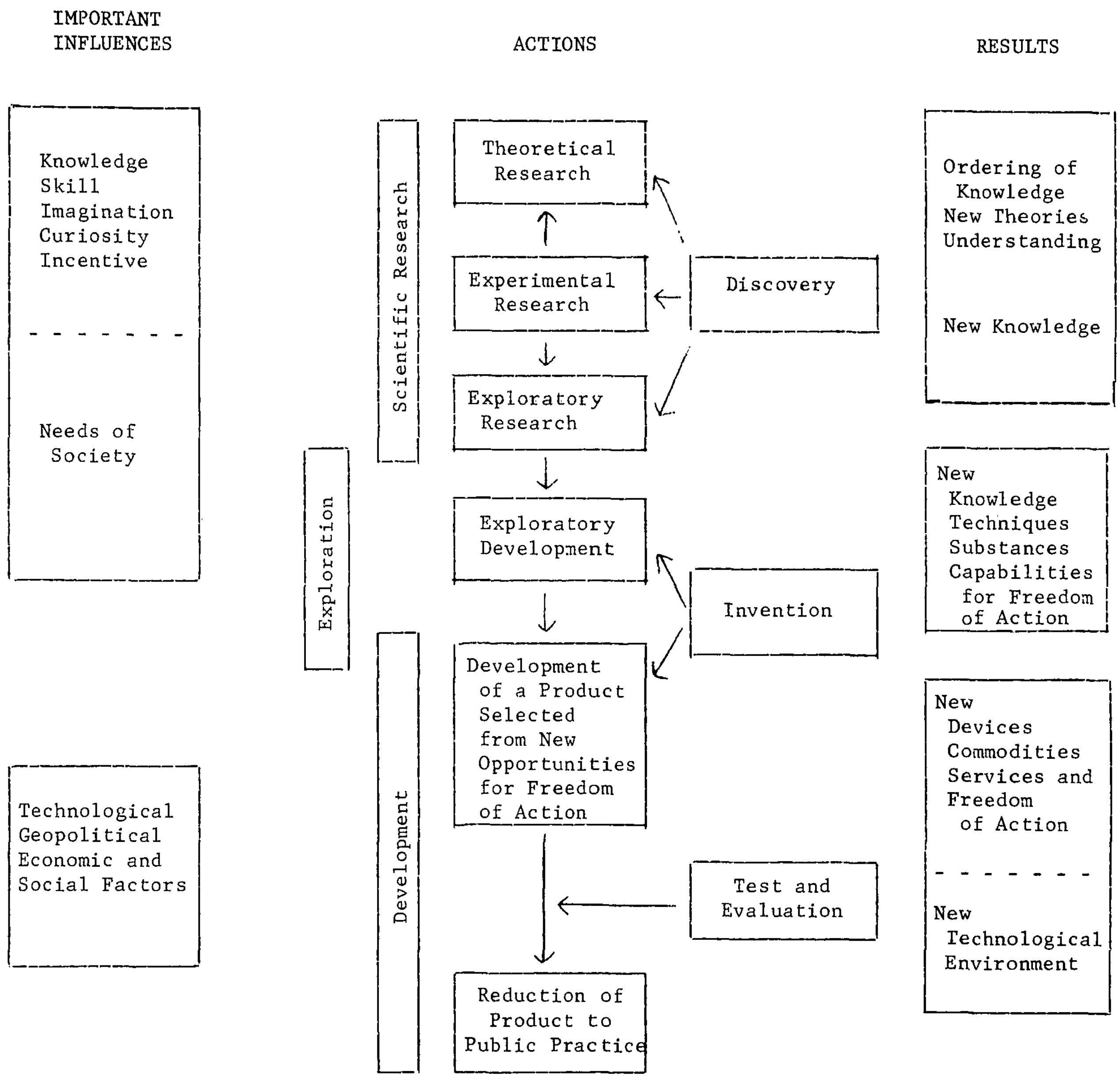

Figure 1. Steps in research and development--important influences and results. (Modified from R. E. Gibson, Research Management 5, 215 (1962); 5, 423 (1962); 6, 15 (1963).

predetermined objectives. It is also the search for new knowledge which is sought deliberately in the hope that a new opportunity for development for public use will result. Included, therefore, is the broad and active field of experimental exploration of possible solutions of specific problems. For example, research may be stimulated by a desire to find an immunological procedure for the prevention of infectious hepatitis. Solution of the problem would require the isolation and identification of the causative agent and the discovery of effective immunological mechanisms and products. The required activity is research rather than development because it adds to knowledge through the use of the incentives and skills of research. Furthermore, it contributes directly to increases in the choices or opportunities afforded for technical application, without necessarily being involved in the process of development for public use.

The term "development" of R&D also represents a series of activities. For example, the Department of Defense, for purposes of financial management and operational controls, recognizes the following R&D categories: research; exploratory development; advanced development; engineering development; operational systems development; and, management and support. Obviously, this is a classification formulated for convenience in funding and, doubtless, has proved its value in the management of research and development in the Armed Forces.

Regardless of the intermediate phases, the fact remains that new research findings are the steppingstones to new developments which, in turn, point the way to the need of additional steppingstones. Each activity supports and is dependent on the other, a relationship that could be shown in Figure 1, if desired, by appropriate "feedback" lines. Throughout this paper, reference will continue to be made to research and development as the primary components of R&D except in the instances in which special emphasis needs to be placed on the intermediate area. Here, the term "exploration" will be used to represent borderline activities which, at times, may be of great importance in forecasting.

II. RELATION OF FORECASTING TO RESEARCH AND DEVELOPMENT

C. Forecasting as a Basis of Planning of Research Programs

> The systematic planning of a research program requires estimates of requirements of personnel and of physical resources. These requirements are usually predictable in the case of plans of short-range duration (one year or less). Their reliability is less in the case of long-range plans. These estimates of requirements serve as forecasts and, in turn, judgments on the availability of personnel and of resources and are aids in the forecasting of the rate of progress in research. Designation of long-range goals in research is important but the time and circumstances of the attainment of the goals cannot be foreseen. Discoveries and the results of specific research projects are not subject to prediction. Research can demonstrate cause-and-effect relationships that may prove useful in the projection of trends. Accordingly, forecasts arising from past and present experiences may have importance but experience cannot foretell the characteristics of the solution of problems.

Success in a research program requires (1) logical planning that is based on familiarity with relevant literature, (2) assembly of sufficient observations and data to permit formulation of specific questions, (3) development of hypotheses that will yield answers to the questions, and (4) experimentation thoughtfully designed to test the validity of the hypotheses. Logical planning normally results in continuing experimentation and in additional programs because the research activity constantly discloses leads and opportunities for gaining more new knowledge.

The effective planning of a research program requires a goal and an evaluation of the potential to reach the goal. Whether the goal is theoretical or practical is immaterial. The goal and the potential must be consistent, otherwise the goal must be modified to be in harmony with the potential in terms of the available research competence and facilities. After the goal or objective has been selected, numerous other decisions enter into the planning. These include (1) the determination of the priority of testing alternative hypotheses; (2) the estimation of requirements of special equipment, of supplies, and of laboratory space; and (3) the estimation of the expenditure of time by the investigator and by each of his assistants. All of this is necessary because the provision of funds is based on these judgments. Some of these judgments represent conclusions from known facts; others are deductions believed justified on the basis of experience. To the extent that probabilities are involved in any of these decisions, projecting or forecasting becomes a part of the planning.

In planning research programs on a short-range basis, six months to a year for example, arrangements usually are revocable and there is sufficient elasticity to allow compensation for errors in the plans or for modifications desirable because of new leads in the research. For this reason, the fact that the assessment of probabilities, i.e., forecasting, has entered into the planning is of little consequence and may not even be recognized as anything more than making proper use of one's judgment or common sense. On the other hand, most scientists place a limit, usually a time limit, on the planning of future research. It is reasonable to conclude that such a limitation results from the realization of the magnitude of uncertainties about research which is to be performed too far in the future. "When is too far?" clearly becomes a crucial question in the forecasting of the progress of research.

If it is reasonable, even essential, to include a forecast in the planning of experiments continuing through a period of one week, what about one month, one year, or one or more decades? Data are not available for the establishment of the rate of loss of accuracy in such predictions. One must turn, therefore, to other factors and, among these, the almost universal custom of operations and budgetary reviews on an annual basis provides a convenient period. Forecasts of research requirements for periods up to one year have no certainty but do have a useful degree of probability, as indicated by the fact that this is the common practice in planning.

The fact that the requirements for the performance of specified experiments can be predicted with reasonable reliability does not imply that the results of the research can also be prejudged. However, research may demonstrate causal relationships between events or factors which then may serve as a basis of estimations or predictions of future situations. Accordingly, forecasts arising from past and present experience may well have importance, but experience cannot foretell the characteristics of the circumstances of the solution.

The statement that the results of individual scientific research projects are unpredictable rests on the belief that the secrets of the unknown can be disclosed by appropriate analytical and synthetic studies but not by clairvoyance. The optimistic opinion that research will solve the problems of overpopulation, of mental retardation,of cancer, and of injury due to ionizing radiations are forecasts with considerable probability but are not aids in the planning of a future technological environment unless the time and the circumstances of the solutions are included in the forecasts. The approximate time that a prediction will come to pass is, of course, indispensable for effective planning.

Long-range research planning is assumed to refer to investigations projected over a period longer than one year. These projects would normally consist of broad topics with many ramifications or of narrow subjects in which new methodology is a prerequisite for the principal problem to be studied. Results of particular aspects of such research are unpredictable but, as in the case of short-range research planning, forecasts of requirements can be estimated usefully. These will lose accuracy as the time period lengthens because of the greater opportunity for interference by the many variables that influence the reliability of the predictions. It would appear to be a safe assumption that the doubling or tripling of the research personnel and of the resources would increase the probability of useful findings but this prediction could not include the time factor required which is essential for the formulation of plans.

D. Forecasting as a Basis of Planning of Development Programs

Technological developments are the natural end-products of discoveries and of scientific breakthroughs. Forecasting of developments is limited by the unpredictability of research results. The requirements of personnel and of physical resources for programs of development that are based on established research results can be estimated and the resulting forecasts have a reasonable probability of accuracy. As in the case of research, estimates of the availability of human and physical resources that will be allocated to programs of development serve as aids to forecasts of the rate of anticipated progress in the utilization of new knowledge. Forecasts of technical developments have potential usefulness if based on discoveries, on inventions, on demonstrated causal relationships, on realistic trends, and on solid experience. The time factor is a critical factor. For this reason, a 20-year forecast can be hazardous unless used with caution and with full appreciation of its weaknesses.

Progress in science and technology depends on successful developmental programs. Insofar as technological developments are the natural end products of discoveries it must be conceded at the outset that the accurate prediction of the technological environment of the future is limited by the unpredictability of the results of research and by the unpredictability of accidental discoveries.

Developmental planning resulting from previous discoveries is relatively straightforward once the choice between alternative possibilities has been made. The opportunities for useful developments are not always evident and trial and error may be involved in the choice of applications for public use. The urgency or priority of need determines the size and phasing of the development budgets. It is tempting to believe, furthermore, that under present circumstances in the United States, the much larger budgets for "development" than for "research" virtually guarantee the application of potentially useful research results. The validity of such a conclusion is uncertain because it is difficult to be sure that current knowledge is, in fact, being exhaustively surveyed for possibilities of important practical applications.

All in all, forecasts of technological developments based on established results of research or on discoveries should prove advantageous in planning the technological environment of the immediate future. For example, once the principle of the controlled release of nuclear energy was discovered, the application of this research finding in the construction of an atomic bomb or of a submarine driven by atomic power was largely a function of men, time, and physical resources. On the other hand, forecasts of technological developments which are based on predictions regarding the outcome of research have no validity and are in error as often as they are accurate. Nevertheless, expenditure of resources on the basis of such forecasts may be decided upon whenever urgent situations demand a shotgun type of approach to the problem of changing the technological environment. Furthermore, it is human nature to rationalize occasional improbable predictions into "informed guesses" or "hunches," especially if the rationalization includes the judgment that the risk is worth the cost. It is important to recognize that these are gamblers' "long shots." The fact that sooner or later one of these yields good results, by chance, is not evidence that a system of prediction exists which remains to be identified and used.

Developmental planning may also be based on forecasts derived from records of previous events believed on the basis of experience to have a cause-and-effect relationship with the technological objective of the planning. This is the well-known prediction or projection from "trends" and such forecasts may have an acceptable degree of probability(7). One of the most ancient examples of this type of prediction was the forecasting of droughts and floods in the lower Nile Valley by the rulers of Egypt. Their success was ascribed to divination but was actually based on the interpretations of the readings of depth gauges in the upper Nile in the light of records obtained over several centuries. Experience showed that prediction was a highly useful guide in this instance. Similarly, a reasonable prediction can be made now of the number of students who will enter college in 1982 based on the present birth rate in the United States and on the present proportion of 18-year-olds entering college. It might even be possible to use present trends to predict the approximate number of these students who will become research scientists. However, less confidence could be placed in predictions concerning the fields of specialization, the incentives for research, or the productivity in research of these future scientists. The prediction of weather has proved sufficiently accurate to make its forecasting an extremely important factor in the planning of weather-sensitive operations. These predictions are in error at times because all of the variables that control the atmospheric environment and their interrelationships are not known. In these instances, experience is obviously inadequate. The element of time is critically important, forecasts of weather becoming more uncertain as the projected time interval in the future lengthens.

The predictions in each of the illustrations are based on previous observations and the accuracy of the forecasts depends on the reliability and completeness of the observations. Forecasts of this type are by no means certain but are advantageous for planning purposes if used with discrimination and caution. It is recognized that the trend relationships which are commonly used represent experiences or happenings rather than findings of scientific research. However, research may be involved in the establishment of the causal relationships. Furthermore, there is no reason why the results of systematic research in some areas should not be convertible into trend curves with the possibility of limited extrapolation(8).

In summary, forecasts of research are feasible if limited to judgments regarding resources in men, money, and facilities. Forecasts of technical developments may prove useful if based on discoveries in research, on inventions, on demonstrated causal relationships, on realistic trends, and on solid experience. As in the case of the role of forecasting in the planning of research programs, the time factor is a prerequisite for the effective planning of developmental programs. This is not to infer that it is valueless to formulate long-range goals in research and development that are without reference to time. Quite the contrary! The long-range goals set the direction and the pattern of short-range objectives which require detailed phasing as a part of useful planning.

The Army Technological Forecast proposes a time interval of 20 years. Commonly, the time interval necessary for the implementation of a discovery in a form in which it contributes to Army capabilities is 4 to 5 years. This may not be true for biomedical developments, such as the development of a vaccine, which do not depend on extensive engineering or instrumentation. With this exception, what the Army will have 5 years from today must be in the process of development now, research having been completed. Hence, in the development area a 5-year forecast represents a fait accompli rather than a prediction. Moreover, unless development engineers know what research programs are planned for the next 10 years, they have little foundation for prediction of new development items 10 to 15 years from the present. It is clear that a 20-year forecast can be hazardous unless it is used with intelligence and with full appreciation of its weaknesses.

III. FORECASTING - GENERAL CONSIDERATIONS

A. The Justification of Forecasting

To the Army planner who is responsible for R&D funds and to the legislator who provides the funds it is unthinkable that either the total amount or the pattern of distribution should be determined in any manner except on the basis of carcfully considered plans. Forecasts based on knowledge and on reasonable estimates of future capabilities are aids in planning. The problem is to take advantage of justifiable predictions. Those who participate in research and development are best qualified to distinguish between valid and invalid forecasts. To refrain from this activity is to turn over the preparation of acceptable and potentially useful forecasting to those who are less qualified to assume the responsibility.

The wise selection of short-range objectives and of long-range goals in research and development is of the utmost importance in military planning, not only to avoid wasteful expenditure of public resources but also to achieve the maximum success in national defense. Proper decisions are essential in the selection of the most favorable future technological environment and of the ultimate research goals which, if attained, will bring the desired environment closer to realization. Such decisions are also necessary regarding immediate objectives in research and development that are established as steps towards the ultimate goals. Admittedly, these may be temporary objectives because they may be changed completely by new and unexpected discoveries. Under these circumstances, the unpredictability of the results of research and the complexity of the forces that control the progress of research and development make the indiscriminate forecasting of probable accomplishments a precarious matter--precarious because the adoption of erroneous forecasts could result in serious losses. By the same token, progress in research and development will be accelerated markedly if legitimate forecasts improve the efficiency of planning.

The temptation to prepare and to give credence to forecasts is understandable. To the Army planner who allocates research and development funds and to the legislator who is asked to provide the funds it is unthinkable that either the total amount or the pattern of distribution should be determined in any manner except on the basis of carefully considered plans. Furthermore, the longer the plans extend into the future the greater their acceptability. There is nothing unreasonable about predicting the state-of-knowledge of tomorrow on the basis of what we know today and of what we have in resources with which to work.

Unreasonableness enters the picture only if plans utilize predictions that are formulated without adequate information or without sound judgment. The problems are to distinguish between justifiable and unjustifiable predictions and to utilize the justifiable predictions most advantageously in planning--planning that takes into consideration the degrees of probability inherent in the predictions.

All who participate in research and development have the obligation to assist in the preparation of forecasts in their respective fields. If they do not do so others, less qualified, will make the predictions on which plans are based and the results could be disastrous. Scientists cannot avoid this responsibility but they must recognize that in forecasting they are not acting as scientists in a scientific endeavor but are serving an urgent public purpose.

III. FORECASTING - GENERAL CONSIDERATIONS

B. The Historical Aspects of Forecasting

The historical importance of forecasting has not been demonstrated. If forecasting has actual value in the planning of the future technological environment, this fact remains to be demonstrated.

The earlier discussion of the historical inter-relationships of science and technology suggested that there is little in the records of the past to instill optimism concerning the role of prediction as a useful aid in planning. This situation raises a number of questions relevant to the role of forecasting in planning. If even a limited type of prediction is advantageous, why has this not been appreciated previously? Or have predictions been made and their effects not recognized either because they were erroneous or inconsequential? Unfortunately, it is virtually impossible to ascertain now the number of predictions made in the past and the outcome of the predictions. Scientists, particularly, are so busy making discoveries that they are prone to disregard the past, even to the extent of "rediscovering" that which was established by their predecessors and overlooked. Perhaps it is also true that there is a failure among scientists and technologists to examine sufficiently the possible applications of new knowledge. The 2-century delay in the development of an acceptable automatic meteorological instrument has been cited. Equally difficult to explain is the 30-year lag in the utilization of chromatography as an almost indispensable laboratory procedure. Discovered by a Polish botanist, Tswett, in 1906 and used over the years with great success in a few isolated technical procedures, its extraordinary value in a wide variety of chemical separations and isolations remained unappreciated over nearly a generation of investigators. Was it realized 40 years ago that the life-saving action of the newly discovered insulin might result in a population in which the incidence of diabetes would be markedly increased? How did it happen that none foresaw that the Land Grant College Act of 1862 would be so successful in agricultural production that the need of the Act is disappearing and Schools of Agriculture are seeking new roles for the future? Is it possible that a more determined effort in 1862 to predict the technological future would have forecast the change in the natural environment resulting from the extended use of chemical fertilizers, pesticides, and detergents? Did anyone expect that the savings in manpower and in resources that resulted from the introduction of hybrid corn would equal in value the cost of the Manhattan Project? Why is it that it is still a lucrative business to examine periodically those patents that have expired without previous development?

These instances of an apparent failure in the past to foresee the effects of changes in science and technology cannot be used as evidence of a lack of value of long-range forecasting. It is clear, however, that if such a value exists this value remains to be demonstrated. It is particularly important to determine the nature of the predictions which may have potential importance. Whether or not further study of the historical aspects of the relation of predictions to progress in science and technology will be informative is not clear.

III. FORECASTING - GENERAL CONSIDERATIONS

C. The Dynamic Nature of Forecasting

A forecast is not a single action. It is dynamic and is a commitment to a multiplicity of direct and indirect actions. The validity of the forecast and the usefulness of the actions must be challenged at frequent intervals.

One of the most important aspects of forecasting is its dynamic nature. This is not true, obviously, if a forecast is prepared and filed without further study. If, however, it is used in planning it cannot be considered a simple isolated action. A chain of events is initiated which must be evaluated very carefully. Such a forecast is a commitment to a multiplicity of actions, some directly concerned with making the prediction come true and some designed to counteract divergent forces that might circumvent the prediction. Each of the side consequences of the prediction must be foreseen and corresponding actions taken. Plans based on the forecast are responsible for initiation of research and development in areas which may be extended indefinitely. Resources of men, money, and material may be committed to such an extent that there are serious drains on the capabilities of the country. The results may be advantageous if the forecasting is accurate and the plans correspondingly sound. If this is not the case the results may be chaotic. A forecast and its consequences must be considered as a totality of actions which should never be allowed to go unchallenged. It must be re-examined and revised continuously in the light of new discoveries and changed objectives.

III. FORECASTING - GENERAL CONSIDERATIONS

D. Interdisciplinary Communication as an Aid in Forecasting

The enormous array of facts and of coordinated relationship comprising today's knowledge makes it impossible for individuals to possess the comprehension and imaginative understanding that are necessary for the visualization of all of the possibilities for progress in science and technology. The better integration of these facts, which can be the basis of effective predictions, can only be achieved by interdisciplinary communication among specialists in the relevant fields. Better communication is needed also between scientists in military, university, and industrial laboratories and between scientists of different countries. Information filing and retrieval systems will be of assistance in the future.

The successful forecasting of the future technological environment requires a thorough comprehension of existing knowledge and the imaginative, yet realistic, extension of present capabilities into the future. Such an understanding on the part of individuals is becoming almost impossible because of the amazing increase in knowledge in science and technology. A complication is the fact that areas of ignorance are still much larger than areas of knowledge. As a result, knowledge is often inadequate, incomplete, and subject to change as the areas of ignorance are further invaded. Advances in specialized fields occur so rapidly that the half-life of "facts" is becoming shorter and shorter. Nevertheless, opportunities for important breakthroughs are greatly increased as the periphery of knowledge expands. The need is urgent for new approaches to learning--new conceptual frames of reference--to facilitate the understanding of today's total knowledge and to bring into view today's total possibilities for accelerated progress. A way out of the present bewildering maze of facts and figures may be possible through the expenditure of much more energy in the integration of the entire body of common knowledge. Such a task cannot be accomplished by specialists working alone but must be a multidisciplinary, or better, an interdisciplinary effort, i.e., a combined endeavor of specialists in all relevant fields. Just as team or group activities have been successful in research and development, especially in the pinpointing of fertile fields to till, so the team approach offers the best chance of success in prediction. The interdisciplinary viewpoint can provide the understanding and the integrated judgments that are essential for the effective projection of today's technological environment into that of tomorrow[9].

A second kind of interdisciplinary communication which would improve the capability for prediction involves the more frequent exchange of personnel between military and non-military laboratories, especially those in universities. University scientists need to be more aware of military scientific problems. Military and civilian scientists in military laboratories can profit from periodic contacts with universities, particularly if the assignments last longer than one year. The learning of new methodology and the opportunity for transfer of information from one field to another have enormous potentials for advances in research and development.

Related to interdisciplinary communication is the need of greater familiarity with scientific and technical achievements on a world-wide basis. It is no asset to be in second place in this regard and first place can only be certain if the technological environment now and in the future is known to be superior to that elsewhere in the world. There is evidence that research in certain fields of science in the United States is trailing that of the U.S.S.R. Reports show that Russian science has made important progress in measurements of the effect of ionizing radiation on the central nervous system. These studies have utilized cybernetics and highly sophisticated techniques in the field of conditioned reflexes. Such an influence of low levels of radiation has now been confirmed in this country but progress in this area is definitely slower than is desirable.

Inherent in a discussion of the important place of interdisciplinary communication in forecasting is the problem of the filing and retrieval of information by computer systems. One may predict with considerable assurance that these systems in time will prove of immeasurable benefit. None is adequate at the present time because insufficient interdisciplinary communication has been applied to the solution of the problems that have arisen.

A common language is needed which can be used by biologists, physicists, and engineers. The preparation of the vocabulary which should be built into the machines may be postponed indefinitely unless it is developed under governmental auspices. The programming of computer systems cannot have maximum importance for the forecasting of research and development unless it is known which questions can be answered. Machine experts have played greater roles in these regards than have scientists up to the present. The hazard is imminent that men will be slaves rather than masters of the machines.

III. FORECASTING - GENERAL CONSIDERATIONS

E. Areas Suitable for Forecasting Progress in Research and Development

Designation of specific areas in biology and medicine that are judged to offer better opportunities for useful forecasting is itself a forecast in which a prediction is made regarding the priority of the military need of a solution of a problem and the probability of finding a solution in the foreseeable future. The clear differentiation between forecasts of progress in research, in exploration, and in development in the selected biomedical areas is also important. Formulation of forecasts of progress in development must take into consideration the forecasts of progress in research and in exploration.

The level of research and development at any given time is a reflection of the existing state of scientific knowledge and of development for public use, i.e., it is a reflection of the technological environment. From the military biomedical viewpoint, for example, the technological environment in the food and nutrition sciences permits the preparation of rations of proved nutritive quality for most but not all operational situations, whereas the technological environment in the field of ionizing radiation has not as yet yielded protective devices or measures of significant operational value. There are similar gaps in knowledge needed for the solution of many additional problems such as the need of greater scope and effectiveness of immunization procedures against bacterial and viral diseases, the postponement of severe physical and mental fatigue; the development of resistance to environmental stresses like heat, cold, and hypoxia; and the preservation of morale and of psychological stability in the face of excitement, discouragement, and danger; the prevention of shock; and the improvement of biomedical equipment by automation and miniaturization. Much more research and development are needed in all aspects of therapeutic, preventive, and environmental medicine before troops can receive maximum protection against the hazards of illness, injury, and impaired physical and mental performance.

Without question, it would be of great advantage to be able to plan effectively a series of future technological environments, each with increasingly favorable characteristics in every one of these areas of research and development. The project areas in which research and development are most active offer the greatest probability of progress. This is an explosive period in biomedical research with a significant increase in the frequency of discoveries, a fact that enlarges the opportunities for predictions but does not increase the accuracy of the forecasts. Insofar as the selection of specific biomedical problems is concerned, the judgment of prediction must be made on the basis of the military need of a solution and the evaluation of competing alternative possibilities.

It is of considerable importance to distinguish between forecasts of progress in research, in exploration, and in development. Preferably these should be presented in separate documents. It goes without saying that forecasts portraying the possibilities in exploration and in development cannot possibly be complete unless they are formulated in the light of the forecast of progress in research in the respective biomedical areas. Similarly, forecasts of developments must include knowledge of predicted progress in exploration.

III. FORECASTING - GENERAL CONSIDERATIONS

F. The Hazards of Forecasting

> Acceptance of an inaccurate forecast as a trustworthy description of a future technological environment can be disastrous because of waste of human and physical resources and because of postponement of essential military preparations.

From one point of view forecasting represents a yielding to the modern impulse to adapt every aspect of human behaviour to scientific methodology. Whether this desire to discover a scientific basis for predictions turns out to be an unhappy response to a siren's call or a commendable effort to move more rapidly to selected objectives depends on how intelligently the forecast is used. A forecast can be the product of one hired for the purpose or of one ordered to do the job. Any imaginative person can make predictions without end on the basis of guessing, wishful thinking, or pure fantasy. Intuition, with or without bias, may play a role. Some of these forecasts might well prove to be accurate predictions of things to come. This has no significance, in itself, because predictions about future happenings in research and development are futile unless they are made according to a process of evaluation that is either accurate in the majority of instances or is accurate in the estimation of its degree of accuracy. If wrongly predicted results are used in planning, the erroneous forecasts can be more than wasteful and futile--they can be hazardous. Acceptance of an inaccurate forecast as a trustworthy description of a future technological environment could influence adversely the provision of funds for research and development by legislative bodies and could lead to poor choices of objectives by investigators and engineers. Even acceptable forecasts, if too limited in scope, could create biases in those who perform and in those who support the performance, could stimulate performance in fallow areas, and could result in overemphasis on new knowledge with neglect of the old. An outstanding example of the result of an inaccurate prediction occurred in this century when a national periodical, "The Literary Digest," forecast an important election from a poll of public opinion obtained by telephone. It turned out that owners of telephones at that time did not represent a true cross section of the voters. The poll was greatly in error and the loss of prestige was so great that the periodical was forced to discontinue publication. An even more pertinent illustration is that afforded by the Ford Motor Company and the Edsel. The construction of this model and its sales promotion were planned on the basis of elaborate forecasts of its superior technical performance and public appeal. Nothing more needs to be said about the inaccuracy of the forecasts than to note the rarity of the appearance of an Edsel on the highway today. The fact that forecasts may go awry is evident and enthusiasm for their use must be tempered with realistic skepticism.

III. FORECASTING - GENERAL CONSIDERATIONS

G. The Problem of Probability in Forecasting

The forecasting of progress in research and development is concerned with achievements that are considered possible of accomplishment, not with those that are certain. Forecasts cannot be dissociated from the question of probability. An expression of the degree of probability of a forecast should be an essential component of the forecast because it permits considered judgments regarding the desirability and the character of support of alternative proposals having different probabilities.

The forecasting of progress in research and development is concerned with achievements that are believed possible of accomplishment within a stated interval of time. Forecasts, by definition, are not concerned with events that are certain to happen, as in the case of the "certainty" that the sun will rise on a following morning. Forecasts, as considered in this discussion, cannot be dissociated from the concept of probability regardless of whether the concern is with research findings or other discoveries, with the development of a new device or procedure, or with the continuance of a cause-and-effect relationship. The degree of probability should be stated in each forecast, if this is at all possible.

An example of the expression of degrees of probability in a forecast is given in the following hypothetical example.

Objective: An inexpensive chemical insect repellent with these properties: effective in man after oral administration; effective against all biting or stinging insects; physiologically safe; sufficiently high renal threshhold so that it will remain in the bloodstream and in the cutaneous tissues for 30 days, at least; resistant to metabolic conversion to either ineffective or toxic products.

Predictions (hypothetical):

1. The agent will be discovered and will be ready for use as follows:

No. of Years	R&D Funds Per Year (Millions)		Personnel	Probability of Success %
	R	D		
2	0.5	None	5	2
2	1.0	None	20	6
10	2.0	None	20	20
10	10.0	None	40	40

2. No single agent will be discovered but 2 agents will be proved successful and ready for use in 10 years if one million dollars are allocated yearly to an R&D unit of 20. Probability--60%.

In this illustration no funds are allocated for development (D) because of the improbability of a successful result. Such a decision would require modification as soon as any compounds were prepared which appeared sufficiently promising to justify "test and evaluation" because the determination of safety after prolonged administration is a costly procedure. On the basis of this hypothetical illustration, it would be clearly unwise to discontinue procurement of the usual types of insect repellents on the assumption that they would be replaced in the foreseeable future.

The token numbers representing probability of success in the hypothetical example can rarely, if ever, be predicted in such detail. At least in the unusual situations in which data are adequate for the calculation of one of the standard coefficients of variability, this can be done. The token numbers are highly illustrative and are believed preferable to less definitive terms such as slight, moderate, or high degree of probability. The actual method of expression is less important than the fact that the concept of probability should be included in a forecast.

It is re-emphasized that forecasts have value even if the estimate of probability is less than one to one, or 50%. Decisions must be made regarding the relative priorities of competing alternative projects. Furthermore, the need of a discovery or device may be so urgent that it is necessary to investigate all possibilities including those predicted to have very low probability.

IV. FORECASTING - HUMAN FACTORS RELATED TO PROGRESS IN RESEARCH AND DEVELOPMENT

A. The Influence of the Nature and Attributes of Man

The nature and the attributes of man determine his desire for changes in his technological environment, his acceptance of responsibility for the support of changes, and his willingness as a scientist or engineer to participate in the making of changes. These characteristics are not fixed but are variable. Their effect is profound and their influence cannot be disregarded as a factor in forecasting.

The nature and attributes of man cannot be disregarded in the consideration of the forecasting of progress in research and development. Man determines the technological environment in which he lives. His state and concepts are of critical importance for progress in science and technology. These concepts are not fixed. Even his genetic nature is changing. Consequently, predictions are necessary regarding probable changes in his medical philosophies, his social mores, his emotional or spiritual state, his concept of purpose and of the future, his religion, and of all the other facets of his complex being. His nature and attributes determine his desire for changes in his environment, his acceptance of responsibility for the support of changes, and his willingness to participate as a scientist or engineer in the making of changes.

Ideally, an investigator should conduct his research at an optimal rate when he is working in a favorable political and social atmosphere, is assured reasonable economic benefits, derives pleasure and stimulation from his cultural surroundings, and finds satisfaction in matters of the spirit. Men differ, however, with respect to the conditions under which their most important work is accomplished and, in many instances, a crisis of one kind or another or discontent with personal circumstances serves as an effective driving force. Irrespective of motivation of the latter type or of motivation that is based on scientific curiosity alone or on an urgent compulsion to find a solution for a specific problem, the degree of success of research scientists is in proportion to their interest, curiosity, imagination, creativity, intensity of effort, intellectual prowess, intellectual discipline, manipulative skill, and keenness of observation. Some of these attributes are natural characteristics, others are gained by diligent study and effort under the guidance of understanding teachers. Their total effect constitutes a potent force in research. For this reason, any incompatible environmental situation that diminishes the effectiveness of these attributes will adversely affect the progress of research.

IV. FORECASTING - HUMAN FACTORS RELATED TO PROGRESS IN RESEARCH AND DEVELOPMENT

B. Influence of Choice of Research Objectives on the Progress of Research

The establishment of long-range goals in research and development results in emphasis on projects directed at the attainment of the goals. This poses a problem for those scientists whose natural inclination is to be quite independent in the selection of research topics. Incentives for research may vary from the desire to investigate the unknown to the desire to find useful answers to specific problems, especially those that are related to the public welfare. Motivation is subject to change, as is demonstrated by the increasing willingness of scientists to accept support for research with predetermined objectives. The forecasting of progress in science and technology requires judgments concerning the influence of the forces that determine the selection of research goals.

1. The Problem of Independent Choice

The predetermination of a future technological environment requires strong emphasis on research that is directed at specific objectives. This is, in fact, the present trend in the support of research and a large proportion of grant-in-aid funds for biomedical research is currently authorized for designated goals like the prevention of heart disease or the maintenance of human performance in a closed space vehicle.

On the other hand, academic communities have taken pride, in the past, in fostering research having no other goal than the increased understanding of man and of the world in which he lives. Furthermore, independence in the choice of research problems and objectives has long been looked on as a cherished privilege, particularly among investigators in university laboratories. For these reasons, a difficult problem faces research scientists who must decide between the alternatives of meager support for uncommitted research in their own fields of interest or of abundant support for committed research in another field. The eager and general solicitation of research support from public and private agencies interested in committed research suggests that the second alternative has considerable appeal. Even the choice of a supporting agency by an investigator may be influenced by his opinion of the flexibility in the use of funds that is permitted by the agency in question. Under these circumstances, the tendency to select a research problem in line with available support, with relegation of personal research interests to the background is understandable whether or not it is defensible.

Attitudes are changing, however, and the future of uncommitted research is uncertain. Hopefully, a proper balance will be achieved between committed and uncommitted research. Even though the immediate results of an all-out emphasis on research directed towards stated goals might prove advantageous for a desired technological environment, the gain could be short lived. The problem is pointedly presented in the following questions asked by Warren Weaver:[3]

> "Are not universities so deeply invaded by the demands for solving immediate problems and by the temptation of income for so doing that there are all too few cases of competent scholars pondering about problems simply because it interests them to do so? Is there not a real danger that the scholars in our universities will lose--and indeed have already partly lost--the maneuvering room for their continuing reanalysis of the universe?"

Prestige in research is also lined with another aspect of motivation which may have a profound influence on the direction of research. It is recognized that the definition of research is frequently limited by such adjectives as "pure, basic, theoretical, exploratory, programmed, and applied" on the basis of differences in the incentives and objectives of investigators. Regardless of the moot question of the validity of the labeling of research in this fashion, the progress of research is affected by the practice insofar as scientists are under pressure to select particular objectives in order to qualify for particular labels. Such pressure is the natural result of their own association of greater prestige with one type of research than with another.

It is clear that the bountiful support of research directed at specific objectives has a potent influence on the choice of research problems by investigators and is a factor that enters into a forecast of the progress of research.

2. <u>The Role of Individual Motivation</u>

The motivation of scientists is important because of its effect on the choice of a research problem and on the nature of the questions that are formulated as a basis of the experimental attack on the problem. For a few scientists, an addition to knowledge is a sufficient justification for research and the practical application of the new facts in the form of new techniques or products is ignored. For the majority, an objective is helpful or even necessary. Some investigators prefer to look forward to the application of their research findings in the better ordering of knowledge, with new ideas for research and new theories as end results. Others think in terms of the needs of society and add the possibility of improvements in public health and welfare to the objective of a better ordering of increased knowledge--this without any necessary participation in the actual application of the new knowledge. Still others are motivated by the desire for financial gain or for the power and prestige considered to be associated with certain types of research.

It is clear that the personal judgments of scientists with regard to research problems are highly important in the determination of the technological environment. Research goals are influenced by individual preferences for studies in specialized areas of science. As noted in the preceding section, goals are also affected by the degree to which investigators are susceptible to persuasion to undertake research on problems other than those that represent their initial or primary interests.

Many scientists believe sincerely that their research is of the uncommitted type even though the financial support has been obtained by adjustment of applications for funds to fit the definitive framework of a particular supporting agency. Some recognize the possible difficulty in working independently within a controlled framework but rationalize their acceptance of the situation on the basis of expediency. To others, the fact that their research is of the committed type is immaterial because they are successful in devising questions within the area of the stated objective and in finding answers by investigations which provide all of the desirable rewards associated with uncommitted research. Fortunately, these individual viewpoints disappear in emergencies, as is the case during a time of war. Then, the end result becomes the impetus for the investigation, whether it be an antimalarial of immediate practical value or a new foodstuff of algal origin.

There is no acceptable basis at the present time for the assignment of priorities to research that is influenced by different kinds of motivation. Each kind makes its own contribution to research progress. Whether it would be advantageous or not if one incentive had precedence over other motivations is not known. The evidence is clear, however, that incentives are subject to change. It is also clear that the influence of these factors that control the selection of the fields and goals of research are of profound importance in the prediction of probable accomplishments in the future.

3. The Role of the Needs of the Environment

The status quo in technology is the starting point from which advances are made, advances which in themselves serve to establish new base lines for further progress. Research objectives are always goals beyond the status quo. Long-range goals simultaneously reflect the strength of human desires for both a better life and for mastery of the universe. In a more immediate perspective, research goals are a response to specific needs, as evidenced by awareness of vulnerability to disease, by the disclosure of an impending hostile threat to national safety, or by recognition of an obvious bottleneck in the development of a more satisfactory understanding of scientific principles.

Short-range research objectives are constantly changing as the findings of research modify the technological environment. This is dramatically the case at occasional and, presumably, at unpredictable intervals when there is publication of findings of unusual importance, such as the discovery of the circulation of the blood, of the transmission of disease by microorganisms, of the x-ray, or of antibiotics. As is to be expected, discoveries of this magnitude are followed quickly by new trends in research objectives and in research emphasis.

Man selects both the short- and the long-range research goals on the basis of a multitude of competing drives and interests. To what extent the research needs of the environment influence the choice of problems by investigators is not known. There is reason to believe, however, that in many instances the younger scientists' initial research interests are subject to chance associations and do not represent the thoughtful analysis of possibilities for the improvement of the environment. It is unfortunate, indeed, if the more worthwhile research problems are passed by for no good reason by superior investigators whose abilities permit them to perform in a superior fashion in any of a number of fields of study. In any event, judgments regarding the most persuasive and appealing of the forces that determine research goals are a requirement of any general forecast of the influence of the actual needs of the environment on the objectives and the progress of research.

IV. FORECASTING - HUMAN FACTORS RELATED TO PROGRESS IN RESEARCH AND DEVELOPMENT

C. Influence of Planning and Management on the Progress of Research

Progress towards a predetermined technological environment can be accelerated by the more careful planning and management of programs of research and development. Whether or not the more carefully planned programs provide better opportunities for accurate forecasts needs to be determined.

Logical and effective planning and management in research are indispensable. For the purpose of this discussion, the term "management" refers to the responsibility of an individual investigator to carry out his own experiments in a precise fashion, even if they are only exploratory in nature, and to the responsibility of a group leader to insure the reproducibility of the work carried on by assistants. Its use is limited to the sensible restrictions on the dissipation of time, energy, and material resources that most investigators believe are proper, whether or not the actions equal the intentions in this regard.

Not all agree that these planning and management requirements are employed universally in research today, whether the problem is the investigation of a relatively simple point in biochemistry or research on the maintenance of a man in space. If a significant proportion of research funds support investigators who carry on laboratory studies without the benefit of rigorous planning and management, including their own management of themselves, it would be of interest to learn why this is the case. Are planning and management accepted and employed, but inadequate, or are they looked on as unjustifiable restrictions on research and as an infringement of the privileges of research scientists? In either case, it is essential to ascertain the extent to which careful research planning and management can or should be established in research programs under present and expected technological environments. Obviously, any intrusion of administrative effort on the time of a productive investigator represents a loss in research productivity and this must be balanced against the gain resulting from his contribution to research planning and supervision.

Charles A. Thomas has expressed himself, as follows, on the subject of management of research: "If we can identify through scholarly studies the factors involved in this delicate art (research management), we will have unleashed on the world a more effective and productive force, one which is now stumbling in a plethora of projects, is sinking in a sea of money, and is being built on a quicksand of changing objectives."[(10)] Possibly, there is need of research on research management, as Thomas suggests. In any event, it is highly desirable to determine whether or not research productivity will change in the coming years as a result of improved research planning and management. The feasibility of improvements in research planning and the benefits that might result from such improvements are difficult but pertinent questions affecting the predictability of probable accomplishments.

IV. FORECASTING - HUMAN FACTORS RELATED TO PROGRESS IN RESEARCH AND DEVELOPMENT

D. Influence of Availability of Qualified Scientists on the Progress of Research

The availability of qualified scientists is a factor in research progress and, therefore, is one of the forces that influences the predictability of probable accomplishments. More than numbers alone is involved. Investigators must possess the education and training that ensure competent research performance, and, in addition, the facility for adjustment to the new situations that have arisen because of new research goals and findings and because of tremendous increases in the number of investigators and in the support of research. The new environment in which research now flourishes often requires a greater degree of participation in group research and more extensive planning and organization of research programs than was previously the case. Concomitantly, there are new problems of collaboration and competition between investigators and different attitudes of investigators regarding their responsibilities for the administration of research. These aspects of qualification for scientific research are closely related to the very important forces that determine the nature of research goals.

1. Education and Training

A shortage of graduates in the scientific disciplines who are qualified to conduct research has been ascribed to the failure of the educational process to develop an interest in science in a sufficient number of students and to the failure to prepare enough interested students for careers in research. This situation has resulted in re-examination of secondary education, in revision of preparatory curricula, and in efforts to improve the preparation of teachers. Funds for the support of research in universities have so increased the emphasis on research that research productivity has become a primary requirement for appointment and promotion in these teaching institutions. Specialized societies have undertaken their own programs designed to attract students to their fields. What will be the outcome of these endeavors on the academic idealism, technical skill, scholarly attainment, and scientific proficiency of the future graduates of schools of advanced training? Involved are questions regarding the most effective methods of increasing interest in learning and the capability of learning in younger students; of developing an appreciation of science and of knowledge, a respect for accuracy, and a disciplined mind in older students; and of promoting broad intellectual curiosity in specialty-trained advanced students.

The latter consideration is important because imaginative originality in research may be more common and fruitful in those who have the benefit of knowledge in varied fields and of correspondingly diverse interests and curiosities.

Clearly, the fact that the needs of the technological environment have not been satisfied by educational processes up to the present is an example of the dependence of progress in research on environmental situations. This problem was not anticipated, whether or not it was predictable, as is demonstrated by the fact that the support of research on a large scale preceded the support of the training of research workers.

Competition between agencies that support research, whether it is research on prevention of a disease or on the maintenance of a man in space will continue to be a prominent factor determining the rate of progress towards specific research objectives as long as the supply of investigators does not satisfy the total demand. The consideration of predictability of probable accomplishments, therefore, must include the recognition of the effect of this shortage and, in addition, it must include an evaluation of the prospects of a solution of the problem of recruitment by present and proposed programs designed to increase the number and proficiency of investigators.

2. Group Research

One of the results of the increased availability of research support in recent years has been the expansion of the research of individuals to group research directed by these individuals. Included in the groups may be one or more postdoctoral fellows, junior fellows, graduate students, technicians, and other laboratory helpers. Training and research have been accelerated by this type of group activity which is quite different from "crash" programs designed to provide answers to urgent questions in the shortest possible time. Related to the problem of group research is the tendency of young and less experienced scientists to move directly from their own doctoral research to the administration of research that is performed by laboratory assistants, thereby losing the opportunity to capitalize on chance observations and occurrences which, in many instances, lead to new and important results.

A study of the anticipated long-range effects of group research is needed in order to be aware of its impact on future research productivity. Such a study should include a determination of the direct benefits of the group programs on technological capability and productivity; an estimation of the adequacy of the planning and management of the research; an evaluation of the role of the director with respect to his scientific leadership, energy, and enthusiasm; and an assessment of the role of this type of research on the development of independence and originality in investigators.

3. Competition Among Investigators

Competition among investigators is a normal and proper driving force for the increased output of more significant research results. In most universities, for example, the junior staff competes for the fewer senior positions and this situation is probably the same in most organizations.

Competition will affect the future quality of scientific investigation favorably if the availability of funds for support of research increases the capabilities of junior scientists vying for senior posts. Its effect may be adverse if individuals are selected for senior posts because they are more successful in the accumulation of funds for research, even if they are less capable in research. There is a possibility, also, that competition would lose some of its potential advantage if senior posts with their increased responsibilities should lose their attractiveness to junior scientists. This might occur if younger men became satisfied with affiliated departmental positions arising from easily obtained research funds and failed to strive for regular appointments.

Competition among investigators, more generally, may be based on the necessity of an improved economic position or it may result from a normal desire for recognition and prestige. Some individuals may be content with the personal satisfaction growing out of finding answers to scientific questions; others may require rewards of a more material nature. In any event, the competitive spirit among investigators will influence progress in research and, for this reason, the factors that influence competition are pertinent to the consideration of the predictability of probable accomplishments.

IV. FORECASTING - HUMAN FACTORS RELATED TO PROGRESS IN RESEARCH AND DEVELOPMENT

E. Influence of Chance and of Intuition

Chance and intuition have been responsible for many important discoveries. Neither is predictable. Hence, these elusive factors complicate the process of forecasting progress in research and development.

The predictability of probable accomplishments involves two unpredictable factors. One of these is chance; the other is represented by the terms "intuition" and "insight."(4).

The scientific literature is replete with examples of experimental findings that "happened by chance." It was by chance that Roentgen discovered X-rays. He happened to leave a bottle of barium platinocyanide near a tube used in studies on electrical discharges in a vacuum. It was his curiosity, however, that made him aware of the fluorescence in the bottle, even though it was separated from the tube by black paper. It was by chance that one of Ringer's assistants placed frogs' hearts in tap water rather than in distilled water, as was intended. Ringer's Solution resulted from the observation that the hearts continued to contract for many hours. It was by chance that Oersted happened to hold the two wires from a voltaic cell in such a position near a compass that the deflection of the needle was seen--an observation that led subsequently to the invention of the electric dynamo by Faraday. It was in recounting this important chance finding that Pasteur made his well-known statement, "In the field of observation, chance favors only the prepared mind." No method of increasing the incidence of chance discoveries is known other than by augmenting the supply of "prepared minds" and by increasing the opportunities and incentives for experimentation.

Intuition or insight is quite different from chance. It is an understanding or explanation of a problem that comes into mind unexpectedly and suddenly. Oersted discovered the effect of an electric field on the needle of a compass by chance but it was insight that made him immediately reverse the position of the two wires so that the opposite movement of the needle occurred. As in the case of chance, no sure method of increasing the incidence of ideas by insight is known. Again, as in the case of chance, a prepared mind is an asset because conscious effort to find a solution of a problem is conducive to a subsequent intuitive explanation. Unfortunately, ideas that come by insight require experimental confirmation because they are sometimes incorrect.

To what extent progress in research is dependent on these factors is not known. Each has been responsible for extremely important advances in knowledge but it appears inconceivable that the planned, orderly attack on scientific "unknowns" has not been responsible for the greater part of the research findings that account for the present technological environment. It is certain that the predictability of probable accomplishments will be influenced by results that are due to chance and to insight but to what extent cannot be determined. It also appears certain that there is urgent need for the training of increased numbers of qualified investigators and for the expansion of opportunities for research so as "to increase the fertility of the soil on which the seeds of chance and intuition may fall."

V. FORECASTING - RELATION OF AVAILABILITY OF PHYSICAL RESOURCES TO THE PROGRESS OF RESEARCH AND DEVELOPMENT

> Money encourages research and development but the degree to which money actually creates discoveries is a moot question. Competition for funds determines the amount allocated to R&D and its distribution between projects. Accurate forecast of the continuing availability of physical resources is essential. The decisions on the relative importance of competing proposals demand a complete array of facts and an interdisciplinary viewpoint on their potential usefulness.

It is a truism that any scientific discovery may result in wholly unexpected technological developments--a fact that argues strongly against the excessive enticement of universities to carry on committed or specialized research. It is also true that the frequency of discoveries is increased by expanded support of both research and technology because research prospers on the problems and other assistance it gets from advances in technology. The increase in discoveries is not proportional to expenditures, however, and the degree to which money creates discoveries is a moot question. Great Britain became thoroughly productive in scientific advances after the support of universities reached adequate levels. The frequency of scientific breakthroughs will certainly increase from its present low level in developing states of Africa, for example, when enough of the gross national product is allocated to the support of science and technology. On the other hand, the actual expenditure of funds in the United States for the development of the Salk vaccine represented a very small fraction of the Polio Foundation's total support of research.

The magnitude of the current acceleration of research and development effort in medical and health-related fields is largely the result of grants-in-aid of research from private foundations, national organizations, and government agencies, the latter providing the major share. Federal expenditures in this area increased from 27 millions of dollars in 1947 to 1072 millions in 1963. The latter amount was 62% of the total expenditure in the United States for medical and health-related research and development in 1963. It was approximately 7% of the total federal R&D budget but less than 0.2% of the gross national product. These funds provide the salaries of many research workers and pay for equipment and supplies and for the construction of laboratories. There is increasing support, also, for graduate and postgraduate training. It appears safe to conclude that the support will continue, probably in increased amounts, as long as the gross national product grows in size and value.

The forecasting of resources for the support of biomedical research and development is simpler than the prediction of progress in this field. Reasonable estimates can be made of the fraction of the gross national product that can be used for biomedical research and development 10 or 20 years from now and what this will buy in laboratories and supplies. Predictions can also be made of the number of scientists and engineers who will use these facilities. The forecasts of the projects in which these individuals will be engaged, however, will have considerably less accuracy than the forecasts of resources because of the unpredictability of research breakthroughs and discoveries.

The relation of money to discoveries or of physical resources to progress in research and development needs thorough study and evaluation. Involved are budget allocations for research and development; research objectives; distribution between in-house and contract research; the nature and effect of specific allocations of funds for personnel, equipment, supplies, and construction; the standard of excellence demanded as a prerequisite for support; and, the over-all effect of the resources on the number, competence, and productivity of scientific workers.

Competition is an essential part of the allocation of funds to R&D budgets and of the subsequent distribution of these funds in the support of separate projects. In each instance judgments are made of the potential worth of the project on the basis of supporting claims. Probabilities are involved, hence the judgments are essentially predictions. Someone must make these choices, choices which may have far-reaching side effects as well as enormous direct results. What proportion of the total national product can be safely used for military R&D? Is it better to develop the civilian economy as a means of improving military capabilities? Should R&D funds be used to give soldiers greater protection by the development of shielding equipment or by the development of biological procedures for increasing endurance and resistance to stress? Is the primary need for equipment for warfare in tropical lowlands or Southeast Asia or for tanks and missiles in Europe or in North America? Is "cost effectiveness" a reliable criterion? How are total costs determined? What if England had figured the cost and had not given the Spitfires the extra 20 knots of speed?

The forecasts of physical resources that will continue to be available for research and development are an important part of the over-all prediction of progress in science and technology. It would be improper to minimize in any way the enormous difficulty involved in decisions on the amount and distribution of these resources. It is doubtful if the total sum will ever be sufficient to provide for all of the projects that are presented for support. As stated by Weinberg, "It seems inevitable that science's demands will eventually be limited by what society can allocate to it."(11) The accurate evaluation of competing proposals requires a complete array of facts. In addition, a rigid examination of the relative potentials of the proposals for future usefulness demands the integrated judgments of specialists having different points of view.

VI. FORECASTING - A METHOD OF PREPARATION AND USE OF A FORECAST

No magic key to open the door to the future has been found. Useful forecasts are the result of hard work in which experts with knowledge, competence, insight, and experience are brought together in an atmosphere of inquisitive enthusiasm for the solution not only of existing problems but also of problems that are anticipated. Forecasts may be restricted to narrow areas or they may be wide in scope, as is the Army Technological Forecast. However, if forecasts are used in planning their results are always complex. Like the stone that is dropped into a pond, the action is simple but the effect continues in ever-widening circles.

The dynamic character of a forecast makes it obligatory that they are based on sufficient facts and wise interpretations of facts to justify the conclusions that are drawn. These need not necessarily have a high degree of validity but they must have a specified degree of probability, whether it is of high or low degree. An element of uncertainty is always present in a forecast. Some of the indirect results may be quite unexpected, even harmful. For these reasons the continuing review and challenge of forecasts are indispensable.

Procedures which are considered essential for the formulation of useful forecasts are listed in Table 1. These include the description of ultimate objectives; the description of new knowledge judged essential for the attainment of the objectives; the planning of successive short-range research projects designed to serve as steppingstones toward the necessary knowledge; the planning of short- and long-range programs of technological developments on the basis of successive discoveries; and the periodic review of each of the individual forecasts, plans, and accomplishments. The requirements of each of these procedures are also indicated.

The underlying feature of the forecasting procedure shown in Table 1 is the step-by-step progress toward the ultimate objectives. This combined type of forecasting and planning, which includes constant reassessments, is essentially the short-range prediction of the requirements of research and of developments made possible by completed research, with whatever aid that can be provided by predictions from trends, all within the framework of the long-range objectives. By this procedure the dynamic nature of a forecast becomes a dynamic method of forecasting in which there is always an expression of the best judgments regarding opportunities for progress in research and development. The frequency of reviews depends on the frequency of discoveries, i.e., with the rate of progress in designated areas of research. In no case should forecasts remain unchallenged longer than one year.

The forecasting procedure described in Table I is illustrated in Figure 2. Also shown are a futile step in forecasting and planning and an unpredictable and, therefore, an unexpected discovery. The discovery might hasten the attainment of the required new knowledge or it might require a change in the ultimate objective. These decisions would result from the periodic review of the initial forecast.

TABLE I

REQUIREMENTS OF FORECASTS

1. Description of long-range or ultimate objectives.

   a. Requirement of selection of a future technological environment representing desirable improvement in human welfare.
   b. Requirement of support of endeavors to achieve a better ordering of facts and better integration of knowledge.

2. Description of new knowledge essential for attainment of ultimate objectives.

   a. Requirement of judgments made without reference to definite periods of time in view of unpredictability of discoveries.
   b. Requirement of familiarity with existing knowledge and with its possibilities for future accomplishments.
   c. Requirement of interdisciplinary communication because of complexity of existing knowledge and of its potential.

3. Planning of successive short-range research projects designed to serve as steps toward the ultimate objectives.

   a. Requirement of judgments on the need of personnel and of physical resources.
   b. Requirement of proposals for increases in availability of personnel and physical resources.
   c. Requirement of encouragement and support of type of research likely to yield maximum productivity.
   d. Requirement of planning based on forecasts with high probability of accuracy and including alternative plans according to urgency of need of solution of problems.

4. Planning of successive short- and long-range technological developments on the basis of established discoveries.

   a. Requirements as in No. 3, above.

5. Periodic review of all aspects of forecasts, plans, and accomplishments.

   a. Requirement of modifications of ultimate objectives as necessary.
   b. Requirement of modifications of research and development projects, in progress or planned, on basis of new discoveries.
   c. Requirement of awareness of the effect of programs of committed research on the incentives and concepts of man and on the relationship of changes in these attributes to continuing developments in science and technology.
   d. Requirement of continuing search for improved method of preparing and using forecasts.

The individual short-range R&D goals in Figure 2 (Blocks B_a-B_x-B_y) are those deemed most likely to answer the question, "What must be known (Block C) in order to make it possible to reach the ultimate objective (Block D)?" The figure assumes that one pathway, the B pathway, is selected because of its high probability, even though an unknown number of intermediate steps may be required. On the other hand, it is possible that the consideration of ways and means of obtaining the necessary knowledge (Block C) would bring to light one or more attractive pathways (not shown in Figure 2) in addition to the B pathway. Whether the decision is made to test these simultaneously or one after the other will depend on the urgency of the need of the new knowledge, on the availability of resources, and on the degree of probability of success predicted for each alternative procedure. In any event, periodic reviews of progress and of the relevancy of the progress in terms of the predetermined objectives are indispensable.

Figure 3 illustrates the same step-by-step process adapted to the forecasting of the technological development of a vaccine and represented as successive programs of research, exploration, and development. Research leads to the unpredictable discovery of a new disease producing virus. On the basis of the new knowledge the prevention of the disease becomes the ultimate objective and knowledge about a suitable vaccine becomes the long-range research goal. The isolation of the virus permits the reasonable forecast that a vaccine can be found but only on the basis of analogy with other similar situations. The requirements for the search for the vaccine are predictable but not the time and circumstances of its discovery. After the discovery of the vaccine the exploration for methods of production begins. Experience with other vaccines is useful in predicting the anticipated progress. Selection of a suitable method is based on the processes of exploration and development. Again, experience coupled with knowledge of available resources of personnel and facilities allows satisfactory predictions of progress. Following test and evaluation of the selected product, it is prepared as a developmental product for use. The presentation in Figure 3 is obviously an over-simplification but it is useful in emphasizing the step-by-step procedure of accelerating progress in research and development by use of short-range forecasts to attain a specific long-range goal.

Figure 4 summarizes the ideas of Table I and of Figures 1-3. Accompanying it is a description of the 7 forecasts (F_1-F_7) indicated in the figure. Only one of these, F_1, is a long-range forecast, viz., that of the new knowledge or goals of research that will make possible the desired ultimate technological environment. Each action results in an action product and these contribute singly and in toto to new knowledge and to the new environment. The requirements and probabilities of accuracy of the 7 forecasts are suggested. It is emphasized strongly that the initial forecast, F_1, is not an assurance of research accomplishments. It does serve to establish research goals which, if attained, will make possible the new level of science and technology. Whether or not the goals are actually reached depends on the success of the intervening steps in research, exploration, and development. Each of the short-range programs can be effective or wasteful depending on the reliability of the forecasts used in planning. It is clear, therefore, that the entire process of changing the environment will rarely be as straightforward as is suggested in Figure 4. This could only be the case if each planning step is based on a completely accurate forecast, a quite unlikely circumstance.

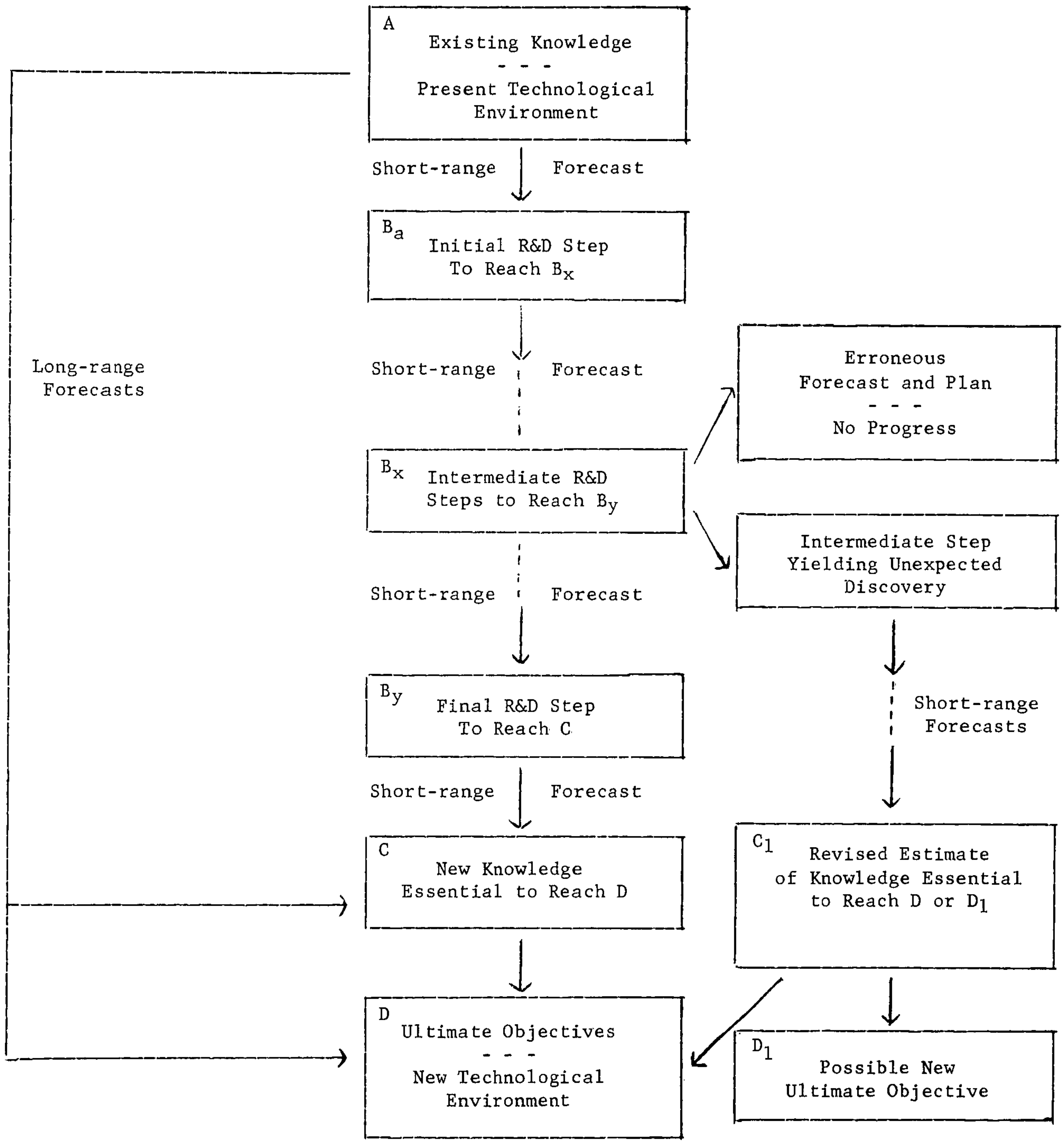

Figure 2. Representation of long-range forecast of ultimate objectives without reference to time by successive short-range forecasts of research and development.

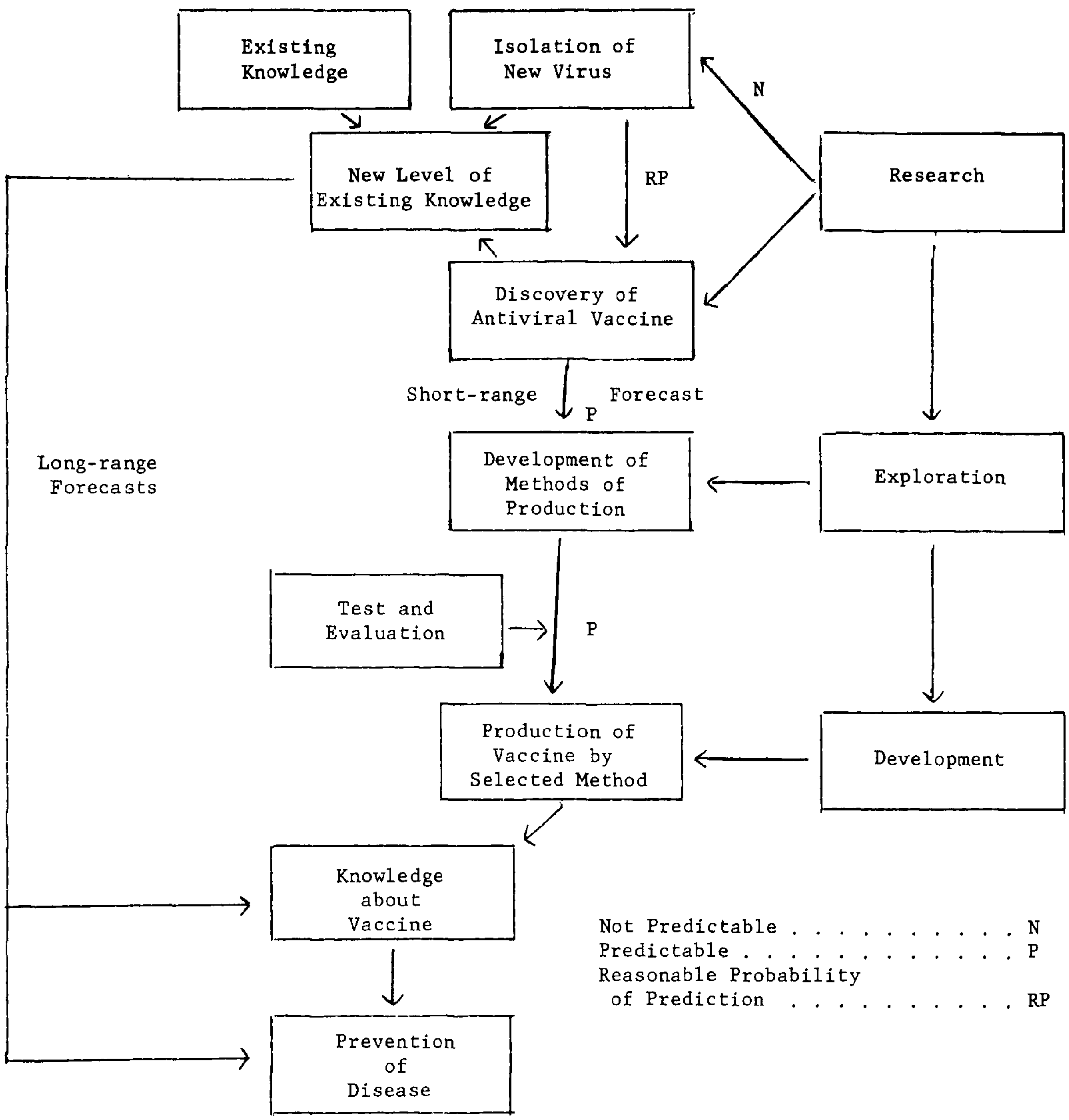

Figure 3. Representation of long-range forecast of prevention of a viral disease without reference to time by successive short-range forecasts of roles of research, exploration, and development in the discovery and development of a vaccine. Time of accomplishment is not predictable except by estimations based on experience in similar situations.

FORECASTING OF RESEARCH AND DEVELOPMENT

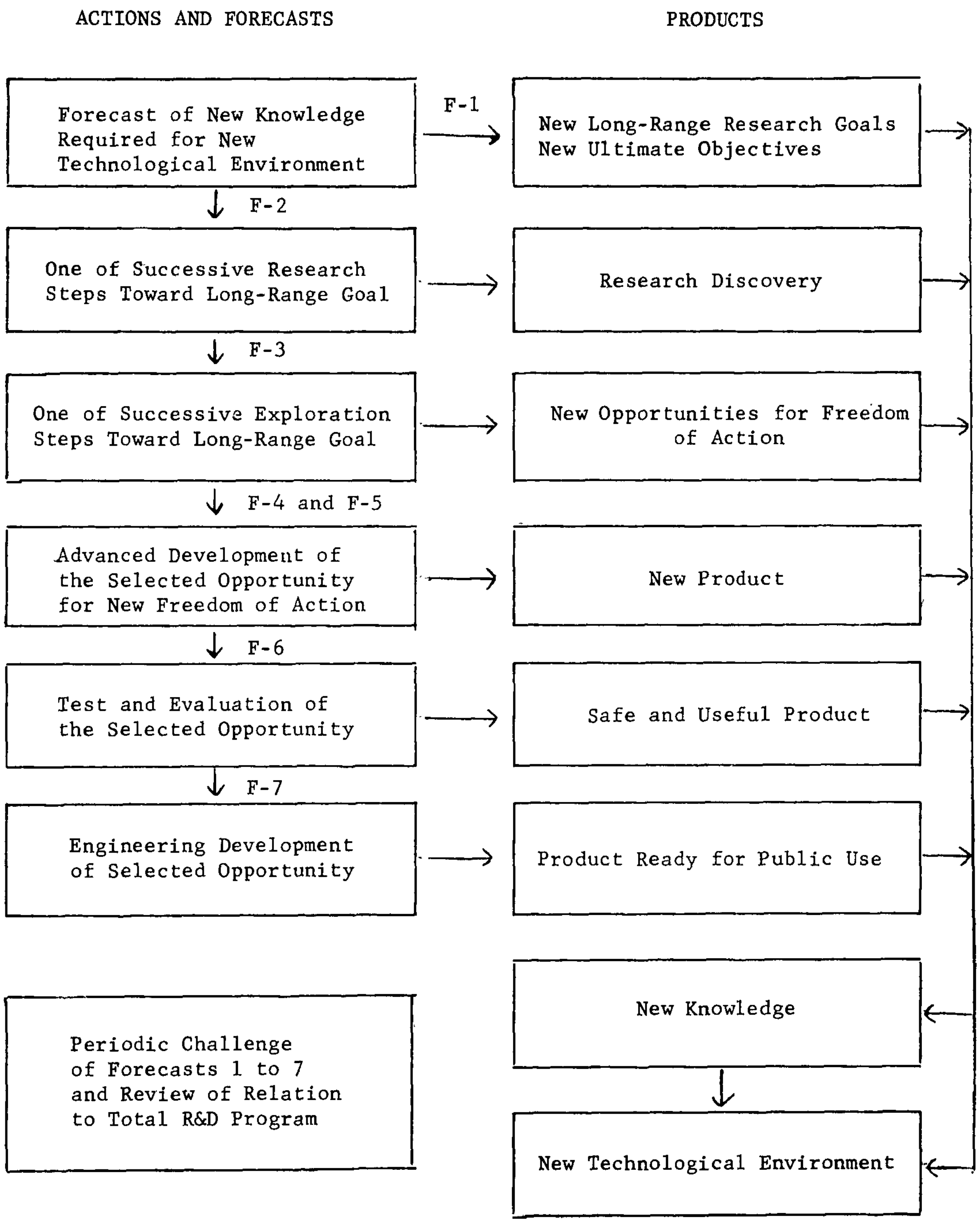

Figure 4. Interrelations of actions, products of actions and forecasts in research and development.

NATURE AND REQUIREMENTS OF FORECASTS IN FIGURE 4

Forecast No. 1 - Description of research goals needed to attain ultimate R&D objectives.
Requirements: Familiarity with existing knowledge and with its possibilities of use in future accomplishments; knowledge of available resources; evaluation of potential of ultimate objectives in comparison with competing proposals; judgments on basis of interdisciplinary communication among biomedical scientists*, engineers, economists, socio-anthropologists, and military representatives.
Probability of Accuracy of Forecast: Accuracy in direct proportion to the satisfaction of the requirements.

Forecast No. 2 - Planning of first research step.
Requirements: Understanding of fact that result of research is not predictable; understanding of step-by-step approach to long-range research goal and of relation of first step to attainment of ultimate goal; knowledge of available resources; experience with requirements of personnel and facilities in other similar situations; judgments by active biomedical scientists.
Probability of Accuracy of Forecast: Discovery is not subject to forecast; accuracy of forecast of requirements is satisfactory; accuracy of choice of first research step is uncertain.

Forecast No. 3 - Planning of studies of significance of discovery for technological applications.
Requirements: As in F-2 with additional requirements of ability to recognize utilitarian possibilities of discovery.
Probability of Accuracy of Forecast: Satisfactory because resources required for developments based on established discoveries can be determined on basis of experience.

Forecast No. 4 - Selection of a particular opportunity for freedom of action.
Requirements: Judgments by administrators, political scientists, engineers, etc., on basis of geopolitical, economic, and social as well as technological considerations.
Probability of Accuracy of Forecast: Satisfactory

Forecast No. 5 - Planning of development of selected product.
Requirements: As in F-3
Probability of Accuracy of Forecast: As in F-3

Forecast No. 6 - Planning of tests of usefulness and of safety of product.
Requirements: Familiarity with methods of use and with methods of determination of possible toxicity or other hazards; knowledge of availability of resources for tests.
Probability of Accuracy of Forecast: Results of tests of safety are not predictable except by analogy on the basis of experience; prediction of requirements for tests have satisfactory probability.

Forecast No. 7 - Planning of engineering aspects of the development process.
Requirements: Scientific and engineering competence.
Probability of Accuracy of Forecast: Very satisfactory

*biochemists, biophysicists, physiologists, pharmacologists, microbiologists, immunologists, etc.

VII. FORECASTING - SUGGESTED PROBLEMS FOR STUDY

The consideration of the many complex and far-reaching aspects of forecasting has made clear the unsatisfactory nature of the evidence of usefulness of the process. In addition, problems that are of fundamental significance in the relation of resources to productivity in research and development remain unsolved. This relationship is particularly pertinent to the predictability of progress in science and technology. Three such problems that need extensive study are listed below.

Problem No. 1 - Evaluation of the Army Technological Forecast.
Comment: Demonstration of significant benefits is needed in order to justify the time required for the preparation of the Forecast. Answers to questions, such as the following, are needed:

Is there evidence of the actual use of the Forecast in planning?

In which instances has the use of the Forecast been effective? What procedures have been used in the preparation of the Forecast? What has been the distribution of predictions between research, exploration, and development?

Have the forecasts of developments been formulated independently of the forecasts of research and of exploration or in conjunction with these forecasts?

How important have been forecasts based on trends?

Over what time intervals have predictions shown the greatest degree of probability? Based on experience with short-range predictions what is the probability of the usefulness of a 10-year and of a 20-year forecast?

Problem No. 2 - Clarification of the relationship between resources and productivity in research and development.
Comment: It is not enough to accept the fact that ample resources favor increased productivity in research and development. Answers to questions such as the following are important:

Is the stimulation of productivity of the same or of a different magnitude for the three activities--research, exploration, and development?

Is there a point of diminishing returns in the expenditure of R&D funds and, if so, what factors determine this point?

Are there differences in the productivity and cost of research and development performed in-house or on outside contracts?

To what extent is it possible "to buy" discoveries? When, if at all, do bountiful funds for research and development affect science and technology adversely by decreasing the potential benefits that arise from uncommitted research or by concentrating research in those areas emphasized by forecasts to the detriment of other important areas.

Problem No. 3 - Examination of the total productivity, cost efficiency, and general effects on science and technology of Federal support of research and development by three well-known types of subsidy, viz., the support of health-related projects of individual investigators by the National Institute of Health, the support of the science-related projects of individual investigators by the National Science Foundation, and the support of research and development in agriculture in Land-Grant institutions by the U.S. Department of Agriculture.

Comment: This is a highly important problem with many ramifications which must be subjected to analysis sooner or later. The study might well include other types of support such as that by NASA and ONR. It is of special significance for the Army to examine the basis of the widespread opinion among scientists that research support by ONR is much more desirable than support by the Army.

VIII. SUMMARY OF FACTORS OF IMPORTANCE IN FORECASTING

1. Although the value of forecasting has not yet been proved, it is a reasonable hypothesis that forecasts based on existing knowledge and on reasonable estimates of future capabilities in research and development can be useful in the acceleration of progress towards stated scientific and technical objectives.

2. Conclusions regarding the role of forecasting in the predetermination of the technological environment require a clear distinction between research, exploration, and development and a definite understanding regarding the time intervals covered by the forecasts.

3. Decisions on projects in research and development which compete for support are commonly based on estimates, i.e., forecasts, of their relative potential for significant advances.

4. No evidence of the common use of forecasting as an aid in the planning of programs of research and development is found in the history of science and technology except in the instance of predictions based on trends.

5. Forecasts of developments based on trends of relationships between previous experiences and subsequent events are valid if the relationships are demonstrated to have a causal and not an accidental connection and if the previous experiences are sufficient in number to insure satisfactory probability.

6. The forecasting of requirements of personnel and of physical resources is an important part of the planning of programs of research and development which has validity if based on knowledge and experience.

7. The accurate forecasting of the nature, timing, and circumstances of discoveries, including the results of specific research projects, is not possible.

8. Discoveries resulting from chance observations or from intuitive knowledge are not predictable.

9. Long-range forecasts are of questionable value unless they are directed towards research objectives without reference to time or unless they are concerned only with technological developments that are based on previously established discoveries.

10. Areas in biology and medicine in which active research and exploration are in progress offer the best opportunities for the successful forecasting of probable accomplishments.

11. Forecasting cannot be dissociated from the concept of probability of accuracy and the degree of probability should be a part of the forecast.

12. Forecasts are dynamic actions whose continuing, direct and indirect effects merit serious study and evaluation.

13. Erroneous forecasts are hazardous because of interruption of progress, misdirection of energies, and waste of resources.

14. A better ordering and integration of the enormous number of facts making up today's science and technology are prerequisites for the preparation of more effective forecasts of the use of this knowledge.

15. Forecasts of probable accomplishments require the interdisciplinary communication and integrated efforts of research scientists, engineers, statisticians, economists, and administrators in order to insure the accuracy and adequacy of observations and the reliability of interpretations, conclusions, and deductions.

16. Planning in research and development must utilize forecasts based on complete knowledge and understanding of science and technology in other nations if superiority in the technological environment on a world-wide scale is to be achieved.

17. Increased exchange of scientists between military and university laboratories will improve the understanding of science and technology, will emphasize military requirements, and will enhance the recognition of otherwise undetected possibilities for advances in research and development.

18. Predictions that depend on the achievements of men must recognize the changes that are occurring in the nature of man and in his concepts of his relation to the world in which he lives.

19. Forecasts of progress in research must include predictions of the influence of other variable factors of importance in human performance, such as education, training, competitive position and spirit, vigor, incentive, efficiency of planning and of research management, and motivation for choice of research objectives.

20. Forecasts of progress in research and development must include predictions of the availability of physical resources for use in expanding the supply of qualified personnel and the laboratory facilities required for their activities.

IX. CONCLUSIONS

1. The factors that are important in forecasting are summarized in the preceding section (Section VIII). They lead to two related conclusions: (a) forecasting can be an important aid in the planning of research and development if prepared and interpreted sensibly and (b) the proper use of forecasts can accelerate the rate of progress toward preselected goals. The preparation of an Army Technological Forecast, as described in Table 1, page 39, is believed justified and this is recommended.

2. The approach to forecasting must be realistic. It must be broad, not narrow, and it must deal with the more general aspects of problems, not with details. Forecasts cannot be expected to include discoveries but the frequency of discoveries may be increased by appropriate support of research. Long-range forecasts of objectives are indispensable but a long-range forecast of actual accomplishments must be accepted as a series of short-range predictions that remain under constant surveillance and modification.

3. Attention to the human factors in the prediction of progress toward predetermined objectives in science and technology cannot be overemphasized. The nature of man determines what he wants, what he will do, and what he will support. An understanding of the changing nature of man and of his concepts is vital to the prediction of his future achievements. Forecasts can only be useful if they are successful in channeling human interest and energy and the resources of the world in the direction of selected ultimate objectives. The extent to which this is possible--and experience shows that it is possible--is determined by the responsiveness of man to the multitude of competing demands that also seek his interest and energy. Recognition of the priority of the demand for progress in research and development directed at national defense is of paramount importance. But this attraction of scientists and technologists to work on committed projects, including those that are a part of the military effort, must be balanced by the simultaneous encouragement of uncommitted research. Only in this way can the forces active in the expansion of knowledge and understanding reap the full advantages of the widest variety of discoveries in many fields.

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