

SOME THOUGHTS ON PLANNING – THE HANDMAID OF DIRECTION

Cogito, ergo sum – Descartes

INTRODUCTION

In his “Discours de la méthode,” a thesis based on systematic doubt, Descartes started with one certainty: “I think, therefore I am.” To think, a mind must be nourished by learning, that is, by assimilation of experiences coming into it through the five senses, providing it with ideas that it can rearrange and re-associate through the processes we call “thinking.” Experience from the past generates thoughts that govern our actions in the present, and present and past experiences generate thoughts about planning what we shall do in the future.

The quality of our thinking depends to a large extent on our mental inventory, and the scientist and engineer are particularly fortunate in having access to a wealth of reliable knowledge of nature,¹ including man, on which to ruminate. It is the business of the scientist to discover new and reliable knowledge (facts) and to relate these facts in a consistent, aesthetic pattern or system. It is the business of the engineer or clinician to apply reliable knowledge to the creation of devices and services which enhance the pleasure, welfare, and security of his fellows sufficiently that they are willing to pay for them. A successful device or service, that is to say one which fulfills completely the objectives set by its creator, attests to the reliability of the knowledge and the skill of the creator in applying it. Furthermore, a successful mechanical device, particularly one that involves the automation of a system, provides additional reliable knowledge to the engineer, food for thought that may lead him to apply this knowledge to other fields. In this essay, I shall apply some thoughts arising from physics and from the systems engineering of relatively complicated mechanical systems to the guidance of the much more complicated systems that are based on the cooperation of human beings.

ASSUMPTIONS

1. An organization of human beings is a system with a definite set of objectives and depends on the intelligent cooperation (however stimulated) of a number of individuals for the success of its operation.
2. An organization of people has so many features in common with man-made and biological systems that principles and even details learned from studies of these latter systems can be applied to human organizations.

3. The human organization is not a *closed* system, but an *open* one whose interactions with its environment are of paramount importance to its continued healthy operation. For example, this system must receive a constant and adequate input of energy from the environment to keep its entropy down. If its entropy reaches a maximum value, the system gets into equilibrium and is dead.
4. Like all mechanical systems, the system we are considering has extension in time, and perhaps the chief function of its control system is to ensure dynamic stability of the whole system in the face of the external and internal changes that it will experience as time goes by.
5. Like the best mechanical system, an organization of human beings must be designed and operated to meet at least two conflicting requirements, requirements that are fundamentally incompatible (Niels Bohr’s principle of complementarity). I make the assumption that the conflicting requirements imposed on a guided missile, stability and maneuverability, are closely analogous to the conflicting requirements of centralized regulation and individual freedom that the human organization, particularly an R&D organization, must live with.

The foregoing assumptions are to me rather obvious, so obvious indeed that they may be called trite, but if the reader does not consider them to be so, I suggest he stop right here.

Before proceeding with my discussion of planning, I must say something more about assumption (5). A well-designed antiaircraft guided missile must fulfill the requirements of stability in flight and high maneuverability to outwit its target. These are really incompatible requirements. In a human system a “balance must be kept between the excess of unbounded centralized power and the extravagance of liberty not enough restrained” (Earl of Halifax, 1717). In 1956, Max Born modernized this statement in physical terms:

Complete freedom of the individual in economic behavior is incompatible with the existence of an orderly state, and the totalitarian state incompatible with the development of the individual. There must exist a relationship between the latitudes of freedom ΔF and of regulation ΔR of the type $\Delta F \Delta R \approx p$, which allows a

reasonable compromise. But what is the political constant p ? I must leave this to a future quantum theory of human affairs. The world, which is so ready to learn the means of mass destruction from physics, would do better to accept the message of reconciliation contained in the philosophy of complementarity.

(From a lecture by R. V. Jones given at the University of Durham, 1971.)

Born obviously had in mind the wave-particle dualism, the basis of quantum mechanics that has been so successful in systematizing the reliable knowledge discovered in studies of the interactions of atoms and molecules. It is important to note that an electron behaves as a wave under one set of circumstances and as a particle under another set of circumstances, but never as both simultaneously.

Applying the principle of complementarity to human systems, we note that the relative weights given to latitudes for freedom, independent thought, and action on the part of the individual and those given to latitudes for centralized regulation also change with circumstances. In other words, $\Delta F/\Delta R = f$ (circumstances) and varies with time.

For example, the Defense of the Realm Act (DORA) passed by the British Parliament in 1915 (I think) was essential to the achievement of the national objective, namely, the winning of World War I. It greatly increased the importance given to governmental regulation and consequently diminished the values set on individual freedom, but was acceptable to the nation as a means of conserving and using effectively its resources to achieve a well-recognized and urgent objective.

However, when this national objective vanished with the end of World War I, DORA still remained in effect, and first lost general acceptance, then became a nuisance, finding support among people who sought to reform the drinking habits of the working classes, and finally became an instrument for the destruction of independence of spirit and morale.

The application of the principle of complementarity to an organization calls for a departure from conventional thinking, both on the part of the "management" and on the part of the people who make up the organization.

To be viable, the system must operate on a compromise between two apparently incompatible concepts, freedom of the individual and centralized control (regulation). This compromise varies with the circumstances that surround the operation and especially with its overall objectives and with the tactical objectives of its component parts (departments, divisions, etc.). Above all, this compromise cannot intelligently be made by a "once and for all" decision (or policy) and in the case of a heterogeneous organization cannot be made by across-the-board decisions or actions, but only on the basis of an intelligent appraisal of the requirements associated with its strategic and technical objectives and interactions

with the environment. All change periodically and unpredictably with time.

This may sound like a rejection of the comfortable doctrine of absolute good and absolute bad, right and wrong. It is. It may be called a doctrine of expedience, but it is not. It really expresses the concept of *adaptability*—a concept fundamental to the survival of a species.

DIRECTION

The chief function of the director² of an organization (and here I shall talk mainly of R&D organizations) is to turn plans into accomplishments. The first question that arises is, "What is the organization going to accomplish?" In other words what is (or are) its prime objective(s)? Formulating the answer to this question is primarily the responsibility of the director, subject, however, to certain important constraints.

1. Has the environment the capacity to support the effort needed to attain the objective? Specifically, is there a sponsor with enough money?
2. Is there a high probability that the environment will support the effort needed? Specifically, will the accomplishment (product) be sufficiently useful to a sponsor in the pursuit of his objective to inspire his self-interest, enthusiasm, and support? Note: It cannot be assumed *a priori* that the merits of a useful product will be apprehended immediately by a potential sponsor. A certain educational effort is needed to persuade the sponsor that the product will be *really* useful to him (vulgarly, a sales job).
3. Is there a high probability that the key personnel in the organization will come to a consensus on the potential merits of an enterprise whose success depends on their wholehearted and intelligent cooperation? If the merits of the undertaking are apparent to all, arriving at such a consensus is relatively easy. However, the value of big accomplishments is seldom obvious at their conception. The leadership of the director and the confidence he inspires in the membership of the organization play an exceedingly important part in the establishment of this consensus. (Here a study of the consensus type of management reputedly practiced by Japan and the various command types of management practiced in this country and elsewhere seems indicated.)

In his biography of Merle A. Tuve, Phil Abelson remarks³ that Tuve considered the principal discovery of World War II to have been the efficiency of the democratic principle in dealing with people, and quotes him as follows:

The democratic principle is this: Tell the worker or the people of the community what

the need is, invite them to contribute in the best way they can, and let them help you and help each other meet that need. Any society or any group always selects men to handle certain tasks, by elections or by hiring them or by some other system. But notice that a boss using the democratic principle does not depend on orders; he asks his men, his workers, to participate. This means that they help him with the whole job, they don't just do what they are told to do. This system of asking people to help with the whole job was what I used in running the proximity fuze development. It worked so well, the whole team took hold so vigorously, that during most of the work it was a struggle to keep up with them. I often felt like a short-legged donkey trying to keep from being run down by a stampede of race horses.

One should note that the "democratic principle" was being practiced very effectively by Dr. A. L. Day in the direction of the Geophysical Laboratory before World War II. Furthermore, George Woolard's statement in presenting the Bowie Medal of the American Geophysical Union "Anyone who knows Merle Tuve recognizes that he is a driver who never spares himself. . . ." is also quoted by Abelson.³ Apparently Dr. Tuve knew how to use Born's "equation," $\Delta F \Delta R \approx p$.

4. Limitations of Resources. Access to three types of resources must be considered before any project, whatever its size, is undertaken: (a) facilities and equipment; (b) people with appropriate technical knowledge, skill, and experience; (c) people who, in addition to technical knowledge, skill, and experience, have the ability to organize groups large enough to handle the work involved. Resources are always limited.
5. Scientific Integrity. The question, shall we or shall we not commit the funds of a sponsor and the resources of our organization to the accomplishment of a proposed project, is the most important one that confronts a director. In the long run, the right answer to this question constitutes the most important service he can render to the client and calls for the exercise of a high order of scientific integrity, especially when the client himself has proposed and is anxious to support the project. Is the goal set by the project scientifically and technically attainable in the present state of the art, or does it require new knowledge and techniques whose acquisition can be readily foreseen, or whose acquisition will require radically new concepts? What is the probability that the organization can produce these concepts? Will the successful achievement of these goals really add to the sponsor's resources to do his job? If so, how much? And how general will be its impact on science and engineering? How will its commitment to the project affect the R&D organi-

zation itself? Will it result in routine challengeless stagnation? Will the unknown stimulate growth in knowledge and skill and the exploration of mind-broadening alternatives? Will the difficulties of the unknown problem be so fundamental that frustration or vast overruns of time and cost are likely?

Answers to these questions can only be given by the director of the organization. They are crucial to its welfare and require a deep knowledge of the organization's resources, knowledge of the client's problems (real, not imaginary), and meticulous exercise of scientific integrity, that is to say, the placing of scientific and engineering considerations beyond the reach of considerations such as money or expediency. To answer these questions, he must be fortified with a good set of plans provided either by himself or by his colleagues.

PLANNING

As I see it, the functions of planning are as follows:

1. Analysis of the five constraints we have just listed, and an assessment (in quantitative terms, if possible) of their relative probabilities, significance, and consequences.
2. Analysis of the client's problems to establish the cogency and stability of the problems as originally stated or of promising technical alternatives, and assessment of the merits of alternative technical approaches.
3. Formulation of a strategic and tactical plan for the conduct of a project (of any size) from inception to accomplishment, including specification of resources and of intermediate goals.
4. Anticipation of contingencies. Formulation of alternative approaches or other changes of course, if the need arises.
5. Formulation of long-range strategic plans. Continuous survey of future "markets" and new technologies.

In developing a discussion of planning, let us first consider two simple cases: (a) that of the individual scientific investigator starting on a new line of work, and (b) that of the captain of a battleship entering New York Harbor without a pilot.

The considerations that motivate the individual to enter a new field of investigation either may be a strong desire to know something about a new area of knowledge or may arise from a conflict in his mind from disagreement between what he has read or teaches and his experience. Whatever the motivation, it stimulates plans in his mind, plans to undertake a trip into the unknown. The individual either has or obtains as sound a knowledge as possible of the area of nature where the field he proposes to enter lies. He imagines that there may be interesting problems in

this field, problems which may be attacked by his expertise. However, his first step in planning is to decide on a small but significant problem, then decide on a promising mode of attack, verify this promise by as simple an experiment as he can devise, and then decide to invest time and equipment in working out a thoroughgoing solution. Where will the money needed to support this investigation come from? All this planning goes on in his own mind, fortified by discussions or reading.

The value of the accomplishment will depend on a number of factors. Did his choice of problem lead to divergent or convergent results? In other words, did the answer to his original question lead to a number of more interesting questions or did it not? Did he encounter surprises, careful attention to which led him to a change in course to answer new and more exciting questions? Specifically, what part did serendipity – accident with sagacity, according to Hugh Walpole – play in influencing the course of his work? In this case, it is obvious that the investigator has, within the limits set by nature and resources, complete freedom to use his imagination and set his own objectives, and flexibility to change his course toward these objectives as experience and circumstances dictate. In anticipation of what follows, I should say that the “time constant” of this worker is quite short.

The captain of the battleship has somewhat the same problems, but their relative priorities are vastly different. His main objective has been well defined either by himself or others, namely, to have the ship moored safely to a certain pier by a certain time. The course to be followed is defined within certain limits by a chart. He knows his present position; the chart shows the channel to be followed and the various aids to navigation marking the channel. However, the environment contains a number of significant factors, tides, winds, and visibility, for example. Whereas the gross effect of the tide is known, local variations in water movement are not. The time constant of the response of the ship to its system is long. It takes many minutes for the ship to respond to its engine and its rudder, the only mechanisms to implement the captain’s decisions. The larger (heavier) the ship, the longer the time constant and the greater the distance it can travel before the effects of an order are apparent. To keep the time constant to a minimum, the crew of the ship must be well trained to respond with rapidity and certainty to the captain’s orders, and the captain must know this – a matter of the internal affairs of the organization. His own decisions must be prompt, his orders clear. Thus the captain’s job in turning the initial plan to moor his ship at a certain dock into the accomplishment of having done so involves many decisions not specifiable in the original plan but dictated by the timely recognition of the significance of weak as well as strong signals from the environment and by the ability of the system to respond. In this case, ΔR is great and ΔF correspondingly small.

CONTINGENCY PLANNING

In the light of the foregoing cases (analogies), our discussion of the functions of planning really starts with function (4), namely, anticipation of contingencies, formulation of alternative approaches, or other changes of course. This by no means degrades the importance of functions (1), (2), and (3), which are currently performed routinely by most organizations with more or less intelligence and effectiveness.

Function (4) emphasizes the fact that any open system must be able to respond promptly and effectively to significant changes in its environment if it is to maintain dynamic stability as time goes by.

Contingency planning is based on the timely and intelligent appraisal of external and internal trends in the environment and in the organization. Among the environmental trends to be studied are the significant changes in the real needs of the client, significant changes in the client’s mission, and changes in the technological environment. Does the device being developed for the client still represent the most timely and effective solution to his problem? Is it likely to remain so?

What new fields of knowledge seem to be emerging, cultivation of which would enhance the organization’s ability to develop another generation of devices to meet the client’s anticipated future needs?

The wise appraisal of external trends is more easily said than done. First, it requires an accurate firsthand knowledge of the environment, not secondhand information of doubtful value obtained from hearsay or reports, but from direct observation. For example, the participation of members of the Laboratory staff in Fleet operations and tactical exercises has uncovered significant problem areas whose importance was otherwise not realized, or realized so vaguely that nothing was done.

Second, the information gathered must be analyzed for consistency and significance and, if possible, reduced to some index whose change with time may be readily apparent and meaningful. Here the resources of modern computers are almost essential.

Economists use these methods. The doubtful value of their predictions is probably due more to the quality of the input data and the rationale of the indices used than to the methodology. In planning for an R&D organization, the devising of experiments for testing the results of prediction and consequent modification of the methods is, I think, not only essential but feasible.

Third, and obviously the most difficult step, is the convincing of those with authority over funds and resources that the results of the studies are reliable guides to future action.

The observations of internal trends, incorporation of these observations into planning, and action to maximize the organization’s effectiveness are most important features of any planning, whether contingent or long-range strategic. Significant internal trends are the growth or decline of the knowledge

and skills of the staff in various areas of science and technology, trends in the variety of these areas, trends toward specialization, the modernization or obsolescence of the supporting facilities, and trends in the quality and flexibility of management at all levels. A most significant index of these trends is the time constant of the organization, by which I mean the time required for the organization to respond to the technical challenges, which, after careful consideration, are supported by a consensus that they will promote the welfare of the organization and the interests of the clients.

The time constant of an organization, like that of an individual, has a natural tendency to increase with age, a phenomenon which is all too apparent in the history of many, but not all, government R&D establishments.

ORGANIZATION TIME CONSTANT

There are many reasons for this phenomenon, of which I shall discuss a few, namely, (a) tyranny of prior commitments; (b) mental inertia of individuals, and especially of groups of individuals; (c) the natural frequency or time constant of the genus *Homo sapiens*; (d) decline in the accomplishment risk ratio in the organization's planning; and (e) subtle undesirable changes in the relative values of ΔR and ΔF in the Born equation for the organization.

There is no better way for an organization to kill itself rapidly than by renegeing on its unfulfilled commitments. On the other hand, slow suicide may be achieved by subservience to commitments demonstrably fulfilled, but prolonged into a region of diminishing returns. This latter may be avoided in the case of the client being desirous of our insisting on a continuation of the organization's work in the field by formal termination of the existing commitment and initiating work under commitments to a new related technical objective. (Examples: the second generation of a device, or the solution of the production problems of the first generation.)

However, it is a fact of history that the prolongation of a project into the regions of diminishing returns is most frequently the result of the mental inertia of individuals or groups of individuals brought about by their laudable interest in the problems and the not so laudable concern over the investment they had made in knowledge and skill, coupled with the fear that this investment has become their sole asset.

The case of group inertia is complicated by management (nontechnical) factors. The expeditious accomplishment of most projects requires the directed or at least coordinated efforts of a number of people reporting to a group leader or supervisor who, in addition to technical knowledge and skill, must have the ability to inspire each individual to give his best toward the timely and excellent achievement of the objective set for the group. It is obvious that the increased responsibility of the successful group leader merits increased compensation through status in the

organization and monetary rewards. However, there is an age-old management doctrine that the rewards of a supervisor depend primarily on the number of people reporting to him and only secondarily on the technical requirements (imagination, innovation, knowledge, and skill) of the project. Although easy to explain to strangers, this doctrine is more treacherous. In particular, it greatly increases the time constant of the organization to react promptly to indicated changes. An able supervisor who could lead a smaller group to a very challenging new assignment not only feels a sense of deprivation in parting from people with whom he has worked a long time, but he may readily feel he has been demoted in starting with another smaller group.

During the war Dr. Tuve, either consciously or unconsciously, kept APL in a constant state of upheaval by frequent reorganization, if the word can be applied to such a loosely organized group. In passing, I might note that after a visit and dinner with the top staff of APL in 1950, Dean Acheson, then Secretary of State, used the term "organized chaos" in a complimentary way to describe his impression of APL.

Tuve was able to get away with his "constant mixing" philosophy in the stress of a dire national emergency. It is questionable in my mind whether this *modus operandi* would be successful or even acceptable in peacetime, but it did keep the time constant of the organization desirably short.

Analogy with a well-designed guided missile system suggests another way of dealing with the time constant problem – a way that gives the hint of an application of the principle of complementarity. A missile must fly stably and also must be able to change its course violently. It is very simple to design a missile that will be stable in flight. A well-designed arrow shot from a bow is a good example; a rotating shell is another; the spherical cannonball is a third. All of these projectiles are designed on a knowledge of constant external forces to follow a prescribed course to its conclusion, perturbed only by variable forces such as wind direction and velocity.

The greater the stability built into the missile, the greater must be the force-time product needed to change its course. Our battleships will keep moving in a straight line essentially forever.

The maneuverable missile, on the other hand, if left to itself, will be almost unstable, subject to being thrown off its attitude and course by small perturbations. Its control system, however, senses these small perturbations and counteracts them in a timely fashion, as do the central control mechanisms in a human being. The missile has dynamic stability. It can also sense the position, course (change of position), and change of course of its target. Information so obtained causes the control system to override the stability requirement and change the course of the missile in a timely way. Not only does the control system change the course, but it anticipates the effects of its action and avoids wide oscillation in the missile's course by further appropriate timely action. In short,

the incompatible requirements of stability and maneuverability are reconciled by timely and appropriate planned reaction by the control system.

Another compromise in the design of a missile is of interest in thinking about human organization, namely, the compromise in strength and weight. During its flight a missile is subject to large mechanical stresses, which it must be able to withstand if catastrophic events are to be avoided. Skillful engineers can predict these stresses within tolerable limits and seek to build a structure that will withstand them. They can add more material to increase the strength of critical parts of the structure, the classical technique used in building bridges, etc., but there are rigid limits within which they can work in the design of a guided missile. Increase in the weight of the structure affects critically the power requirement of its propulsion system, the demands on its guidance system, and its overall performance.

There is another possibility. The structural engineer can use materials which have themselves a high strength-to-weight ratio, and this is what they do in the aeronautics industry. This may seem very commonplace until one remembers the generations of scientists and engineers who have labored to find materials such as aluminum, titanium, and their alloys, to extract these materials from nature, and to process them in useful form. Lightweight materials did not drop like manna from heaven.

Getting ahead of myself somewhat, I shall apply this latter analogy to a human organization. The addition of mere numbers of people to an organization increases its ability to stand the demands of the environment linearly and its inertia (time constant) exponentially. Richard B. Kershner explored this phenomenon in his 1957 paper.⁴

The structure of an organization depends on the “strength-mass ratio” of the individuals that make it up. Each member must be chosen for his innate attributes – knowledge, skill, imagination, industry, etc. But that is not all. Individuals must grow by being assigned work that stretches their ability to the utmost and by appropriate educational opportunities to broaden their knowledge and sharpen their skill. The word “appropriate” covers so much that it would require a dissertation to bring out its implications.

The analogy of the missile system suggests some fundamental requirements for the control system (direction) of the much more complicated system, an organization of human beings, each individual of which is itself a complicated system. The ideal control mechanism takes timely and appropriate planned action based on reliable information from external and internal sources, including timely information from a feedback network, giving the results of the actions taken and what steps are necessary to reduce overshoot in the organization’s progress to its objective.

In an organization, as in a missile, timeliness, phasing, and planning are the key words, and decisions to take action in one direction must always be

accompanied by decisions to take counteraction at the proper time.

Even a superficial study of the implications of what I have just said will give the reader the feeling that the job of director of an organization is an impossible one. And this would be so except for the concept of hierarchical control exemplified in the human body. The functions of the various organs are first under a local control that keeps their actions within certain limits; for example, the baroreceptors in the major blood vessels keep the pressure of the blood within certain limits. When the local control system can no longer do its job, a regional control system, capable of controlling organs which cooperate closely with the affected organ, makes adjustments. Thus, stresses on the local control system are reduced. When these fail, the central nervous system, which has control of the whole system, is brought into play.

This concept is not a new one by any means; in fact, the management of any organization is built on a hierarchy of controls – top management, middle management, foremen, etc. Some are not successful at all. I suggest that factors which differentiate the successful from the unsuccessful are timing (phasing), anticipation, readiness, and excellence of performance.

STRATEGIC PLANNING

This discussion of planning started with contingency planning and really ends there. On two occasions the German General Staff prepared excellent and exhaustive strategic plans for the conquest of Europe and probably the world. On both occasions the enterprises based on these plans failed completely, largely due to the fact that they did not foresee, or perhaps ignored, possible contingencies, such as the capability of radar and the “Spitfire,” the use of modern techniques to “break” the code, the tenacity of the Russians in defending the motherland, and the intervention of the United States. Possibly the failure of the German rulers was due to an inflexible obsession with one means of achieving their grand objective.

Strategic planning for an R&D organization always has in mind one grand objective, namely, the survival of the organization as a first-class institution. Many ways lead to this objective, posing choices which must be explored imaginatively and made wisely. For in addition to the five constraints mentioned earlier in this paper, there are these questions: What has the future in store for the environment (world)? What resources can the organization mobilize to take advantage of the opportunities the future may bring? The crystal ball is not much help, but a careful study of trends may be. Here I suggest that we might classify appropriate trends into four categories.

1. *Inevitable*. For example, the application of microchips to new systems; radical improvements in naval warfare in air, on the sea, and

- under the sea; industrial and military applications of microorganisms and their cells, etc.
2. *Highly probable.* Evolution of new knowledge and techniques for exploring the oceans. Evolution of new knowledge and techniques for exploiting natural resources, particularly nonfuel resources (e.g., strategic materials). Naval use of space, particularly near-earth satellites.
 3. *Probable.* For example, novel sources of free energy for transportation and for other domestic and industrial uses.
 4. *Nebulous.* I cannot recall a good example of this category. This category of trends can be observed best, perhaps, in the literature and thinking coming from basic research laboratories and requires a very fertile imagination to perceive the significance of emerging ideas. However, I might suggest the emergence of new theoretical and experimental techniques of possible application in military technology or the development of a greater understanding of problems in human education and communication as fields of great interest.

Strategic planning for an R&D organization involves the timely matching of the organization's resources to environmental trends, and probably the most profitable part of strategic planning is the building up of resources, men, and material, to facilitate this timely matching to a variety of opportunities when they arrive.

In passing, one may note that the decision to link the fate of an organization with a field in one of the categories of environmental trends I have just mentioned is really a complicated and difficult one involving all the constraints I have already outlined. For example, to join an "inevitable" trend, the organization must already have experience and expertise (I might almost say unique experience and expertise) in the chosen field. The potential payoffs are already obvious to many and competition is ruthless.

As we go from category (1) to category (4) the possibility of a relatively small initial effort growing into something big increases. The organization may get a jump ahead of its competitors, and the potential payoff increases, as does the risk of tangible success. Constraints (1) and (2) loom large in these decisions.

Here I intended to discuss the impact of basic research on strategic planning, but find that the discussion takes so much space that I am putting it into the Addendum that begins on p. 53.

A professor at the Johns Hopkins Medical School whose judgement I respect greatly once said to me, "History has shown that the medical school and hospital have seldom been the first to enter a new field and never the last, but they have always taken on something new when they could do a better job than anyone else." This suggests one way of handling the timing problem.

No matter how far strategic planning may look into the future, it must always see the way from here to

there. An organization which is currently producing useful and wanted results in a given field usually has acquired a vast amount of experience and expertise in that and related areas. It has built up a "technical momentum" of considerable magnitude and fairly well-defined direction. To change even the direction of this technical momentum will require considerable force, and the stresses involved will be proportional to the rate of change of this momentum. Thus, when an acceptable long-range objective is chosen, the time and effort required to change the organization's technical momentum appropriately is a prime consideration. I am tempted to suggest that, other things being equal, the course to follow is that which requires the minimum rate of change of technical momentum.

In concluding this rather inadequate discussion of long-range strategic planning, I would again emphasize the contingency planning. Strategic plans may explore and find new desirable long- and short-range objectives, but if they do not contain thoughtful anticipation of future vicissitudes, both internal and external, foreseeable and unforeseeable (the better the planners, the fewer the latter), and prescribe possible courses of action to cope with these vicissitudes, they are hardly worth the paper on which they are written.

GROUP LEADERSHIP

So far I have linked my thoughts on planning with the direction of the whole system of human and material resources represented by an R&D organization, but I must emphasize that what I have said may be equally pertinent to the leaders of the subsystems of which the whole is generally composed, namely, departments, divisions, groups, etc. By consensus, the leader of a subsystem accepts (unreservedly, we hope) certain constraints such as the overall (strategic) objective of the organization, the specific area in which his responsibility for achieving this objective lies (for example, the development of an engine for a missile, an RF transmitter for a satellite, research in a given area of science), an upper limit to the resources available to him, and a time scale.

Within these constraints, the leader of the subsystem is free to formulate the tactical objectives he deems appropriate for his task and assign technical objectives to his subordinates. I think his plans should include plans for contingencies. To be successful he must be perceptive of external and internal trends and have available, at least in his thinking, courses of action to be taken in the event of the occurrences of vicissitudes these trends may portend.

In a fascinating essay on *The Art of Management*, Vannevar Bush selected Horatio Nelson as a superb example of a past master in the art of management. Nelson could inspire the loyalty of his men and their admiration for his consummate ability in formulating strategic plans of action to suit the occasion. Before an engagement, he explained his plan of action to his admirals and captains and then sent them off to implement these plans with full authority to

conduct the detailed operation of their ships as they deemed appropriate to the circumstances that arose in the course of battle. The analogy between group leaders in an organization and the captains of ships in Nelson's fleet is very suggestive and valuable in developing a good management policy.

SUMMARY

To sum up this admittedly rambling discourse, I repeat a few of the points I have tried to make about the functions of planning.

1. Since an R&D organization is an open system whose objectives are achieved by the cooperation of a number of individuals for whom extension in time is important, the overall function of planning is to safeguard the viability of the organization by realistic anticipation of future needs and opportunities and of the organization's potential to meet those needs or grasp the opportunities.
2. Intelligent planning is based on firsthand information about trends in the environment from which the organization derives or may derive its nourishment, its clients and their real needs, prevailing economic and social conditions, the technological environment, etc. Planning requires expert analysis of these trends to bring out (a) their consistency, (b) the significance and viability of the organization's current pre-occupations, (c) anticipation of troubles arising from environmental changes, (d) anticipation of new and desirable opportunities that may arise from environmental changes, and (e) examination of trends in technological and social areas that may enable the organization to create for itself opportunities for service and benign growth. These studies should result in a listing with appropriate detail of actions, one of which the director of the organization may implement promptly, if and when the necessity of a change in course arises.
3. Intelligent planning also involves perceptive observation and analysis of trends in the organization's own abilities to use its resources effectively and of the capacity of these resources to respond to external demands, to capitalize on new opportunities, or to create desirable new opportunities for service. I call particular attention to the time constant of the organization.
4. All planning contains so many partially known or understood elements that wherever possible its "theoretical" output should be verified by experiment. Three ways suggest themselves. Historical studies, if conducted perceptively, can tell about the fate of plans made for projects or programs that have been successful or unsuccessful. A study of the feedbacks in the system can provide information about how well plans are working. In general, particularly in the area of research, the progress of a well-planned investigation is characterized by positive feedback; the output augments the input. Finally, experiments may be devised with the cooperation of the hardware people to test the validity of critical elements in a set of plans. Incidentally, discussions between those responsible for building devices and those assessing their effectiveness in meeting the client's objective have refined both operations. Modelling and the use of computers are very popular for assessing the consequences of a planned course of action before it is put into effect. This technique, however, depends on the realism of the model and requires unprejudiced inputs from the planner, the implementors, and users; otherwise the results can be quite misleading.
5. Significant words (those responsible for the preparation and implementation of plans) that should always be kept in mind are anticipation, timing, phasing, feedback, response time, readiness, and preconsidered action.
6. The principle of complementarity suggests that the operation of any system requires the maintenance of balance between two fundamentally conflicting concepts important enough to be called requirements. In a system composed of human beings, these conflicting concepts are freedom of the individual on the one hand and centralized authority on the other. The proper maintenance of this balance depends on the environment and current tactical objectives and is a function of time. Applications of the principle of complementarity call for a change in management lore, imposing on the manager requirements for greater vigilance, flexibility, and contingency planning (anticipation) for a change of course. In particular, the manager of an innovative program should plan for success and be prepared to adjust the $\Delta F/\Delta R$ ratio as applied to his subordinates as the program moves from thought to specific action. Furthermore, the application of the principle of complementarity must be well understood and accepted by the individuals concerned, particularly in a heterogeneous organization, a very difficult thing to accomplish since people tend to look for across-the-board policies that are set in concrete like the Laws of the Medes and Persians. I have a suspicion that very successful managers have applied the principle of complementarity instinctively, but I have not thought the matter through. I must leave that to younger minds.
7. The function of a good helmsman is to keep the ship on its course to an agreed-upon destination and to avoid large oscillations in the course from environment forces, the ship's inertia, etc. The positive feedbacks from a successful

project can produce explosive growth. When to introduce the negative feedbacks that counteract this trend and dampen the oscillations that may prejudice survival is a function of the director, fortified by intelligent planning.

CONCLUDING REMARKS

Although useful for illustrating some points that I consider important in planning and directing the course of an organization of human beings, the guided missile analogy must not be pushed too far because it would lead to the conclusion that a strong, indeed an almost tyrannical, central control system is essential to the successful operation of any system. This, of course, is at variance with all I have said and arises from the fact that the missile has been designed (by human beings) to achieve a certain predefined and very specific objective and having done so, the missile's career ends abruptly.

An R&D organization has at least a dual objective: (a) *research*, the discovery of new knowledge and understanding, and (b) *development*, the reduction of new knowledge and understanding to useful devices or services. Both of these strategic objectives imply innovation, journeys taken into the unknown at some risk but with the hope of great profit. All innovations begin by the coincidence in one man's mind of two ideas, the realization of a problem or need and the concept of how this problem might be solved. This association leads to a new idea, which after gestation in the individual's mind, makes its public appearance, providing tactical objectives for the exploitation of groups of workers, perhaps to explore expeditiously the new knowledge lying in a specific field of nature, perhaps to develop new devices or services that promise greater usefulness to society as a whole. As these tactical objectives mature, they become more specific, desirable technical options are reduced, and the completion of a device or the prescription for a service becomes a matter of urgency requiring strong control, as in the missile.

In an R&D organization, there is present at any one time a spectrum of activities ranging from the individual conception of new ideas to the collective reduction of ideas to reliable practice. The problem is "how do you design and operate an organization to take into account this degree of heterogeneity?" In other words, how do you achieve an overall value of $\Delta F/\Delta R$ in such a way that local values of $\Delta F/\Delta R$ can be maintained at optimum levels? I don't know the answer. But, I am convinced that whoever finds it should (but probably will not) be regarded as a benefactor of the human race.

ADDENDUM: THE IMPACT OF BASIC RESEARCH ON STRATEGIC PLANNING

It may be appropriate to say a word or two about basic research in the context of strategic planning.

It is often said, particularly by people soliciting funds from poorly informed sources, that basic re-

search must be strongly supported in order that the depleted reservoir of scientific knowledge be kept filled up so that technological applications (of great value to the national economy) may be found and promoted.

This plausible statement is, however, so naive that it may almost be called misleading. Where is the reservoir? And is knowledge depleted by use?

Generally speaking, scientific research is the search for new, reliable knowledge and validated facts, and is the search for a mental model or structure that accommodates all validated facts in a consistent and aesthetic pattern often called a theory.

The establishment of one or two valid facts is a laborious process which may take a man a lifetime. The nugget of gold is hard to refine from the dirt in which it is found. Even then, until the new fact fits neatly into the theoretical pattern, both are suspect.

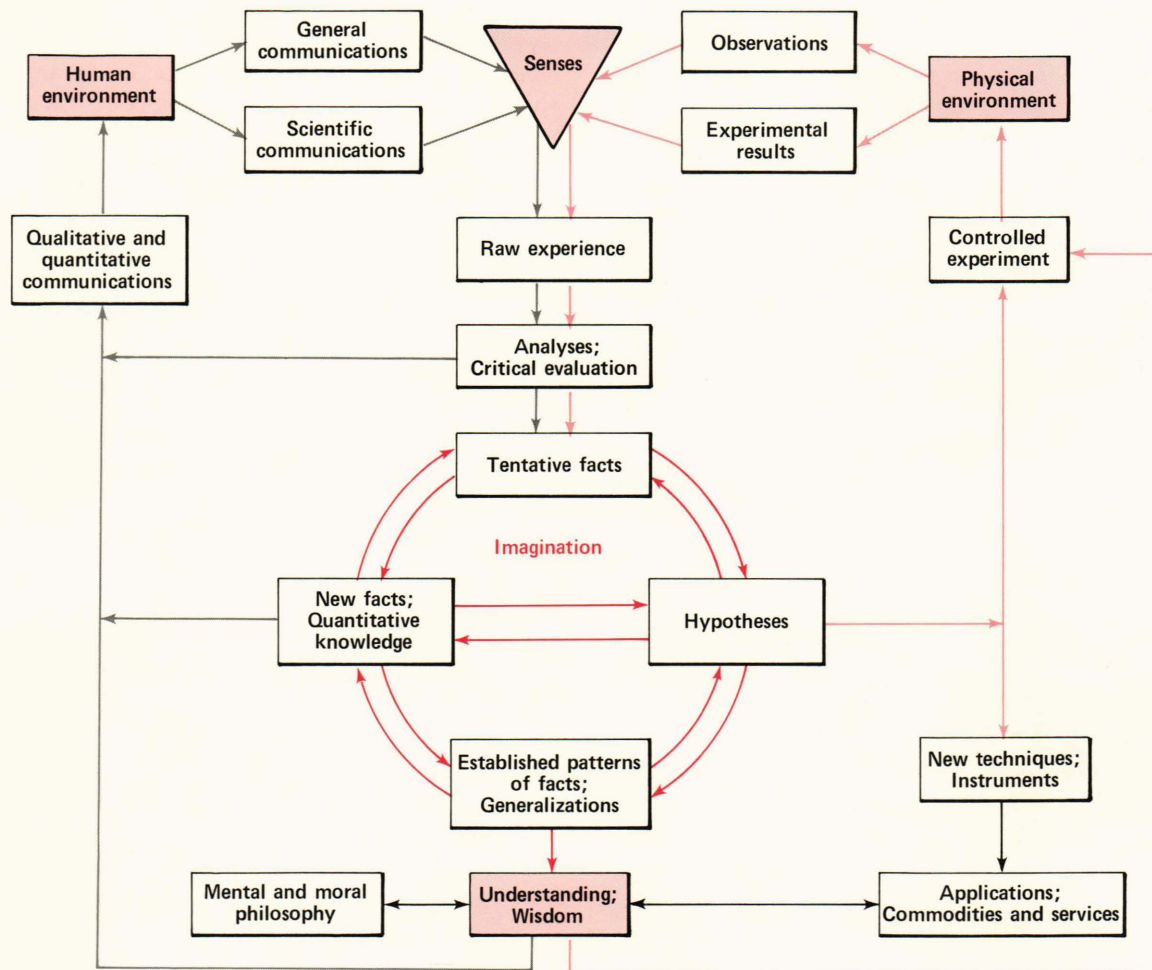
The established patterns called physics and chemistry now accommodate consistently millions of valid experimental facts. We have great confidence in the validity of both fact and theory, but this confidence cannot be infinite. A new fact can still overthrow the most powerful theory we have; a new and more powerful theory may change our perspective on the interpretation we may give to experimental facts. There is still more reliable knowledge to be gained than we can imagine, a thought that should keep us humble. Basic research is still full of surprises.

I think that the mainspring of basic research is human curiosity, curiosity in the mind of one person, be it a director, a group leader, or a student, and this leads me to suggest three types of basic research:

1. Research motivated by curiosity aroused by conflict.
2. Research motivated by curiosity catalyzed by opportunity.
3. Research motivated by pure curiosity fortified by sagacity.

Examples of each abound. Many people, including Galileo, Planck, Gibbs, Einstein, and Pasteur, to mention a few, have taken seriously major or even minor discrepancies between experience and established dogma and devoted their lives to attempting to resolve the conflicts so generated.

New instruments such as telescopes, microscopes, rockets, sensitive techniques of analysis, high voltage accelerators, etc. have opened up to exact observation vast areas of nature hitherto essentially closed to direct observation. The curiosity of men has led them to capitalize on these opportunities to gain reliable knowledge in hitherto inaccessible places. Pure curiosity fortified by sagacity implies the wonderment in an alert, reflective mind that encounters something brand new and strange and abhors mysteries, characteristic of many of the natural scientists, geologists, paleontologists, and anthropologists, and indeed, all pioneering explorers.



The systems of thought and action in scientific research.

Of course, these types of basic research are closely interrelated not only among themselves but with applied research, development, and finally engineering. I have attempted to show these interrelations in diagrams,⁵ the latest of which is shown here.

There are two characteristics common to all basic research in its purest form:

1. It is full of surprises. Indeed, if surprising and unexpected results do not come out of this activity, one may say it may add routinely valuable contributions to reliable knowledge, but it has not achieved its highest goal.
2. It is untameable. It cannot be “cabined, cribbed, confined;” a first-class investigator must follow his results wherever they lead, and not be subject to the goals set by administrators, arbiters of fund allocations, nor even by the dictates of peer review committees.

Strategic planning is based on predictions or forecasting, and in my article⁵ in the *California Management Review* I called attention to the fact that fore-

casting in the area of basic research is possible but risky.

To be sure, a study of research in progress, and especially of established lines of investigation, does enable us to predict in broad general terms what the state of knowledge in a particular field will be five years hence. At least we can set a *lower boundary* to what our knowledge is likely to be. However, Box A [basic research] is the region of discovery – the opening up of unexpected new fields of knowledge. In this sense its outputs are really unpredictable. Hence, an attempt to forecast the *upper boundaries* of the outputs of Box A is likely to be so hazardous that the wise man refrains from doing so.

In these days when the acquisition of new, reliable knowledge takes place at an ever increasing rate, one cannot rely on perusal of current journals or other publications as a means of keeping up to date with advances in scientific knowledge. Discoveries may be a year old, at least, before they appear in print. There have grown up a number of what has been called “in-

visible colleges,” meetings or symposia attended by a small number of experts in a specialized field who communicate to each other their latest findings, which may or may not be completely validated, but which are always thought provoking. Economic necessity generally limits the circulation of the proceedings of these meetings to preprints sent to those in attendance or interested friends of the authors.

I submit that even to have available to it information about the possible boundaries of knowledge five years hence on which to base tentative plans, an R&D organization should have access to one or more of the “invisible colleges,” which means that some members of its staff should be able to make contributions to the agenda of the “college,” the price of admission. In their heyday, General Electric, Bell Laboratories, and du Pont, to mention only a few, followed this practice very profitably.

Basic research, or as it used to be called, “pure scientific research,” lies at the intellectual end of a spectrum of activities called technology. It is an art, and the excellence of the product depends on the creativity and the industry of the practitioners. They become fascinated with a subject outside themselves, but leave on their product the stamp of their individuality. The well-known dictum of that pragmatic man of the world, Francis Bacon, “Knowledge and power meet in one,” represents a consequence of the work of pure researchers, but must not be mistaken

for the motive that inspires their work. Without the poets, the painters, the musicians, the writers, the philosophers, and others who enrich us with intangible gifts, the life of the genus *Homo sapiens* would lose its savor. We may add to this list the intangible contributions stemming from the curiosity of the pure scientist.

REFERENCES and NOTES

- ¹The adjective “reliable” is most important, since the minds of most of us are constantly supplied with “knowledge” whose reliability as a guide to future action is, to say the least, open to serious question. This is not the place to embark on a discussion of what is implied by the term “reliable knowledge.” The reader who asks for more will find it in the book, *Reliable Knowledge*, by John Ziman, Cambridge University Press (1978).
- ²I use the term “director” to mean an individual who has the responsibility for the decisions affecting the course and the welfare of the organization. The analogy with the captain of a ship is almost perfect. The term “president” or “general manager” etc. might be used. Which one is unimportant, but concentration of responsibility in one person is. Management by committee has been proven ineffective. The well-educated and well-stocked human mind exceeds any computer in its ability to assess and put together information and generate action. The rapidity, clarity, and synthesis of communications within a single brain give it a capacity no committee can approach.
- ³From a paper by P. H. Abelson published in the *Johns Hopkins APL Tech. Dig.* 3, 214-217 (1982)
- ⁴R. B. Kershner, “The Size of Research and Engineering Teams,” in *Proc., 11th National Conference on the Administration of Research*, 77-88 (Sep 1957).
- ⁵R. E. Gibson, “The Strategy of Corporate Research and Development,” *Calif. Manage. Rev.* 9, 33-42 (1966); R. E. Gibson, “A Systems Approach to the Management of Research and Development,” *Johns Hopkins APL Tech. Dig.* 1, 252-263 (1980).