PROJECT RAND

MILITARY RESEARCH AND DEVELOPMENT POLICIES

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PERSPECTIVE

During the past several years, RAND has been conducting a study of military research and development in an effort to discover what kinds of policies are most effective in providing the military services with new weapons. This report summarizes the findings of that study.

Our conclusions are based on an examination of past programs. We have looked in detail at the development histories of many of the postwar fighters and bombers, most of the postwar engines, and all the postwar bombing-navigation systems. Somewhat less detailed attention has been directed at a number of other programs, some involving development of operational hardware and some aimed at advancing basic technology. Most of the cases we have examined were taken from the history of research and development in the Air Force, as our choice of references and illustrations, and the structure of some of our arguments, will make clear. Our general investigation of the field, however, suggests that our findings are relevant to all military R&D.

Section I of this report discusses what we have found out about the general nature of military R&D. Section II indicates the shortcomings of some present development policies and suggests a more fruitful approach to weapon-system development. Section III deals with the question of providing technological building blocks for future weapons.

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MILITARY RESEARCH AND DEVELOPMENT POLICIES

I. The Nature and Function of R&D

R&D IS AN UNCERTAIN BUSINESS

Research and development is characterized by uncertainty or unpredictability at every stage. Given such radical advances as are being sought nowadays, there are bound to be many changes in early ideas about the best configuration, the best compromises among military characteristics, and the usefulness of any particular system. There will be changes because it is impossible to predict at the start of a program what major problems will arise during development. In almost every system development that we have examined, early estimates of expected performance, cost, and development time have turned out to be optimistic by varying factors, sometimes as great as three or five. It is such evidence that drives home the great uncertainty characterizing every development program.

R&D BUYS KNOWLEDGE

Research and development is a way of progressively reducing uncertainty by buying information. Its purpose is to obtain knowledge. If answers to all the problems of developing new weapon systems were known in advance, there would be no need for R&D; procurement and production could be started the moment there was a requirement for a new capability.

It is clear, however, that merely having a requirement is not enough. Development decisions depend not only on what is wanted but also on what it is possible to get. That is where R&D comes in: it tells military planners what they can get, i.e., what weapons and capabilities they can buy with limited funds. R&D is neither a substitute for production nor a method of procurement; it is rather a *search*, a process of discovery.

Money spent on R&D is not intended to buy airplanes or missiles; it buys *knowledge.*

THE NEED FOR MANY KINDS OF KNOWLEDGE

That the purpose of R&D is to buy information is true throughout its spectrum, from the most abstract theoretical work to the final equipment tests in an operational command before a new weapon is phased into the force.

Basic, or "non-objective," research (the kind of work done by an Einstein or a Fermi) investigates fundamental physical principles.

Applied research, or exploratory development (the kind of work done by an Edison or by the Bell Telephone Laboratories), is concerned with experimental application of the findings of basic research. It seeks to determine whether the conclusions of theoretical calculations, or the devices constructed in the laboratory, can be translated into a workable piece of machinery—hopefully a new tube, a new fuel, or a new airfoil.

Technical, or component, development (the kind of work done by the M.I.T. Instrumentation Laboratory or by the experimental shop at, say, Pratt & Whitney, or Lockheed) seeks to find out whether the demonstrated models of exploratory development can be modified and combined into major equipments of immediate or potential military usefulness. It tries to build a new engine, a new guidance system, or a new airframe of radical design, and to test these in as realistic an environment as can be constructed.

Finally, at the upper end of the R&D spectrum, there is the full weapon-system development program. *System development also is primarily a means of obtaining knowledge.* Clearly, the knowledge bought here is quite different from the kind of knowledge obtained in basic research or in technical development. System development seeks information about how to put subsystems together to form a reliable, smoothly functioning whole; about how much a system will cost; about how it will perform in an operational environment; and about the best ways of preparing to produce it. System-development programs, in short, do *not* yield airplanes or missiles that are ready to go into the force; production does that. System-development programs, like all R&D, produce *knowledge.*

R&D IS A STEP-BY-STEP ACTIVITY

Since the purpose of R&D is to obtain knowledge, it is most efficiently conducted *sequentially*, because the problems of any given stage in the spectrum are not fully apparent until the previous stage has yielded its test information. This is not to deny that several different lines of inquiry can and usually must be pursued concurrently. But in any *given* investigation, the nature of the learning process dictates the order in which problems should be taken up. It is difficult to construct a successful full-scale prototype of an engine or bombing-navigation system until breadboard demonstrator models have first shown how the major technical problems are solved. Similarly, to undertake production of a complete weapon system before critical component tests have yielded knowledge of the inherent performance and reliability of its chief subsystems is to run a serious risk of failure.

Any attempt to schedule an entire R&D program at one time is likely to lead to inefficiency, either because plans for the later stages may have to be scrapped and remade on the basis of information yielded by early tests, or because, in pursuing premature plans, a development program may fail to profit from new information gained along the way. Either case will cause delays, or raise costs, or both.

Planning for research and development, in short, is very much like planning for war: it is a matter of putting first things first. In our war planning we have, and should have, a fairly clear idea of what will be done immediately after hostilities begin. We do not have, and cannot have, a detailed operational and logistics plan for fighting the war month by month up to the time of the victory celebration. If we did, we would probably lose the war, because we might not have the flexibility to adapt to a new situation, either in time or at all.

The sequential character of R&D requires (as will be seen below) that development *decisions* also be made sequentially, so that each will profit from new knowledge gained since the last decision. It is clear, however, that the need to expedite development also means that a decision should be made quickly after the necessary knowledge is at hand. Vacillation at that point may result in constant and unjustified postponement of decisions at the risk of never getting new systems into development at all.

THE FIVE RULES OF A GOOD DEVELOPMENT POLICY

The essential character of R&D as described above has some clear policy implications. A good development policy will be based squarely on recognition of R&D as a sequential, knowledge-getting activity. It will therefore be designed to obtain information as quickly and as cheaply as possible, and it will provide opportunities during the course of development for maximum exploitation of the knowledge obtained. Adherence to the following five rules would constitute such a policy.

1. A good development policy will seek to give contractors a clear idea of the role that a system is expected to play *without specifying in detail exactly how the system must look and perform.* The best configuration and capabilities of a new system are not known until the early phases of development have provided reliable test information about the probable performance and cost of any given design. Attempts to anticipate such findings by fixing a final system configuration in advance, therefore, force development to proceed along a predetermined course, thus making it difficult to take full advantage of what may be learned later. (As will be explained below, a move away from rigid, detailed requirements will strengthen rather than weaken service control over contractor behavior.)

2. A good development policy will frequently have *two or more alternatives under development early in a program*, and will call for a decision about which major components will be integrated into the final system *only after initial test data have provided information about the relative merits of the alternatives.* At the start of development there are usually several different approaches to the solution of any given technical problem, and no one can know with certainty which will prove most successful until test results provide a basis for evaluation. (Evidence presented below indicates that planned duplication of this kind early in a program is economically sound.)

3. A good development policy will make only modest financial commitments to a specific configuration *until there are test results available to provide a sound basis for determining the potential usefulness of that configuration.* Otherwise there is a great risk in backing heavily a design that may not test out successfully. (An implication of this rule is that

expensive tooling and production facilities should not be bought until the completion of initial tests. It will be further suggested below that this need not delay a program, and may in fact—along with the other rules—*save* time.)

4. The fourth rule concerns administration. A good development policy will ensure that *those in technical charge of a program have the authority to take quick advantage of new information gained during development.* Such a policy will guard against the tendency for technical decisions to be made at the upper echelons of the military services, or higher. Technically trained contractor and service personnel who are in close touch with a program are in the best position to translate new knowledge into concrete plans for the next stage of development.

5. A good development policy will require that equipment be brought to a test *as early as possible at each stage of a development program.* Tests—be they tests of a transistor, a full-scale guidance system, or a complete missile—are the only fully reliable source of information about the technical aspects of a program. System studies, preliminary engineering designs, and scientific predictions, though essential in every development program, are not adequate substitutes for test data. Hence the emphasis on early tests.

Current military R&D policies are often not well calculated to derive the maximum payoff from the resources devoted to research and development, because they do not reflect sufficient recognition of R&D as a means of buying information. That is not to say that most of the officers in development establishments do not appreciate the uncertain character of their business. We know that they do. The difficulty is rather that the actual policies pursued are often not designed to take account of all the *implications* of that uncertainty. We believe that reorientation of policy in the direction indicated by the five rules given above would yield better weapons more quickly for given resources than do present policies. Two points need to be made in this connection.

First, we cannot say in advance *exactly how far* the services should move in the new direction, because a considered evaluation must depend in part on the results achieved after the shift in emphasis has had a

chance to be effective. We are quite confident, however, that the *direction* indicated is the right one, and that the move should be a fairly major one.

Second, we recognize that higher agencies in the Government play a considerable role in determining military R&D policies, and that they are therefore in considerable part responsible for the shortcomings of those policies. Nevertheless, we believe that the services can do a great deal on their own to improve the situation. For example, although we do not consider such questions specifically in this report, we think that changes are indicated both in the organization and personnel assignment policies of the R&D establishment.

II. Weapon-system Development

ON EVALUATING THE EVIDENCE

The history of every weapon-system development exhibits features all its own. It is therefore difficult to make precise comparisons among different programs, and unqualified statements can be misleading. No one, for example, can say with confidence that a particular development was a total failure or a complete success. Issues are rarely that clear cut.

Yet we have found that most of the programs we have studied can be roughly but meaningfully characterized as more successful or less successful *on the whole*. The more successful programs have generally produced systems that cost less, took less time to develop, and performed better than comparable systems resulting from the less successful programs. Though far from rigorous, such comparisons can be useful, especially as they are given concrete meaning in the following discussion.

Most of the successful programs we have studied were conducted in general accord with the development approach recommended in Section I, above. The less successful ones, by contrast, followed procedures now current that tend to deviate from that approach. Such evidence supports our conclusion that the policies and procedures governing development can have a considerable bearing on the probability of success of any given program.

THE MAJOR THEMES OF CURRENT POLICY

Current R&D policies attach great importance to (1) developing subsystems that are *specialized* to a particular weapon system, i.e., that are matched or mated to each other in the *earliest*, preliminary-design stage of development; (2) tooling up for quantity production *concurrently* with the development and construction of prototype test articles; and (3) ordering *large quantities* of test vehicles from the very start of a program.

These procedures were introduced in the dual hope of avoiding the necessity for such extensive postdevelopment modifications as had characterized many programs immediately after World War II, and of minimizing the time needed to bring new weapon systems from initial concept to operational use.

It was argued that if subsystems were designed to match from the start of development, weapon systems would not only be inherently better (because all the components would be new and the final systems would therefore be the most advanced possible), but they would also be operational from the time of their first appearance in the force, because problems of integration and reliability would have been solved in the process of development.

Similarly, it was held that a production buildup on the basis of preliminary system design would save the tooling-up time traditionally intervening between development and procurement, and that availability of several test vehicles would save testing time by making it possible to conduct many tests concurrently instead of consecutively as had been the practice in the past.

Most of the cases where this approach was used, however, illustrate several undesirable consequences of such practices. The frequently haphazard character of some of the developments undertaken immediately after the war no doubt justified alerting development agencies to the importance of giving timely attention to initiation of subsystem programs. Also at a time when technology advanced perhaps less rapidly than it does today, early tooling and many test vehicles may have been worth trying in the hope of saving time. But these new procedures have often created more problems than they have solved, because they have failed, in application, to take sufficient account of the uncertainty that pervades research and development.

THE EMPHASIS ON EARLY INTEGRATION OF SPECIALIZED COMPONENTS

There is nothing about the nature of weapon-system development that *requires* a great emphasis on early integration. One way to seek a new missile capability, for example, is by developing a highly integrated system

with very specialized components. Another way is to develop a much less specialized system that relies on better inherent performance in the individual subsystems. In the first case, major attention would be devoted to making subsystems work well with each other. In the second case, more effort would go into improving the individual performance of the various components. Specialization, in other words, is only one way to get a new capability, so that the more money spent on achieving specialization, the less will be available to buy advancement in other forms. It is therefore important to determine the costs of specialization and the value of the benefits it yields.

The stress on early integration makes development very expensive. Among the programs we have studied, those that began with a highly organized plan requiring development of integrated subsystems have generally cost much more than comparable programs in which integration was not a primary objective at the start. The reason is not far to seek.

In an age that demands the ultimate in weapons, the improvement in performance required of every new engine, guidance system, or airframe taxes the ingenuity of the best scientific and engineering talents that industry can provide. It is difficult for them to predict whether development of a new device will succeed at all, let alone how the equipment will look and perform, how much it will cost, and when it will be ready.

The effect of early insistence on integration is to require that time, cost, and performance predictions be made *before* even preliminary test data are available on which to base a prediction. In a highly integrated development program, the weight and dimensions of electronic equipment are usually spelled out to the last pound and fraction of an inch. Power supply and cooling capacity are determined in advance and allocated among the various components. Size and shape of missiles are tailored to the missile bays that will carry them. Such added constraints can greatly increase the difficulty of development, for the developer is now required to come up not only with a subsystem of spectacular performance, but also with one that will achieve the performance in a certain configuration, at a certain cost, and by a given time. This means that equipment must remain in development much longer than would otherwise be necessary before it can be brought to a test, and that the task of modifying it in accordance with the test results will become more difficult than need be because of the greater complexity imposed on it by the extra requirements.

The effect is to multiply the difficulty of a job, with the result that something usually has to give—the cost, the time, the performance, or all three.

In addition, overspecification means that the subsystems that *are* successfully developed will be so highly specialized for their intended use that it will be difficult, expensive, and time consuming to adapt them to *other* uses if the system originally planned is not procured for one reason or another. Specialization, in other words, limits the *versatility* of major components.

Despite the high cost of specialization, its net payoff is usually small. The many examples of engines, airframes, or missiles that ended up in applications quite different from those envisioned for them when they were first put into development suggest that the value of a subsystem depends much more on its actual performance than on whether it was originally intended for a particular use. The big payoff in system development, in other words, lies not in emphasizing subsystem integration but in developing subsystems with better inherent performance and reliability. It would therefore seem more efficient to save on the high cost of early integration and spend the money instead on more independent development of advanced subsystems.

A word of caution is in order at this point. We do *not* argue that new subsystems must be completely developed and exhaustively tested before any attention is given to combining them into a reliable weapon system. That is the road to unending research and experimentation with little hope of ever getting advanced weapons into the force. The lesson of the early postwar years is clear: there must always be some over-all control of subsystem programs on pain of returning to the practice of completely undisciplined component development with only a vague hope that everything will automatically fit together in the end.

But this does *not* mean that integration of subsystems into a complete weapon can be done most profitably at the preliminary-design stage of development. The time to start matching subsystems to each other is after preliminary models have gone through first tests. The advantages are twofold: the components to be integrated (although not yet in their final

form) will have proved their *inherent* capability, and there will be actual models available that can be modified to match each other. Engineers are notoriously more successful when they can tinker with pieces of machinery than when they are asked to make all their decisions at the drawing table before there are any test data on which to base them.

ON PRODUCTION TOOLING AND THE PROPER NUMBER OF TEST VEHICLES

Analogous reasoning applies to the questions of when to tool up for production and how many test vehicles to order at the start of development.

Tooling up for high rates of production can be done with minimum risk only when one knows exactly what will be produced. Such knowledge is not available before a new piece of advanced equipment has been tested. Making large investments in tooling before equipment is brought at least to a first test, therefore, involves a considerable gamble. The evidence we have examined suggests that it is not a good gamble because winnings are rare and small and losses are frequent and large.

Unlike the time of World War II, when huge quantities of aircraft, ships, and other weapons were needed and tooling capacity was meager, tooling per se no longer poses a major problem. Aside from an occasional long-lead-time item (which of course *should* be anticipated) and the need to *plan* the tools and production floor for full-scale manufacture (which of course *should* be done early, since the cost is small relative to that of full tooling), it takes very little if any more time to acquire the actual tools than it does to work up the labor teams and get deliveries of the raw materials necessary to go into production. Even in the lucky case of the system that requires no substantial modification after first test, therefore, the potential time saving from early *full-scale* tooling is likely to be relatively small—probably about six months, in the case of aircraft.

But the lucky cases are rare. Normally, because of the inherent uncertainty of development, initial flight tests disclose a need for modifications sufficient to upset any previously set up tooling line. The disadvantages that result are of two kinds. First, since it takes longer to

produce a test vehicle on production tooling than in the experimental shop, the first test results become available later than they might, with a consequent loss of over-all development time. Second, there is the actual loss resulting from the need to discard already-purchased tools in order to make the production-line changes necessitated by the test results. As a consequence, the *cost* of the program goes up.*

As suggested above in passing, an efficient tooling policy would not emphasize full-scale tooling until test results had provided a reliable basis for estimating eventual tooling needs. But it would call for early *planning and design* of the expected tools and production layout, and would authorize immediate *R&D on tools* that might be needed for production. That kind of anticipation pays off, because tool planning and R&D are the main lead-time items in production buildup, and because they are, at the same time, its least expensive phases.

Like premature tooling, the present tendency to order large numbers of test vehicles right from the start of a program involves gambling much to gain relatively little.

There is no question that a thorough large-scale test program involving many vehicles must be carried out at some point before a new system becomes operational. But first-test time is not that point. The purposes of early testing are to discover what over-all performance can be expected from a system, and to expose major deficiencies. These are the kinds of things that are important to know *before* too much is committed to a program. If a large inventory of vehicles is under construction before such information is available, modifying them in accordance with the major lessons learned from early test can be both expensive and time consuming. This tends to make for a good deal of expensive scrap if initial test results are so disappointing as to dictate abandonment of a program.

*The argument is sometimes advanced that production tooling is needed from the start simply because modern weapons are so complex, tolerances so exacting, and materials so tough that no other tooling will do. Of course, the day has passed when prototype airplanes, for example, could be built essentially "by hand." But there is still a significant difference between the nature and cost of the tooling needed to build three or a half-dozen aircraft for initial test and that required for a production rate of three or a half dozen *per day*.

PRESENT SHORTCOMINGS AND A SUGGESTED REORIENTATION OF R&D POLICIES

Undue emphasis on early matching and specialization of components, early large-scale tooling, and production of large numbers of initial test vehicles reflect inadequate recognition of the inherent uncertainty of research and development. Such overemphasis makes it difficult to observe the five rules of good development practice listed in Section I of this report.

We believe that the emphasis of R&D policies, therefore, should be shifted from the themes now stressed, to the guidance inherent in those five general principles.

1. The Planning Process Needs To Be Simplified

Spelling out all the details of a weapon system at the preliminary-design stage of development contributes to the current tendency to prolong the planning phase of a program. But the consequence of spending two or three years in an attempt to pin down all the details of a program before development gets under way is that the planning itself can become obsolete before it is put into effect. More serious is the direct time loss. Since the course of development is in fact always determined by what is learned *during* a program, much of the two or three years of elaborate planning often turns out to be wasted time.

It is sometimes argued that specific and ambitious requirements are necessary for maintaining control over industrial contractors. That might be so if the military usefulness of a new weapon or component were a function of only one or a few performance parameters. If accuracy alone were important in a bombing-navigation system, or if speed alone mattered in a bomber, then a requirement might profitably ask for a specific degree of accuracy or for a specific minimum speed.

But the military utility of a weapon system is more complicated than that, because neither accuracy nor speed will be very useful if the eventual system is unreliable, for example. The proper objective of development is a sensible compromise or trade *among many interdependent and often conflicting performance characteristics*. The effect of specifying rigid requirements for each factor, therefore, is not only that the technical

problems are multiplied (as we have seen), but also that the developing agency is constrained from making the trade-offs among performance characteristics indicated as desirable by the knowledge gained during a program.

How, then, *should* a requirement be written? We do not pretend to have a satisfactory answer to this very difficult question. It may be, indeed, that there *is* no satisfactory answer as far as a *written* requirement is concerned, because the knowledge on which a complete written requirement must be based does not begin to become available until *after* a program is under way. This suggests that a well-planned R&D program would begin with careful analyses to define, *in broad terms*, the kinds of systems that seem most promising, and to ensure that no important considerations are neglected. But detailed decisions about specific performance parameters would be suspended until development and tests provide an adequate basis for making them.

Such a procedure could work, of course, only if there were adequate provision for *sequential* decisionmaking—which brings us to the second general policy rule.

2. There Should Be More Authority in the Project Offices

Precisely because of the current emphasis on advance planning of all the technical details of a system, the project offices in direct charge of developing systems have suffered a serious diminution of authority and control over development programs. The planning function has tended increasingly to gravitate to service headquarters in the Pentagon, and even higher. Yet most of the detailed technical knowledge about any particular program is concentrated in the project offices. That is where the contractor's engineers can exchange ideas and plans with the technical representatives of the services. The effect of shifting the locus of decision from the project offices to higher levels, therefore, is to bypass the technically-best-informed group.

The result is that with the best will in the world, many detailed decisions made are more or less uninformed. When these decisions are then given official status by headquarters, which cannot be changed without appeal to the high-level originating office, the effect is to tie the hands

of the project office and thus lose the advantage of its more detailed knowledge of the program.

Of course, the headquarters groups are most knowledgeable about general policy, strategic planning, and the budget situation at any particular time. Their function in R&D, therefore, should be (1) to determine in general terms what kinds of weapon systems are likely to be most useful in future time periods, (2) to secure funds and allocate them among broad projects, and (3) to participate in major policy planning among the various commands and with their sister services.

They should receive periodic technical and financial reports about how various programs are proceeding, but they should not be concerned with detailed expenditures or insist on reviewing and approving in advance detailed changes in requirements or specifications that are made necessary by what is learned during a program.

Conversely, the higher echelons of the R&D establishment should keep project officers fully informed about the thinking at headquarters on matters of strategy and over-all budget, so that those in charge of a development program can do their detailed planning in as realistic an environment as possible.

These considerations amount to the fact that the project offices should be given much broader authority than they now tend to have, both in connection with technical decisions and on such questions as the conduct of test programs, detailed budgeting, and contracting. Running a project office for a new weapon system is one of the most important jobs in the military services. There are many able and experienced officers in R&D, and staffing the project offices should have first claim on that talent; but talented people will stay in the project offices only if the job is given a status commensurate with its responsibility.

3. Alternative Approaches to Difficult Problems Should Be Encouraged

One of the most serious consequences of a strong emphasis on specialization is that it severely limits the number of alternative subsystems that can be put through at least the first phases of development. That is true for two reasons. First, it is pointless to carry on parallel early developments of, say, a new bombing-navigation system or engine, if both

programs must aim at producing, to the last detail, a design that has been specialized for a particular new application. Duplication of that kind provides little protection against technical uncertainty, because failure of one such development will probably mean failure of both for the *same* reasons. Alternative approaches to the solution of difficult technical problems are most effective when *different* solutions are tried, thus increasing the probability that at least one will succeed, because they are then vulnerable to different uncertainties.

Second, overspecialization, as we have seen, raises the cost of development to the point that it may be impossible, with the resources at hand, to afford more than one program.

But there are cheaper ways to buy information than by trying to decide in advance on all the characteristics of a "final" system (which rarely turns out to be final in fact) and proceeding directly to build an operational version. For example, in order to find out whether an engine of advanced design has a reasonable chance of being satisfactory, it is not necessary to commit 150 million dollars for a full-fledged program and wait three or four years until it comes to a 150-hour test. Development to the 50-hour-test stage, in much less time and at much less than a full program, will permit more reliable estimates of such crucial characteristics as weight, durability, and cost than are possible before any test information is available at all.

Similarly, to decide whether or how a new guidance system can be made tolerably reliable, a preliminary version can be tested in one or two years, and at much less than the expense required for an elaborate five-year program aimed at getting a seemingly complete model ready from the start. Two or a half-dozen prototypes are usually adequate to test the gross performance characteristics and major deficiencies of a new design.

In other words, it becomes economically possible to follow a multiple approach in the early stages of a program if the alternatives under concurrent development are prototypes of the crucial components instead of duplicates of entire system programs. One value of a multiple approach is that it is a highly efficient method of buying time. If a preferred subsystem does not test out as hoped, it may be possible to switch to a more successful alternative under concurrent development rather than

having to hold up the whole system program in order to start developing some part of it all over again. The added cost of carrying two—or maybe three—prototypes through early test is usually justified by the improved ability to select a system with the best possible combination of performance, development time, and cost.

It is impossible to establish any general rules about the number of projects that should be run in parallel, and we certainly do not argue that there should be alternatives under development for every single component of a new system. Such decisions will depend on the nature of the problems encountered in each case, and on the probable cost of carrying parallel projects to the point where they yield enough information to improve the basis for choice. If the development problems are difficult, and there are several possible approaches to their solution, and if it is possible to bring alternatives to an early test, then contracts probably should be let for several different prototypes—perhaps three or more. But if the problems are relatively easy, and the different approaches limited and costly, then the number of competing alternatives should be fewer—perhaps two or one—or even, in some cases, none at all.

4. Financial Commitments to a Single Design Should Be Kept Modest in the Early Stages of Development

It follows without need of further argument that purchase of elaborate tooling and large numbers of test vehicles in advance of preliminary test results represents a large financial commitment to a development program at a time when its future course is most uncertain.

Emphasis on early integration also increases the early commitment to a program, not only because, as already mentioned, it makes development of advanced subsystems more difficult, but for two other reasons.

First, the administrative costs alone become very great. The sheer magnitude of the task of organizing, monitoring, and co-ordinating all the aspects of a highly specialized major system from conception to operational availability is staggering. Complicated reporting systems must be devised and made to function. Each subsystem program must be carefully monitored to make certain that no unilateral action is taken that upsets other subsystem programs. Changes in plans for one part of a system must be co-ordinated with the agencies or contractors charged with developing

complementary systems that will necessarily be affected. When hundreds of contractors and subcontractors and thousands of individuals become involved in a program, it is easy to see that administration of a completely integrated operation can become very costly indeed.

Second, as noted earlier, premature matching of subsystems raises the cost of a program by requiring delays or modifications in several or all of the programs when obstacles are encountered in any one. If the volume that, say, the fire-control system will occupy has been specified in advance to the last cubic inch, and if it is discovered during development that the desired capability can be achieved only by a change in configuration that must increase the volume, then the over-all design must be changed, the air-conditioning plan may have to be modified to yield more cooling capacity, and so on down the line of the rest of the optimally matched components. It is expensive enough to modify one subsystem along the way; it could well be ten times as expensive to change ten subsystems when only one ran into trouble.

Besides the simple risk of losing large sums of money, large premature commitments to a single preferred design have the added disadvantage of making weapon-system programs so expensive that the services can no longer afford to explore alternative roads to a desired capability. The differences in cost are usually expressible by factors of three to five. A 300-million-dollar prototype program can become a billion-dollar affair when the costs of full-scale early tooling and thirteen or fifteen initial test vehicles are added to the extra costs generated by a decision in favor of early integration.

Yet, paradoxically, such behavior is sometimes defended on the grounds of economy. It is argued that going quickly and directly to an operational capability is the cheapest way to develop systems, because the costs of alternative developments are thereby saved.

But that kind of economy can easily boomerang, because the program may fail, leaving no alternative, or it may be salvageable only with reduced performance or at inordinately increased cost. Two design proposals may appear to involve equal expense, but one may turn out in fact to be twice as costly as the other, not only in development but in unit cost of the finished article. Suppose, then, that by happy accident, the choice is in favor of all-out backing of the cheaper of the two. Perhaps six

months of time and 30 million dollars will be gained as a result of early tooling and cancellation of alternatives. But if, by unlucky accident, the choice falls to the wrong system (and we are assuming that either one will be developed successfully), five or fifty times the amount saved may now be required if large operational quantities of the more expensive weapon are procured.

5. Quick Tests of All New Equipment Should Be Insisted Upon

The advantage of getting systems and subsystems into test as early as possible after the start of development—required by the fifth rule of a good development policy—is that it is made easier thereby to observe the first four rules. Insistence on early tests ought to be regarded as a cardinal principle in the conduct of research and development.

Early tests provide a reliable and inexpensive means of determining what the military are likely to get for the money they spend on development. They can reasonably ascertain whether a contractor is going to come up with a satisfactory system before vast sums have been committed to a program—more reasonably than on the basis of a design study. This means, in turn, that the services can effectively control the results of the program and ensure that their money is well spent at every step of the way. By contrast, imposing a very ambitious and detailed requirement on the contractor, and telling him in effect that there is no interest in anything but the "final thing" is the opposite of effective control. Tests are thereby delayed (until every last detail of the requirement has been taken care of), and long periods elapse and large sums are spent without test data to indicate what kind of job a contractor is doing.

Getting equipment into test before detailed requirements and specifications are written also makes it easier to pursue alternative approaches to difficult problems. When the desired system is not specified in rigid detail, multiple alternative subsystems can be carried into the initial phases of development, thus providing some insurance that a program will have something to fall back on if a preferred design fails to work.

The multiple approach during the early stages of a program is desirable, for the reasons given, even if all the alternatives are investigated by a single contractor. The advantages of competition can be *added* to those

of parallel development, however, if *more* than one contractor is used. Running two competitive full weapon-system programs concurrently to meet the same requirement is in many cases so expensive that it is out of the question—as many people in and out of the services have pointed out. But, as we have seen, developing only *one* rigidly defined system is not the answer, because it deprives a program of fresh solutions to difficult technical problems. The best course is to put more than one manufacturer to work on the components whose expected performance is most uncertain, and insist on early test demonstration of their respective achievements.

In so insisting, the services will in effect be telling contractors the following: "If you do not deliver a preliminary version of this new system for test within two and one-half years, we do not know whether we will be able to finance the project any further. We do not expect more than a rough approximation of a final system in that short a period, but whether or not development is carried forward will depend on what you come up with at that time."

In other words, instead of fixing the detailed configuration of a final system in advance and then paying and waiting, and paying and waiting some more, until a single contractor (at his leisure if he wishes) does the best he can (or wishes to do) with the requirement, the services should tell two or three likely contractors what general improvements in capability they want and then *fix the date* for a preliminary test demonstration. The most enterprising companies will see to it that they have something worth showing by the required date, because that will be the only way they can expect to get a production contract. Under present practices, companies are encouraged to exaggerate their claims so that they can tie the military down to expensive programs before either has the slightest idea of what kind of system will finally be procured—and when, and at what cost.

THE MORE SUCCESSFUL PROGRAMS

What we have sought to do in this part of the report is discuss some of the undesirable consequences of policies that sometimes guide the conduct of research and development in the military services. This has inevitably required undue concentration on unfortunate experiences. We

should not therefore be understood to be saying that there have not been some highly successful programs in military development history: there have been many, and we have examined a number of them with care.

We have found in general that the conduct of an overwhelming proportion of the successful programs reflected the approach that we have recommended. They tended to observe one or another of the principles elaborated above, even though none, to be sure, applied the entire five-point strategy in any formal or consistent way.

The methods used in these programs were sometimes no more sophisticated than the "cut-and-try-and-then-decide-what-to-do" variety. Some inefficiency was the inevitable result, and modifications were frequently needed to compensate for insufficient planning. Yet, in time, cost, and performance, most such programs compare very favorably indeed with those that reflect the kinds of policies that we have criticized.

It is our hope that in reorienting the conduct of development along the lines that we have suggested, the military departments can regain some of the good effects of such past programs while at the same time guarding against their undesirable features.

III. Advancing Technology

The responsibility of the military services in research and development does not end with providing the weapons of the next generation. It has the equally important task of keeping filled the pipeline of scientific ideas and technical equipments that will feed the generations *after* the next. While the capabilities of the coming decade depend chiefly on the weapon systems now in development, the longer-run military capability of the nation is much more a matter of the effort devoted to advancing technology *apart* from any specific system objective. We believe that the current effort is inadequate, and that it should be increased immediately and considerably on pain of always remaining behind in the international race for technological and military supremacy.

ADVANCING TECHNOLOGY IN THE CONTEXT OF SYSTEMS

The development of almost every new weapon system inevitably involves some advances of a basic technological nature. That is one way in which the services contribute to the increase of technical knowledge. There will always be some need for advancing technology in the context of systems, either for special reasons that apply in particular cases, or because the system approach will disclose the need for knowledge heretofore neglected. We have seen that one of the genuine values of conducting development in a system framework lies precisely in the insurance that it provides against neglect of important problems. But we have also seen that weapon-system development is most efficient when such technological problems are tackled *first*, because the new knowledge gained from solving them is essential to proper guidance of the later and more expensive stages of a program.

During the first two or three years of their existence, in other words, system-development programs that aim at major advances in the state of the technical arts—if they must be run as full weapon-system programs —should resemble nonsystem programs designed solely to promote

technology. Many experimental projects should be started, and many, of course, will be dropped, since the objective in the early stages would be to obtain the information necessary to build the hardware components of the desired system.

ON THE IMPORTANCE OF A GREATER INDEPENDENT EFFORT

To the extent that the necessary technical knowledge is at hand when a system program is undertaken, weapon-system development itself can become faster and cheaper. That is one of the great values of pursuing a vigorous independent program to advance technology *apart* from particular system projects. By providing a wide and tested menu of advanced components for incorporation into weapons, such a program reduces the number of technical problems to be solved when a specific requirement is issued, thus shortening development time. Development cost is also reduced, since, as already noted, it is much cheaper to remove uncertainties by building and testing experimental prototypes than by launching prematurely into full-scale system-development programs.

Another great value of an independent research program in technology is that it is a more effective way to get new knowledge than is concentration on highly futuristic *weapon-system* development. Major technological advances do not come about by "orderly extension of the state of art"; they result rather from a willingness to take long shots—many long shots. But, as in poker or horse-racing, a long shot is a good gamble only when the ante is relatively small. A military service would be rightly unwilling to investigate an exciting but unlikely prospect for technical advance if the only way of doing so involved a system program costing hundreds of millions of dollars. It would be more willing if the cost of drawing another card were a few hundred thousand or a million dollars.

If there is inadequate provision for making the smaller bets, therefore, betting tends to become conservative, and the occasional long shot that could pay off handsomely never gets the chance to run.

The preceding points argue for increased support of what we have referred to earlier as the areas of basic research and exploratory develop-

ment. These points apply equally to the importance of undertaking more independent projects aimed at developing major components such as engines, missile frames, and electronic systems—i.e., equipments that can later serve as tested subsystems ready for incorporation into complete weapons. Such items, even when technologically feasible and potentially useful, are sometimes put into development late, or not at all, simply because there is no specific formal weapon-system application for them, with the result that lead-time and costs may be increased when a system requirement is issued calling for a large number of advanced subsystem designs.

We are *not* saying that every scientifically interesting idea should be explored, or that every conceivable engine, radar, or guidance system should be brought to prototype test. With very few exceptions, considerations of military usefulness should play some role in all R&D decisions. But that is a quite different proposition from the argument that a project in basic technology should await formulation of a specific system application, or that a new engine should not be put into prototype development until the configuration and mission of the vehicle that it will power are known in exact detail.

In general the decisions likely to involve the greatest expense (full-scale weapon systems, for example) should be scrutinized most specifically from the point of view of probable military usefulness. But such scrutiny might profitably be less specific as the bets become smaller. When they are smallest, as in basic technology, the indicated course is to suspend judgment about military worth long enough to gather experimental data on which to base reliable judgments. Only thus is technology likely to advance at a rapid rate.

RECOMMENDED POLICY CHANGES

Present policies do not emphasize sufficiently the importance of advancing technology outside the context of weapon-system development. The funds that are now devoted to this purpose are inadequate, and administrative practices are oriented so sharply to system development that they do not facilitate initiation of independent programs apart from systems.

1. More Money Should Go Directly to Advancing Technology

Funds currently going to research test vehicles, to independent subsystem-development programs, and to research and exploratory development account for less than ten per cent of the total annual service budget for R&D. Although there are differences among the services, we believe in general that all of them could profitably devote more money to such purposes.

The main reason that it is difficult to provide for adequate support of basic technology is that its promised payoffs are more distant and seem therefore less urgent than the more immediate needs of weapon-system programs. No matter how strong the belief that more long shots should be taken in R&D, the money even for small bets is the easiest to move from this year's budget to the next. Even projects that clearly promise major savings only a few years hence are easier to sacrifice than the items needed for system programs today.

The only way out of the dilemma lies in a firm and continuing policy decision to devote a *higher proportion* of total R&D funds to the needs of technology apart from systems. Stated differently, money for this purpose should be taken out of system projects.

We have indicated in Section II of this report that a reorientation of weapon-system development policies could easily release that amount of money for technology without endangering system programs now under way. *Our recommended shift in the R&D budget does not imply a slowdown in system development.*

2. Some Needed Changes in Administration

For the same reasons that the demands of system development dominate allocation of the R&D budget, they also tend to determine the pattern of program administration in the R&D establishment as a whole. Most of the subcommands and offices within a service development establishment have dual responsibilities: for weapon-system development and for advancing technology apart from weapon-system programs. The result is a tendency to undervalue the needs of technology in the face of urgent demands from system programs.

The solution, we believe, is to carve out of the existing R&D command

structure a strong organization devoted solely to the interests of technology. To be sure, not all primarily technological investigations would be conducted by this group. As pointed out earlier, some work on advancing technology will always have to be done in the context of system programs. But the advantage of a separate organization engaged *exclusively* in advancing technology is that the needs of the future will not be subordinated to the more urgent demands of the day. Given an adequate budget to discharge its task, such an organization would have the clear responsibility and authority to exploit quickly the most promising ideas for the future enhancement of the nation's military power.