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The recent pattern of military competition has emphasized qualitative rather than quantitative factors. This is placing an increasing importance on the research and development phases of military hardware development. American defense planners should give consideration to the basic Soviet research and development technique: building competitive prototypes before a commitment to production is made. Such a program would realize not only a lower dollar cost, but would have the added advantage of increased design flexibility, less leadtime to production, and the creation of a considerable subsystem menu from which to choose proven components for future needs.

RESEARCH AND DEVELOPMENT ON THE SOVIET MODEL:

THE CASE FOR THE PROTOTYPE

A research paper prepared in the Defense Economics
and Decisionmaking course at the Naval War College
by
Lieutenant Commander Gregory V. Gushaw, U.S. Navy

INTRODUCTION

Scenario. Since the end of World War II, when the highly successful Manhattan Project catapulted the United States into nuclear superpower status, this country has enjoyed the fruits of an excellent military research and development (R. & D.) program. Sophisticated and effective new weapons have been available when they were needed, practical applications by the military sector kept pace with the state-of-art advances in basic research, and U.S. forces could point with pride to their long technical lead in weapons systems over their closest challenger, the Soviet Union. Unfortunately, this clear-cut advantage no longer exists, and we have also been made painfully aware that technological advantage does not necessarily imply military success. Indeed, the Vietnam war made clear that at least some future conflicts would be fought on terms offering no overriding advantage for technical superiority.

As a result, previous high-level sources for R. & D. funding are beginning to dry up. As budgets are reduced or level-funded with dollars of less buying power, military R. & D. is caught in the same squeeze affecting other areas of Federal spending. This has one important implication for the U.S. approach to R. & D. management: as our military R. & D. resource base shrinks in proportion to total Federal spending, we must emphasize a broader technological base, even at the expense of dropping systems in being or reducing contracts for large procurements. We are driven to a scheme which rewards innovation in the use of proven (thus less costly) technology to fill new military requirements—a scheme already successful in Soviet military R. & D., whose boundaries were set not by competition within the state budget but by competition with U.S. military strengths and by R. & D. funds thinly sliced from a smaller national wealth.

Unlike the Soviets, our approach to military R. & D. insists on large technological advances for each new system and incorporation of the latest state-of-art advances whenever they become available during the course of a development program. This increases uncertainty during development, lengthens program times, causes cost overruns, makes system integration more complex, and increases the risk of failure. Resulting program cost uncertainties have decreased the willingness of Congress to fund and industry to undertake these highly speculative, potentially very expensive new weapons programs.

As we move into an era of international arms limitation agreements, the R. & D. effort becomes less important for end products than for overall program viability. The more deployment limitations we negotiate, the keener the competition for systems development short of actual deployment in order to maintain a full and varied potential weapons menu.[1] In addition, arms limitations to date have dealt with absolute numbers of units, not with the technological improvement of existing systems. Implied in these agreements is confidence on the part of U.S. policymakers that the U.S. qualitative edge in weapons development would more than compensate for frozen numbers. We have begun a technical competition, upgrading and making incremental improvements in weapons systems rather than deploying large numbers of new systems. The approach is more characteristic of Soviet R. & D. and is, for the United States, a competition governed by an unaccustomed set of rules. It is, nevertheless, a competition of the fiercest and most fundamental sort which we cannot avoid, the outcome of which may well determine our national survival.

Frame of Reference. It is perhaps appropriate to place some bounds around the area of R. & D. treated in this discussion. Since Soviet aerodynamic systems R. & D. is one of the most heavily documented areas reported by any source, it is the area from which most of the examples will be drawn.

While acknowledging such a restricted view, a comparison of Soviet R. & D. philosophy, approach, and methods with those of the United States should, nevertheless, yield lessons which could improve U.S. techniques. This is particularly the case when one considers that the U.S. military R. & D. effort will have to become more competitive with other Federal sectors for scarce Federal dollars.

COMPARISON OF SOVIET AND UNITED STATES R. & D. EFFORTS

Soviet Approach. The Soviet military R. & D. effort—the response of a nation with a relatively small economic base to the pressures of international competition—concentrates national resources in weapons development programs at the expense of consumer goods. Competition for the tightly held production resources has also forced accommodations and the less ambitious incorporation of expensive, complex, state-of-art gadgetry in new weapons. Yet, the Soviets are still spending nearly as much as the United States on R. & D. projects and have actually exceeded our efforts in certain areas. Aerodynamic missile developments, for example, by their sheer proliferation, have undoubtedly been one of the high priority areas for Soviet R. & D. expenditures.

It appears that the Soviet approach to development in aerodynamic missile systems R. & D., as well as most other areas of R. & D., has produced a 10-year cyclical pattern—one alternating between heavy R. & D. expenditures with few deployments of new systems, followed by a heavy procurement/deployment period during which few new R. & D. efforts are undertaken. Thus, in the period 1958-61, the Soviets tested and deployed their first-generation antiship missiles; by 1968 they were able to begin testing and deployment of a second generation, which included advancements in electronics, propulsion, and a submerged-launch cruise missile capability.

To a large degree these rapid deployments are the result of an incremental, largely off-the-shelf development approach that acts to minimize both delay and uncertainty. Subsystems available at the time designs are initiated are used without improvement unless they simply fail to meet the overall design requirement.[2] This is a crucial difference between United States and Soviet views of development and probably reflects the only logical Soviet compensation for the differences in economic base. Achieving a capability, however crude, is still achieving that capability, something usually forgotten in the United States. However, the more fundamental the interest in achieving a capability, the more likely are U.S. project managers to operate along similar lines. Such was the case when the Navy sought to develop the ability to deliver a strategic nuclear strike by carrier aircraft. The project managers involved were advised to show *any* capability that could be quickly achieved by modifying an existing aircraft. Once proven, the Navy role in strategic strike could be vastly improved with newer, more sophisticated systems.[3]

Not only do the Soviets rely on off-the-shelf components and subsystems, but they go to prototype before any commitment to series production of an aerodynamic system is made. In some cases, two or more prototypes may actually compete in tests to determine which best satisfies the military requirement. Normally, however, the competing designs are weeded out beforehand, and only one design is tested.

Throughout the design process, the crucial difference from U.S. practice is that *no weapon is committed to series production* and there is *no funding for anything but R. & D.* until the prototype is shown to fulfill a service requirement. Series production requires a high-level policy commitment after prototype acceptance. Not only do the money sources seem to be separate, but indications are that the facilities for R. & D. (design bureaus, prototype production plants) are totally separate from series production facilities. In any event, because of this physical and conceptual separation between Soviet R. & D. and production, no aerodynamic missile is marked for series production before the final and exhaustive design bureau and service testing for reliability and simplicity, qualities apparently prized by Soviet planners.

In addition to direct cost, time, and reliability advantages, the proceed-to-prototype approach has secondary propaganda value. Display of various prototype aircraft and strategic missiles—which either were successful in tests but not committed to series production or were prototype test failures—has produced the wildest speculation in the West about the direction of Soviet weapons development.

Consequences. There are, however, drawbacks to the way the Soviet system of R. & D. operates. The Soviets have been criticized, both in their own literature and by outside analysts, for unimaginative, inflexible development practices, for lacking common-system crosstalk, and for the overcentralization of management. They also have difficulty bringing fresh discoveries from the realm of pure research into practical application. Factors contributing to this inflexibility are the pressure to use off-the-shelf subsystems and adherence to strict design specifications. At the same time, the institutional separation of R. & D. from production appears to have produced the awkward, often unbridgeable, separation between research and applications.

But these problems, as well as the opportunity costs to civilian R. & D. programs, are not prevalent in American R. & D. and can be overcome. A more important consequence of the Soviet approach is the varied and imaginative application of existing technology. The approach often achieves the desired capability at a much lower cost by using point designs rather than multimission designs. This approach is probably used for airframes generally and antiship missiles in particular. Lacking carrier aircraft for the long-range engagement of naval targets, Soviet designers provided a weapon which can perform a similar task less expensively—fight surface-to-surface engagements at far longer ranges than naval guns. The development of antiship missiles from existing technology also extended the useful life of otherwise obsolete systems: the Soviet Army's Shaddock battlefield cruise missile when given a radar terminal homing system became SS-N-3, a missile which was introduced to the fleet in 1961 and is still operational. It has allowed major Soviet surface units and some submarines to "outgun" U.S. units for 13 years.

United States Approach. In contrast to the Soviet programs, the U.S. military R. & D. effort could best be described as reactive, a dangerous approach to R. & D. in the 1970's. If we wait to start weapons developments or shelve them too early, we risk losing the necessary technological advantage dictated by our arms/force level limitation agreements.

The Sputnik scare and perceived threats of a missile gap in the early sixties led to an expansion of U.S. R. & D. efforts in order to guarantee dominance in strategic nuclear delivery systems. Many other development programs rode the coattails of that expansion. The systems which insured strategic dominance have now been deployed at great cost. Meanwhile, the Vietnam war demanded funds which, once shunted away from R. & D. programs, have never really been restored. The budgetary breathing room needed for strategic systems R. & D., which allowed full development of those and many other (tactical) systems, has now vanished. We have been criticized for spending fewer dollars than the Soviets for military R. & D. programs, for stretching those funds out over too many projects, and for permitting unusually lengthy development phases.[4]

Against an austere funding environment, there is precious little room for the traditional U.S. approach to providing new weapons systems. We are hardly in a position to insist on the "best for our boys": full development and deployment of a large number of multipurpose systems incorporating, to the maximum extent possible, the outer fringes of state-of-art improvements. We have not yet come to realize the hard lesson that shrinking funds should teach: that a change in approach to R. & D. involving more prototype, off-the-shelf developments could well save enough money to keep a wider range of projects alive.

Although we seem to have ignored the real lesson, the pressure of austerity funding has caused adjustments to R. & D. programs, but in the wrong direction. The formal system of requesting bids on separate development contracts has been to an extent bypassed, and R. & D. costs tend to be made contingent on final systems buys. At a very early stage in the R. & D. cycle, U.S. planners settle on *one* industrial activity to develop and produce a new weapon, using subcontractor assistance as necessary. The prime contractor does not consider he is entering into a sound business arrangement unless series production in his plant or under his direction is guaranteed, usually well before all system integration uncertainties are understood. This contrasts starkly with the Soviet system which makes no commitment to series production until late in the R. & D. cycle.

Clearly defining and controlling these ever-escalating large system contracts has become such a headache that an overstructured R. & D. management bureaucracy has developed to deal with the problem. The resulting reporting system and methods of negotiating contracts to contain fees, incentives, and cost margins are clearly counterproductive of a healthy R. & D. effort.[5] [6] Industry is looking for ways to shake off some of the governmental control systems while, at the same time, Government is in need of a greater, more innovative R. & D. package from industry for the money invested.[7]

Consequences. As perhaps overdrawn above, the U.S. methods and approach to military R. & D. lead to long development times, uncertainty over final project success, and higher R. & D. costs. Development time often spans 6 to 10 years and is generally characterized by confusion as to when a project will be actually completed. The resulting higher costs are in part caused by lumping R. & D. with the total system production contract and in part from commiting resources to preproduction tooling before the final system configuration and deployment have been resolved.[8] [9] Therefore, in the face of the uncertainties of cost, technology, and development time, manufacturers are understandably hesitant about going ahead with R. & D. on a complex, expensive new system when there is no series production contract or significant Government underwriting of the R. & D. project. Being left holding the prototype bag is too costly to undertake without Government financial help, particularly when hundreds of millions may be spent to "fly before buying."

Congressmen have shown a willingness to provide R. & D. funds as part of a total procurement package, probably because tangible results (numbers of aircraft or missiles, new project starts in his state) can be seen by his constituents as direct results of his vote. Forcing already meager R. & D. funds to compete with procurement in a one-producer, total system contract has the additional shortcoming of cutting off options early by channeling the program toward a specific weapon, not toward a specific capability. However, parallel path development, if appropriately funded, could achieve the same capability with several quite different systems whose costs, reliability, and effectiveness could be compared *before* commitment to series production was ordered. As conducted and funded now, much industry (and especially Government laboratory) R. & D. is emasculated by a not-invented-here syndrome: there are no incentives, and little desire, to series produce someone else's R. & D. product.

In addition to the funding and contracting inequities already discussed, exhaustive feasibility studies are equally effective option killers. Even a technically feasible program can be shut off by a study which predicts too high a final systems cost, too complex a marriage of technical factors, or too long a development time.[10] All too often these studies are made at a point in system life when uncertainty about the key issues is so great that useful judgments are not really possible.

Finally, the U.S. approach fails to capitalize on a larger R. & D. manpower and capital equipment base than that of the U.S.S.R. Confining the R. & D. effort within a larger manufacturing contract or cutting it off at feasibility study restricts the number of industrial entities who actually bring their experience to bear on providing a military capability. Combined with the tendency toward defense industry conglomerates who specialize in certain systems, the result is an ever-narrowing technological base from which to draw in the future. In what is supposed to be a learning industry, there are too few students in an R. & D. "class" to benefit from exchange of experience at solving complex but common problems.

STRENGTHS OF THE GO-TO-PROTOTYPE APPROACH

In general, an approach to U.S. military R. & D. which stressed maximum prototype development should solve more difficulties than it creates. It has advantages of lower costs, better design flexibility, less leadtime to production, and greater capitalization of technical breakthroughs.

Figure 1 demonstrates the life cycle costs of a typical defense system. The figure plots system costs against time, showing with idealized curves the proportions of total lifetime cost used in the R. & D., investment (procurement), and operations phases of weapon developments.[11] Note that initial operational capability (IOC) date corresponds roughly to the point where maximum R. & D. expenditures are being incurred and that feasibility studies are conducted very early in the R. & D. phase.[12] Increasing the technological intensity of new U.S. weapons systems has not reduced manpower, maintenance, and operations costs, as it was optimistically predicted, and no matter what proportion of total systems cost is included under the R. & D. curve, increased technology or technical complexity/intensity of new systems will increase the entire system cost, not simply the R. & D. cost. All three parts of the total cost depicted by the curves will rise (even if not in direct proportion) with the technical complexity of new systems. Thus, one can raise a fundamental argument for going to prototype; if R. & D. will always be one-fifth to one-sixth of the total cost (as the figure shows) and the cost-to-prototype about one-half the R. & D. cost, then R. & D.-to-prototype cannot reasonably be expected to cost more than one-twelfth to one-tenth of final systems cost.

Arguments have been made that prototype production and parallel development paths are useful only when the R. & D. cost is a sufficiently small part of the total system cost. But R. & D. is

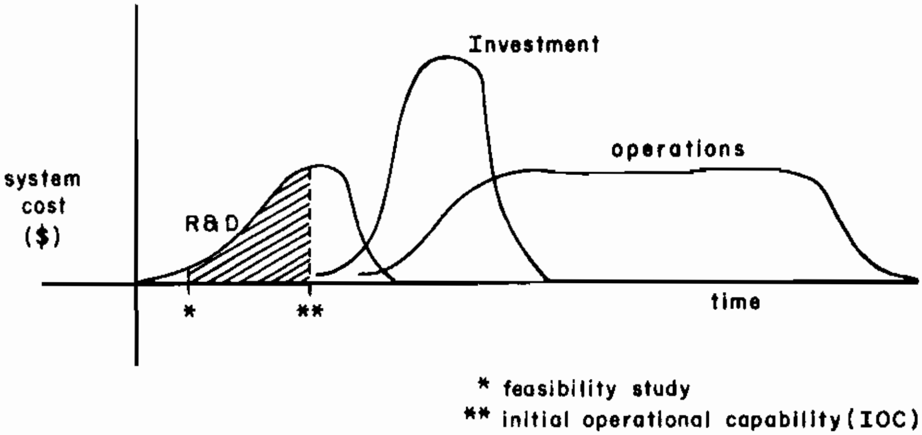

Figure 1

always a small protion of total system cost. Even when the R. & D. cost is a larger proportion of total cost, the cost to reach prototype (shaded in the figure) will still be only a fraction of the total cost. Similarly, in terms of the time the system will be operational, it will be only a fraction of the total system useful life.[13]

Proceeding at least to prototype production before deciding on final system application has the further advantage of permitting an unsuccessful project to be cleanly terminated or a successful prototype simply shelved if no current service application is seen. Government funding of prototype production, in whole or in part, is certainly less expensive than entire system production/deployment, and it reduces the industrial interest in seeing a system deployed regardless of its perceived military usefulness. It should also discourage industry tooling up for production during the R. & D. phase, which perhaps reflects inordinate concern for production problems before the development problems are solved.[14]

The existence of a competing, perhaps better, system still in exploratory development is another possible reason for deferring production of even the successful prototype. This allows for competitive comparison of the two developments before either one enters active deployment and is produced in large numbers. For example, the successful performance of the latest Sidewinder air-to-air missile, the AIM-9L (now in R. & D.), need not mandate widespread introduction for fleet use until the prototype Agile missile undergoes extensive testing. Both missiles offer the same ability to attack targets from positions forward of the beam, producing better engagement geometry, but Agile incorporates the significant advantage of rocket motor thrust vector control for greater maneuverability. Also, the Agile launch aircraft need not be pointed at the target before firing.[15] Current series (AIM-9D,G,H) Sidewinders have proven more than adequate to handle air-to-air engagements in Vietnam and, by Israeli Air Forces, in the Middle East, so little is gained by deploying AIM-9L when the money might be better invested in the completion of R. & D., testing, and procurement of Agile. The decision to hold AIM-9L at the prototype stage until Agile reaches that stage would be eminently reasonable and cost-effective.

In a larger context, encouragement of R. & D.-to-prototype results in more proven prototype subsystems and complete systems being available to meet new service requirements. It has the advantage of being able to reduce the cost-to-produce for any new requirement. Sunk R. & D. costs are not lost since the larger arsenal of proven systems increases future design flexibility and gives a greater range of low-cost choices to fill an operational need. In addition, the smaller dollar costs to reach prototype should encourage Congress to appropriate funds for R. & D. Since procurement is often reduced after service acceptance of a system (e.g., the reduction in F-14 total aircraft purchase), appropriations are actually tending to resemble funding-to-prototype at the operative level. As congressional timidity over large system expenditures reduces the final buy, the only remaining significant cost is the R. & D. work leading to production and testing of a limited number of units, similar to the Soviet minimum prototype production.

A slight redrawing of the system life cycle curves from figure 1 suggests two basic strengths of the go-to-prototype approach—savings in time and savings in money. Consider first the time savings:

1. If a new requirement can legitimately be filled by an existing prototype system, that system can be brought into series production (see figure 2A) in far less time (t_2) than it would take to complete an entire new R. & D. cycle

(t_1), even starting from a completed feasibility study.

2. Even if production is delayed because of snags in testing—stretching out the R. & D. curve (figure 2B)—the time to reach procurement from prototype (t_2) will still be less than if the project had been initially stopped at the feasibility study in anticipation of problems, thence restarted and carried through to production (t_1).

Next the money savings: if procurement is delayed for additional testing, as in figure 2B, and the project fails or a more attractive candidate system displaces it, money will have been spent to learn that the system will not meet the requirement. However, considerably less will have been spent (total cost to reach T_O= shaded area in figure 2B) than if production tool-up and investment had begun before the actual utility of the prototype was finally proven (total cost to reach the same T_O shaded in figure 2A). The customer benefits from proceeding to and proving the need for the prototype system. In this case, and in general, "buying time" will save money.

Since R. & D. is considered an industry heavily structured toward learning, an emphasis on developing prototypes would enhance the overall industrial learning base and reduce the number of project false starts. In short, prototype competition is an effective method of promoting industrial knowledge and experience in the type of technology necessary for military projects. It should also discourage the not-invented-here behavior of single producer industries. Given a case where several firms attempted to assemble prototype antiship missile systems, for example, getting a competitive bid for series production of one of the designs, even the design of a Government lab, should be easier. Increased industry crosstalk on related problems could also be expected.

Until the existing approach to military R. & D. is changed and until more industries gain the requisite knowledge, making series production commitments or forcing manufacturers to absorb considerable portions of the R. & D. cost in the total contract before prototype testing leads to situations where funds are committed with considerable uncertainty. Nevertheless, uncertainty can be significantly reduced by subsystem and prototype testing.[16] [17] This points to perhaps the greatest strength of the prototype approach: *reduction of both external and internal uncertainty* before committing huge sums of money to a system. External uncertainty is reduced because the military is better able to

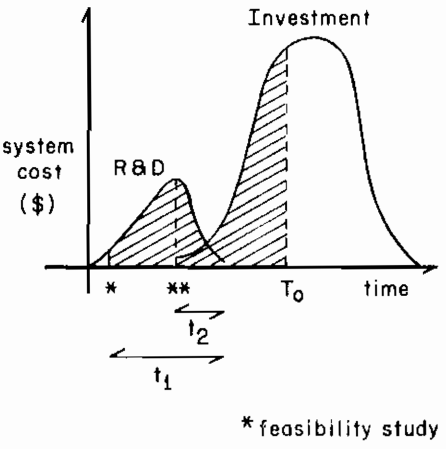

Figure 2A

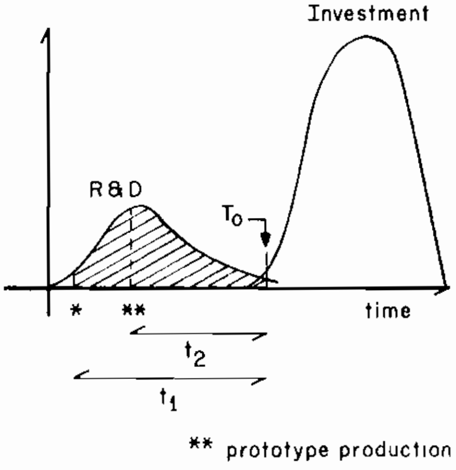

Figure 2B

deal with future scenarios with a larger menu of proven weapon systems. Internal uncertainty is reduced by the greater likelihood that proven systems can be used off the shelf or integrated with minimum R. & D. time/cost to meet new requirements.

New solutions to old problems often occur to engineers and applied scientists when difficulties are encountered on the way to solving an entirely unrelated problem. Simply deciding to *build something* can resolve the previously unresolvable.[18] Parallel path prototype funding will also encourage more independent study of each proposal. Competitors sniping at each other's programs can illuminate more problems than program managers' feasibility studies ever will.

From the decision to increase prototyping should come two types of beneficial cross-fertilization. Either defense conglomerates may recognize civilian applications for some military programs, or the reverse may hold. For example, manufacturers of microminiaturized logic circuits of the type used in pocket calculators have indicated a willingness to *sell* part of their production runs to military consumers, even though they have a strong aversion to restructuring any part of that production to meet military specifications. These circuits are, even now, more compact, rugged, and reliable than those in common military use. Interestingly, the services appear to be relaxing requirements at operational levels by allowing use of many pieces of critically needed equipment (mostly computer ancillary hardware) which was not designed to meet specially written military specifications. Military acceptance of more off-the-shelf components for other systems cannot be far behind. It is also possible that one service may see uses for another's weapon in the way that the Soviet Shaddock missile found a home at sea as SS-N-3.

RECOMMENDATIONS

What recommendations can be made based on this comparison of United States and Soviet approaches to R. & D. and the strengths of the prototype systems approach?

- Perhaps the most important is that the United States should be prepared to *increase R. & D. funding ending in prototype production/competition*, without the present commitments to series production purchases. The money saved on production false starts alone could pay for the necessary boost in R. & D. funding. A two-track bidding system could be used which contracts one or more R. & D. projects ending at prototype and solicits separate bids for series production. In the end, both contracts might be awarded to the same manufacturer, but the two-track system would force the contractor to separate his production cost considerations from the R. & D. bid.

- Secondly, the defense procurement program should insist on prototype production by more than one producer and testing by the producer similar to the Soviet design bureau field tests, followed by service testing to establish a level of acceptability. Only after this testing should series production contracts be awarded.

- Parallel path R. & D. projects should be encouraged in order to compare different design approaches to the same capability. Avoid specifying detailed weapon system characteristics since desires can change in the face of innovative proposals; it is also possible that the entire service requirement will change in the course of the R. & D. cycle.

- Formalize cross-service participation in R. & D. so that weapons of use to more than one service can be identified and tested. Maintain wide dissemination of on-shelf capabilities to encourage new applications.

- Deemphasize multimission designs incorporating the "last ounce" of state-of-art improvements. Instead of redesigning for a specific system, more commercial components which already incorporate state-of-art advances can be used off the shelf to establish a military capability in less time and at lower cost.
- Insist on adherence to relatively tight time schedules in producing prototypes to encourage the use of off-the-shelf systems.
- Finally, if military R. & D. funds cannot be transferred from other Federal sectors and must increasingly compete within DOD for money, funds must be shifted to R. & D. to increase the diversity of the technological base from which systems will be drawn to satisfy new requirements. This must be done even at the expense of operating funds for current systems or reducing the size of some procurement contracts. Failure to adopt this approach will surely result in the enemy achieving a technological surprise.

* * * * * * *

If these recommendations are used to establish guidelines or rough out a framework for military R. & D. in the United States, the result may well be a viable research effort for future technological competition within the predicted increasingly austere military resource environment. International military competition has shifted from numbers to quality, from comparison of existing force levels to evaluation of strengths based on technology. It is not too soon to predict that the measurement of military supremacy in our world will hinge on a kind of negative standard: preventing a potential enemy from acquiring a technological edge. Our effectiveness in this technological competition will surely determine whether the U.S. military will remain capable of supporting national foreign policy objectives and, ultimately, of defending our free society.

BIOGRAPHIC SUMMARY

Lt. Comdr. Gregory V. Gushaw, U.S. Navy, did his undergraduate work at Harvard University in applied physics and is a graduate of the Defense Intelligence School. He has served in destroyers and, specializing in intelligence, has served as the analyst for Soviet naval cruise missiles and as Intelligence Officer for the U.S.S. *Whitney*. Lieutenant Commander Gushaw is a recent graduate of the College of Naval Command and Staff.

NOTES

1. Robert W. Buchheim, "Problems of Planning and Decision in Military R&D," P-3021 (Santa Monica, Calif.: Rand, September 1964), p. 4.

2. Pushing technology and attempting to manufacture unproven designs have been the most important causes of high production costs in the United States . . . the Soviets avoid both of these pitfalls. Prototype testing, simplicity, commonality, and design inheritance, minimize technological advance in each program, shorten the development period, and hold the development program to a relatively small size.

Arthur J. Alexander, "Weapons Acquisition in the Soviet Union, United States, and France," RAND P-4989 (Santa Monica, Calif.: Rand, March 1973), p. 11.

3. Vincent Davis, "The Politics of Innovation: Patterns in Navy Cases," Richard G. Head and Ervin J. Rokke, eds., *American Defense Policy*, 3d ed. (Baltimore: Johns Hopkins, 1973), p. 398.

4. These allegations are treated in detail in several unclassified and at least two classified sources. Overall funding levels are discussed by the Institute for Defense Analyses in study S-397 (note 12 below).

5. If the government wants to impose a much stronger system of incentives, it should insist that prototype models be built before full-scale development contracts are

> awarded, and that production contracts will not be let until the system in question is well in hand. In other words, . . . winning or losing a 500 million dollar contract might prove a stronger incentive to most contractors than a possible variation of from 4 to 10 percent in the profit rate of that contract.

Burton H. Klein, "Policy Issues Involved in the Conduct of Military Development Programs," RAND P-2648 (Santa Monica, Calif.: Rand, October 1962), p. 26.

6. "A management system which structures large, expensive programs containing even small amounts of critical unproven technology, or which encourages people to play down or hide technological risks, can only lead to wasted resources on the average." National Security Industrial Association, *New Initiatives for Defense R&D; a Summary of Suggestions from the Defense Industry; Second Report,* REAC 2-71 (Washington: 1971), p. 4. SECRET (Quote unclassified.)

7. "Normal procurement procedures (cause) . . . proposed contracts . . . to accumulate restrictive clauses that are designed to insure compatibility with operational systems. This compatibility must certainly be the ultimate goal, but should not be applied until feasibility of the concept is demonstrated and weapons applications determined." REAC 2-71, p. 52. (Quote unclassified.)

8. "According to testimony presented at the 1964 budget hearings, the Air Force has cancelled programs representing an investment of over $4 billion. . . . about 15% of all expenditures on R&D." Thomas K. Glennan, Jr., "An Economist Looks at R&D Management," RAND P-2819 (Santa Monica, Calif.: Rand, November 1963), p. 8.

9. The requirement for the (B-70) aircraft was ultimately deemed to be invalid. The program, subject to many reorientations because of questions about the value of the system, slipped badly in time and escalated in cost. . . . The result is the expenditure of approximately $1.5 billion for B-70 bomber aircraft technology, a substantial fraction of which went to what in retrospect is *wasted effort* at coordinating subsystem development, integrating logistics and training considerations into the design, and *laying out a production capability.*

Thomas K. Glennan, Jr., "Policies for Military Research and Development," RAND P-3253 (Santa Monica, Calif.: Rand, November 1965), p. 10. (Emphasis supplied.)

10. Very seldom indeed have studies alone led to the decision to go ahead with the development of a major technological advance. In fact, in many cases the effect of conducting long drawn-out "scientific" investigations has been to dampen enthusiasm for trying out a really good idea. Beginning in the late 1920's, for example, almost every study that was made of the jet engine came to dimmer conclusions on the feasibility and value of a jet engine than the study preceding it. . . . an experimental engine had been developed in Britain (and) . . . It is of interest to note that the amount of money the company risked in demonstrating the feasibility of the jet engine came to something like $20 or $25 thousand. This, essentially, is the amount scientific committees spent nearly 10 years arguing about.

RAND P-2648, pp. 19, 20.

11. E.S. Quade and W.I. Boucher, eds., *Systems Analysis and Policy Planning: Applications in Defense* (New York: Elsevier, 1968), p. 131.

12. Institute for Defense Analyses, Science and Technology Division, *Analysis of Research and Development Trends in the U.S. and USSR* (Arlington, Va.: May 1972), IDA S-397, pp. 18, 19.

13. Yet, it is usually well *before* IOC when a U.S. weapons system is committed to a series production schedule and a total unit purchase.

14. The belief that production problems are likely to be the dominant problems leads to the initiation of large-scale production preparation early in a development program, even at the expense of minimizing the program's flexibility. Moreover, initiating programs in this way is so costly that the number of options that can be carried into development is substantially smaller than it could be if the programs were initiated on a different basis.

RAND P-2648, p. 23.

15. Clarence A. Robinson, Jr., "Navy Spurs Industry Missile Role," *Aviation Week and Space Technology,* 18 March 1974, pp. 12-14.

16. The possibility of acquiring significantly improved estimates at relatively low cost suggests that it may be economical *not* to choose one design or contractor for an R&D job on the basis of first estimates, which experience has shown to be notoriously unreliable, but rather to initiate parallel development efforts, cutting down the list of competing projects as estimates improve.

R.R. Nelson, "The Economics of Parallel R&D Efforts: a Sequential Decision Analysis," RAND RM-2482 (Santa Monica, Calif.: Rand, 12 November 1959), p. 3.

17. "Early cost estimates of proposed airframes, engines and electronic equipment have tended to be wrong often by a factor of five or more. Performance estimates and development-time estimates have also been extremely unreliable." RAND RM-2482, p. 2.

18. . . . the scientific basis for radar was established before World War II . . . one wonders why it hasn't been possible to discover what kind of radar would be most useful for some particular purpose, and to proceed forthwith to build it. . . . consider . . . side looking radar, and more specifically one that operates at very short wave length frequency, say, .86cm . . . it would have fantastically good resolution, almost approaching that of photographs. . . . An experimental 1.25cm radar had been developed toward the end of the war which, while it gave quite remarkable resolution, had a range of only several miles. And it was generally concluded that a radar with a wave length less than 2.0cm would have no military utility . . . experiments conducted by British scientists . . . proved that the choice of 1.25cm as a radar frequency was a very unfortunate choice indeed, for at frequencies slightly higher (1.8cm) and slightly lower (.86cm), atmospheric attenuation was far less serious. . . . *When an experimental .86cm side looking radar was finally put into development, it took just 90 days to get it ready to be tested, and the cost of finding out what resolution it actually would provide came to some $3 million. As it turned out, this was actually a much smaller amount that the government had spent on past studies.*

RAND P-2648, pp. 7, 8, 15. (Emphasis supplied.)

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It is by devising new weapons, and above all by scientific leadership, that we shall best cope with the enemy's superior strength.

Winston Churchill: Memorandum for the War Cabinet,
3 September 1940