Evaluating Defense Department Research

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EVALUATING DEFENSE DEPARTMENT RESEARCH

Background Paper

Prepared by the

Office of Technology Assessment

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SUMMARY

Congress must vote funding for-- and sometimes choose among--an extremely complex assortment of highly technical proposals for supporting the defense technology base. Any type of systematic approach that could make this task more tractable, rational, and transparent would be attractive. One approach that seems, at first examination, to hold much promise is some sort of "decision-support system" or, as it is sometimes called, "risk analysis" often used by commercial research groups. Closer examination reveals that such approaches are too limited in scope to apply across the whole range of projects that Congress must consider, although the approach can still be applied to specific cases.

One limitation of using a decision-support system is that the method requires a quantitative measure of "benefit," which is very difficult to produce when dealing with questions of national security. This is not to say that members of Congress do not have clear ideas of national security objectives, just that these ideas typically are not readily quantifiable.

Much of the reward of a quantitative decision-support system could be had without the artificiality and implied precision of quantitative measures if the Congress could apply its judgment to questions of military research. Congress, however, has judgments about military missions but research money is allocated by technology area and there is no easy uniform way to connect the two. What Congress requires is a clear statement of defense policy, either from the Department of Defense or formulated by Congress, and a 'road map" that allows Congress to trace how research proposals intend to support that policy. Congress can require that the Department of Defense demonstrate how the forces that it wants for the future will support the military policy and, finally, how its research programs will make those forces possible. Congress should be able to review those goals that the DoD develops. It does little good for Congress to make certain that research is supporting defense goals if Congress does not support the defense goals themselves.

The criteria for evaluating and correcting research programs include:

(1) the length of lead time before the technology will produce results,
(2) likelihood of technical success,
(3) number and importance of the technology's military applications,
(4) the time required to develop countering technologies or tactics,
(5) number and difficulty of required ancillary technologies,
(6) the risk of being overtaken by parallel technical developments,
(7) the extent of civilian spin-off (or unintended civilian costs),
(8) alternatives to U.S. government support including industry and allies, and
(9) the overall threat posed by potential adversaries.

Congress also has a role in assuring a robust research program and in assuring that research programs are well run. For example, it could designate some fixed percentage of procurement funds to research budgets or designate some floor for research to forestall raids on the research budget. Occasionally, Congress may want to specify funds for particular research areas. An important objective of Congress's oversight is to discover problems as early as possible. Even if Congress is itself not well-suited to detect problems, it can require that procedures be followed which will help assure earliest possible warning of any problems that do occur.

In short, setting military research priorities will never be easy. The task has three parts. One is setting strategic goals. The second is judging which particular research programs will best help reach those goals. Once a research program is approved, there is the separate task of monitoring it to see that it is well run. Congress is best at the first and third tasks but less capable of determining the structure of research programs. Through mechanisms such as hearing, reports, and oversight, Congress can satisfy itself that DoD is selecting and managing research programs on a rational basis.

Evaluating Defense Department Research.

Setting priorities is seldom easy. Intelligently setting science and technology research priorities--where success is uncertain, where the return on investment is years in the future, and where even the objectives often are vague- -can be very difficult indeed. This paper discusses briefly (1) the purposes of research directed toward support of the defense science and technology base and (2) criteria for **judging** government-supported, military-oriented research portfolios. It will not cover, except tangentially, research into science and technology directed to civilian uses or the more advanced development phase of military weapons and systems.

Why Should Congress Fund Research at All?

Justifications for government support of research vary. In some cases of scientific "pure" research, we may hope for some long-term benefit but no practical application is clearly foreseeable, except perhaps the training of science graduates. For this type of research, support cannot be expected from commercial concerns interested in future profits. Some have suggested that disciplines such as cosmology that have no discernible benefit should be supported by the government for aesthetic reasons, that is, for the same reasons used to justify government support of the fine arts.[1] In any case, if pure research for which no one can see practical benefit is to receive any support at all, support will have to come almost entirely from the government.

More often, the general advantage of some research project is clear, or at least accepted, but no one company may be able to collect the extra profits generated by the research. In this case, an economist would say that the benefits of the research are not "appropriable." Again, lacking expectation of profits, commercial concerns will not support such research. However, government funding of this type of research is easily justified if it creates a public good.

Justification for the support of research in defense science and technology is different yet. Providing for the common security is a constitutionally-mandated responsibility of the government. More importantly for this discussion, however, the government is the only direct customer for the products of military research. The government determines the 'market" and what 'product" to buy and how many (or even whether to buy them). Moreover, the government can, through fiat, limit future profits derived from the product of research performed by companies, but-it 'cannot be relied upon to

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1 C. F. Carter, "The Distribution of Scientific Effort," Minerva, Vol. 1 (Winter 1963), pp. 172-190.

guarantee return on investment. In this situation, some companies will fund and perform some research but the government can never depend on industry to do all of the research required, especially long-term research.

The government must sometimes fund scientific research in areas having military application which, on their scientific merits alone, would not normally be supported strongly. Sound propagation in the sea is a good example. The intricacies of sound propagation may be of marginal scientific interest but are vital to anti-submarine warfare.

Far more often, however, government support will go to technological research important to, and often unique to, military application. The technologies of nuclear weapons, stealth, and electronic countermeasures are all examples. Given these requirements for support of military science and technology, the government must maintain and constantly revise methods of devising research goals, of evaluating on-going and proposed research projects, and of setting research priorities.

The government is also justified in investing in research to reduce the cost of manufacturing weapons. Peculiarities of the weapon procurement system discourage companies from investing in research to improve efficiency of manufacture. For example, with cost-plus compensation, a company's eventual earnings are actually lowered if it invests in research that reduces production costs. In addition, uncertain" and generally small production runs, government control over specifications and output, and veto power over foreign sales, all combine to reduce companies' incentives to spend much research money to improve the efficiency of manufacture.

What Is Technology?

Before describing means of evaluating military science and technology research programs, it is useful to divert long enough to consider the definitions of "science" and especially "technology." Science is not a collection of observations. Rather, it is the organization of those observations into a systematic stricture. The structure is what allows us to believe that we "understand" the observations that we see.[2] Technology is the sum of knowledge, tools, skills, even organizations, that allows mankind to bend resources to its use. One way to put it, simplifying very much, is to say that science is concerned with "why" and technology with "how."

Scientific understanding does not automatically result in technical progress. Nor does technical advance require a scientific understanding; the first few thousands of years of progress in metallurgy and animal husbandry

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2 From the large literature on the theory of science, see Karl Popper, The Logic of Scientific Discovery (London: Hutchinson, 1972) and Thomas Kuhn, The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1970).

took place without a whit of understanding of either atomic chemistry or molecular genetics. However, without the science behind it, technological progress depends on a tedious and inefficient trial and error approach.

The military enterprise is filled with examples of both science and technology. Our previous example was anti-submarine warfare. It requires for its, success an understanding of sound propagation through the sea, that is, how sound is affected by salinity, temperature, currents, and ocean bottom conditions. These are scientific questions that are as important to the tactics as to the technology of submarine hunting. The ability to make, hook together, and use the hydrophores, amplifiers, computers, and appropriate software, constitute a technology of anti-submarine warfare.

Technology is concerned with the means to ends. Technology allows us to achieve desired ends reproducibly with predictable means. In more specific, practical terms, a level of technology is measured by the ability to achieve reproducibly a desired performance, whether in a computer chip or a jet engine, for a certain cost. Cost is, therefore, an important (even if usually implicit) part of any definition of 'technology." Technology allows us to do things; better technology allows us to do the same things more easily or to do better with the same effort. In terms of cost, an "advance" in technology can be defined as a new process that allows higher performance at any given cost or any given performance at lower cost.

Of course, new technology makes possible performance that just wasn't possible before no matter what the cost. In 1900, even unlimited resources could not allow the millions of calculations per second or supersonic flight made routine by today's computer chips and jet engines. Because the military, especially since the Second World War, has almost always used advances in technology to increase performance, high cost and "high" technology are closely linked in many minds. However, it is the performance that is expensive, not the technology. For example, a modern fighter plane like the F-14 is fabulously more expensive than the first Wright flyer bought for the Army Signal Corps for $25,000, but advanced technology is not what makes an F-14 expensive, it is the desired performance (made possible by the technology) .

Keeping the effects of technology and performance separate is always difficult whenever we compare the costs of two airplanes with very different performance and built with very different technologies. The distinction between technology and performance would be plainer if we instead compared, say, the cost per horsepower of engine (keeping weight constant) or the cost per ton of payload delivered a given distance in a given time. Then we would see that advances in technology make any given performance cheaper. This is clear when we consider that modern technology--aluminum, carbon composites, nylon, light-weight engines--puts within the financial reach of a weekend hobbyist a motorized hang-glider with much the same performance as the first U.S. military aircraft.

Just as new scientific understanding does not automatically lead to new technology, new technology does not automatically lead to new products. Research can create new technology, which is the **ability, or know-how, to do** things but it is not the things. Usually, a development program is needed to

apply the new ability to the construction of something. Sometimes, this division between research and development is very clear cut. More often, however, the development process is mixed up with bits of research along the way. We set off to build a new thing, say a tank, airplane, or computer, believing that we know how to do it but in the event discover that something does not work quite as expected. (After all, in a development project, we are building something we have never built before.) This forces us temporarily to back up in the development program to do additional research on new approaches.

In summary, two distinctions should be maintained. The first is the distinction between technology and performance. The second is the distinction between research and development. Both distinctions are important if we are to think clearly about the problems of science and technology research. The first distinction is important because what is clear in the civilian sector is often forgotten in the military sector: an advance in technology makes any given level of performance cheaper; however, greatly increased performance made possible by a new technology, will be more expensive. In short, performance is a measure of the development of technology, not the technology itself. The second distinction is important because the research budget is small and long-term compared to the development budget and is, therefore, sometimes neglected bureaucratically or 'raided" to cover more immediate financial needs. (And exactly the same problem appears one step further up the ladder. Because the overall R&D budget is small and long term compared to the procurement budget, it is often neglected compared to procurement or raided to help pay for procurement.)

Knowing what technology is does not tell us what technology we should try to develop nor how we ought to obtain it. Research is needed to develop new technology and it must be directed to get us where we want to go. The next section discusses how we should decide what technology we need and how to set goals for research.

Strategic Planning for Military Research.

Any enterprise must have clear goals before its priorities can be set and its efforts organized.[3] To apply such a top-down approach to military research, we must first define a strategic framework: What is the relative strategic threat from a single large European war versus a multitude of possible smaller third-world conflicts? Do we need to be able to fight tomorrow or can we allow a long mobilization? Such a framework allows us to set priorities and to debate intelligently overall levels of R&D spending, how it should be distributed between military and civilian economies, how the military budget should be divided among research, development, product improvement, and so on, and finally the funding of specific research programs.

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3 This was one of the principal points of the Grace Report; see Task Force Report on Research and Development (Washington, D.C. : President's Private Sector Survey on Cost Controls, 8 December 1983).

A pure top-down approach, although perhaps appealing in theory, is infeasible in practice. While policy makers can decide what is desirable, they cannot decree what is possible. For instance, putting a man on the moon by the end of the decade is an example of a clear objective for a research and development program. Whether it was a realistic goal when it was proposed depended on what was possible at the time. In 1960, it was a challenging but plausible goal. In 1950, it would have been overly ambitious if not outright impossible. And in 1940, it would have been hare-brained.

The scientists and engineers at the laboratory bench are best placed to judge what is technically possible. We can be most certain that we are pursuing technically promising paths by fallowing a bottom-up approach. A pure bottom-up approach would be to collect all the proposals for research projects from workers in the laboratory, and let them decide which ones to fund. (This is similar to the 'peer review" system used in allocating most U.S. government-funded civilian basic research.) This approach is guaranteed to remain somewhat unfocused and contentious, with contributions to specific overall goals being strictly fortuitous. However, if there are more projects than available money can fund (and there always will be), then a set of priorities based on some "cost-benefit" scale, is required. It is at this point that the top-down and bottom-up approaches meet because the strategic goals define "benefit."

The overall research effort is shaped, then, by iterative accommodation of three considerations: Hardware needs expressed by the users of military technology, new technical possibilities described by the scientific and engineering community, and the strategic criteria of benefit determined by policy makers. Moreover, researchers will soon learn what projects meet the criteria and, since they want funding, will attempt to accommodate the established criteria. Thus, our overall direction is a compromise between where we want to go and where we can go. And, the objectives of military research can change over time, either because the strategic military objectives change or the technical possibilities change.

This section described how we decide what we want, but it does not tell us how to get it. The next section describes some of the mechanisms we could use to guide us in making concrete decisions about funding.

Allocating Research Funds

Some of the same ideas that have been developed to set research priorities within individual companies can be applied to government support of military research. We must keep in mind, however, that commercial companies can have quite narrow research goals while Congress must evaluate military research proposals within an enormously broad context.

An approach, often called "decision support" or "risk analysis," attempts to systematize and, in at least some cases, quantify the allocation of finite resources across an entire portfolio of research projects.' Computer programs are available commercially that find solutions meeting the complex set of conditions. Even when this quantitative approach is inappropriate for direct use by Congress, an outline of the logic helps us understand the allocation process.

To compare research proposals using such an approach, we must first determine the possible funding levels within any individual project and estimate the benefit from funding of each level. Some projects will have natural and obvious steps, or "break points," in the level of support. We can use, as an illustration, a research program to improve jet engines for aircraft. Jet engines contain three basic components, a compressor, a combustion chamber, and a turbine; we can picture the overall program as being made up of three projects, one for each component. Imagine that we wished to investigate the nature of some new type of high-temperature turbine material that has two important characteristics. Measuring one characteristic uses an inexpensive instrument and measuring the other characteristic uses a much more expensive instrument. Four possible support levels are obvious: no support, low level support to buy the first instrument, high level support to buy the second instrument, and a slightly higher level to buy both instruments. Not all projects will have clearly defined break points but they "can be created at arbitrary levels of support between no funding and high funding.

For each support level for each component project, the expected benefit must be estimated and a numerical value assigned to it. In the case of jet engines, "benefit" can be quantified in just a few simple measures, for example, weight, fuel consumption, and cost. Moreover, all of the components can be compared by these same measures. Judgments must be made about the relative importance of each of these benefits; for example, we may decide that a one percent improvement in fuel consumption is worth a two percent improvement in weight reduction. (These relative benefits will be determined, in part, by the application; for example, weight is more important for a fighter but fuel efficiency is more important for a transport.) Now, by examining the contribution to expected benefit from each project at various levels of research support, we can allocate support, within a given overall budget, among the three projects to maximize the overall improvement in jet engine performance. In our example with only three projects, only a few possible funding levels, and simple benefit measures, we might be able to find the best allocation by inspection. As the number of projects increases, the complexity of finding the optimal allocation grows very rapidly. In these cases, we can use a computer' to evaluate all possible combinations of allocation to find the best one.

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4 Howard Raiffa, Applied Statistical Decision Theory (Cambridge: MIT Press, 1968) and Howard Raiffa, Decision Analysis (Reading, Mass. : Addison-Wesley, 1968).

In principle, exactly the same approach could be applied at the next higher level. For example, the performance of an airplane depends on (among other things) the performance of its engine and the weight and strength of its structure. We can calculate the benefit to overall airplane performance from various levels of funding for both structure and engine research projects and then find the best mix to achieve the greatest improvement in overall aircraft performance.

At this level of analysis, the difficulties with this approach are already becoming apparent. Whereas engine performance is objectively quantifiable, aircraft performance is more difficult. Not that quantifiable measures are not available; they are, for example, top speed, turning rate, rate of climb, range, and so on. The difficulty arises when we try to relate these performance parameters into a quantifiable measure of benefit, such as combat effectiveness. Estimating combat effectiveness from aircraft performance requires various assumptions about the theater, tactics, the adversary, and objectives. We could use computerized models of air-to-air combat or the results of simulations and exercises to determine quantitative relations between the various performance parameters but these methods are as likely to obscure as to reveal underlying assumptions.

We could, again in theory, continue this process up the ladder to ever larger scales, assigning numerical values to the contribution to security from aircraft and tanks to find the optimal allocation of research funds between the two areas. This could be done, for example, by applying large theater-level computer models of combat, through historical analysis, or through results from military exercises. We could then compare land and sea forces, and finally compare research in conventional and strategic weapons. The difficulties are formidable and apparent. As we move up to larger scale comparisons, the estimate of ‘benefit” becomes more diffuse, less easily quantifiable, more subject to assumptions and judgment, and hence less subject to consensus.

If we wish to use a quantitative decision support system to allocate resources, two limitations are clear. First, application of the system is increasingly inappropriate as we get further from the laboratory bench level of technology development. At higher levels of comparison, evaluation is more judgmental and speculative. (The method requires numerical values of benefit. But assigning numerical values does not turn fundamentally subjective evaluations into quantitative ones.) Second, wherever such a quantitative resource allocation model would work, it would best be applied by those closest to the research management, for example by Service or National laboratories.

Even where quantitative models cannot provide clear-cut answers, resources must still be allocated. And even allocation based on subjective judgment needs information to be sensible. The problem with a quantitative approach is that one end of the resource allocation problem is quantifiable but the other end is largely judgmental; and now there is no way to connect the two ends. If we try instead to use a judgmental approach to military research funding decisions, the problem arises that Congress makes judgments about military <u>missions</u> but funding is distributed by <u>technology area</u>.

For example, one may have an opinion about the relative importance of air defense and anti-submarine warfare, but that does not answer the question of how those priorities should be translated into research dollars for the development of new technologies. This requires, first, a complete set of technology areas, second, a set of military missions, and, finally, the best attempt at a clear and explicit set of connections between the two. With a set of connections between research area and military mission, we can see how technology funding is distributed by mission. The DoD is now preparing just such a set of connections. This effort was inspired, in large part, by Congress's request for a list from DoD of the twenty most important military technologies.[5]

One way to represent the connections is a checkerboard matrix. Along the rows would be listed technology areas and along the columns would be listed military mission areas. Each square in the matrix represents the connection between the technology and mission; the numerical value in the square would be the degree of benefit to that mission area deriving from progress in that technology area. A matrix allows us to see at a glance how resources should be distributed to technology areas based on whatever priorities we hold for military missions.

However, many assumptions would still lie hidden in a matrix. Specifically, the degree to which a technology contributes to a mission depends on how we decide to accomplish the mission. For example, if the mission is air defense, we may decide to make this primarily an Army mission, in which case the most relevant technologies may be skewed toward those supporting ground-based, direct-fire, surface-to-air weapons, or we may decide that this is primarily an Air Force mission, in which case the most relevant technologies may be skewed toward those supporting aircraft and missiles. This mission assignment may have more to do with service rivalries and historical roles than with any objective technical evaluation and it may remain a hidden assumption, but its effect would, nevertheless, show up in the degree of connection between a technology and a mission.

Evaluating Research Projects

Having decided what technical areas to support, we must still evaluate specific proposals for research. By what criteria should we assess a proposal? We have included criteria in estimates of expected benefit. Clearly, the policy makers' security desires and strategic goals discussed thus far define 'benefit." But caution also must be exercised because a lot of judgment is hidden in the word 'expected." Any estimation must consider lead time, technical feasibility, application and ancillary technologies, military mission, countermeasures, civilian contributions and applications, and, of course, cost.

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5 The most recent report is, Critical Technologies Plan (Washington, D.C.: U.S. Department of Defense, 15 March 1990). Appendix B contains the Congressional request for the critical technologies plan.

Lead Time

The first critical consideration is lead time: when do we expect to see the benefit of the research program? Time is important for, among other things, the simplest accounting reasons. The amount of money we are willing to spend now in order to save money later depends on future real interest rates. Financial fluctuations that have nothing to do with the technology in question could at times be more important to our calculations of cost and benefit than technical uncertainty. In many cases, we can trade time for money. (That is, a 'crash" program can accomplish the objective fast but with less efficiency, hence greater total cost, than a longer, more methodical approach.) In general, as the cost of capital increases, so does the economic incentive to delay research as long as possible.

The length of time before we see the results of a research project also determines the degree of technical uncertainty in expected benefit. In general, the earlier in the research phase a technology is, the longer it will take to get to an application, the more potential unseen pitfalls can lie ahead, and the more likely that the research project is to result in nothing at all. This uncertainty in success reduces the expected benefit. If, on an arbitrary scale, a research portfolio would have a benefit of 100, assuming everything went as hoped, but we estimate only half will succeed, then the expected benefit of the portfolio would average to 50.

Likelihood of Technical Success. .

Estimating the likelihood of success is probably the most difficult and subjective part of any assessment of a technology research proposal. Research is, by definition, exploring new territory; no one can make accurate predictions based on experience. Scientists and engineers can argue by analogy, estimating the difficulty of some project by comparing it to whatever past project seems most similar. For example, by looking-back at the development of silicon-based transistors and integrated circuits, they can guess at some of the problems that will be encountered during the development of similar components using gallium arsenide. In the end, some estimates of feasibility will be little more than guesses. Moreover, since a certain amount of confidence is required to propose a research project and a natural human tendency is to discount pitfalls that cannot be seen, there may be a bias toward underestimating potential difficulties.

In practice, we also argue by 'analogy" with respect to people and laboratories. Even without understanding the details of the research, we can recognize success when we see it and we can see that certain people and laboratories develop proven track records. We are naturally more likely to assume a higher probability of success for research projects under their direction than for those under the direction of less successful researchers. In short, we trust them. Moreover, research projects can be difficult for Congress to oversee and audit once funds are allocated. Thus, an important

function of testimony and documentation presented to Congress is not to provide detailed reviews of research projects but to establish and justify the trust in the people and institutions that are to do the research.

Military Mission.

As we look further into the future, other uncertainties extraneous to the research and technology itself increasingly come into play. First, will the military mission still be needed when the technology is ready? For example, before the development of the intercontinental ballistic missile (ICBM), the Air Force devoted considerable resources to intercontinental-range cruise missiles and both the Army and Air Force devoted much effort to continental air defense. With the demonstration of the ICBM, however, both missions suddenly were much reduced in importance. At the time of the first ICBM flight, it must have appeared in retrospect that research resources expended up to that point on intercontinental cruise missiles and air defense were, to some extent at least, wasted.

Multiple potential applications are a hedge against the loss of any particular application. If a technology is applicable to a single future mission, its utility stands or falls with that mission. Typically, however, a technology will have multiple applications. When calculating benefit, the total potential benefit from all end uses must be included. When estimating the likelihood that a need for the technology will remain when the technology is finally ready for use, the overall likelihood of the collection of potential uses must be considered. In general, the later in the development process a project is, the more specific the application will be. A materials scientist investigating high temperature materials in general may be able to see in rough outline application in many types of engines, although perhaps years in the future. On the other hand, an engineer designing a specific new part for a specific engine must be able to achieve some predicted gain immediately.

The future utility of a weapon depends on the possibility of countermeasures. No weapon is useful forever; obsolescence is inevitable. The question is one only of degree: how long before some new weapon overtakes the one we have now. An important consideration regarding a new technology is how long will countermeasures take to develop. In some cases, countermeasures could be fielded faster than the new technology or a new technology could be countered by a quick change of tactics, suggesting that we should not even bother to set off down that road.

Ancillary Technologies.

Just as some technologies have several applications, some applications require several new technologies and a balanced approach to developing the technologies must be maintained. The ICBM can once again serve as an example. Many new technologies were required to make ICBMs possible; three important ones were efficient rocket motors, accurate **guidance systems, and high-**temperature reentry nose cone materials. If any one failed, then all were

useless (for application to our example of ICBMs, at least). Therefore, while setting priorities for any particular research program, the interaction of other parallel technologies must be considered. Funding one critical technology does no good if the others are not funded, too. Each must be funded at the level required to assure a reasonably high probability of success to have any hope of getting them all. (If a mission requires three new technical capabilities, each of which is only fifty percent likely, then the likelihood of getting all three is only one in eight.) Finally, we must consider the cost of the whole package of required technologies. Funding for rocket motor development alone may seem a reasonable price to pay to get an ICBM, likewise for funding of development of either nose cones or guidance systems, but the costs must be considered all together.

Technical Obsolescence.

Long-term research projects also run the risk of being overtaken by other technical developments. We can imagine a research project being started in 1945 to develop very small, low-power vacuum tubes. With the invention of the transistor in 1948, almost all of the work on such a hypothetical project would have been in vain even though the overall application, electronic devices, was very much in demand.

The risk of technological leapfrogging, as the transistor leapfrogged the vacuum tube, does not mean that we should never pursue two routes to the same goal. Multiple routes often are the rational choice. For example, one technical approach may be preferred but its success judged uncertain, while another approach, although a less attractive technical solution, may be judged very likely to succeed. The first may be funded because we want it and the second funded as an insurance policy. However, we must keep in mind the reason we develop a new technology. We spend money for capability, not technology, per se. Therefore, when we pursue parallel technical solutions, we have to sum the cost of developing each approach to fully account for the total resources being expended to achieve the new capability.

Revolutionary breakthroughs like the invention of the transistor or the development of the ICBM are unlikely in any particular year, but the further into the future we must go before we reap the benefit of today’s research dollar, the less certain we can be that our planning will account for all of the future changes that will occur, and the less certain we can be that there will be any benefit at all. This does not mean that we should let uncertainty stop all long-term research; indeed, research and certainty are to a large extent mutually exclusive. Rather, we have to be able to expect greater <u>potential</u> return, or “leverage,” on investment the farther into the future we plan. It may be worth spending a dollar this year to save a dollar and ten cents next year, but if our return occurs in decades, we need to be able at least to <u>hope</u> for ten dollars in savings to make up for all those cases where our research doesn’t pan out at all.

Spin-off.

The government must consider civilian spin-off (and indirect civilian costs) when comparing the costs and benefits of military research. "Spin-off" is any technological bonus that the civilian economy can collect from military research. There is little agreement on the extent or net benefit of spin-off from military research to the civilian economy.[6] Some argue that military research drains limited resources, both in money but especially in skilled manpower, from civilian tasks. If this is the dominant effect, then military research actually retards civilian technical development and we might speak of "negative" spin-off.

Others argue that money spent on military research should be seen as a pure addition to civilian research, not as competition for civilian research funds; that if the resources were not justified by military needs, they would not go to any kind of research at all. If this is true, then civilian research is getting a no-cost bonus.

Most studies of spin-off come to an intermediate position, concluding that the civilian technology base benefits somewhat from military research but not as much as if the resources were applied directly to civilian ends. Another way to express this intermediate position is to say that every dollar of military research benefits civilian technology by some fraction of a dollar. What that fraction is varies from one technology to another. It may be high for jet engines and near zero for nuclear weapons technology. However, most agree that the relevant data are difficult to obtain, conclusions are not firm, and often the support for a position is" anecdotal.

Experience with spin-off from the recent past may not be a reliable guide to the near future. If we look over the last few hundred years, we see that sometimes military technology has led civilian technology and sometimes civilian led military. Immediately after the Second World War, the superpower competition was new, the U.S. military industrial base was primed while the civilian industrial base had been starved, and many new technologies were ripe for incorporation into civilian hardware. Many of the oft-cited cases of military to civilian spin-off may be artifacts of those peculiar circumstances and we should not necessarily expect comparable spin-off from military research projects started today.

Alternate Sources of Support.

Support of military research does not have to be provided entirely by the government. In most cases, most research funding will come directly or indirectly from the government, because military security is a public good and

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6 A great deal has been written on the economic effect of military spending. A recent report containing a review of the important existing literature is David Gold, The Impact of Defense Spending on Investment, Productivity and Growth (Washington: The Defense Budget Project, 1990).

the government is the sole customer for the results of military research. In some cases, however, industry may calculate that support of research is justified. This will occur whenever industry sees predictable military need or some non-military application. Whatever the reason, when estimating the level of necessary government support one must judge the total level of support required and the amount that might come from non-government sources.

Similarly, government support need not mean only United, States government support. We will wish to develop strictly independently a few technical areas. However, in most cases we share common security interests, hence common military technology interests, with allies. When judging the level of U.S. government support for those research activities that must be supported by government, we should keep in mind the potential contribution from allied governments.'

The Threat.

The military enterprise is inherently competitive and what we choose to do is inextricably bound up in what other nations choose to do. In a hypothetical, isolated economy, decisions about research funding would depend primarily on choices between consumption now and more consumption later. That is, we could reduce our present consumption to invest in research which would increase future efficiency and hence allow increased future consumption. In contrast, decisions about military research funding depend critically on external factors: our estimates of the future threats. We must judge the likelihood that potential enemies will develop some surprise threat or some countermeasure to an important technical capability. This likelihood will be roughly proportional to what they are spending on research. We must also judge the importance of maintaining a relative technological lead. Not all technologies will be equally important; in some areas we can accept a lag if we have compensating strengths. In any case, we cannot judge research spending in absolute terms of adequacy but must compare it to the levels of funding for research spent by potential adversaries.

Summary.

In summary, the criteria for evaluating research programs should include at least:
(1) the length of lead time before the technology will produce result,
(2) likelihood of technical success,
(3) number and importance of the technology's military applications,
(4) the time required to develop countering technologies or tactics,
(5) number and difficulty of required ancillary technologies,
(6) the risk of being overtaken by parallel technical developments,

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7 U.S. Congress, Office of Technology Assessment. Arming Our Allies: Cooperation and Competition in Defense Technology OTA-ISC-449 (Washington, D.C.: U.S. Government Printing Office, May 1990).

(7) the extent of civilian spin-off (or unintended civilian costs),
(8) alternatives to U.S. government support including industry and allies, and
(9) the overall threat posed by potential adversaries.

Managing Defense Research,

Explicit research goals allow evaluation of research proposals and explicit, perhaps even quantitative, ordering of research priorities. However, even if Congress knows what it wants to do, how can Congress see that it gets done? The problems are formidable. Research is funded at a very high level of aggregation. Almost all of the details of distribution are left up to the research managers in the services and in the defense laboratories. Any more specific allocation increases the burden on Congress, which is ill-prepared to consider the details of research projects, and is bound to be resisted by the research institutions. Institutional resistance to redirection of effort is difficult to overcome (or even detect!) in the case of research because the categorization of research is so flexible.

If Congress wishes to use a quantitative resource allocation model (or decision support system, as they are sometimes called), then one approach would be to provide the research directors with the judgments necessary to create a quantitative 'benefit scale" that the researchers could use to rank research projects. In commercial firms such a benefit scale can be profits. For example, a researcher may develop a new process and the process engineers, considering the overall production process, would determine how much the process would reduce production costs. Alternately, a researcher may develop " an improved product and the marketing department would determine the -increase in sales for each degree of improvement (or determine the more complex case of increase in sales for each degree of improvement relative to the competition) .[8]

What are the analogies for Congress? The benefit of military research is security which, like a production process, is a composite of many things. In analogy with the process engineer, Congress must specify how much a reduction in short-term threat is worth relative to a long-term threat, or a nuclear threat compared to a conventional threat, and so on. In analogy with the marketing department, Congress must specify how some new capability would increase this thing we call 'security." Any particular individual would have some difficulty devising such a set of values. Anyone can quickly see that reaching agreement among a group on such an explicit statement of values could be impossible. (For example, there is much debate about whether a bilateral deployment of partial ballistic missile defenses would enhance or detract from deterrence and whether tactical nuclear weapons increase or decrease the

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8 Many companies use some version of this method. One case described in the literature is Philip V. Rzasa, Terrence W. Faulkner, and Nancy L. Sousa, "Analysing R&D Portfolios at Eastman Kodak: A methodology based on decision and risk analysis can improve R&D productivity," Research and Technology Management, Vol. 33, No. 1 (January-February 1990), pp. 27-32.

likelihood of strategic nuclear war.) In any case, this essay argues that this quantitative approach is more applicable the closer we get to the lab bench, where Congress is least suited to provide direction. And where Congress is best suited to provide guidance, the approach is least applicable.

In principle, such an explicit evaluation process also allows determination of the appropriate level of total funding. In a commercial firm, an analysis of this type produces a specific, quantitative value for expected return on investment. Any firm with discretionary spending available could fund research projects in the order of their expected return until the expected return is not greater than what the firm could receive from, say, the commercial bond market. Perhaps the analogy for Congress is the decision it must make between spending on research to meet some hypothetical future threat and spending on procurement to meet a present threat. In practice, commercial firms are more likely to decide on some general level of funding for research and find the best allocation within that level. This might be an absolute level of funding, so many dollars a year, or calculated as a type of "tax" on profits; that is, say, 10 percent of profits will be plowed back into the research budget. Congress could adopt a similar approach to help assure adequate research funds. Congress could designate some fixed percentage of procurement funds to research budgets or designate some floor for research to forestall raids on the research budget. Of course, such formulas will inevitably be somewhat arbitrary. They also cannot be permanent; they need to be adjusted for changes in the threat.

The quantitative, somewhat mechanical, approach decision support systems will not work for Congress but that does not mean that Congress must just rubber-stamp whatever the Department of Defense suggests. There are intermediate courses available, between specific direction of particular research projects and accepting the entire research proposal as presented. Congress can require that the Department of Defense formulate clear defense policy or, failing that, Congress can develop defense policy itself.

At the very least, Congress should be able to review those goals that the DoD develops. It does little good for Congress to make certain that research is supporting defense goals if Congress does not support the defense goals themselves. For example, the first of the "major long-term goals of the investment strategy" expressed in the most recent DoD Critical Technologies Plan, listed under the heading "Deterrence," is the development of 'weapon systems that can locate, identify, track, and target. strategically relocatable targets."[9] The consensus of four decades of scholarship on strategic deterrence is that untargetable systems--for example, ballistic missile submarines- -enhance crisis stability, so presumably systems that make them targetable detract from crisis stability. This particular goal implies, then, a relatively greater weight for deterrence through capability for war-fighting and relatively less weight for deterrence through threat of retaliation. There are strong arguments on either side of this debate, but whether this weighting is appropriate or not is a question with which Congress may very well wish to involve itself.

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9 See Critical Technologies Plan, p. 3.

With a statement of policy in hand, Congress can require that the Department of Defense demonstrate how the forces that it wants for the future will support the military policy and, finally, how its. research programs will make those forces possible. That last step is the subject of this essay: determining how research priorities ought to be set to assure that future military needs are met. In principle, Congress could direct resources to particular research projects. In practice, it is more effective to relieve Congress of the burden of having to look over the shoulder of" the scientist at the laboratory bench and allow those scientists--who are most familiar with their work- -to make their own research decisions, but within a context of priorities approved by Congress.

Congress can do much to clarify discussion of research by specifying formats for questions and presentations about research programs. The list of questions presented earlier is an example of a type of 'checklist" that could provide a format for evaluating research programs. If testimony before Congress always used a similar format, then comparisons among them would be easier.

Also, format can influence thinking. The recent Congressional request from DoD for descriptions of the most critical technologies forced DoD to be explicit about what technologies it considers critical and why. So even before Congress read the report, DoD had learned something that Congress wanted it to learn.[10]

Probably the greatest requirement for intelligent oversight is understanding the connection between military technology and military missions. Such understanding allows Congress, which can render judgments about the importance of missions, to rationally allocate resources to technology research areas. The "linkages" described in the second annual DoD Critical Technologies Plan[11] is a good start on this problem that should be maintained, encouraged, and expanded.

An explicit statement from Congress of its objectives allows the research community to try better to reach those objectives. The community can hardly be faulted for not responding to the strategic goals of Congress if it does not know what they are. If Congress could reach consensus on overall strategic goals, then the Department of Defense would find itself under severe pressure to respond directly by showing how it planned to meet those goals.

Occasionally, Congress may want to specify funds for particular research areas. The direct approach is to identify particular research projects in budget line items. Alternately, research funds could be specified for particular missions. Such mission-specific direction often can be

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10 A description of the general approach is contained in Les Aspin, "The Power of Procedure," in Alan Platt and Lawrence D. Weiler, eds., Congress and Arms Control (Boulder: Westview Press, 1978).

11 See Critical Technologies Plan, p. 4.

circumvented in research because the area boundaries are poorly delineated. Another approach is to direct funds to particular organizations or laboratories. For this approach to be most efficient, the missions of the organizations or laboratories must match up with the way that Congress wants to be able to emphasize research priorities. For example, if laboratories are organized by military service, as they are now, it is simple to shift research priority from Navy research to Air Force research by shifting funds from Navy laboratories to Air Force laboratories. However, shifting funds from one technology area to another will be very difficult if the shift conflicts with the priorities of the Service running the laboratory. It would be easier to shift funds from one technology area to another if the Department of Defense had laboratories organized by technology area. The relative advantages and drawbacks of various laboratory organizations has been discussed in other recent OTA reports.[12] Simplifying allocation of research funds is unlikely to be the determining factor in organization decisions but it should at least be included for consideration.

An important objective of Congress's oversight is to discover problems as early as possible. To illustrate the challenge of oversight of the research and development process, we can use a pipeline as a simple model. In this metaphor, research gets shoved into one end of the pipeline and military capability comes out the other end. There are many intermediate steps including development, testing, and production. The danger is that, if we suddenly stop shoving the research in, do the wrong research, or do it the wrong way, then new products will still come out of the pipeline for years because of the research we had done years ago. When the flow out of the pipeline finally stops, we might look in only to see the pipeline empty for years to come. Just as it took years to empty the pipeline, if we begin corrective action immediately, it could take years to get the pipeline full and producing again.

Even if Congress is itself not well-suited to detect problems, it can require that procedures be followed which will help assure earliest possible warning of any problems that do occur. Many mechanisms could help alert the Congress to potential trouble. (The research community is aware of the danger, so these types of mechanisms are already in place in one form or another.) For example, Congress could:

(1) Require outsider review to avoid parochialism. This means people from other research groups, outside laboratories, other military services, civilians, and other scientific disciplines.[13] These outside groups can also try to find innovative countermeasures to new technologies.

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12 U.S. Congress, Office of Technology Assessment, Holding the Edge: Maintaining the Defense Technolgy Base, OTA-ISC-420 (Washington: U.S. G.P.O., April 1989).

13 Alvin M. Weinberg, "Criteria for Scientific Choice," Minerva, Vol. 1 (Winter 1963), pp. 159-171.

(2) Require clearly defined milestones and consider the research project in its entirety. Reviews should include comparison to previous long-term projections of progress. In other words, in addition to reviewing progress over the last year, compare initial plans with progress over the entire research project. This will illuminate research areas that are not on track, expose the project that is 'just one year from completion" year after year, and encourage more realistic appraisal. Also, when research projects fail, a careful case study could help avoid similar problems in similar fields. (But be careful defining 'failure." A research project may seek "to answer the question, "Is X possible." The answer may turn out to be "No," but that does not constitute failure.)

(3) Act as the guarantor of research and keep research visible at a high level. Some research problems will not fit neatly into the mission of any particular laboratory or service. Someone at a high level must make certain that these problems do not fall between the cracks. Another problem of managing research is that its importance is long-term and greater than its proportion of budget suggests. Bureaucratic attention, in contrast, tends to be proportional to immediacy and dollars at stake. Also the military, which will tend to worry more about present threats than potential future threats, will tend to favor procurement over research. Therefore, when forced to make the choice, often wholesale cuts in research will be proposed by the military to avoid moderate cuts in procurement. Congress could help maintain continuity in military research and could help to keep research high-profile.

Conclusions

Setting military research priorities will never be easy. The task has two parts. One is setting strategic goals. The other is judging which particular research programs will best help reach those goals. (Once a research program is approved, there is the separate task of monitoring it to see that it is well run.) Research by its very nature deals with unexplored terrain so we will never have the pleasure of being absolutely certain of where we are going or how we will get there. On the other hand, we can reduce ambiguity whenever we can set explicit goals and agree on explicit criteria.