Science and National Security*

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WE are sometimes inclined to think that the phenomenon of science being of service to the nation is a new development of recent years—say since 1940. However, science has always been a national asset and has always been in the nation's service. Science and technology have been among the decisive influences that have improved the lot of the common people and thus made this nation fine and strong.

Even in the restricted sense of science directly serving the government, rather than the people generally, we are not dealing with a new phenomenon. From the day the postage stamp was first put in general use, the United States Government has needed and has used science and technology, not only to promote the general welfare, but also to advance the special functions of the Federal Government, including the function of national defense.

This long experience has led this nation to the conclusion—a conclusion not always expressed or adequately implemented—that the government and the nation are heavily dependent for strength, welfare, and security on science and technology. Hence, the government must do two things: (i) make provision within the framework of government to carry on scientific and technologic activities directly related to specific government needs and (ii) encourage and support throughout the country a strong nongovernment science and technology. Both of these activities are necessary for a strong and prosperous nation in time of peace; both are vital to military strength in time of war.

However, it is one thing to recognize these two facts and obligations; it is quite another thing to do anything sensible about them. The history of government relations with science is replete with examples of good intention and bad implementation. A desirable agency is established and then deprived of funds. Laws enunciating high-sounding ideals are passed, and then the men who are appointed to implement them are denied secretarial help and travel funds. Scientists are saluted as being essential to the nation's progress and then are drafted as privates in the army or employed by the government under policies set up for postal clerks.

I am told that men who have been in politics for many years do not expect political institutions in a democracy to be either reasonable or consistent on any policy matter. The implementation of policy will be in the hands of many people who have differing views on various aspects of the policy, and these people will change with time; hence, there is bound to be confusion.

Those of us who are less experienced, however, still think of "the government" as an entity with a mind and a purpose. And we shall never get over the shock of seeing the government do one thing with one hand and a wholly contradictory thing with the other.

Some people react to this paradox by turning their backs on government entirely, refusing to have anything whatsoever to do with it—except when forced to serve in the Army or to pay an income tax. However, most reasonable people, although they sigh sadly at the vagaries of a democratic government, realize that it is the only government we have and try to put up with it, to help it, and even try to make it better. In time of war we all do this. In time of peace it is more difficult to do, but it is no less necessary.

It is from this point of view that I wish to discuss the question of the ways by which science and technology can contribute to making the nation strong enough in a military sense so that it can achieve its national aims and ideals in the face of opposition from potential enemies. In short, what are the ways in which science promotes national security?

This simple question does not have a simple answer. The roots of national strength extend deeply into the national pattern of living, into the nation's industrial, social, educational, and economic systems. We can not trace all these roots. But we may select a few essential items that bear on the answer to our question.

In the first place, we now understand that both a strong military and a strong industrial technology must rest on a solid base of *fundamental* science. Indeed, all technology grows out of discoveries in basic science. There are thus very practical and immediate reasons why the government should encourage the building of a strong science: (i) As I have said, new knowledge is essential to future progress. (ii) The corps of scientists engaged in basic science will be among the most able and imaginative in the country. Their numbers should increase, not only because of the scientific advances they will produce, but because they stand as a "ready reserve" available to accelerate the development of military technology when emergencies arise. (iii) Basic science in universities is the sole source of education of future scientists and technologists. The well-springs of future talent must be maintained and enlarged. (iv) Basic science in relevant fields has an important place in laboratories of applied science. It improves the intellectual tone, stimulates the imagination, satisfies the curiosity, helps attract new scientists, and, of course, fills in essential gaps in knowledge.

* Based on an address delivered at the dedication of the new building of the Applied Physics Laboratory, The Johns Hopkins University, Silver Spring, Md., on 16 Oct. 1954.

How do we stand in this area of basic science? Are we as a nation doing all right? I am convinced that the answers are in the negative. But I am also aware that there have been important forward steps during the postwar years.

First, the military services, realizing the importance of scientists to wartime technology, have aided mightily since World War II in financing a rehabilitation of university research. The funds made available for equipment, for services, and for graduate student support stimulated a spectacular postwar development in university science laboratories. We will forever owe a debt, particularly to the Office of Naval Research, for what was done in this respect. The Atomic Energy Commission, the Public Health Service, and others have made similar contributions. They have shown that government can encourage university science without the necessity of damaging political interference, provided, of course, that the universities themselves remain alert.

A second step in advance since the war is that most American industries have increased their research expenditures by an amount which, for the country as a whole, adds up to a very large factor. Industries have increased the amount of basic research in their own laboratories and have made substantial grants to support research in universities. This is excellent, and I hope the latter practice especially will greatly increase.

In the third place, the government itself, especially the military agencies and the AEC, have put their research programs on a long-term stable basis and have made provision within the government laboratories for such basic research as is appropriate and relevant in each case. I am not saying there is agreement on how much and what kind of basic research is proper or relevant in each laboratory, but I do say that the principle has been recognized.

Fourth, the Congress finally passed the National Science Foundation bill and has given it modest funds to get its program started. Its fellowship program is already an outstanding contribution.

All of these steps have helped to strengthen research, have helped in the education of students, have helped to attract young men into science, and have also brought about a degree of contact, of friendship, of understanding between the scientists and the military that was unheard of before 1941 and will be a mighty element of strength in future (and current) conflicts.

But there is much that has been left undone; much that has been done inadequately. The National Science Foundation has been treated with shameful niggardliness by the Congress, and its role and potentiality seem to be almost wholly unrecognized on the Hill. Someone on the Hill started the idea that the National Science Foundation was to "eliminate the tremendous duplication" in basic research. This is one of the most tragically mistaken delusions of recent years. Duplication in basic research is, in the first place, a contradiction of terms. Research is the seeking of new knowledge. If the knowledge has been found, no one else will seek for it—so duplication is most unlikely. If someone wishes to check up on a discovery and repeat an experiment, this is certainly not duplication. Science by definition, deals with those phenomena which anyone can duplicate at will.

Should one then say that it is duplication for two persons to be searching for the same knowledge? This is like saying that it is duplication for more than one person to search for a child lost in the woods! We all know that when many people are used the search is greatly accelerated. And so when one is searching the infinite wilderness of the unknown for an uncountable number of undiscovered pieces of knowledge, it is clearly evident that the more people engaged in the hunt, the better. And no one will find the same child twice. If two scientists happen to run onto the same discovery at the same time this is good, not bad, for it gives an immediate confirmation of the validity of the finding.

As long as science is done in the open, not in secrecy, duplication is, by the very nature of science, nonexistent. If all Congressmen could learn this one simple fact, it would make the task of the National Science Foundation much easier.

But the duplication idea leads to another illusion: Science will be more efficient or effective if it is all under one management. In order to move in this direction, the Congress, when it increased the National Science Foundation funds this year, imposed an almost equal decrease in the funds allocated to other agencies. This was most unfortunate, for it caused disruption of existing programs and a net decrease in the amount of research in progress. Furthermore, this policy ignores the fact that in science—as in education—*diversity* is our most precious asset. No single agency with a particular policy, a particular program, a particular group of advisers, a particular staff can possibly accommodate all the diverse needs of science. It is almost as bad as trying to decree that all children should have the same father!

So I suggest that in the field of basic research we have an educational job to do. We need to convince industry and the government of the value of encouraging basic research. We need to convince the executive and legislative branches of government that all agencies concerned with science and technology should encourage and support basic research, in their own laboratories and in universities; that the more agencies that are doing it, the more effective and productive our program will be; that the dividends that will be repaid in new knowledge, in more scientists trained, and in more scientists brought close to the government will be worth a thousand times the investment—and the dividend may indeed be the survival of the nation.

Let us turn now to some problems of *applied* research conducted by the government, confining our attention to research carried out for military purposes by the Department of Defense and the AEC. Applied research is in many ways a very different animal from basic research. It is true that the scientific training required is much the same and quite similar techniques and equipment are involved. But the east-bound and west-bound sections of a streamlined train look similar too. It is just that they are not headed for the same place.

Applied research is research aimed at a goal—a better or improved weapon, a new industrial product, or a cure for a disease. Because the goal is established or agreed upon (this is important, of course!), it is possible to organize the attack on the problem, assigning a number of specific tasks or areas of investigation to different individuals or groups. Usually also it pays to avoid duplication, for it is usually less efficient to have two groups assigned to the problem of developing the same weapon than to have them combine forces. Also, since applied military research is usually necessarily secret, it is important to establish coordinating mechanisms to avoid the waste of unknowingly repeating what others have done.

For these and many other reasons it is important, when talking about government activities in science, to distinguish sharply between pure and applied science. What is good for one may not be good for the other.

Now it is not surprising to note that the military services and the AEC have developed not one but many patterns for carrying on applied research. A democracy never does anything in a unified or monolithic way. This is both our strength and our danger. The strength is that diversity of approach and individuality are conducive to new ideas. The danger is that in military research resources are scattered and ineffective, policies are confused, and high priority tasks are neglected.

It is proper that the direction of all military research has *not* been consolidated under one office in the Department of Defense, but that there has been created there one office to give guidance and coordination to all the service agencies.

It is also neither surprising nor disturbing to find the research pattern different in the three services. Yet each service needs one office that gives rather close attention to the supervision of the whole program of that service. And each service needs an effective mechanism for keeping new developments closely tied to plans, to requirements, to logistics, to tactical development. This is probably the area in which there is greatest weakness. New weapons are produced in ignorance of tactical requirements; they are introduced without adequate study of their tactical possibilities, without adequate logistics and maintenance and training. Military plans, on the other hand, are sometimes drawn up without taking into account new weapons that will shortly be available. There is inadequate attention given to clarifying the goals of the military research program and making clear to every agency its part and purpose in the program.

Many people are aware of these shortcomings, and many in the military establishment are trying to remedy them. It is not my task today, or any day, to tell them how to do the job. We certainly do wish them success.

The *mechanisms* by which military research is carried out also show great diversity, and this is both desirable and troublesome. Broadly speaking, military research may be carried on either in a government-owned and government-operated laboratory, in a government-owned contractor-operated laboratory, or in a contractor-owned contractor-operated facility. In true military style these are, I understand, called respectively: GOGO, GOCO, and COCO.

Examples of GOGO are the Naval Research Laboratory, the Army Ordnance Arsenals, the Fort Monmouth Signal Corps Laboratories, the Naval Ordnance Test Station (Inyokern), the Wright-Patterson Air Base Laboratories. In each case the facility is under the command of a military officer, and there is usually a chief civilian scientist to whom varying degrees of responsibiilty for the technical program may be delegated.

In the GOCO class, there are such laboratories as Los Alamos, Argonne, and, indeed, all the AEC laboratories, plus such facilities as the Jet Propulsion Laboratory operated by California Institute of Technology and the Lincoln Laboratory operated by Massachusetts Institute of Technology. In these laboratories the land, buildings, and equipment are owned by the government. However, after the general goals of the technical program have been agreed on jointly between the government and the contractor, then the contractor is given managerial responsibility for all operations. All personnel and other administrative policies are set by the contractor—usually to be in line with those he employs in other operations.

Finally in the COCO class come the great mass of contracted projects in industries and in universities. Some are small basic research projects related to a military problem. At the other extreme are vast industrial projects for development of a new airplane, a new radar, or a guided missile. Then there are establishments like the Johns Hopkins Applied Physics Laboratory and the Rand Corporation, where a privately owned facility is wholly devoted to government work—but in these cases to a broad program of applied research rather than to the building of a single device.

Now it is easy enough to classify military research centers in this way. But it is not easy to draw any general conclusions from such an exercise. It would be nice if we could say that all laboratories of one class were dismal failures, while all of another were great successes. But in human affairs things are generally not so simple. One can find examples of extremely successful and productive centers of all three kinds. One can also find members of each class that have not been as productive as could be expected.

The reason for this is not hard to find: research success is the product of good ideas; and ideas cannot be manufactured like automobiles in a production line. Ideas arise in the brains of individuals, and they arise under circumstances that no one—not even the individual himself—understands. We *do* know how to increase the *probability* of new ideas arising. There are some simple rules for this: (i) Find some well-trained people who have been successful in getting ideas in the past. (ii) Give them full information about the nature and importance of the problem being tackled. (iii) Keep them in close touch with one another and with others engaged in similar work in a way to allow the maximum interchange of ideas, for out of such interchange and stimulation new ideas are frequently born. (iv) Provide these people with the facilities and help they need in developing and testing their ideas. (v) Keep the environment, the atmosphere, and the administrative arrangements such that there is the maximum stimulation to imaginative thought processes and the minimum of interruption and frustration.

Even these rules are not very specific. They do not tell how to find the right people, just how they are to be thrown together, what facilities they will need, or just what arrangements provide maximum stimulation and minimum frustration. All of these are delicate and subtle matters. They are also variable; a combination that works in one set of circumstances with one set of people may not work with others. Arrangements that are most satisfactory during an all-out war may prove hopelessly unsuitable in time of peace. An organization operating beautifully under one director sinks into mediocrity or worse under another. A research team that has delivered an outstanding contribution in the form of one new weapon falls to pieces when that job has been done and never quite "clicks" on another.

All of these are, as I have said, subtle and difficult problems. Their solution depends on the ability, the intuition, the adaptability, the imagination of relatively few people, possibly of only one person, in each organization. One person who can judge people, who can sense the spirit of the group, who can anticipate difficulties and avoid them, who can stimulate enthusiasm can make a successful team under almost any circumstances. And wherever you find a highly successful group I suggest that you seek the causes for its success not in the organization chart, not in the budget book, not by counting uniforms or rank but by finding a man or a small group of men who have created the spirit of the place and who know how to preserve that spirit.

If one keeps in mind the essentiality of this intangible "spirit" of a research organization, one can understand why there are so many arguments about the best way to "manage" military research. There are, in fact, many ways to "manage" it if the spirit is there. There is *no* way to manage it if the spirit is absent. From this point of view, many familiar arguments fall into proper perspective.

For example, the argument about whether it is best to have a military or a civilian director of a laboratory misses the point. A *good* military officer will clearly be better than an incompetent civilian, and vice versa. The main advantage of a civilian—assuming equal competence—is the matter of *continuity,* and continuity is very important in maintaining spirit. Also one is more *likely* to find inspired research directors among civilian scientists, because many years of experience in research is helpful in developing this innate feeling for its spirit. Experience in a fighting organization is not intended to develop this same characteristic.

Again, the arguments about the relative merits of government operation versus contractor operation now fall into perspective. The government can and does find and employ good civilians, and it has created some excellent research centers. However, the difficulties are somewhat greater. A government civil service bureau set up to employ a million clerks and secretaries may not be an efficient mechanism for employing a few hundred scientists. And a government financial system geared to a budget of 70 billion dollars may lack some of the flexibility needed for the rapidly changing needs of a research laboratory. Thus the government, in delegating research operations to a contractor, buys an important asset in this flexibility of personnel, administrative, and financial policies which a contractor may supply. And this flexibility is, in turn, an attraction to good scientists too, thus increasing the probability of finding and maintaining high-quality leadership. The objective must always be to find a mechanism that is appropriate to the task and has the greatest probability of developing and encouraging the creative spirit of research and bringing that spirit to bear on the specific military problem at hand. An able team, ably led, that understands where it is going and why will surely get there.

This brings me to a problem that I think neither scientists nor military agencies have solved. This is the problem of trying at every stage in the development and use of new weapons to bring together scientific *and* military experience. Every time an intensive effort has been made to do this in a particular area, the results have been most fruitful—sometimes spectacularly so. But this should be a *continuous* process. It seems to me that it is the responsibility of every scientist and engineer who is working within a laboratory devoted to military purposes to keep himself continuously informed of the nation's broad military problems and of the specific ones in his area. If he is working on a radar, he should inform himself fully of the military situations in which it is to be or could be used. If he is working on a missile, he should know what it is for, what other equipment it will be used with, what military problems it is intended to solve. A weapon designed in the dark, no matter how technically clever it is, may be of little or no military utility.

Conversely, of course, the military agencies have a responsibility. A weapon dreamed up as desirable by a soldier who is without access to knowledge about technologic possibilities may also be a dud—or at least less effective than it could be. The point is that military and scientific people should do their dreaming together. They should be continually exchanging ideas about problems. The scientist may hear of military situations he did not know existed, and potential aids to confronting them may come to mind. Similarly the military officer, hearing about new weapon possibilities, may see new possibilities for their tactical use or how they could be adapted to new situations. The idea that the function of the military is to tell the scientists what weapons they need—and that the function of the scientists is to deliver them without argument—is as obsolete as the idea that the scientist can toss new weapons at random at the military services and expect them to find a use for them.

Weapon development is a tough business and requires the best combined talents we can muster at all stages of the enterprise. If military secrecy is interfering with this intimate meeting of minds, then secrecy is working against national security, and it is time that *real security* considerations come first.

If I were to express a hope for the future of the Johns Hopkins Applied Physics Laboratory, it would be that, as it maintains and develops the spirit of research that I have been discussing, it also becomes a meeting place where scientists and military men meet together to discuss broadly and intimately and vigorously the problems of the military defense of this country. Out of such discussions will come new and important concepts in the field of military weapons and their uses. For your business and my business is not just a better device for this or that purpose, it is, rather, nothing less than the safety of this nation. And it is your responsibility and mine—not someone else's—to insure that each of us is making his most effective contribution to that end.

New Horizons in Cancer: Cytology in Research and Practice*

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GENERALLY speaking, the early diagnosis of cancer offers the most hope for successful treatment. This doctrine has been the principal theme of efforts by the National Cancer Institute, the American Cancer Society, and other cooperating groups to improve case-finding. Educational programs to alert the public and to aid the physician in cancer diagnosis and treatment are yielding valuable dividends. Education alone, however, is not enough. Urgently needed to ease the cancer case-finding burden are practicable screening methods. An ideal solution to this problem would be a simple, inexpensive chemical or blood test as useful as the Wassermann test for syphilis. Over the years, many attempts have been made to devise a laboratory procedure that would show whether or not an individual is harboring a cancer. Altogether, hundreds of such tests have been proposed.

Since 1948 a program to evaluate the old tests and to develop new ones has been conducted, with financial and technical assistance from the National Cancer Institute, by investigators at the medical schools of Tufts College and the universities of Washington, Alabama, Tennessee, and Kansas. Much good work has been done by these and other workers in this field and reported in the literature (*1, 2*). None of the tests evaluated so far has been found sensitive and specific enough for clinical use. However, the approach seems hopeful. The fact that certain tests are effective to some extent is an indication that tangible changes do occur in the body of the cancer patient, and that these changes may be measurable in a diagnostic procedure. For instance, it is known that there are changes in the body chemistry of cancer patients. In some patients with cancer of the prostate the acid phosphatase level is increased. Measurement of prostatic acid phosphatase has been developed to the point where several laboratories are evaluating it as a means of diagnosing prostatic cancer. Other promising procedures being investigated include a serum flocculation reaction, the use of radioactive tracers, and means of detecting abnormal steroid in the blood or urine (*1*).

Although a practical general diagnostic test for cancer appears to be still in the future, considerable progress has been made in the development of tests to aid in detecting cancer of specific sites. The most useful of these is the cytologic examination developed largely by George N. Papanicolaou (*3*). In the past few years, Papanicolaou's "baby" has come of age. Today it is established as a valuable complement to other clinical procedures in early diagnosis of cancer, particularly of uterine cancer. Many qualified persons have been trained in the cytologic test, and numerous clinics and physicians in general practice are employing it routinely in cervical cancer diagnoses. Variations of the original cytologic technique have been developed to aid in the detection of cancer of the lung (*4*) and of gastric cancer (*5*). These variations show considerable promise when used in combination with other procedures. Cytology is being evaluated as a screening test for cancer of the genitourinary tract, the rectum, and the colon. Also under study are applications of the cytologic examination to breast secretions and spinal fluid.

The value of vaginal cytology as a detector of early