BASIC RESEARCH AND THE SCIENTIFIC METHOD

13 December 1957

CONTENTS

	Page
INTRODUCTION--Rear Admiral S. R. Clark, USN, Deputy Commandant, Industrial College of the Armed Forces	1
SPEAKER--Dr. Alan T. Waterman, Director, National Science Foundation	1
GENERAL DISCUSSION	13

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Dr. Alan T. Waterman, Director of the National Science Foundation since 1951, was born in Cornwall-on-Hudson, New York on 4 June 1892. He received an A.B. degree from Princeton University in 1913 and a Ph.D. in physics from the same university in 1916. During the next year he was instructor in physics at the University of Cincinnati. After two years military service (private to first lieutenant) with the Science and Research Division of the Army Signal Corps in World War I, he joined the faculty of Yale University and remained in the Department of Physics there until 1948, with leave of absence during 1927-28 on a National Research Fellowship to King's College, London; to Massachusetts Institute of Technology in 1937, and to the Office of Scientific Research and Development from 1942-46. From 1946-51, Dr. Waterman was with the Office of Naval Research, Department of the Navy, in the position of Deputy Chief and Chief Scientist. Dr. Waterman holds honorary degrees of Doctor of Science from Tufts College, Northeastern University and the University of Vermont and State Agricultural College and an honorary Doctor of Law from Cornell College, Mount Vernon, Iowa. This is Dr. Waterman's third lecture at the Industrial College.

BASIC RESEARCH AND THE SCIENTIFIC METHOD
13 December 1957

ADMIRAL CLARK: This morning we have as our speaker Dr. Alan Waterman, the Director of the National Science Foundation. The Foundation was established by act of Congress in 1950 to promote the progress of science; to advance the national health, prosperity, and welfare; to secure the national defense; and for other purposes. The first general purpose listed in the act reads as follows: "To develop and encourage the pursuit of a national policy for the promotion of basic research and education in science."

Dr. Waterman is an eminent scientist, who has served with distinction both in the Government and in the academic world. He served in the Army in World War I, and in World War II he was with the Office of Scientific Research and Development. From 1946 to 1951 he was Deputy Chief and later Chief Scientist in the Office of Naval Research. Besides being Director of the National Science Foundation, he is a member of several other important boards; and only recently the President appointed him as chairman of the Interdepartmental Committee on Science, Research, and Development.

It's with great pleasure that I introduce Dr. Alan Waterman, who will speak on "Basic Research and the Scientific Method."

DR. WATERMAN: Admiral Clark, Gentlemen of the Industrial College: It is always a pleasure to return to the Industrial College. Indeed, I feel quite at home here, having addressed the College many times over a period of years, first in my capacity of Deputy Chief and Chief Scientist of the Office of Naval Research, and more recently in my present position. The fact that the College has, year after year, included many phases of scientific research in its curriculum is an indication of the military's appreciation of the increasing importance of science in national life.

Since the sputniks, science and the problems of scientific training have become very much the topics of the day. Many of the issues that some of us have been trying for a long time to bring to the forefront have almost overnight been brought sharply into focus. The topic that I have been asked to discuss today, "Basic Research and the Scientific Method," was selected long before even the first sputnik went up. But certainly the events of recent weeks have clothed old problems with a new sense of urgency.

Perhaps the most conspicuous element in the situation today is the extent to which science has emerged as a truly national issue; and we can be sure that in the months to come, many proposals will be put forward and many decisions made regarding science that will have a profound effect upon our national life. With your permission, therefore, I should like to broaden the scope of my topic to include not only basic research and the scientific method, but national science policy as well. In fact, I plan to go one step further and demonstrate that there are inherent in the methods of scientific research certain rules of conduct and tradition that must necessarily mold and influence national science policy.

The terms "scientific method" and "national science policy" suffer from ambiguity. The "scientific method" is sometimes assumed by nonscientists to be a sort of mystic process by which scientists produce miracles. "National science policy" is a more recent term, often used rather loosely by those who feel that we should have something, without being entirely sure what it is. As a matter of fact, a friend of mine, who knows the situation pretty well, has said that people don't talk about "policy" until there is something that they don't want to do, or something that they think is being done badly. Otherwise the word is not used.

For the purpose of a common base of understanding, we might begin by defining a few terms. By "science" we really mean the cumulative knowledge that man has acquired of himself and his environment. Here the key word is "cumulative." It distinguishes the body of facts we call "science" from the type of noncumulative knowledge that reposes in our literature and art, for example.

The distinction has been ably described by Crane Brinton in his history of Western thought, in the book Ideas and Men, as follows:

> ". . . our contemporary men of letters are today writing about the very same things the Greek men of letters wrote about, in much the same way and with no clear and certain increase in knowledge. Our men of science, on the other hand, have about astronomy and physics far more knowledge, far more ideas and propositions, than the Greeks had . . . . To put it another way: A modern American college student is not wiser than one of the sages of antiquity, has not better taste than an artist of antiquity, but he knows a lot more physics than the greatest Greek

scientist ever knew. He knows more facts about literature and philosophy than the wisest Greek of 400 B. C. could know; but in physics he not only knows more facts--he understands the relations between facts, that is, the theories and the laws."

I might just interpose a comment on this passage. This thought expressed by the author, that we have more ideas than the Greeks, is perhaps a subject of suspicion. Anyone who has looked up Greek literature knows that the Greeks had more ideas than they could do anything about. They mostly just discussed them in abstract form; but certainly in science we find that the Greeks had proposed in their own way, in a qualitative way, many of the theories that have finally been proved. So you could almost truly say that the Greeks had a word for it. But, of course, they couldn't decide which was which. They were just full of ideas.

After describing the process by which cumulative knowledge grows, Dr. Brinton goes on to say: "The result is a discipline, a science with a solid and universally accepted core of accumulated knowledge and a growing outer edge of new knowledge."

The process by which this outer edge of new knowledge is made to grow is, of course, called research. In fact, one of the simplest and most effective definitions of basic research is Vannevar Bush's definition: "Basic research is the search for new knowledge."

Basic research proceeds in an orderly and effective way, and the established techniques by which it progresses may be termed the scientific method. As Conant puts it in his excellent book On Understanding Science: "I should prefer to speak of the methods by which science has been advanced, or perhaps we should say knowledge has been advanced, harking back to Francis Bacon's phrase, 'the advancement of learning.'"

In general, the scientific method consists essentially of four steps:

First, fact-finding and data-gathering by observation, experiment, and search through published literature. For science the important thing is experiment. That is what started modern science.

Second, analysis of these findings. This involves examination and classification of data, weighing the importance and relevance of each, and discovering their relation to each other and to existing knowledge.

Third, statement of an hypothesis or tentative generalization to account for the new and the old facts.

And finally, testing of this hypothesis by following its logical consequences and proving or disproving them by experiment. If these consequences are found to be verified, this hypothesis becomes theory and, when properly tested and proved, a physical law.

It should be borne in mind, of course, that not every scientist follows this outline in detail. Scientists work in all sorts of way. In general, however, these steps apply to the development of science itself as a whole, and one can see this pattern as you review the history of science. It's far from true to suppose that all scientific research workers work carefully and logically along a predetermined course in their discoveries. Some work that way; others work much more like artists--almost by inspiration. They get bothered by an idea or a problem, and they sleep on it and think it over; and the solution may come to them when they're taking a shower in the morning. There's nothing logical about that except that their mind won't let go of it, and they finally get the answer. That's a sort of experience we've all had. So I don't want to give the impression that all this is completely laid out in advance. Some kinds of science and some scientists work that way, but by no means all.

The power inherent in the scientific method lies in its ability to provide quick applications, important predictions, and, above all, new ideas for further advances. Once these generalizations are put into words and recognized, then the thing seems to take on new life, because from the generalization one can make predictions about things that have never been tried before, with some degree of assurance that if the law is true, the predictions will be true. These in turn can be tested; and that is the great power of the generalization which we call a physical law.

On exploration, these inevitably lead to new hypotheses which continue the development of the science. An important additional factor is that, as time goes on, we find by exactly the same process physical laws that may be combined into a single one of greater

generality and therefore of greater power, such as the principle of the conservation of energy, for example.

Even as the scientific method has evolved, so too has a traditional "policy" grown up among workers in science that is almost as old as science itself. It is really a combination of a philosophy of science--the scientists' attitude toward their field--plus a tacit code of behavior for the scientist both as an individual and in his relation to his colleagues. Although a scientist would be startled and probably amused at calling this "policy," no policy for science that ignores or violates it can be tenable.

But what is this philosophy or policy concerning science? Briefly, and in oversimplified form, it is this: The efforts of science to obtain better knowledge of our universe are completely objective, as far as may be. They are not based upon opinions, even of the world's leaders in science, but upon observations and experimental demonstrations that any competent individual can verify for himself. Each new scientific finding, even by a Nobel Prize winner, is subject to challenge and to critical examination and test by others in the same field. The process is thoroughly democratic.

In fact, scientific research has always been a "free enterprise" system. Any adequately trained research scientist may and does make his own contributions to science, which may be large or small. In fact, this is the research scientist's goal--to make an original contribution to his field of science; and his standing in his profession depends directly on that. But note that this is a highly individual matter. The scientist makes his own decision as to where and how to explore. Since he is a specialist, the importance and feasibility of what he wants to do and the significance of his final results can be properly appraised and evaluated only by his peers--that is to say, his scientific colleagues in the same field.

An important part of the research process is adequate information about the state of the art. Before a scientist undertakes an original piece of work, it is clearly necessary for him to know, to the extent possible, about all other research in his special field--past, present, and planned. It is obvious, of course, that work in his field will not necessarily be confined to a given country or group of countries. Science is, by its very nature, quite international in character. It always has been.

All scientists are in the same boat as far as the need to know is concerned; so they are all strongly motivated to keep in constant touch with each other. These necessary contacts are maintained in various ways--sometimes direct and personal, through visits and correspondence; sometimes through conferences and meetings; and regularly through papers published in scientific journals. The moral of this is that any attempt to isolate scientists and to interrupt the spontaneous process of the exchange of scientific information can only handicap the advance of science itself.

These, then, are some of the factors that have entered into the development of the scientific method. The next question is: What is the significance of the scientific method in terms of a national policy, say, for basic research in science? It is that no agency, governmental or otherwise, can rationally attempt to decree what individual scientists should do, and still less how they should do it. The Soviets tried to do this and failed. The Germans tried to do it during the war and failed. When the Communist hierarchy, realized that scientists must be free to pursue their own inclinations, Soviet science began to take giant strides forward.

Scientists know best, then, what can be done in their own fields and how to go about it. A national policy in pure science must, therefore, be an enlightened one. It must find out what scientists consider important to do and see that they have the means to do it. This includes wholehearted approval of providing support for competent basic research wherever needed, and also for the capital facilities which science needs in such fields as nuclear research, radio astronomy, and the scientific exploration of outer space--a very recent one. A national policy should also assist by providing better means for the dissemination of research reports, by providing opportunity for conferences and travel to scientific meetings, and by helping scientists to renew research study and personal communication with other scientists, both at home and abroad.

But one may ask, What of the priorities of different sciences? Are there not some more important than others? To this, science itself can make only one answer. No field of science should be excluded from encouragement and support. Capital discoveries in science may occur in any field. History is convincing upon this point. In fact, the more novel and far-reaching the discovery, the less one can predict where it may occur. How could one have foretold the discovery of magnetism, X-rays, and penicillin before knowing their

existence? One cannot even say with certainty that important discoveries will be made by those from whom they might logically be expected. It was a biologist who first discovered an electric current and the principle of an electric battery. It was a physician who first formulated the broad principle of the conservation of energy--one of the major laws of physics.

This thing happens, too, in its application for development. I might digress for a minute just to give you one illustration of that.

One of the problems directly after the war was to provide sufficient power for microwave radar. At the time the only power tube available was the magnetron, but that had practical limits at about one megawatt. Now, at that time the klystron was also in existence, which as many of you know, is a vacuum tube affair that also could provide power for radio waves; but it was impossible, according to the electronic tube manufacturers, to make this do very much below a wave length of about 20 centimeters. The minute you got below that, you couldn't get any power out of it. This was a problem that bothered the services and it bothered industry tremendously. They did their best to try to improve the tube, without success.

At this time, a physicist, Bill Hanson, went back to Stanford University with the idea that it might be possible to accelerate electrons to enormous speeds by one of the aftermaths of the cyclotron family. He was thinking of a method of accelerating electrons to enormous speeds by a linear array instead of the cyclotron's circular orbit. The problem then was, How do you put power into a stream of electrons to get them up to a very high velocity, maybe a billion volts speed equivalent?

In the process he looked over the situation of power tubes. There, as a basic research man, he took a look at the klystron, made a new design going back to fundamentals, and came out with the possibility that a klystron could be built that might have 25 to 50 times the power of the magnetron.

Well, of course, the Navy--I happened to be in the Navy at the time--encouraged this, because we had the support of the project for the linear acceleration of electrons he was talking about. They encouraged him to perfect this. So it was done. And now we have in the klystron a very powerful tube for microwaves. But, you see, this wouldn't have happened if this basic research man had not wanted to do something of the sort in his own field.

I mention this as a typical case where somebody who is working in a completely different way on basic research comes out with something which the practical people have had great difficulty in solving. They're a little too close to it. So although this isn't universally the rule, it's a very important thing to bear in mind.

The only distinctions that can be made as to what should be emphasized in developing sciences are in terms of generality of findings, techniques available, current rates of progress, available skilled manpower, and occasionally neglected or overemphasized special areas when so identified by the scientists themselves. This is science's own answer to the matter of priority.

But there is, of course, another and completely different question, namely: In what ways can science best serve the Nation, the Department of Defense, or all mankind? Here a wholly different set of criteria apply. This question involves an appraisal of the Nation's needs or the departmental needs, and a matching of progress in the fields of science to these national or human needs. For the service of science to the Nation, or to mankind, is almost exclusively a practical matter and therefore concerns the applications that can be made from scientific discoveries and scientific principles. This is the primary business of applied research; and after applied research has pointed the way, engineering development takes over to prepare for production or other economic application.

This whole sequence properly goes under the name of technology, and its role is very often confused with that of science itself. But "technology" is the proper word for this whole sequence. We note that applied research in general has to start from the basic facts of science and engineering; therefore, the more advanced our basic research, the more advanced can be our technology.

I mentioned the fact that this talk would have to be oversimplified in this short time. I don't want to leave the impression that all things start with basic research and that applied research doesn't have a hand in it. There is always a feedback, and a very important feedback, between what is done practically in applied research and in development. This feeds back into basic research and provides those people more tools and more insight into the problems they have. So the two assist each other. I don't want to imply that one calls for more ability than another. They just call for different kinds of ability; that's all.

Now, as a good instance of feedback of this sort let me give again an illustration from radar, because this is a very striking one indeed. Radar itself was a matter of applied research during the war. The principles were all contained in Clerk Maxwell's theory of the electric current and in electricity in general back in the middle of the 19th century, and scientists knew this pretty well. The possible application to microwave power, however, brought all these things in the open. That was applied research very largely, and very successful, too, of course.

Now, then, after the war, the principles involved in radar, the practical principles of how to handle these waves in the usual plumbing, and so on, were known by nuclear physicists. And they proceeded, two of them--one of whom got a Nobel Prize for it--to develop the use of radar waves in containers to measure directly the magnetic moment of the atomic nucleus, which is a highly basic thing. That was done, then, as a feedback from what had been done in radar--the measurement of the degree of magnetism, you might say, of the atomic nucleus. It was a beautiful job, and came out with very precise values. This could not have been done, then, without the development of the art of radar. That's the type of thing I mean.

The needs of any nation can be described in terms of fundamental human needs and wants. These are represented by such broad essentials as food, water, health, defense, transportation, housing, communication, and, above all in the modern age, available power. The countries of the world differ little in their fundamental needs; there are great differences, however, in the degree to which they have been able to apply modern technology to the satisfaction of those needs. The United States and other industrial nations are, of course, most nearly similar in this respect. Our standard of living is commonly considered to represent the extent to which we are satisfying those needs. By the same token, the standard of living we wish to attain is regarded as a measure of priority of future needs. To the extent that this has to be centrally done, it is one of the responsibilities of the Federal Government to see that these broad needs are met. Therefore, Federal departments and agencies have been set up whose primary purpose is to do what is necessary to meet this responsibility.

Government Departments, such as Defense, Commerce, Interior, Labor, and Health, Education and Welfare, therefore, have responsibilities for science and technology in their respective fields. To meet them, these agencies have had to develop and support appropriate technology. By and large they are active in science only as necessary

to provide the background for that technology. Thus the Department of Defense is concerned with military weapons and devices of warfare and supports science underlying the corresponding fields of technology. Most Government departments and agencies have had considerable experience in these matters and have specialized in knowledge of the relation of science and technology to their overall missions. It would be neither logical nor feasible, therefore, to establish a Department of Science and Technology with overall supervision of these activities. Such a department could at best be only an administrative department superposed on existing ones, with confusion and frustration resulting in the operation of each separate department.

Federal research and development activities do, however, have a common element, namely, basic research. This is true because the findings of basic research may be useful in many and unexpected directions. It is logical and reasonable that a Federal agency, for example, such as the National Science Foundation, which keeps itself informed of the basic research activities of the Federal agencies, should exercise an appropriate degree of coordination among them and be in position to take the lead with respect to common programs of the Federal Government in the various sciences.

Now, the present policy of the Federal Government with respect to technology among its own agencies is simply that each agency has responsibility for what it does in technology that is directly related to its mission.

It is also the policy of the Federal Government to permit and encourage each agency to conduct and support basic research in fields of science related to its mission and, of course, to conduct and support applied research aimed at solving its technological problems. Both these policies--these two that I just mentioned--are set forth in Executive Order 10521, issued by the President in 1954.

In addition to the research conducted in its own laboratories, the Federal Government supports basic research in institutions throughout the country. In order to achieve its objectives, the Federal Government must utilize the highest available research competence in science in the country wherever this can be found. Consequently, Federal departments and agencies should and do have authority to support basic research in colleges and universities, and at research institutes and research laboratories as appropriate. This is accomplished through a selection of applications for the support of basic

research that are received by the Federal research offices, and also, where appropriate, by enlistment of research scientists to work on pressing agency problems. This is the usual way in which the Government supports basic research outside its own laboratories.

In addition to purely practical considerations, however, the Federal Government recognizes that progress in pure science is fundamental not only to the strength and welfare of the Nation, but to its intellectual growth as well. Congress demonstrated this conviction when it established the National Science Foundation in 1950, to support basic research comprehensively throughout the country, so that science would go forward actively on all fronts.

Policy on the part of the National Science Foundation is simply to support high-quality research by competent scientists in all fields, taking into account support by other agencies. The Foundation has in view the distribution of support among fields of science and among various types of institutions. It also considers such factors as geographical distribution and the encouragement of promising young research investigators.

In practice, this policy on the part of the National Science Foundation, and, indeed, of all Federal agencies, consists of the encouragement of proposals from any competent scientist or group of scientists in the country. With the advice of expert consultants, the agency makes a selection from among these proposals on the basis of the scientific merit of the research, the competence and experience of the research personnel, the endorsement of their institution, and the relative importance of the field of research in question. The proposals selected then receive support in the form of a grant or contract to the institution on behalf of the persons wishing to do the research. Generally speaking, the grant or contract provides funds for equipment, materials, and personal services required for the job.

This procedure enables the Federal Government to follow the lead of the scientists in the country in determining what research should be supported, since the ideas originate from them, by and large. The result is a truly national policy for the encouragement of science. Most important of all, it is consistent with the best tradition in research, as I outlined it earlier; and permits, to a maximum degree, the freedom and independent action on the part of the scientists themselves in the choice and conduct of research that are so necessary to the progress of science.

It should be noted that Federal policy with respect to the support of science has, in general, drawn the line at providing unrestricted research funds for institutions or the departments of institutions. This policy has been deliberate, and is subject to constant review and appraisal. It rests upon two major considerations: In the first place, it is endorsed by the great majority of the country's scientists as being in the best interest of progress in science. In the second place, the provision of unrestricted funds for research to educational institutions or their science departments would be a strong precedent for direct Federal aid to higher education.

Although the policy of support "by project," as it has been called, has been criticized by some university administrators and some scientists, majority opinion still holds it to be the most satisfactory. The question should, however, remain open. It is important to make certain, before any change is made, that direct support by the Federal Government to educational institutions in some manner other than by projects would be a wise move, since such a new policy, once inaugurated, would be very hard to reverse.

In all these matters it is of the greatest importance that any steps taken in support of science should have the understanding and the backing of the people of the country. Just 10 years ago, in the book I mentioned earlier, Dr. Conant laid special stress upon this point, under the heading, "Science and National Policy." He said:

> "In a democracy, political power is widely diffused. National policy is determined by the interaction of forces generated and guided by hundreds of thousands if not millions of local leaders and men of influence. Eventually, within the limits imposed by public opinion, decisions of far-reaching importance are made by a relatively few. These men are almost accidentally thrown into positions of temporary power by the forces working throughout our benignly chaotic system of political democracy. Because of the fact that the applications of science play so important a part in our daily lives, matters of public policy are profoundly influenced by highly technical scientific considerations. Some understanding of science by those in positions of authority and responsibility as well as by those who shape opinion is therefore of importance for the national welfare."

Our citizens must be able to understand and appraise the urgency of the international as well as the national situation and to take and to urge effective action. Under our system it is not possible for the Federal Government to take adequate steps to strengthen our science education and our research unless these measures have the wholehearted support of all citizens.

Furthermore--and this point cannot be too strongly stated--we must realize that the national security depends not only upon our military and defensive strength, critical though these are. Fundamentally it depends upon our economic strength, which in this day and age depends upon our progress in technology. Still more basically, this in turn depends upon the will and the determination on the part of our people to put forth a maximum effort in science and technology, which is at the same time consistent with our broad ideals and goals.

Thank you.

CAPTAIN STEVENS: Dr. Waterman is ready for your questions. He told me to tell you that you do not need to limit your questions to what he talked about this morning, but anything in his field of competence he will be happy to take a crack at.

QUESTION: Dr. Waterman, I think it becomes the responsibility of us in the military to advise the country on certain aspects of the international situation and their implications on the ocean and on land, such as was done in the case of Korea. What action have scientists, either in your group or in other groups within the Nation, taken to advise the country, and to advise the leaders of the country, on the situation in which we now find ourselves?

DR. WATERMAN: The first, of course, is advising the President as to what things scientists consider to be important. Those things have been mentioned in his speeches, many of them.

The country is pretty well aware, I think, and has been for several years, of the national shortage of certain kinds of scientists and engineers. This has been oversimplified. The press has talked, and most other people have talked, about the general shortage of scientists and engineers. That's not a good way to put it, because in civil engineering, for instance, the situation is not very different than normal, but in the new field of nuclear engineering there is a very

bad shortage indeed of people trained along those lines and the same holds true for aeronautical engineering, electronics and other new fields.

The National Academy of Sciences and the National Science Foundation published a book called Soviet Professional Manpower, which appeared about three years ago. That got pretty wide circulation and called attention to the higher rate at which the Soviets are producing specialized personnel. Of course, industry has alerted the country, and Government speakers of all kinds have dwelt on the shortage of scientists and engineers. So the country has been made aware of the situation.

More recently the matter of scientific education has hit the country in a number of ways. Officially all I can say is that we in the Science Foundation and the Science Advisory Committee, now reporting to the President, have presented these facts very clearly. The agencies know them through various media.

Again, every opportunity is used to place them before the country. What has not been done, I suppose, is to adopt 10 or 12 points or anything like a 5-year program and present it to the country. It has been done more informally. That's the way the thing stands.

QUESTION: Doctor, you mentioned that the first part of your program for insuring a good scientific progress was a free flow of information between people working in scientific fields. Since such a large part of our research and development in this country is sponsored by the Government and specificially by the military departments, and much of that is necessarily classified, what effect does this have on the flow of information? Do you think this is good, bad, or indifferent; and what would you recommend that we do about it?

DR. WATERMAN: That's a very complex situation indeed. All you have to do is to realize the volume of available material in research and development and you really are stumped to know how best to solve it. Let me take it by parts.

Insofar as work is classified, as it must be in many cases, especially in development and on the application side, this, of course, is a handicap to progress to some extent. You all know that very well. Anything that gets highly classified suffers under a handicap in some respects, because the classification does impede progress.

Now, I don't mean to say that this shouldn't be done. You do have to have high classifications; but one has to realize when he imposes such a classification that it does slow things down, and can't help but do so, because only a limited number of people are in position to act on the matter, whereas, as you know, in industrial development things are a little more open. The more people that can get in it and work on it in their own way, the faster progress occurs. So to the extent that classification is necessary, it must be admitted that it does impede progress to some extent.

On the basic research part, classification is seldom required, or certainly it should seldom be required. Here is where it's much more important, it seems to me, to be sure that we make rapid progress than to try to overclassify.

A good illustration of that appeared--and you perhaps know the reference--in the Geneva Conference that was held on the industrial use of atomic energy a few years ago. Facts were disclosed there by several countries that had been kept classified on the part of each. One outstanding case was the measurement of what is known as nuclear cross sections. That is to say, "nuclear cross section" is just a word for the possibility of capture by an atomic nucleus of a neutron which could cause fission. Obviously, this is a matter that we did not want to disclose unless we had to at the time. So all the work that was done on these measurements during the war and since the war was kept classified in this country and very closely so. We had progressed to knowledge of this with reference to all sorts of nuclei, and all this came out at Geneva.

Well, our measurements were reported at this meeting and so were the Russians; and they turned out to be identical. We had a little edge in progress, but not much. The Russians had gone ahead and measured the same thing and got the same results, and so had the British.

So these facts of nature are open for everybody to discover, and you can't prevent another nation from discovering them by classifying your own results. In fact, one can say roughly that the progress of science in all civilized countries is comparable. I don't mean to say that they are that close always. But if you discover a thing first in basic science, you can pretty surely suspect that another country will do it within a few years at least, because that's the way these things go. The basis is available to everybody.

So I would say that it's a mistake to classify basic research to the same extent that you classify applied work, because it's much more important to make progress. If we get started on a line of basic research and lead other nations, and if this is unclassified and our people get enthusiastic about it, the chances of another nation catching up are very dim even if it is in the open. It's like the old system of giving a battle order right out in the open. If you move fast enough, nobody can catch you. If you try to overclassify your research, you may slow it down so it's unsuccessful. It's a little like that. That's on the classified side.

Now, coming to the general picture, right now we have an agreement between the Department of Commerce and ourselves--John Green's Office of Technical Information--and the National Science Foundation--to try to do what we can to get better dissemination to scientists of Government reports, that is, unclassified reports from Government agencies. The Department of Commerce will do this for technical information and we will handle the basic side. So that is a move in the general direction of better dissemination.

The most troublesome problem right now is the translation of Russian scientific and technical literature, because very few of our people read Russian. The people who can read it intelligently in science and technology and translate it well are few in number and are apt to be busy with other things. So we have been making a determined effort to try to get the most needed items translated. We have been supporting the translation of the major publications in physics by a contract with the American Institution of Physics. It's being extended and we hope this will cover other fields of science very shortly. All this is a very large and troublesome subject. You know how it is in the Department of Defense. All we can do is to try to do the best we can.

QUESTION: I have two related questions, Doctor. You indicated that in the basic research field, I believe, you reviewed some proposals and found some progress and the Department does some of this on its own, but you try to keep abreast of what others are doing. Would it be better if this basic research, which has no directed end use, were all put under your wing?

DR. WATERMAN: No.

QUESTION: The second question is this: We have been severely criticized for the lag in basic research. Have budgetary restrictions over the past several years forced you to turn down proposals which you consider should have been supported?

DR. WATERMAN: Well, that I will answer very quickly and then turn to the other.

We have been handicapped, and so have most of the research offices in the Government. The reason is an obvious one. Basic research is moving on the frontier and you don't know for sure what it's going to come up with. You can't stake it out clearly, and therefore it is a hard thing to get money for. In fact, one of the definitions of basic research, you may have heard, is that it's the kind of research you can't get funds for!

The trouble is, you see, a Congressman wants to know what you're going to do. You tell him and he'll pick out a project--you've seen this happen--and say, "What earthly good is this?" Well, you can't tell him. The title may sound absurd. The idea is not. But you can't tell him definitely. So the only thing you have to go on is: "It's like an investment. You don't want to put all your eggs in one basket in an investment. And what looks like an awfully good thing may be just the wrong thing."

So what you do is make a comprehensive program covering all the kinds of things that you think might be invested in, short- and long-range ones, and you don't know that any one of them is going to pay off. You can't know it. But you set up your program so that statistically something does pay off. And that comes from various parts of the program. Industry knows this very well. So that's the real answer.

Well, specifically, in the Science Foundation for a number of years, almost since we started, we have not been able to come anywhere near financing all the good proposals that come our way. We know this statistically, because we have to analyze them all, appraise them all; and then when we get through, we find that of the number that come to us--and about 75 percent of them are really good, and in good hands--we've been able to award grants or contracts to only about one-third or one-fourth. A similar situation exists in other agencies.

So on that the answer is very clear. We have not had nearly enough funds. The country could stand right now, I am sure, an increase of 50 percent more support in basic research. It would be a very fine thing.

Now the other question: why the National Science Foundation shouldn't do all of this. Well, for one thing, it would be against all the instincts of scientists to have one agency responsibile for all research. No agency is omnipotent enough to do a good job of that. It's better, then, to have a number of agencies.

But I can speak from experience that it is a very good idea anyway. When I was with the Office of Naval Research I could see this. What any agency needs, what the Department of Defense particularly needs, is close contact with the advances made in basic science just to provide a better understanding of what science can do toward working out the developments in which the Department is interested. This is the basic knowledge that enables us to make a decision. It'll save you money, too, plenty of times. Basic research can come through here and enable a wiser decision to be made about what is feasible in development.

Now, I believe firmly that an agency can't get this information second hand. If we were to try to do it, in the Foundation, then you would have to appoint a liaison officer with us, or something like that. Then even with the best intentions, that contact would get worse and worse, and you wouldn't be getting it in the best form. We in the Foundation wouldn't know your problems well enough to do a good job, for one thing; and no liaison officer could really make a go of that for very long. In other words, I believe an agency should have this information first hand and not second hand.

I seem to be full of radar this morning, but let me give you an illustration of the importance of basic research to a military project during the war. It's a really classical instance of where the inability to carry on the basic research adequately at the time cost the country a great deal of money and retarded a development which was quite promising.

The first radar developed was, as you know, the 10-centimeter band. Then the wave length was moved down to three centimeters, because that got better definition. Then the next step was to see if one couldn't go further and make a still shorter wave length, the K-band, about one

centimeter. This was projecting ahead to see what the state of the art could do. The prediction seemed favorable. You could see that from the definition improvement from the 10-centimeter to the 3. Well, they settled on about one centimeter and went ahead with the development.

When the thing was perfected, it was found that the range was very limited. It could hardly go any distance at all. The answer happened to be that this particular wave length ran into an absorption band, as the physicists call it, of water vapor in the air. So any water vapor in the air absorbed these waves and they wouldn't go any appreciable distance at all.

Well, there was a development that cost tens of millions of dollars for the production of equipment, and when it was finished, it couldn't even do the job it was intended to do. If there had been time to go into basic research--this was during the war; so there wasn't--this matter could have been explored by basic research methods to find what there was in the air and how it affected these short waves; and then a change of half a centimeter would have made the thing all right.

I seemed to get eloquent on that one, but I really think that an agency should have the right to do its own basic research. But notice that the Executive order says "in fields closely related to its mission" and that makes sense, because then you can defend it in the budget.

QUESTION: I am concerned about the allocation of the available scientists, especially those at universities. Since World War II we have had a tremendous increase in the contracts let by the Department of Defense, very largely with universities, for basic research. Yet we hear now that we do not have enough science teachers to teach science in our universities. Is that a problem of the diversion of the available professors to contracts for research and away from teaching, so that therefore the students don't get enough teaching? Or am I bringing up a problem which does not exist--the diversion of science professors and some other talent to Government work to the detriment of students?

DR. WATERMAN: That's a good question. It doesn't have a very simple answer. It could be, unless there were special circumstances, that a given man who has a Government research contract might get out of teaching too much. There are two ways of meeting the situation: One is that the university has to watch out and see that this doesn't

happen to too great a degree. The other is the attitude of the agency itself. If it operates wisely, it would know something about the situation in appraising the project and ask the university, "Aren't you really taking this man and putting too much of his time on research?" That's the answer to the first question.

Now, the second is one that I should like to discuss and I am glad you asked that question. This situation, even at the worst, is not as bad as one fears, for this reason: The way a professor at a university works in research is this: As soon as he gets any stature at all, he gets a number of graduate students to work on particular problems that he outlines for them and lets them go to work on their own. So each good research man on the university faculty will have some teaching. This will usually be advanced teaching where he is a specialist, because he's in the graduate school. He'll want that anyway, and he'll hold on to it, because he feels that this keeps him fresh and in contact with students.

Then he will map out, say, during the summer or some free time research problems that he things are feasible and good. Then he gets graduate students, if he can, to work on those. So he ends by having--depending on the kind of work that he does and on the capacity he has for this--anywhere from one or two to ten or a dozen graduate students working under his direction.

This takes very little of his time. All he does is outline the problem. They're supposed to work on it on their own. If they can't do it on their own, they don't get their degree. So this takes a limited amount of his time. In other words, when he comes in with a program for support from the Government, it is probably on some additional problem for which he needs money, and he's going to get a graduate student to work on it. It won't take but a small amount of his time. So it doesn't have the effect that you mentioned at all.

QUESTION: I am a little more perturbed about freshmen and sophomores. It seems to me that they would have an interest in it.

DR. WATERMAN: Such a man, if he's really good at research, seldom does much teaching of freshmen and sophomores. He may, depending on the policy of the institution, teach a class in freshman and sophomore work. But he won't be given a major responsibility for teaching the entire subject or be made head of the whole works or

the elementary department. You see, he's not at that level. He's on research, which is a higher, more advanced plane. Very often he's not very much good at teaching beginners.

So that brings out the other side of your point which I wanted to mention, namely, that there is a difference, not sharp, between those who specialize in teaching in colleges and universities and those who specialize in research. Most do a little of both. Some do a great deal of research, concentrate there because that's where their ability lies, and almost no undergraduate teaching. Others do their share and do the research that they have time for; but that isn't the major part of their time. Still others are there only to teach the elementary people. They're usually the ones that have charge of the whole program of elementary teaching. They do almost no research. They just don't have time. So that's what happens here.

QUESTION: I have two questions. One is, can you tell us what is the extent of the financial resources available for grants to research institutions for scientists? The other is, What is the mechanism by which a scientist can be assured that he has all the available basic research knowledge on his subject when there are so many thousands of research institutions all over the United States, and the world for that matter? How can he lay hands on all the available information? What is the mechanism by which you assure him of that?

DR. WATERMAN: On the first one, our present budget--and this is our seventh year of the National Science Foundation--is $40 million. The President applied for $65 million for this year, but we ran into a budget squeeze last year; and so we were kept where we were. It was $40 million last year too. Of that a little over one-third goes to the support of basic research of the kind you mentioned, and about one-third goes to the training of scientists, which is now so important. We started that some years ago. The remainder is mainly a separate item, that we have separated out from basic research in general. These are capital research facilities, of which the outstanding one at the moment is the construction of the radio-astronomy telescope at Greenbank, West Virginia, with a 140-foot dish, which will cost around $5 million. This item, which comes out of funds for research, constitutes the major part of those funds.

Now, on your other question, it seems appalling when you look at the number of scientific papers and reports that come out. But this

a very good one. Brookhaven National Laboratory under the AEC has a 25-billion machine under construction. The Russians have a 50-billion machine under construction. And that's as far as we've gone. We haven't yet taken the next step, but you can be sure that Russia is making plans.

So this is a very important thing to continue and yet these things are very costly. The ones contemplated for the immediate future may cost $100 million over a period of five or six years. Notice that this is basic research. So there is a question that I would ask to which the answer is uncertain: How much money would we need for these capital facilities? You have to face it; we'll need a lot.

Well, those are some of the things that the Foundation would be interested in. Incidently I think you may have seen in the paper this morning that we've been given the old AEC building for an office coming up in the spring. That will enable the three branches of the Foundation, now scattered around Washington, to get together again.

QUESTION: Going back to the classification question but for analysis purposes, the newspapers have been reporting an apparent conflict between England and, I believe, the United States over the release of discoveries in the control of the fusion process. Can you tell us what position we should take? Should we release this, or are we right in opposing the release of this information?

DR. WATERMAN: I don't know enough about the details of that story to really comment on it. I think it would be helpful if both England and the United States moved in the direction of releasing more information. That is my personal inclination in the matter. But, of course, one can't really decide those things without going into great detail.

CAPTAIN STEVENS: We would like to keep you here all morning, but I think we'd better call a halt on it. On behalf of everybody here in the College, I want to thank you for putting aside your busy duties and coming down here and giving us a clear, concise, and first-hand view of what your Foundation is doing and the state of science in general in the United States. I want to thank you very much, Dr. Waterman.

(21 Feb 1958--4,100)B/sgh:ljt

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