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BASIC RESEARCH--A NATIONAL RESOURCE

Dr. Alan T. Waterman

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Reviewed by: Captain W. P. Schoeni, USN

Date: 2 November 1959

INDUSTRIAL COLLEGE OF THE ARMED FORCES
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16 October 1959

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16 October 1959

ADMIRAL PATRICK: General Houseman, Gentlemen: This is the second lecture in our series of science and security. Today we take a comprehensive look at the basic research as a foundation stone of our national scientific effort.

Our speaker is a most distinguished scientist and scholar. Moreover, as Director of the National Science Foundation for the last eight years, he is in a unique position to convey to you not only the nature and importance of basic research but also the role of the Government in this field.

Dr. Waterman, it is a pleasure to welcome you back to the College for your sixth appearance here. Dr. Waterman.

DR. WATERMAN: Thank you, Admiral Patrick. It is a great pleasure to be invited to return again to the Industrial College.

My talks before you have been generally on a topic of this kind for a number of years, and the topic that I have this morning is still much the same, Basic Research - A National Resource. I think it is very significant that the Industrial College continues to take an interest in this phase of science.

This title is one which appeared on a National Science Foundation study in 1957, which was made to the President. Incidentally, it was released just before Sputnik, and their ideas changed very little except

in emphasis, I should say. But, since then, for those of you who are interested in pursuing the subject in greater detail, there have been other reports which you might find interesting for further study. Among them is perhaps one of the most important, the report issued by the President's Science Advisory Committee about a year ago, entitled "Strengthening American Science." Last spring a series of important papers on this topic were presented at a symposium on basic research under the joint auspices of the National Academy of Sciences, The American Association for the Advancement of Science, and the Alfred P. Sloan Foundation. The proceedings of that symposium will be available in public form very shortly.

For those who wish to gain a good understanding of basic research and its consequences in our technology, I might mention a first-class report entitled "Basic Research in the Navy," which was made to the Secretary of the Navy by the Naval Research Advisory Committee and was undertaken by an O and R contract with A. D. Little of Boston. In this report I would especially call your attention to several case histories showing the evolution from the earliest discoveries of basic research to the end products that are so useful to our industry in defense. I suppose I perhaps ought to apologize for this plug for the Navy, but I assure you that the report has equal bearing on the other services as well. It happens that this one has gone very thoroughly into how basic

research starts and how it develops, and brings out the applications to finally whole industries, almost, in the process. This is usually a period of 50 to 75 years as the important subjects have emerged.

It is common knowledge that the Government has, in the years since World War II, built up a very strong program in scientifical research and development, in its own laboratories, in national centers, such as the AEC's national labs and those of the services, in large and numerous contracts with industry, and by extensive contracts and grants with the universities.

At the present time the Federal Government is supporting nearly half of the total research and development effort in the country, which currently amounts to about $10 billion. That is doubled in about six years. Of this, industry is performing about 73 percent of the work. However, the fraction of Federal support of basic research is small, between 7 and 8 percent. It is worth noting that many of the leaders in modern technological industries regard this percentage as too low for the fields in which they are engaged. A canvass has been made recently by the A. D. Little Company of leading industries, and that seems to be a fairly unanimous feeling among the leaders of industry themselves, that what we should do is back the basic side of research even more strongly than we are doing. They suggest that perhaps twice this figure would be an optimum one.

That is instructive, I think, and important, because industry knows very well the value of money and the effectiveness of it.

Not only the Nation's security but its long-term health and economic welfare, the excellence of its scientific life, and the quality of American higher education are now bound up with the care and thoughtfulness with which the Government supports research.

Now let me quote from this little pamphlet, "Strengthening American Science."

> "If this support is halting and erratic, if it emphasizes mechanism and hardware to the neglect of fundamental understanding, if it lavishes money onto few popular fields and starves others of importance, if it fails to encourage exceptional men and exceptional programs, the net result could be an impoverished science and a second-rate technology. One of the clearest lessons to emerge from the history of science is that various scientific disciplines, seemingly unrelated, have a way of stimulating and fructifying each other in an unexpected manner. This complex back-and-forth interplay is the life and soul of science and technology. There can never be too much of it. The most impractical thing that can be done in designing and directing programs of scientific research is to worry overmuch about how practical they are. The secrets and treasures of nature are hidden in the most obscure and unexpected places. It is clear, therefore, that the strongest

"scientific program is the program with the greatest breadth and scope. It is impossible to predict from which quarter the next scientific advance will come, but we can try to make sure that the Nation has able people at work across the whole scientific frontier."

Again, later in this report:

"A strong case can be made for intensifying the Nation's scientific effort in a dozen or more fields, ranging, for example, from geophysics and biophysics to linguistics and social psychology. A better understanding of geophysics would reduce the cost and difficulty of finding new raw materials which are needed in ever-increasing volume. A deeper knowledge of biophysics would contribute to the understanding of heart disease and cancer, which together cause nearly 70 percent of all deaths in America. Advances in linguistics could greatly simplify the unresolved and growing problem of formulating and communicating new knowledge. And advances in social psychology might help to reduce tension and conflict at every level of human intercourse, in our communities, in business and industry, in government, and even among nations."

Let me define the terms we are using. The word "technology" has come into greater use as a helpful expression for a nation's effort extending from research, both basic and applied, through development, test, evaluation, production, and use. Of these, the most troublesome

to define is basic research, and indeed no short definition has been found which is entirely free from objection. The definition used by the Department of Defense is as follows:

> "Basic research is that type of research which is directed toward an increase of knowledge in science. It is research where the primary aim of the investigator is a fuller knowledge or understanding of the subject under study."

This definition has been pretty generally accepted. For our surveys of the country's research and development effort, the National Science Foundation uses this definition, with an additional qualifying phrase.

I'll repeat that last sentence. "It is research where the primary aim of the investigator is a fuller knowledge or understanding of the subject under study"--we add, "rather than the practical application thereof."

One of the best explanations, in brief form, of the relationship of basic research to the mission of an agency was stated in the report on basic research in 1953 by a Department of Defense Committee, of which Dr. Warren Weaver was chairman. I'll quote from that report:

> "It is essential to recognize that there are two aspects of basic research, depending on who is viewing it. From the point of view of the research worker himself, basic research is research motivated by curiosity and interest carried out because it promises to add to knowledge and without any necessary

"interest in or concern for the practical applicability of any results that may be obtained. Nevertheless, it is most strikingly and emphatically true that basic research is not impractical research. The whole history of science constitutes the most impressive proof of this statement, and the research administrator, informed as to the history of research and aware of the interrelationships between various fields of science and various fields of application, or end products, can, concerning a given body of basic research activity, reasonably make judgments concerning probable practicality, these being judgments which may be quite foreign, if not meaningless, to the individuals actually doing the research. Thus, it is quite obvious, if one is interested in, say, the development of new materials which will maintain strength at high temperatures, that there are certain areas of pure research which have probable relevance to such problems and other areas which are unlikely to yield results useful for this purpose. Thus, without in any way abandoning or contradicting the concept of basic research as viewed by the researcher, the research administrator can discriminate between various areas of basic research and can sensibly judge that certain of these general areas have a high probability of producing results useful for the purpose, while others have a very low probability. In other words, having a field of application in

"mind, it is meaningful and sensible for a research administrator, without in any way influencing the creative atmosphere within which the researcher, himself, operates, to judge that certain areas of basic research have, with high probability, relevance to his practical interests."

It is on this basis that Executive Order 10521 of the President in 1954 justified basic research for Federal agencies, as follows. After a first statement that the National Science Foundation was responsible for broad, so-called general-purpose basic research which did not have to be related to particular missions, the section concerning it states the following:

"The conduct and support by other Federal agencies of basic research in areas which are closely related to their missions is recognized as important and desirable, especially in response to current national needs, and shall continue."

This has been my philosophy in the National Science Foundation all along. Industry knows very well the importance of this. They have their research departments, and the Government would do well to follow that, even though an agency's attention is on end items.

Events of the present century have pointed up with increasing insistence the importance of technology both for the Nation's welfare and for its security. This is popularly regarded as emphasis upon science, whereas the emphasis should really be upon science and

engineering, since it is engineering that makes possible the applications of science. One of the difficulties in dealing with the whole subject of basic research is this lack of clarity in the popular mind.

So one of the first points I wish to make is that, while most of us, especially this audience, fully realize the importance of technology, militarily and otherwise, most people do not have a clear idea of basic research nor of its importance in undergirding the entire technology. In the popular mind basic research means science, and science means miracle drugs, jet planes, color television, and so forth. To be sure, there is known to be professors at universities who are doing so-called theoretical work, which is somehow related to all of this, but there is a tendency to believe vaguely that these individuals will go on working in any case and whatever remote connection they have with modern development will somehow be realized.

The point is that, partly because of this lack of understanding, and partly because basic research cannot predict what it is going to find, exactly, when the chips are down, this kind of research suffers in comparison with urgently needed development for which the need is obvious. As Dr. Vannevar Bush remarked in his book, "Science, the Endless Frontier," which came out just after the war:

> "Applied research drives out basic, and that is at the root of many of our problems today. When money is tight, the applied research and development get the money because

"one can see clearly what this is for and knows it is necessary. The problem, then, is how does one get funds for this vague thing, the progress of science, where one can't foresee exactly what is going to come about."

That's the problem, essentially. I remember hearing that basic research can be defined as research for which you can't get funds. Actually, it is not easy to establish a close connection between basic research and technology in most cases, except in a very general way. This has been done rather skillfully in the Navy report I mentioned earlier, by case histories for the particular examples of shock waves, radar, and the transistor. Let me add to these examples, just briefly, the development of electronics and the story of helium.

Electronics began in a very interesting way back around 1870 in the hands of Clark Maxwell, who was a theoretical physicist and who was dealing with the equations governing the motion of electricity and the behavior of electricity. He saw in his equations, with no experimenting, the appearance of something that looked like a velocity, and inferred from his equations that there must be such a thing as an electrical wave. This was theoretical prediction. About 15 years later, Heinrich Hertz, a German, aware of this, set up an experiment where he showed that, by an electric spark discharge, this actually did produce waves, so he verified that there were these things that were predicted.

From then this thing went rather rapidly. Of course it went in the direction of Marconi to transmission of signals using these waves. In another direction it went off into electronics by the discovery of the emission of electrons from filaments, in the very early days, you remember, in the hands of Edison. Then it went on the theoretical side to the discovery of the electron by J. J. Thompson in England, and the working out of the theoretical equations which controlled electrons when they traveled through a vacuum, by Sir Owen Richardson, another British physicist, who spent a great deal of time in this country. Then, on the practical side, there was the Fleming valve, the first tube, and then the De Forest free electro tube went immediately over into the possibilities of amplification, generation, detection, and so on. It started off a host of these things.

This origin, then, was from a theoretical equation, first verified in a laboratory, and fanned out to produce this enormous industry that we have now.

The story of helium is interesting because we haven't seen the last chapters of it by a great deal. But it is a fascinating one. The gas, helium, was discovered on the sun. No one knew it existed on the earth. It was discovered because of a particular spectrum in the sun that we couldn't duplicate on earth, and therefore the element was named helium, which means the sun. Some time later it was discovered that this is a very tiny, minute part of the earth's atmosphere. So we

do have some helium in the earth's atmosphere, but a mere trace. However, we proved that it did exist, because it had the same spectrum. Considerably later it was discovered that, of course, helium was associated with oil deposits, and that gave us in the United States the opportunity of using helium commercially. We were the only country, for years, that had such a supply. Now Soviet Russia has found supplies in its country, and we are the only two countries that have helium commercially, at all.

In the meantime, the point is that, to begin with, helium could not be liquefied. Nobody could do it, apparently. For years it was supposed to be the one gas that could not be liquefied. Finally they found out how to do it. They found out that the trouble was that it had to be cooled to a point beyond which any other gas had been cooled up to that time. As a matter of fact, it had to be cooled right down near absolute zero, the lowest possible temperature. The advantage of that was that we then had an opportunity to experiment at those very low temperatures.

Then some very curious things began to happen. This liquid helium in one form, for example, showed the property of perfect fluidity. It could do the most amazing things in the way of flow, apparently flowing without friction. If we could find out how to make liquids flow without friction by proper treatment of them, that, of course, would be a revolution. Helium does it in this liquid form. We don't yet know

why. To illustrate this in a definite fashion, as was early proved in this game, if you take a test tube or a closed cylinder which has a small hole in it, so small that if you put air in it it would take about an hour for half the air to come out through that hole under pressure, and put liquid helium at these low temperatures in it, the helium is out in half a minute, in liquid form. There is apparently no friction of flow whatever.

That's an amazing property and, if we could understand it, we could certainly use it. But we haven't understood it yet.

For another thing, the heat conductivity of liquid helium is about ten times that of copper, at those low temperatures-- a nother thing we could use, if we knew how it happened. Still another thing--you know, I am sure, of superconductivity. Certain metals—not copper and silver, the best ones--but tin, lead, and others like that, if cooled to liquid helium temperatures, have apparently no resistance to the flow of electricity. We take a ring of tin, cooled to these temperatures, and put a magnet up to it. We draw the magnet and that induces a current in the ring, with no battery, of course. If it is cooled to these low temperatures, we can go away and come back tomorrow and the current is still going around.

Again, if we knew how to produce this lack of resistance in conductors, just think how that would revolutionize power lines. Not only that, but the fundamental theories of the structure of matter, when you

get near absolute zero, take very simple forms that can be tested. That is a fundamental approach to the most basic things in the subject.

You know some of the applications of helium, of course. It is used for welding, and it is used as a non-inflammable gas, which is very light, and so forth. So it is a most remarkable substance. It is in limited supply, and I am very happy to say that the Department of the Interior has taken steps to conserve it because of these remarkable things.

My point here is that you can see what the possibilities are by research at these very low temperatures and the properties of this mysterious matter. Some day we will be able to understand this and apply it, but we haven't done that yet. That's the kind of promise which basic research can show.

I don't want to weary you by pounding on the table about basic research. You have heard me talk about that before, and you've heard others, I know. I should like to take this occasion to go into some broader aspects of this whole subject and set before you some of the large problems which are opening up in this age of science and technology. While each age of man regards itself, I am sure, as being unique with respect to the future, I guess we can say with some assurance that ours is certainly more unique, if there is such a thing, than others before ours. One needs only to refer to the fact that for the first time we have learned how to get off the earth entirely and do something in outer space, which

itself is certainly impressive.

Well, in the first place, science and technology have become overwhelmingly important for national defense. But the new forces now available to us--and I am not saying anything original in this--have raised very grave questions about the survival of mankind, or at least civilization as we know it. If these forces are fully unleashed, after that the dangers inherent in even the preparation for war, such as nuclear testing and the possibility of unacceptable fallout, and the implications of unrestricted preparation for warfare, become formidable, to say nothing of war itself. This question is on us with more insistence than ever before.

On the economic front, each country with any aspirations at all for its own improvement has recognized that the main avenue to this end lies in science and technology and the strengthening of its industry. This means a degree of technological competition in the world, far more intense than ever before. Furthermore, as man exhausts his capacity to feed, shelter, and protect himself with the natural resources at hand, he must increasingly turn to science and technology for help in meeting these fundamental needs. He must look for ways to increase the productivity of the soil, to increase the nutritive value of existing foods, to synthesize foods, possibly, both plant and animal, to make duplicate synthetic substances to supplement all natural ones. He also turns to science for new sources of energy, in anticipation of the day

when fossil fuels approach depletion. These are all well known things.

Despite the spectacular progress in basic research in recent years, there are still large areas intimately affecting man and his environment where science has scarcely begun to probe. I heard a talk by Lee DuBridge the other day. The main thesis was how ignorant we are. He said we think we know a lot, and when we come to analyze it, we know very little. Just think how much remains to be done.

Notable among these areas where we should learn much are those of meteorology and oceanography. Meteorology now is being regarded as perhaps too special a word. We are really talking about atmospheric physics, everything about the atmosphere that governs the weather. Meteorological studies to date have given scientists tantalizing insights concerning the vast forces which influence our weather, as well as some hint of potentialities for modification and control.

The earth satellite has demonstrated its usefulness in acquiring and transmitting back to earth a wide variety of data influencing world weather conditions, and these, of course, must be incorporated into our theory and into our practice.

The importance of both the economic and the security aspects of atmospheric physics notwithstanding, research in this field has been very meagerly supported thus far. A special committee of the National Academy of Sciences has made a thorough study of the situation and has urged that much greater support be given to our national efforts in

the entire field of atmospheric sciences which are now opening up.

Similarly, a report by a special committee of the National Academy of Sciences has been made in the field of oceanography, which strongly urges systematic plans on the part of the Federal Government to develop this field intensively, over the next 10 years, with participation by many Federal agencies, and, of course, especially by the Navy.

The splendid and comprehensive work during the international geophysical year has uncovered not only unprecedented global data concerning the whole field of geophysics but has also uncovered new areas of great significance, notably in oceanography, in the Antarctic, in atmospheric physics, and in research observations in outer space.

There is still another even more fundamental aspect of this matter which really deserves serious consideration, and now, for the first time, this is beginning to be very important, and we must not overlook it, in my opinion. This is the possible influence that man's experiments may have upon conditions on his planet. We like to think that in the progress of science we may begin to learn to exercise intelligent control over nature, but now we are beginning to realize that in certain areas, unless we are extremely careful in our experiments, we may produce unplanned effects that may have far-reaching consequences which are unfortunate for us. A notable example is the one I have mentioned, radioactive fallout. This subject is receiving serious attention, and, of course, careful analysis. It is not regarded as a

serious danger now, but it is one which is causing great apprehension.

However, I want to bring up another side of this. Attention has been concentrated to date upon the effects of fallout on the human race and possibly on life in general. This is, of course, most important. Nevertheless, there is a broader view which must not be forgotten--the possible physical effects of the continued presence of radioactivity in our atmosphere. I am just illustrating the kinds of problems that are going to arise.

Recent studies of the composition and behavior of the entire atmosphere surrounding the earth, made possible by rockets, satellites, and other modern techniques, show that the physical composition of the atmosphere and its degree of ionization, for example, are not entirely stable but are very responsive, for example, to radiations and emissions from the sun and outer space. Everyone knows this, I am sure, from the difficulty with radio communications, which sometimes have blankouts and sometimes fade, due to fluctuations in the ionized regions. Extensive fluctuations caused by the sun manifest themselves to us directly by variations in the earth's magnetic field and by effects upon our radio communications.

These influences may also be important in determining variations in weather and climate, to a degree that we cannot as yet begin to understand. If widespread and concentrated nuclear explosions on and above the earth's surface should be undertaken, our atmosphere would be

exposed to continual and protracted radiation from radioactive particles which, wherever they went, would cause copious electrification of the atmosphere and the production of ions. These radioactive nuclei don't fall out. They are just atoms and they are around among the gas. So they may keep on for thousands of years giving off their radiations, and when they do they produce ions. So we are producing ionospheres all over the place in a mild way, you might say. That's my point.

This is not a transitory thing, then, but might continue in some measure for years. Such a widespread phenomenon conceivably might disturb this uneasy equilibrium in our atmosphere and bring about changes in climate and temperature, or severe storms such as living creatures could not possibly tolerate.

Careful forethought, experimentation, and analysis on the part of scientists provide the best form of insurance against any such happening. A point to be stressed, however, is that the onset of a nuclear war would not permit time for this type of careful research and analysis. Although the possible effects of a continuing presence of excessive radioactivity in our atmosphere are by no means certain, we should note them as a most ominous possibility.

It is a very troublesome field to do research in. I merely call attention to the fact that it conceivably might be very important indeed. An illustration of this point occurred in the first contemplated experiment of a nuclear explosion--going back to Alamagordo. The question was:

Was there any possibility of this novel release of energy detonating the atoms of its surroundings--the earth or the atmosphere adjacent to the bomb? This possibility had to be examined very carefully, and the evidence was found to be overwhelmingly in the negative, before this experiment was performed.

In this connection it is interesting to go back somewhat in history and quote a remark by the English physicist, Dr. Aston, a pioneer in the subject of isotopes, made in 1923. At that time the theoretical possibility first emerged of the utility of the famous Einstein equation E equals MC^2 in the case of the fusion of hydrogen into one of helium. Any physicist at that time, knowing Einstein's equation, could see that if you could put hydrogen together to form helium you would get a tremendous amount of energy. But everybody thought the whole idea was rather absurd and that there was no possibility of ever doing it. Even Einstein said that. But note. Aston says this in his book:

> "If this experiment were ever undertaken, the success of the experiment might well be published to the universe at large as the birth of a new star."

This was 1923. This is an illustration of the kind of thing we have to consider.

Perhaps the best instance of the point, the modern one, I am trying to make occurred in Project Argus, which you surely know was a brilliant undertaking in both conception and realization, last year. The

young theoretical physicist, Nicholas Christofolus, proposed that a nuclear explosion beyond the atmosphere might result in a trapping by the earth's magnetic field of the electrified particles produced by the explosion. This would be expected to result in a belt of electrified particles, at whatever altitude this might be above the atmosphere, oscillating to and fro, roughly between the Arctic and the Antarctic Circles. This would be caused by the earth's magnetic field. Such a natural so-called radiation belt was discovered at higher altitudes by VanAllen in our early satellite experiments. These are much higher altitudes, of course, with much greater intensity. Nature had already done it. But this experiment that we could make was carried out by the Department of Defense, as you know, and the prediction was verified. That is to say, the radiation belt was formed at lower altitudes, which persisted for some time, and it was confirmed by a high-altitude rocket probe. As a matter of fact, we sent one satellite on purpose to see whether this artificial radiation belt was present, and it verified that and made a pass through the belt every time it went around. So we have evidence of this.

The experiment resulted in an artificial aurora, so man was able to duplicate that. But the great significance is that this opened up the possibility, if it was realized in time, of man being able to establish a layer of electrified particles that might, for a definite length of time, interfere with radio communication with a satellite, so that there would

be a possible blanking out of the satellite and it wouldn't behave the way we would like to have it behave.

That's a rather ambitious experiment and we have to watch out, if we do such a thing, to be sure that we know what the consequences might be if we proceeded to send up or establish such a radiation belt and found that thereafter we couldn't communicate outside of the earth. That would be a pretty important thing to know about ahead of time. This illustrates my point.

The conclusion one can draw from this line of thought, getting more philosophical, would be that, in the age-long struggle of men and nations between world competition and world cooperation, mankind had better learn to cooperate, the farther these experiments go and the broader scale they run. The sooner man learns to use his power over nature, not against his fellow man but in the common cause of mankind, the greater will be his mastery of nature and the less the risk of his early destruction at his own or nature's hands.

It is here that the scientists in the world should feel a solemn obligation to cooperate fully in making progress in this truly global problem. That this cooperation can indeed take place is evidenced by the outstanding success of the IGY as a worldwide cooperative undertaking. While the detailed programs of the IGY do not require the same continued intensity of effort that occurred during this period, cooperation on major features of this program, which are truly global in extent,

and form focal points for continued cooperation, is highly desirable on these worldwide problems.

You are aware that right now, in one feature of the IGY, namely, the work in the Antartic, the United States has invited 11 other nations to confer with regard to a treaty in the Antarctic. This is now being held in Washington and it is hopeful that we can establish cooperative scientific programs there in the Antarctic and carry on in much the same spirit.

It is clear that considerations such as these lend additional emphasis to our thesis that science, particularly our basic research, needs full support to look into some of these matters. I should have mentioned earlier, and I am sure you know, that, in the process of supporting basic research, we are also supporting the training of all scientists and engineers, because it is via basic research that they get this training.

I have often been asked--making a play for basic research--what I mean by an adequate program for basic research. I don't like the word "adequate." It always seems to me a weak word. It's just enough, and you can't be sure that it is enough or not. The logical thing, of course, is to be sure that those who are capable of doing basic research and are not needed for applied research have what they need to do their work. That means everyone who is really competent should be given a chance to do his utmost. That's the only way that we can really do, I

would say, an adequate job. An adequate job mowadays is the best we can do. It isn't just a passable job. There's no question about that.

Then, why are we justified in urging those who feel this way to support all competent scientific researchers? Won't this lead to an impossible economic situation? Obviously we don't have funds, manpower, or facilities to carry out all the ideas that may be generated. It is true, of course, that the security of the country depends not only on its progress in science and technology and its defensive strength in the military weaponry but upon many other factors, the chief of which is the strength of its economy. The latter in turn depends upon many factors, including the proper balance among its activities. Are we then in a dilemma? If we make the maximum effort in research and development, do we jeopardize our economy and therefore our security? On the other hand, if we withhold adequate support of research and development, do we jeopardize our security by failing to maintain technological supremacy? That's a real question.

The answer, I believe, is clear. We should encourage and support basic research to the limits of the abilities and capabilities of our scientists and engineers. By so doing, we make available to ourselves the full potentialities of all new discoveries of science. We should then give careful attention to which of these potentialities we should emphasize and support for development and ultimate production. This is one of the

best ways, then, to control the national budget. In this way we should be able to maintain a sound economy and at the same time achieve our highest priority goals. To support basic research fully requires relatively modest amounts of money, except for capital facilities. It is in the latter stages of technology, especially development and production, that the large costs occur. Therefore, we are not jeopardizing the national economy if we provide full support for basic research. In fact, I should go further and declare emphatically that, unless basic research is adequately supported, we are certain to miss opportunities for development and application that may make all the difference between success or failure in the race before us, whether for war or for peace.

As history amply records, the most epoch-making scientific discoveries have come from basic research, but this, being exploration into the unknown, by its very nature cannot predict exactly where these break-throughs will occur. Therefore, comprehensive support of research has to be undertaken in order to overlook no opportunities. This can be regarded as investment, the precise spots where high returns occur being unknown in advance.

Then, to recapitulate here, we can only insure the possibility of full protection of national security by giving every encouragement to scientific research and the training of scientists and engineers who have that aptitude. It is only in this way that we can achieve the ideas and the break-throughs which promise this clear superiority and be

sure that the developments we undertake are modern and up to date in every detail. The results of such research in competent hands are never without value, because, even when no break-throughs appear, the total effort always brings a possible break-through clearer.

How do we proceed with this? You will notice that I said the key to protecting the economy is the selection of the important things that are expensive and have the highest priority. That, of course, is where the rub comes. You in the Department of Defense know this very well. It has always been a struggle. I have two minor suggestions to offer here. This problem is not by any means new either to industry or to government. The technical industries especially have developed considerable competence in dealing with it.

The most important point is to make certain that we take advantage of modern techniques for this purpose. Industry knows them quite well. There are two modern techniques that stand out especially promising. One is the use of systems analysis, as it is often called, and the other is the use of operation research techniques. Both warrant further study and more intensive application.

In systems analysis, analysis and evaluation of an entire program for development is laid out in detail in advance, showing all the component items, their scheduling, their difficulties, and their needs, individually and collectively, where the bottlenecks lie, and so forth. This is already an advanced technique, but I want to point out that it

has been greatly enhanced by the adaptation of modern computer techniques which can handle complicated arrays of data and lengthy calculations with incredible speed. In fact, on a modern computer those who know how can almost have the machine build a device in imagination and know how it will function, and then modify the parts of it and show how that will affect the output. Now, this is crude, to be sure, but it eliminates a great deal of the old-fashioned, tedious model-building.

Operations research provides a different approach in that the degree of success of a proposed item or system can be evaluated in relation to its effect upon the operation. For example, as you know, operations research methods can give an estimated quantitative prediction upon the success of a military operation or a scientific undertaking by the introduction of a new weapon, such as a new missile. The Department of Defense has, of course, its weapon systems evaluation group, and the three services have individual operations research units.

I just point out that these are two techniques that we could, by intensive study, take fuller advantage of to solve this highly difficult problem of selection of where you are going to put your priority and your biggest cost.

The Federal Government is now better prepared than ever before for the consideration of matters of this general type, largely because of the concentrated thought that has been given this subject by so many

people. In addition to the interested departments and agencies, there is a new Federal Council for Science and Technology, recently established by the President, there is the Special Assistant to the President for Science and Technology, now Dr. Kitsikovsky, formerly Dr. Killian, in the White House, and there is the very active President's Science Advisory Committee.

In fact, responsibility for science matters in the Federal Government on an overall management problem which is related to how we do these things might be described best as follows:

With respect to the role of the Federal Government in the support of basic research throughout the country, the National Science Foundation, with its National Science Board, has the prime responsibility for dealing with policy concerning Federal support of basic research throughout the country. The Federal Council for Science and Technology deliberates on matters of policy coordination and future planning among the principal Federal agencies concerned with research and development and makes recommendations to the President. The President's Science Advisory Committee is drawn from nongovernment scientists and engineers and considers important scientific and technical matters in relation to government policy, with special reference to national security and not confined to military departments. And, finally, the Special Assistant to the President for Science and Technology, in the White House,

as I have said, makes available to the President at all times advice and counsel on the wide range of scientific and technical affairs.

Well, I have talked at considerable length and rambled a little bit between various topics. Perhaps in closing I should just point out that those of us who thought of these things, particularly those of us who have been engaged in the field of science, boil this thing down pretty much to a very general proposition as far as this country is concerned. We are not the largest nation in the world, by a good deal. Russia is larger, and Russia is making faster progress than we are in developing some of these things. After Russia is India, which is still larger, and China which is still larger yet. China is moving fast.

The role of the Foundation, obviously, is to make use of its know-how, of which we have a great deal, and the competence of its people, but, unless we can all understand that what everyone does has got to be the best he can do, we are not going to get very far. If you stop and face facts, with the determination that other countries are showing, particularly, right now, Russia and China, we can't get on with this job in any half-hearted way. We've got to go at it with our very best in view. Many of us have thought that perhaps a good slogan for this would be the achievement of excellence. This doesn't mean just in science; this means throughout. People have just got to learn that they must do the best job they know how to do, they must get the best training to do it, and we have got to see that they have the wherewithal to do it, whether

it is money, equipment, or personal assistance, or what not. This is a very important matter. There is just no question about it. That needs consideration not only from scientists but from others.

We are aware of the goals we would like to attain. We are a little confused about how we get there. It appears more and more that we continually seem to be complacent over results. Each person thinks someone else will do it, or Uncle Sam will do it, or some one. But it is everybody's job in a democracy, and, while I don't want to make a sermon on this, it is something that we've all got to think about seriously.

As I wrote to the President last year, whether our objectives as a Nation are to deter our enemies, to achieve and maintain world leadership, and to extend a helping hand to underdeveloped nations, or are merely to maintain our peace and prosperity at home, the first essential is a real determination to achieve better education, better science and technology, and above all the development of quality, quality of training and quality of performance; in other words, the achievement of excellence. Unless we can succeed in accomplishing these things, we can maintain neither our national objectives nor the personal objectives of our people.

Thank you very much.

COLONEL FORBES: Gentlemen, Dr. Waterman is ready for your

questions.

QUESTION: I read a good deal in the newspapers in the last two or three years about the shortage of scientists. I wonder if you can expound on that a little bit for us, and tell us what we are doing about it as a Nation, if anything.

DR. WATERMAN: Well, anyone who has looked at this subject knows that we still have a shortage of scientists, engineers, and such. The overall numbers are not very meaningful although they are with respect to what Russia is doing overall. It seems to be true that Russia is training about 2 or 3 times as many scientists and engineers per year as we are, and turning them out. In the case of our own shortages, they are spotty. It is not fair to say that there is a shortage of every type of engineers, for example. Civil engineering is much as usual in its output and its demands, as far as we can make out. On the other hand, for electronic engineering, aviation engineering, and aeronautical engineering, the demand is very high. The same is true for this new brand, called nuclear engineering. Those are critical spots.

The same way in the sciences--there is a shortage of physicists. They are in great demand. And we are short of mathematicians of the applied variety. There is less of a shortage in chemistry, and so on. So the shortages are spotty.

As to what is being done about it, we have, of course, in the long run, the fact that careers are more and more rewarding in the financial

way and there are opportunities in the critical areas where there are shortages. So that first has its effect in industry, by people moving over to those areas, and that gets the universities exited, because they are losing their best men, and they begin to raise their salaries. This makes the career more attractive. That is our common way of doing it.

Specifically, what we are doing is we are offering fellowships in the sciences in a big way. We are offering training for teachers to stimulate that profession. And we are offering research grants and contracts in a big way to see to it that those who are doing research can get on with it. To the limit of the funds that we get, those are the ways we are going about it, really, plus, of course, propaganda to this effect, which takes place rather naturally because industry and the universities are talking it up all the time, and the Government, too.

I hope that answers your question.

STUDENT: Yes, sir.

QUESTION: In recognition of the fact that a person in pure research might work a whole lifetime and never produce anything, how do you as an administrator evaluate the people you have engaged in this type of work to see whether you should continue to spend limited funds?

DR. WATERMAN: This depends on what you want for your evaluation. In the Defense Department you might want to use scientists for the results they can give you in their own research which could have a bearing on plans

for development. A good man will almost certainly give you data and ideas which make it worth while. But they can't be judged by the hardware they produce, because generally they don't produce any, except in engineering. What you have to judge, then, is the quality of the man's work, his published work, as judged by other scientists in the same field. If he is making a record in his field, then his ideas are good and he is influencing the field, and he is a good man to have on your side in Defense if any questions come up in that area. He may make some capital discovery, and that's fine, but at first it will be only announced as a paper on maybe a new theory, or something that he has observed, or some effect you could get. You watch for those things.

Generally speaking, you judge basic research by the output of the man , the quality of his output, rather than the quantity. This is judged best by his colleagues. They mull it over, and other people repeat it, to see if he is right, and they carry it on. That is really the life blood of research to such a person, anyway. His job, as he sees it, is to do something or find something which is original, that no one has ever found before. That's what he is trying to do. The extent to which he succeeds in this measures the extent to which he is a success. And that can only be judged by others who know the field, really.

That's kind of a vague answer. If he is making that kind of progress then, you see, he is stimulating others, and he gets to be a good consultant, because he is an authority on this field, a head and shoulders above the

others. And chances are that something may come up where he is very badly needed. An instance just occurs to me. We had problems of vision during the war, night vision. The questions were: How do we test for that? What people have it? What can we do to improve it? It was a very critical matter when flying at night got to be so important. How did we get the best answer? Not by a rather direct approach. Word went out asking what we could learn about it. Everybody did a great deal of experimenting, by the cut-and-try method, more or less, but that's a wasteful method. We had to try it. In the end we got the answer out of a couple of specialists who had been working on the eyes of the horseshoe crab. That's a hobby you wouldn't think would have any great interest, but they learned more about the horseshoe crab because of the simplicity of the horseshoe crab's eye, which could be applied to the human eye, than they could have learned by direct experiment, because the human eye is too complicated, you see. It's that kind of thing that comes out.

QUESTION: A previous speaker of authority in this field indicated that he felt that the present government effort in the field of basic research is handicapped by emphasis on the operational aspect, which is naturally given by agencies of the Government which have a primary mission of an operational nature. He felt that we needed a Department of Science and Technology in the Government. What do you think about it?

DR. WATERMAN: Yes, that question has been discussed rather fully. I have some very definite ideas of my own about that. In the first place, when you are talking about emphasis on these relative fields, the operational and the end-products side, I think this ought to be left to the agencies that know their missions. How can anyone else decide for the Department of Defense the right kind of research to do for defense? The Navy and the Air Force know their problems. They are expert at that, and they know the fields in which they would like to start development. They should have first hand, not second hand, the information which gives them background to go further. So I say this is a problem for them to decide, not an outside central agency. We would never want to do it in the Foundation for that reason.

Basic research will always have to struggle, as I said, against the pressure of events. If you have a limited amount of money, you must do something for the things that have got to be done today, tomorrow, or the next day. Basic research you always feel you can postpone. It's too vague. But the very fact that this is true, and the very fact that basic research in the long run will produce capital things for you, and start whole industries, shows that we can't keep putting it off. We've just got to go ahead with it. It is not an expensive thing to do.

So much for that. Therefore I think the National Science Foundation is in the best position to look at that. Here is an agency that doesn't talk about end products. It's talking about backing this kind of thing

which is involved in the training of scientists and in keeping the long range going.

As for a Cabinet department of science and technology, I feel this way about that: It is a natural thing to think of just because science and technology are important. But that isn't reason enough. You have to analyze a little further. When people talk about a department of science, they don't really mean that, I am sure. They mean technology. They are thinking about the things that a science department could do, and that really is technology. So let's be clear. It means that when they say a Department of Science and Technology, or a Secretary of Science and Technology.

In the first place, this doesn't seem to me to be appropriate, because research and development is not an end in itself anywhere. It is a service function, it seems to me, for the departments with their practical missions. Each agency, as I say, knows its mission and should have the opportunity to do what it chooses to do along the line of research and development. Research and development is a means to an end in those agencies. I don't see how that can be dictated from a central agency. That agency won't know the problems well enough.

A second thing is the administrative difficulty. If you had a Department of Science and Technology and gave it fairly broad powers, what about the Office of Air Research, for instance, or Army or Navy research? They would have two masters, it seems to me. They would have to report

to their own agency and in some way report to this central agency, and the central agency would somehow control or supervise what they were doing. I don't think this is administratively possible. It seems to me it would just cause confusion.

So I would say that, to have a research and development function in each agency is the right way to do it. Now, is it possible to have a central agency? Well, yes, it is, if you make it science and basic research, because that is pretty general and common to all the departments and is not related to end items. You can centralize that. But we don't make Cabinet departments out of things like science, any more than we would make a department of economics. Science is a field of study. It is a discipline where you go ahead regardless of end products and try to make all the progress you can. It is an academic sort of thing.

It doesn't seem the right sort of subject for a Cabinet department. A Cabinet department wants practical results for its people, like Labor and Health and Defense and Commerce, and so on. Science is not that kind of thing.

So I think things are quite well off by having an independent agency which will stand for science in this way but will keep out of the way of the mission-related departments, because it can't know as much as they do about their jobs.

QUESTION: Dr. Waterman, many scientists have complained that

the climate in government employment has not been conducive to the same freedom of work and expression that is obtainable in the university atmosphere. Can you suggest to us, sir, how this may be improved?

DR. WATERMAN: That's a large question and a very important one, I believe. I know that those who are employed in government laboratories are very well aware of it. A simple answer is this: If you have a good director of the laboratory, he does pretty well by it. He knows what the importance of this is. Of course government laboratories are subject to civil service requirements, and that's a drawback, when you try to match industry for the top people, for example. Then the general practice that civil service adopts gives less flexibility than you can find outside.

So those are things that have to be reckoned with. But that's an old story to you. On the whole there has been a healthy move in the right direction here, except the demand for scientists and engineers, which has robbed some of the government laboratories of their best people. I say the trend is good. People are realizing that a healthy research and development laboratory must do some basic research. There are many reasons why.

One is it gives them an insight into their development work as background work which can effect good efficiency and economy. Another is it enables them to hold some very competent people. Competent

scientists and engineers depend a great deal on their reputation in basic research. One must give them opportunity, then, to achieve standing in their profession, you might say. Also, basic research is a way in which the work done at the laboratory gets to be known throughout the country, and that's an asset to the laboratory.

All those things a good director knows. I suppose the chief difficulty in government laboratories is that, once in a while, if directors change too fast, or if the head of the laboratory changes too fast, and if a director gets in who isn't aware of these things and doesn't handle them well, then during his term of office this can be serious. It is necessary to have a continuity about this to be sure that these things are handled well.

There is a continuing study on the part of people interested in government laboratories on how to deal with this situation.

QUESTION: My question has to do with, I guess, the field of information retrieval. The amount of money we are spending on research in this country has increased. I wonder if you might comment on what is being done to simplify and modernize the exchange of research among the members of the scientific community.

DR. WATERMAN: This is a question that concerns us in the Foundation right now very much. Last year we were given this assignment of what to do about it. We were given it by two sources, one from the President, who got out a letter to the agencies and specified that we should serve

as a center in dealing with this, and the other was that in the National Defense Educational Act there was a title which gave us the same kind of authority.

What we have done is, under the leadership of a very able man, Dr. Atkinson, we have set up a scientific information service office. We are not trying to do the whole job. We have canvassed this, and no one thinks we should try to copy Russia by starting an all-union institute to do everything. This isn't our way. What we are trying to do is to make sure that competent agencies, both public and private, know these problems, are working at them hard, and are aware of what each other is doing, so that we have better coordination.

We have an interdepartmental committee in the Government with representatives from agencies concerned with this, and we also have a council, which is stipulated by a Congressional act, of people in private life as well as in government, to get their wits to bear on the whole thing.

We are making progress. Now, specifically, there is the human problem, the organizational problem of how to make people understand this and work together. We are working hard on that, taking the lead in this. Then there are definite ways in which one can get at it. For example, there are modern machine methods of identifying material in a library, reproducing it, and getting it to the person, or how you locate it in the first place. This involves a lot of work on classification,

storage, and then quick devices for retrieval, copying it in some way, and getting the material to the person.

It is a large problem, and not an easy one at all. We are trying to find the competent people and giving them all support to work out these things.

Another problem which is related to what you ask is the matter of translation of foreign papers. There the Foundation is taking a major position in this, along with having a lot of pure scientific work translated. The Department of Defense is having a lot of it done. The CIA has contributed its share. This is being pooled, and we are making sure that this can be available all around.

On that mechanical translation is getting a good deal of attention. There are several methods, believe it or not, that seem to have some promise of success. True, you can see the machine hunting around sometimes for the right word. At the present time, the machine comes into trouble with grammar occasionally, and then gives you alternatives. It sort of explains to the reader what its troubles are, so he can figure them out. Or it will present three different words and say the machine doesn't know which one it is, and you choose.

That's as far as we've got. I don't know whether you saw this illustration of what machines can do along this line--not translation, but something similar. The Manchester Guardian quoted two love letters that were written by a machine. They gave the machine a vocabulary

and the rules of grammar, and told it to go to work. The love letters looked curious. You could just see the machine hunting around for a word, and it didn't come out too well. It wasn't poetry. The article closed by saying, "Perhaps these aren't very good love letters, but at any rate the machine can turn 200 out a minute, all different."

QUESTION: Sir, a current topic is the Nobel Prize. My question is two-fold, sir. Is that Nobel Prize primarily to promote basic research? Second: If so, what part does our Government play in providing such incentives for our basic researchers?

DR. WATERMAN: Yes, the Nobel Prizes are for research. Well, they call it science. They are for science in the medical and biological fields, in the physical science fields, and so on. It is for advances in science as such, not the practical applications.

We have been doing pretty well, by the way, in Nobel Prizes in the last few years, as you know. The majority of them are coming to this country.

What we are doing to encourage this is, of course, the whole Government system support of research, by grants and contracts, is a great incentive, because this gives our people what they need to work with, and that is distributed rather widely. Then we have fellowships, to junior people, graduate students, those who have got their degrees, younger people, and senior people, to spend the time wherever they like to carry on their research. This is a big incentive. Just recently,

as you perhaps saw, Congress passed an act creating a Presidential award for excellence in science or engineering. This would establish up to 20 awards a year for excellence in the work of science and engineering. That is thought by Congress to be an additional incentive.

So these are the ways in general.

QUESTION: Previous speakers have commented on the subject of getting research done overseas, in countries other than the first 48 of our 50 states. The pros and cons vary. Some are: It's a good idea to get all the information you can but don't let them build any expensive equipment other than in this country. Can you give us your thoughts along that line?

DR. WATERMAN: That's a very important question, and, I think, a troublesome one. I have been in the middle of discussion of this now for some time. I don't know. I suppose the first question to ask is whether I can say a lot about it. The work we are supporting abroad is very good at the present time, I think. The programs are very good on the whole. The first question to ask is: Why do we do it? I don't think we have any policy. Each agency does this when it feels it is a good idea, but I don't think we have any Federal policy as to why we do it.

We do have a policy with respect to underdeveloped countries, in the hands of ICA, and so forth. That's quite definite. But I take it that isn't just what you mean. You mean supporting research in

countries like France, Belgium, Germany, and so on. Here we do a little in the Foundation. Our criteria are about as follows:

In the first place, if it is a top quality of research, better than what we could do in this country, and if the people are really tops in their fields, we would do it there, in the interest of our wanting to get on with it, and in that the results would be useful to us. Top quality is the first thing.

Second, provided the location is unique, we could support research, in the Tropics, for instance. We haven't got the right kind of Tropics to do research here. Or we could support research in the North. In regions that are particularly adapted to research, we could support that and it would be to our benefit.

That's about as far as we have gone to the present time in the Foundation.

A cooperative plan between countries would be nice. I don't think most of us think of it in that way.

Those are some of the considerations that go in. I think that what we should do, really, is to know why we are doing it. This is not being critical of programs that are there. The programs of the military department in Europe now are very sound. It is a good quality of research. There's no question about it. Just why we do it is a question that should be faced. Is it to help the other country? Is it to bring research returns to us? Do we do it because it is cheaper? The other countries may be

apprehensive about this. They may think that by providing support we ultimately have the idea of drawing people from their country to ours, and so on. Those are questions that come into this.

QUESTION: Sir, Dr. Bush, in his book, "Modern Arms and Free Mem," published, I believe, about 10 years ago, stated that time was on our side. In view of the expanding population and the great emphasis being given to science and industrial production in the Communist world, do you feel that this is a true statement today?

DR. WATERMAN: I wouldn't say so. I think things have changed. Ten years ago even it was hard to be sure, with Russia going ahead as fast as she is, and we know she is doing first-class research in many fields--not in all--and that her technology is pretty sound. It has one characteristic which I think is worth remembering. It is not as complicated as ours. They usually get a simple developmental plan, make it rugged, fool proof, and get it fast. And it is reliable and easy to reproduce. We are after more sophisticated development. We try for something much better, but we are apt to be a little more sluggish in getting it out and postoning the decision as to when we get the final article, which stands a little in our way.

In the long run our method is much the better, undoubtedly, but in the short term they have a pretty formidable approach.

I would say that statement is no longer true.

QUESTION: You spoke of trying to develop an age of excellence.

In general your support starts at the graduate level. Is there any place in the Government where there is a program to try to reverse the trend that we believe has been instituted by the colleges of education and the teachers' colleges of developing the common man in social adjustment?

DR. WATERMAN: Yes, we are doing a lot in the Foundation, as a matter of fact. Most people don't know that. After Sputnik the executive branch got into a huddle to see what could be done about education. The result was a division of effort between the National Science Foundation in science education and the Office of Education in HEW. The difference was that HEW didn't have the authorization to do it. They had to go out for a bill to get money, the National Defense Education Act, and authority. We already had the authority. We just simply got more money, and we are carrying out that side of it.

We deal with the sciences themselves, rather than with educational theory. What we do we do for particular sciences separately. What we do for physics and what we do for chemistry and biology are separated. This gives one advantage, and so the two programs, our own and that of the Office of Education, supplement each other very well. The Office of Education customarily deals with the state boards of education and that chain of command, you might say, in the whole educational field. That's the recognized agency to do that.

But that is a troublesome and important area. It is where we get the fundamental things done in our public education system, but it is a complicated one to deal with. The states are all different. The state boards of education are different. Then you get into problems of what to do about public schools, what to do about private schools, and parochial schools, and all that, where you are dealing with education in general.

If we want to do something with the field of physics, we can do it directly with the school, whether it is a parochial school or a private school or a public school. This is a good complement to the other, you see.

Actually, what we are doing now is, this last summer we had 320 summer institutes, most of them for high school teachers, and most of them separated into one for physics, one for chemistry, one for biology, and so on. These are established and are in the hands of, usually, colleges or universities. They invite 40 or 50 teachers of that science, bring them in, get a staff to coach them in the subject, and they have a chance to compare notes on how they teach it.

This has been happening for the last 2 or 3 years, and it reached that number last year of over 300. This stimulates the high school teachers to learn more science, and they can compare notes. When they go back home we find this tends to give them promotions, and so on.

We have another program where we finance a limited number of universities, about 20, to establish academic year programs for teachers. They commonly then bring in 40 or 50 teachers to study for the year, graduate courses--not undergraduate--in their subject, in the latest subject. They come out with a Master's degree.

Then we have fellowships, too, for teachers to take time out, including fellowships for high school teachers to spend the summer at various places. We have started a new program on interesting undergraduates in research under the guidance of their faculty members. It is not maybe real research, but it is research to them in that they have to do something new, and that is stimulating to them.

One of the most important of our projects, I think, is what we call Course Content Studies. The most advanced is in physics. It was started by Professor Zacharias at MIT. He got together, believe it or not, some of the leading physicists of the country, and they put their attention on how to improve secondary school teaching of physics. They have come out with experimental texts. These have been tried at our summer institutes. They have been tried at typical schools. Teachers have been brought in to see whether these are feasible and whether they ought to change them.

In this way the Government is making available a new approach to the teaching of secondary school physics. The publisher can pick it up or not, or other writers can pick it up or not. That is not our problem,

distribution, but this is a research job which we are making available.

The biologists are following suit, and the mathematicians are well along, and the chemists are talking it up. If we can get modern, up to date, clear texts that have had thorough trial, this will do more for the elementary teaching of science than almost anything else, because it will jack up the students and jack up the teachers.

Those are some things that are going on. I didn't mention the National Defense Education Act programs. I think you are familiar with them. They have had a lot of publicity. They are doing a good job, too. They have more money than we have, and they have lots of opportunities to improve.

COLONEL FORBES: Gentlemen, practical considerations require us to close this question period. Dr. Waterman, on behalf of the Commandant and the students, thank you very much for a most informative and comprehensive coverage of this subject.

DR. WATERMAN: Thank you very much.