THE COORDINATION OF RESEARCH AND DEVELOPMENT IN GOVERNMENT AGENCIES

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Dr. Lawrence R. Hafstad was born in Minneapolis, Minnesota, 18 June 1904, and was graduated from the University of Minnesota in 1926. In 1933 he received his doctorate of philosophy in physics from Johns Hopkins. He was associate physicist at Carnegie Institution of Washington, D. C., from 1928 to 1933, and in 1931 with Dr. Tuve was winner of an award by the American Association for the Advancement of Science for Research for the development of the million-volt vacuum tube. His work also included research and development in propagation of radio waves; the measurement of the height of the radio-reflecting layer and its relation to magnetic storm; atomic disintegrations; and artificial radioactivity. In 1940 Dr. Hafstad started work on the proximity fuze for the Army and Navy and in 1946 was awarded the Medal of Merit by the Secretary of the Navy for his wartime activities in connection with the development of ordnance devices. Dr. Hafstad is the first Director of the Reactor Development Division of AEC, charged with the program of designing and developing nuclear reactors for the practical application of atomic energy for power, for propulsion of ships and aircraft, for production of isotopes, and for research on reactors themselves. Dr. Hafstad, with two colleagues--Drs. Richard Roberts and Merle Tuve, demonstrated uranium fission for the first time in the United States in 1939 in Washington following reports from abroad that German scientists had split atomic nuclei. Dr. Hafstad has been on leave of absence from Johns Hopkins University where he holds the position of Director of Research of the Applied Physics Laboratory. It was in this laboratory that he helped perform much of the work leading to development of the proximity fuze and also took part in research and development work in the guided missiles.

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COLONEL SEAWARD: Gentlemen, in your course in technological progress you have heard of the importance and of the quantity of the research and development that is performed within the agencies of the Government, as well as that sponsored by agencies of the Government and by agencies and institutions outside the Government. This morning we will hear something of the coordination of research and development.

Our speaker this morning, a scientist, is also an administrator and a coordinator. He is on leave from Johns Hopkins University, where he was Director of Research of the Applied Physics Laboratory. Currently, he is Director of the Reactor Development Division of the Atomic Energy Commission. He is a past executive secretary of the Research and Development Board and is the Chairman of the Interdepartmental Committee on Scientific Research and Development.

It is a pleasure to welcome again to the college, Dr. Lawrence R. Hafstad. Dr. Hafstad.

DR. HAFSTAD: General Holman, gentlemen: Every time I talk to this group I envy you gentlemen the time you have to think about these fascinating problems that are before us at the present time. Those of us who are in the middle of the day-to-day grind just don't have an opportunity really to think hard about the problems that we are handling.

We improvise as best we can from day to day. It is encouraging to me, therefore, to come down and talk to a group like this and to give you my impressions for what they are worth. I can always hope that out of these discussions and out of the thinking and discussions that you gentlemen have following my remarks, we may learn something about what we really ought to be doing and how we ought to be doing these things better. There is not time for those of us in the middle of the struggle, so to speak, really to think through the implications of the things that we are engaged in.

My subject for today is the "Coordination of Research and Development in Government Agencies," as you have heard. This implies at least that science is being coordinated. Sometimes I question that!

At the same time, I would like to ask the basic question, Is it necessarily desirable that science should be coordinated? We should not take things for granted in this business. Especially in a group

like this, we ought to get down to fundamentals, to brass tacks, to question everything and see that we understand everything as well as possible. So here we are coordinating science, and I raise for you the question, Should it be coordinated? I happen to think it should, and I will try to give you my reasons. Again I stress the fact that I give you only my impressions. I make no claim of knowing any of the answers.

Let us back up and take a look at the dollar figures that are involved in this business of ours. The last figures I have conveniently available show about 1.5 billion dollars spent by the Nation for research and development. This is important, particularly because just before the war the total figure for the Nation, including industrial research as well as governmental research, was 166 million dollars. Look at the change in the economics of the situation in 10 short years! We have grown from a 166-million-dollar activity to well over a 1.5-billion-dollar activity in 10 years. And the figure I have given you was pre-Korea and before the more recent expansion of the atomic energy program. So you can throw on top of the 1.5 billion dollars an increment to cover recent advances and expansions.

The reason for concern arises from two things. First, research and development is no longer an incidental activity. When you are talking about a billion dollars or two billion dollars, you are talking about amounts of money that are appreciable in any man's game. Such amounts represent an appreciable effort, even in the military activity.

I don't know what figure you carry in mind for equipping a division, but suppose we use the figure of 250 million dollars. Two billion dollars, then, would represent eight divisions. That means that we are spending something like eight divisions per year for research and development.

This is the kind of choice you gentlemen face, and this is what raises the basic question, Are we doing this in the right way? Which would you rather have--to go back to our childhood game--eight divisions of armored troops or a strong research and development program? This is the fundamental question.

Dollars, then, is one of the things you have to keep in mind in connection with the over-all program. A still more critical one, from my point of view, is manpower. I have already stressed the fact that we have grown from 166 million dollars to 1.5 or 2 billion dollars in this activity. Remember, however, that the 166 million dollars kept our best-trained, key scientists busy back in the old days and kept them effectively busy during the war. Where are we going to get the additional talent that is going to spend effectively this 1.5 or 2 billion dollars?

True, we are raising more people and getting them out of the universities. But this is a process that really takes time. If you look back and read over the history of the developments, I think you will find that the people who pay off, in applied research particularly, are those with, first, a very good fundamental training and, second, about 5 to 10 years of practical experience.

Well, we just do not have a lot of people now with 5 to 10 years of practical experience, and it is not a foregone conclusion that we can expand our research and development activity merely by expanding the dollars invested. There is a saturation effect in this picture that I want to call to your attention.

The two limitations on a research and development program, then, are dollars for their own sake, and manpower because in the last analysis it is good men who will make the developments which you people need.

The key question, therefore, for either civilian or military research, is, How do we get the most effective men working on the most important problems? This raises the priority question and puts the fat in the fire. The reason this happens is that as soon as we start talking priorities we get into arguments; we raise the basic question of applied research versus basic research. Our good scientists will argue heatedly that we cannot put priorities on basic research; that we cannot foretell what use basic research is going to have, and, therefore, it must be supported independent of priority. The people on the other side of the argument insist that first things come first and that, somehow or other, one must rank up all these requirements that we think we have and make sure that the most important ones are well supported. I think both schools are right. And as we go on through the discussion today, I hope to begin to make clear to you that basic and applied research are two different animals and must be handled in different ways. Both are important.

Basically, however, Congress--and the taxpayer--is putting up hundreds of millions of dollars for research. This is done on the assumption that someday there will be a compensating return. It is probably vaguely understood that this is a long-term investment, but investment it must be. It is certainly not a gift. The money that Congress puts up is intended, somehow or other, to return to the advantage of the American people. It is not given over as a grant to me and my scientific colleagues to play with.

Unless some usefulness can be foreseen, therefore, the money is not forthcoming. And if the use can be foreseen, the research in question is really applied research. This leaves basic research completely neglected, and basic research, we all agree, is absolutely essential for real progress.

This is the dilemna, and the problem, the Government is now struggling with. Many committees and boards have been appointed, and, to varying degrees, they either grapple with, or evade, the fundamental issues involved.

The problems are government-wide, but let us start with some examples in our own field, the military. It is generally recognized that our accomplishments in the field of new weapon developments since the war have been somewhat less than spectacularly successful. Yet, while money has been tight, the flow has been enormous compared to the years before the war. (I am referring here to the fact that our expenditures for research and development have been running along at about 500 million dollars a year ever since 1945.) For comparison, we might take the 75 million dollars spent by OSRD to give birth to a host of useful devices during the war, against the more than 2.5 billion dollars (500 million dollars per year for five years) we have spent since that time. Yet most of the people involved are the same! This suggests, among other things, how critical in this field a few keymen can be.

We have all seen examples of a very "hot" organization beginning to fall apart because a half-dozen keymen leave it. This is characteristic of the kind of activity we are in. It also suggests that our available mechanisms for evaluating programs and projects have been inadequate. Our applied research is not sufficiently "applied," and our basic research is not sufficiently "basic." This is one of our troubles.

On this matter I would like to quote Bob Wilson of Standard Oil of Indiana, writing in "Mechanical Engineering," January 1950:

> "/The value of a given research project,/ the appraisal ratio,...is the product of the probably value to the company of a successful result, multiplied by the probable chance of success, and divided by the estimated cost of the research and development--not merely the research cost."

Here is a hardheaded businessman trying to indicate how to assess a research project. All these things have to be taken into account: the value of the success, the probability of success, and the total cost. This is realistic thinking. This would certainly be a good index for the military activities as well.

Wilson goes on to say:

> "/Another/ requirement of a research director is that he keep the company business and potentialities in the forefront of his thinking at all times. Too many research directors have private hobbies. They may have a reputation to maintain in a certain

field, and they want to continue to publish in it. Such considerations must not enter into the direction of research. We cannot, in industrial research, afford a lot of hobby riding."

These quotes may sound as though I am opposed to basic research. Nothing could be further from the truth. The Nation can well afford to subsidize good basic research wherever it may be found. The trouble is that too many of our research activities are neither "bread and butter" applied research nor creative basic research. E. O. Lawrence, for example, has started to use the term "responsible basic research," implying, at least, that there is such a thing as irresponsible basic research. I am afraid he is right. The trouble is that, whereas basic research certainly need not be useful, the converse does not necessarily follow; namely, that just because an activity is pointless it is necessarily good basic research.

Let me give you some examples. I refer here to an article in the 31 May 1950, issue of "Pathfinder," a news magazine. It reports a news item on a military show that had been held. I will skip the first part of it and give you only the last part:

"The Air Force contributed a pneumatic rubber building; the Army presented a portable ice cream plant; the Navy had a diesel-action pile driver. As for the Signal Corps, it stole the show with a video-phone hookup that enables callers to see each other as they talk."

There is nothing wrong with that. It is a good demonstration. It is probably good public relations. I have no quarrel with the action that was taken so far.

It happens, however, that about 30 years ago I was working for a telephone company, and shortly after that the television industry, or technique, started to grow. For 30 years my friends and I have been talking about the possibility of having a television device attached to a telephone. There is nothing fundamental about this. It is just a trick that anybody could do any time he put his mind to it. It is not a question of not being able to do this. It is a question merely of its not being worth-while.

Let me go on with the rest of the news item:

"Asked what military use the invention has, [the sponsor] quoted Ben Franklin: 'What is the use of a newborn child?'"

This I object to. Up to this point it was frankly a publicity stunt, and it probably earned its keep as a publicity stunt. But here somebody is trying to jump on the basic research band wagon and palm

off a 30-year-old idea as something that is brand new and has all the value of "a newborn child." Here, you see, is where we jump the track. Here is something which is plain technology and masquerades as basic research.

There are other examples. Driving home in my car just last night, I was listening to the radio, and over the radio came the announcement of a remarkable development. It was right up my alley. Somebody had discovered how to convert nuclear energy directly into electrical energy. This is something that certainly would be of the greatest value to me, and, if it were really new, it would be a very fundamental first-rate discovery. The fact is that somebody was publicizing a thermocouple; a thermocouple, as near as I can make out, is something like 80 years old. It is one of the oldest ways of converting heat into electrical potential. Trivial amounts of current are provided. It is not an invention and it is not a new development--it is not anything, so far as I am concerned. Yet this is palmed off as a major scientific advance.

These are the things that happen when government money is spent. Note that I did not refer to government laboratories. My point is that the highly advertised efficiency of industry seems to refer to industry spending its own money. For the kind of work we are interested in, the proper comparison is not between industry spending its own money and a civil-service laboratory. The proper comparison is between industry spending government money and a civil-service laboratory also spending government money. In this case, the difference becomes vanishingly small, and the peaks and valleys of performance overlap. We have a spotty situation all over the country. Some of the government laboratories are good and some are weak; some of the laboratories are good in spots and weak in other spots. The same is true of our industrial contractors.

There is no easy solution to this problem. The solution lies neither in complete conversion to contract operation nor in conversion to a civil-service type of organization. There must be some best proportion of each. What is it? Where is the dividing line? Here, again, are fundamental problems that I would like to leave with you.

Returning now to the military research picture, we have seen some examples of how diversions of effort occur. Those of us within this room all know the steps that are normally taken to reduce such diversion to a minimum. In general, the work is sooner or later broken down into projects. In each department or agency, review committees are set up to review all these projects and to insure that each project can be justified. Within any one agency this works fairly well, and each feels, when its review process is complete, that it has a tight defensible program. It is when many such agencies approach a single source of funds--such as the Budget Bureau or Congress--that the weakness becomes apparent and charges of wasteful duplication and lack of coordination are raised.

This is the problem for which the Research and Development Board was organized within the military establishment. You all know the history of this; I won't belabor it. The important thing is that some mechanism was needed to insure that the different programs of the departments were all brought together, compared, the obvious undesirable duplication eliminated, and shifts of emphasis from lower priority to higher priority assignments carried out.

You probably all know, too, that this did not work out too well. There has been enough talk about RDB so that it is pretty well known that the mechanism bogged down in what we refer to as the "paper battle." I have referred to this as being the battle between the staff people who choose to question certain projects and the agency people defending those projects. It was an endless battle trying to get anything shifted, or to get money shifted from one thing to another. This was the weakness of the original RDB approach.

It is true that RDB has succeeded in doing a number of very useful things. They prepared a paper which is called a master plan for research and development, which is certainly better than no plan at all. It indicates those areas where, by and large, people are agreed additional emphasis should be given, and other areas where people, by and large again, are agreed that less emphasis would be sufficient.

The problem of evaluation is a tough one, and RDB, with the Joint Chiefs of Staff, has set up the Weapons Systems Evaluation Group to help in this evaluation problem. There are two stages of this. One is to evaluate a new weapon after it is essentially complete and its performance can be found. This is a relatively easy thing. You want to compare one kind of gun against another kind of gun, or one airplane against another airplane, and so on. This is evaluating a completed device. It is not quite so useful, however, from the research man's point of view, as getting a preview of how valuable a thing would be if it could be made. This is a much more subtle problem. You see, in the first case you have already spent the money; and you may have spent tens of millions of dollars developing something, then you take it out and test it, and the combat officer says, "It's no good." He should have the right to say it is no good and throw it out. But the sum of 10 million dollars has been lost. If we could learn more about this technique and learn to evaluate these things before they are completed, we would save ourselves a lot of money and save our laboratories a lot of waste effort. This is a direction in which we must struggle.

My conception of a development project is something like this: Everything goes through a growth curve. You know how a tree starts out slowly, then grows fast for awhile, and then tapers off. A research or development project is the same kind of thing. It grows slowly for awhile, then it flourishes, and then the point of saturation is reached, when it is extremely difficult to make any further progress.

The ideal way of handling this, it seems to me--it may be unrealistic, but it is something to shoot toward--would be as follows: In the early research stages, when the activity is small dollarwise and manwise, you can afford to parallel; you carry a number of things in parallel and explore the possibilities of new approaches. As you approach the knee of the curve, where your costs are bound to rise rapidly, just before you get to the hardware stage, then you should stop and think. That is the time when WSEG, and other systems organizations within each of the departments and agencies, should come into the picture and really sit down and think hard as to whether this thing would earn its keep if it were completed. It would save us a lot of money and effort if we can learn to do that.

There are all kinds of difficulties in evaluation. I will list some of them.

You can get a panel of "experts" to do the evaluating, but, by and large, they will have a bias because they have been in the business before; otherwise, they would not be experts. The other alternative is to pick neutral laymen who may have judgment but lack knowledge. These seem to be the only two choices we have. Neither solution is ideal.

You can imagine that RDB, with all of these problems to struggle with, had its difficulties. It was supposed to sort out all the research and development projects of the whole military establishment--I remember that when we started there were some 18,000 of them--then emphasize the most valuable and cut out the weak sisters. That was not an easy job, particularly since the bookkeeping systems we have had so far have been fairly inadequate.

Here is a report by a committee that was looking into the cost accounting situation, because anybody looking at a program would like to know what is being done, where, and how much it costs. I brought the report along, but I won't read more than a page. Here is the result of months of study by a fairly large and strong committee:

> "Study shows that little information is available to the Board indicating the rate of current effort on research and development, either in total or by categories. That which is available, namely, obligations, unliquidated obligations, and expenditures, does not reflect the rate of activity during the current or any other fiscal year because obligations precede, and expenditures lag behind, accrued costs by varying periods of time."

That is fancy wording for saying we don't know how much anything costs.

"Aside from this inadequacy, the figures submitted to the Board are prepared by such diverse methods as to make impossible meaningful comparisons and combinations between the Military Departments. The utility of the information is further impaired through its failure to reveal the cost of the many operations supporting research and development that are covered by appropriations other than those out of which direct costs are met. Moreover, the lack of standard definitions leaves room for varying interpretations as to which cost elements should or should not be included." (Abstract of Report by Ad Hoc Committee on Fiscal and Budgetary Information Requirements, Research and Development Board, 8 June 1949.)

I won't bore you with more of this. I merely wanted to rub your noses into the fact that it sounds easy to say that research should be coordinated, but this is the morass you bog down in when you try to find out who is doing what and why.

I would like to pass now from the Research and Development Board to the Interdepartmental Committee on Scientific Research and Development. Remember, the Research and Development Board has representation from the Army, Navy, and Air Force and is the top coordinating agency for the military establishment alone. The Interdepartmental Committee is government-wide. It has membership from the Commerce Department, State Department, Agriculture, Federal Security, Smithsonian Institution, Atomic Energy Commission, and several others.

The study from which the Interdepartmental Committee grew is that of the Steelman Board, which started in about 1945 to investigate scientific research in and outside the Federal Government. You should review this study when you have time. The important thing here is that it was recognized that science was growing apace in all the departments. You all know that science is flourishing in the Agriculture Department, we have all the antibiotics coming out of the medical activities in the Health Institute, and so on. So science is moving forward on all fronts, and we have exactly the same problem government-wide as we have within the military establishment, as we have just been discussing.

Fortunately or unfortunately, most of us who were on the original Interdepartmental Committee had some experience with RDB and its troubles. We had, so to speak, burned our fingers. So the Interdepartmental Committee did not start out with as bold a program as the RDB had. We did not try to set up a staff really to coordinate science in the Government. We agreed that the best progress would be made if we would focus our attention on a series of problems of common interest. We would avoid trying to referee interdepartmental fights such as those between the Army, Navy, and Air Force that RDB got into. We would confine our

attention to the questions involved in introducing some modicum of standardization, for example, in the procedures of grants and contracts from the Government to industry and universities.

We all knew--and we know it is still true--that the different departments and agencies had different rules and regulations, and that one university or one industry played one department against another. Some gave 8; some, 30; some, 50 percent overhead; and so on. That indicated that the Government simply did not know its own mind, and it would be in order for someone to study this problem, see what a fair and reasonable overhead should be, and make it government-wide. This is something which could be done and we are making good progress in this direction.

Similarly, we are all concerned with the manpower problem. Since our research demands are going up continually, each agency is becoming increasingly concerned with the problem of adequately staffing its laboratories. Money is not the whole story. You can buy facilities with money but you cannot always staff them. So a really thorough study and careful evaluation of manpower needs is very much in order, from the government point of view. We are looking into this problem.

Another one is the matter of presentation of budgets. Congress and the Budget Bureau are continually annoyed and impatient with the differences in the presentations by different parts of the Government, and a degree of standardization there would really be helpful to all of us. It would help the Budget Bureau understand what we are doing and why. It would help each of the agencies when they come up with their budgets if there were some preferred form that we all understood.

Let me indicate some of the questions that have been raised by the Budget Bureau with and in the Interdepartmental Committee. The representative of the Budget Bureau noted five principles that should be carefully followed in presenting research budgets. These are as follows:

1. A clear exposition of the program and the budget.

2. Indication of priorities in terms of needs.

3. Clear statements as to the basis for the cost estimates.

4. Presentation of convincing evidence of interagency coordination to avoid duplication.

5. The relationship of the research program to the mission of the department or agency.

The representative went on to explain that the Interdepartmental Committee could assist the Budget Bureau by the following:

1. Formulating general standards for evaluating research programs within the originating departments and subsequently by the Budget Bureau.

2. Appraising the programming and scheduling methods and techniques within the bureaus and departments doing research; making recommendations as to where improvement was needed and showing that methods had proved successful.

3. Critical evaluation of prevailing appropriation problems in different types of research programs.

I think this will indicate to you the nature of some of the problems with which the Interdepartmental Committee is struggling budgetwise.

So far, then, I have talked about the Research and Development Board, which is the organization within the military establishment; and the Interdepartmental Committee, which is the government-wide organization. Remember, however, that the members of the Interdepartmental Committee are, by and large, the representatives of the bureaucrats--the government laboratories, the regular government departments. They do not represent national science as a whole, by any means. The largest part of science is still uncovered in our governmental coordination mechanism. This is where the National Science Foundation comes in.

I continually get queries as to the status of the National Science Foundation. It is my understanding that the names of the people on the board have been selected. As many of you probably know, a little money to get this thing started was made available by Congress. The sum of $225,000 is available to start the National Science Foundation. This will at least enable its members to have a few meetings and start thinking.

They are needed for many reasons. So far as my own thinking is concerned, in these times and these days, the real reason we need the National Science Foundation is to get its guidance in the mobilization of science for the present international situation. Basically, the National Science Foundation was set up by law for what we hoped would be a peacetime activity. But the immediate need for the Foundation, so far as I am concerned, is to come to grips with things like the man-power problem, to speak authoritatively for the nongovernmental university people in the science field, and to help us set up an adequate mechanism for mobilizing science in this coming, or present, emergency.

I would like to dwell on that point a little. Too many people jump to the conclusion that, since the Office of Scientific Research and Development was effective in the last war, the natural, required thing to do is to set up immediately a full-blown OSRD of exactly the same type that we had last time. I think it is not too early to start thinking about mobilization, but the fact is that the technical and military picture now is enormously different from what it was in 1940.

I urge those of you who are interested in this problem to probe deeper into it. I think you will find that in 1940 practically no scientists in the country knew anything about military problems. They hardly knew which end of the gun to look into. In this day and age, there is hardly a scientist of stature and competence who has not been approached by and who is not tied into some part of the military establishment. So you already have your fingers on the scientists to a large extent. You have already tapped their brains. What is needed is to pick a few things to drive through with real conviction and real pressure, rather than to look around and try to think of new ideas that might be explored.

The National Science Foundation, then, has these two primary jobs, (1) to help us with the present mobilization, and (2) to put a protective hand over basic science.

In the early part of my talk I indicated that, so far as the military people are concerned, we need very much applied science. This does not mean that we cannot stand a little basic science, carried along as a leaven. But so far as the services are concerned, basic science must necessarily be small. For the National Science Foundation, it should be the other way around. Its main aim and end in life should be to make sure that our national science is in a healthy, vigorous state. It should support basic science where it grows best, in the universities. It should concern itself with training young men coming up to fill the gaps left by old fogies like myself who move out of the picture.

I would urge on you that you take a look, just for your own amusement, at the "Bulletin of Atomic Scientists" dated 1 November 1946. I took a look at that last night just for fun, and I found that there is a very good article in it on the original arguments leading to the setting up of the National Science Foundation--that is four years ago, note. There is an article in that paper predicting a great future for the Lilienthal Commission, there is an article on the United Nations' control of atomic energy, and so on. It is quite amusing to read these articles now and see what has changed and what has not changed. There is some homework for you if you have time.

Now let me summarize and list the problems common to all three of these agencies--the Research and Development Board, the Interdepartmental Committee, and the new National Science Foundation.

The first and fundamental problem is adequate evaluation of what is going on. The second is planning. This has to be done with real tolerance. We cannot have the kind of planning that we at least attribute to the Russians, whereby we tell each scientist what he is supposed to invent and on what schedule, but we do need some kind of planning to insure that we are moving in the right general direction.

This requires that we have flexibility in our program as well as stability. The research director must be able to shift gears promptly when one thing looks much more promising than another bet. At the same time we cannot have a research laboratory with an over-all budget that fluctuates up and down by a factor of two or three from year to year. This is not the way to get a good team working together.

We have to work out some solution to the problem of optimum proportions of basic and applied research. On this point, I have toyed with an idea that some of you might have some fun exploring. Roughly, one might put down the proportions as something like this: Ten percent of a total operation of any kind--military, industrial, or anything you like--might be justified for applied research if it is a vigorous, growing, active outfit; and 10 percent of the applied research might be justified as basic research to really keep the applied research vigorous.

Just for fun, I made for myself a nomograph, with three vertical lines. I see there is no blackboard here, but I think you can imagine what this is. The first vertical line is "total operations"; the second line, an inch or two away, is "applied research (dollars X 10)"; and the third vertical line is "basic research (dollars X 100)."

If you try this out, you will find some very interesting and significant differences between different kinds of activity. Take the military establishment as a whole. You will find that the line slopes markedly downward to the right. The total activity is high; the basic research is small, relatively. This is understandable because in a big military activity you have a large standing Army, Navy, Air Force, and so on, and you don't need a lot of research to maintain them. So I think we could expect and understand a downward trend in that case.

Let us take a look at those numbers for the military establishment before the Korean expansion. It is about 15 billion dollars for the total; something between a half billion and one billion dollars for applied research; and somewhere in the neighborhood of 50 million dollars for basic research, which would include the activities of the Office of Naval Research and quite a number of the smaller activities in the different departments.

That gives us one line for comparison. Now take an outfit like one industrial system. The total operation is somewhere in the neighborhood of between 1 and 2 billion dollars; the applied research is somewhere in the neighborhood of 100 million dollars; and the basic research within the laboratories, about 5 million dollars. That gives us another line on our chart.

You might take a look at the Atomic Energy Commission. Here we have a total operation of roughly a billion dollars. Much of it is applied research; nearly half, I imagine. At least several hundred million dollars would be applied research. Something like a hundred million dollars would probably be basic research. This line slopes the other way. Here we are heavy on the basic research end and light on the operations.

I do not have time to play with this idea, but it is a rather intriguing one. It might be the kind of thing that will give us some hold on what a sensible proportion should be. This is something you should consider in your seminars: What percentage of the total military budget should be protected, against the encroachments of operations, for applied research and development, and what percentage of that should be protected, against the pressure groups, for basic research in order to have the value of the leaven of basic research in your military organization?

Finally, I would like to suggest how you can tackle any big research budget. It does not matter whether it is for a department, for a laboratory, or whatever it is. I have learned that the best I can do--there may be better ways--is to look at the over-all activity and start slicing from both ends. At one end you peel off anything that you can say is an honest-to-goodness, legitimate project, something for a real end result, something useful, essentially along the lines of Wilson's definition that I gave you. At that end you peel off the useful projects, which is fairly easy to do. Starting at the other end, you can pick a few individuals by name, people who are really outstanding scientists, who have shown a record of real creativeness, and whom you can afford to subsidize in basic science. This you can do with no embarrassment. So at this end you peel off the really creative basic science. In the middle, and here is where your savings can be made, you will have an accumulation that I call "miscellaneous research." It is the same as happens in any file cabinet. In mine I always seem to have a big file labeled "miscellaneous."

Thank you.

QUESTION: Doctor, do you think that the universities and like groups that have a requirement for scientific personnel should have representation on a board set up to allocate scientific personnel?

DR. HAFSTAD: The university does not need scientific people for its own sake; it needs them for the Nation. The university is serving a training function.

I think it is a moot question, but if I were really running the show, I would leave the university people off because to do otherwise would

weaken the argument of this board. We all know in advance that the university president is going to "yowl" for his boys. I would rather have the military man, whoever he is, get up and say, "We've got to have a steady flow of technical people. We want to see to it that the universities have a steady flow." To have the steady flow of youngsters, the universities must have teachers--you see there is the need--suggesting what should be done.

One can always have representation there, but I doubt if it will be by vote. This is a psychological matter.

QUESTIONER: Doctor, there is a possibility that the military representative may not have that attitude. You may be running a rather severe risk.

DR. HAFSTAD: Surely. Anything important is risky, I have found. At the same time I think there certainly are people within the military who have just as broad gauge an approach to this as any civilian. I would say that these people must be very carefully selected. Unless they are broad-gauge people, it simply won't work. This applies to the civilians as well as the military.

QUESTION: Dr. Hafstad, in one of your previous talks here you mentioned the difficulty that universities and other nonprofit institutions have as to their budgets, the reason being that the tax structure is such that the endowments are falling off every year. The tendency on the part of the services is to go to universities and other institutions that obviously have know-how, and they have ended up going to MIT, Johns Hopkins, Cal Tech, and so forth. Is anything being done to try to spread out the research and development contracts of the services to these lesser-known universities?

DR. HAFSTAD: This is very difficult to do through the services' mechanism because the Congress and the Budget Bureau give the money to the services to defend the Nation, and the services are constrained properly, to go where they can get their services best, cheapest, and fastest. This makes them gravitate toward the big, safe, well-known organizations.

I think it is one of the real functions of the National Science Foundation to rectify this. Its money is supposed to go to the grass roots. That money should go to the small colleges. We should build up in all the small colleges around the country adequate strength so that it will be available in the future. I hope the National Science Foundation lives up to this responsibility.

QUESTION: Dr. Hafstad, you mentioned that the OSRD spent only about 75 million dollars during World War II and made a great deal of progress;

since then we have spent a great deal more money but have made less progress. You implied that, perhaps, this was due to the fact that we do not have as good leaders at the present time as we had during the war.

DR. HAFSTAD: Partly. It is a mixture of things.

QUESTION: Isn't it also possibly due to the fact that during the war our intense effort exploited all the basic research we had and that the effect of the law of diminishing returns now shows up, so that we have to put in a great deal more effort than we did during the war to get something out of it? There isn't much basic research left unexploited

DR. HAFSTAD: I think all these things contributed. Certainly, the going is much harder now. During the war we essentially moved in on a vacuum. We had a lot of scientific technique in the back of our heads, so to speak. The military had not exploited or used this, so we made very rapid progress for awhile. Since the war, things have dropped off. However, I cannot go all the way and say that the reason is that there has been no real basic research available.

Let me cite you an example again. You probably saw the article in "Life" on the "Ram," the rocket developed at Inyokern. I think it was a remarkable development. In 24 days after the boys' bull session, the rocket went from the laboratory to use in combat. I think it is fine and somebody ought to pat them on the back. But why wasn't it done three years ago?

You see, we have to haveincentive; otherwise, these things don't get done. And this is one of the things we lack in peacetime.

QUESTION: Doctor, you said quite a bit about coordination of research and development within the Government. Would you care to say something about coordination of research and development between the Government and industry?

DR. HAFSTAD: That is rough. What you raise is the fundamental question of how far the Government should control and guide industry. Since we have a free-enterprise system, and most of us believe in it, I think the Government should be very circumspect in upsetting and controlling the actions in industry. The things I have in mind there are control via the patent clauses, and things of that kind.

There is an argument going on in the Government as to whether or not, when a government agency makes a contract with some big industrial outfit, the patents that are developed should be wholly owned by the Government. This raises a very deep-seated question, because the reason we go to Westinghouse, GE, or some company like those, is to bring their

know-how to bear on our problem. If we take from them their know-how, incorporate it into some new patent, hang a government label on that, and then deny the industry the use of this patent--take it away from them, so to speak--industry will no longer kick in its know-how. This is the problem you run into when you try to coordinate these things.

I would say that the best procedure is to be just as tough as the Government knows how to be in getting its dollar's worth for every dollar it gives to industry for a given job. Industry doesn't need subsidy. If we subsidize it, I think we weaken rather than strengthen the fiber.

Now, we must subsidize universities somehow or other because we have taken away their income tax supports. But that is an entirely different thing from industry.

QUESTION: Dr. Hafstad, I am wondering if the National Science Foundation could not help the long-range scientific manpower problem by an extensive program of scientific scholarships. Has any thought been given to that?

DR. HAFSTAD: Very much. This is their ambition. But I think Congress has become very skeptical of the National Science Foundation, and unless we have really commanding names on it, they will have a hard time getting money enough to do all these things you gentlemen are suggesting.

I think this is what needs to be done. We have to support scholarships, and we have to support the small universities. But it is going to cost a lot of money; we cannot do it on the $225,000, I am sure. We are not going to do very much on the 15-million-dollar ceiling that was placed on the National Science Foundation's budget. This will take a long campaign of education.

QUESTION: To carry the question of coordination between the Government and industry a little further, I would like to ask whether you feel the holding of symposiums and membership in the various scientific societies by the Government, university, and industry scientists does not coordinate enough to take care of the unnecessary duplication that might take place. Don't you think that probably handles a lot of it?

DR. HAFSTAD: I agree that all of that sort of thing is helpful. When we have technical people exchanging ideas, by and large they will try to channel their efforts into the most profitable directions and will shy away from the unprofitable things. This is fine so long as there is no dollar incentive in the other direction. But if the military roar around the country saying, "Please, Mr. Industry, take a few hundred thousand dollars and study something," we are going to

need coordination because, otherwise, you are going to be wasting money. This is the other side of the difficulty.

QUESTION: Doctor, since we frequently see a large volume of technical papers in journals such as those of the American Physical Society, in your opinion what percentage of increase in fundamental, basic knowledge are we getting as a by-product of our applied research?

DR. HAFSTAD: A very large percentage. I think one of the most instructive exercises along that line is to take a look at the "Physical Review," which is our most high-brow physics journal, and see how it has increased in size and in number of publications since the war. We have a tremendous output of this kind, but that is not the whole story. That is only one of the indices, so to speak.

It might amuse you if I give you a figure that my colleague at AEC turned up. Ken Pitzer was wandering around in the laboratories, and just for fun he took the cost to the Government of a certain laboratory's work in basic science, divided it by the number of professional papers written, and he came out with a figure of something like a half million dollars per paper. I claim that is costing a lot of money.

Before the war we used to think we did very well if we got a few hundred dollars or a thousand dollars with which to carry out research. The game has changed enormously, our standards have changed, and I fear that we are in the middle of scientific inflation as well as dollar inflation.

QUESTION: Dr. Hafstad, you mentioned that one of the things you are looking forward to getting from the National Science Foundation is the mobilization of scientific manpower in the present emergency. In your opinion, is not the National Science Foundation, if properly set up, in the best position to be the operating agency for controlling any form of mobilization of our scientific manpower?

DR. HAFSTAD: I would answer that in two parts. We are already planning to place the scientific roster, so called, under the National Science Foundation as soon as it is ready for business. It is now planted temporarily in the Department of Education and will be moved over to the National Science Foundation so that it will have its fingers on all the technical talent. This is the first step. On this I would agree with you.

The question of allocation of these people between the military and the civilians--that is where the hot battle is going to be--is a notch above the level of the National Science Foundation, and, according to my thinking, the National Security Resources Board was set up specifically to handle such problems. I think the NSRB would be looked

upon as neutral, whereas the National Science Foundation would be looked upon as biased.

Except for that difference, I think the National Science Foundation would be an excellent place to locate this function, because it has the raw material.

QUESTION: In looking into this matter of the Department of Defense placing contracts with the universities, there seems to be a growing tendency on the part of the universities to set up separate establishments--apart from the university itself and not particularly connected with the university in the training of scientists. Would you care to discuss how you feel about this trend? Do you think it is dangerous or valuable in our present setup?

DR. HAFSTAD: I will give you my own opinion on this because it is a very controversial question right now. I think it is understandable and natural that as a university takes on many small contracts it needs some kind of mechanism to pull them together. The contracting agencies themselves would like to be able to go to some one person in the university and get the university's policy on overhead, travel allowances, and other things of that kind. So long as it is a service agency in that sense, I think it is a gain rather than a loss.

The danger starts creeping in when this administrative group begins to go into business for its own sake, begins to control the policies of the people doing the research, and gets in between the man who can do the research and the government agency that wants the research done. I think, however, this will be a self-healing difficulty, because the Council of Commercial Laboratories, among others, is very violent on this particular subject. As soon as these things become institutes which intend to have a sort of profit-making function of their own, they are encroaching on the private industry domains, and they are endangering themselves so far as the tax exemption situation is concerned vis-a-vis the Government.

I think that after some oscillations this will settle down to a not-too-dangerous situation.

QUESTION: Dr. Hafstad, in our reading of other lectures and discussions here, as well as your own, it has been brought out that many agencies and groups, if not all, are concerned with the mobilization of scientific personnel. You also brought out the fact that we have a sort of limited emergency at the present time. Is the NSRB actually taking any steps toward the formulation of a plan for the allocation or use of these people; that is, calling in all the different information of the various groups and agencies and actually accomplishing something on it?

DR. HAFSTAD: I think I can report fair progress on that. The NSRB, within the last couple of weeks, called a meeting, under the auspices of the National Research Council, at which there was representation from the American Chemical Society, the American Physical Society, the Council of Engineering Education, and several other similar organizations. I was present at that meeting. We spent all day at it and tackled exactly this problem of: Which is the best way of doing this? Should there be an allocation? Will the civilians accept the responsibility in meeting the needs of the Nation in this area?

The second draft of this is now being prepared. It will go back to the parent organizations for their scrutiny, then back up to the National Security Resources Board, and may emerge either as an Executive order or as legislation--I don't know which.

COLONEL DIEHL: Dr. Hafstad, on behalf of the Industrial College, I thank you for a most interesting and informative lecture and question period.

DR. HAFSTAD: Thank you.

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