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RESEARCH AND DEVELOPMENT IN ELECTRONICS

4 December 1947

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CONTENTS

	Page
SPEAKER--Dr. Julius A. Stratton, Director of the Research Laboratory of Electronics, Massachusetts Institute of Technology....................................	1
GENERAL DISCUSSION..	13

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CAPTAIN ROWLEY: Gentlemen, so far in the course most of our lectures on the subject of technological progress have been rather broad and have cut across the whole field pretty much on a functional basis.

This morning, however, we are going to pin-point our attention on research and development in the field of electronics. To deliver a talk on that subject we have been very fortunate in obtaining the services of Dr. Stratton, who has come down from Massachusetts Institute of Technology, where he is the Director of the Research Laboratory of Electronics. He also holds the post of Chairman of the Electronics Committee of the Research and Development Board.

I take great pleasure in presenting Dr. Julius A. Stratton.

DR. STRATTON: Captain Rowley and gentlemen; I have been asked to discuss with you this morning the relation of research and development, in the field of electronics, to the national security. Immediately I am confronted with the extremely difficult task of stating exactly what I understand to be the scope of electronics. Certainly I should be at a loss to give you any concise definition of the term. An airplane is a tangible object that can be described in terms of operating characteristics or costs of development and manufacture. Even though you may never have had occasion to deal with a guided missile directly, the word certainly conveys a concrete idea and you will find it relatively easy to grasp the essentials of our National Missile Program, though perhaps you may not always concur in its wisdom.

But electronics is not something that flies through the air or drops like a bomb. Rather, it is a vast multitude of intricate devices that enter into the production, test, and operation of nearly every major weapon of war, and which are playing an increasingly dominant role in our so-called mechanized era.

To a very great extent, the primary function of electronic devices is that of communicating intelligence. We can classify our field, perhaps, according to the nature of this intelligence and the manner in which it is obtained. Telegraphs and telephones deal with code, word, or voice signals that transmit command or information from point to point. This is communications in the restricted, conventional sense.

Or, again, the presence of a distant object may be disclosed by a reflected signal, an echo; this is radar. Radio and radar, aids to navigation, constitute another group of devices for transmitting intelligence to the navigator, some broadcasting it automatically and some responding to the challenge of a pilot.

Finally, all the devices which belong to the vastly important field of electronic control serve, in fact, to communicate intelligence of one sort or another. For example, a signal may report the fact that an indicator needle has deviated from a preset position. This signal, by actuating a servo mechanism, may restore a gun to its target or an airplane to its course.

The heart of all electronics is the electron tube itself. In recent years, these tubes have been developed in many forms and produced in stupendous quantities. They serve as generators of electromagnetic waves up to frequencies exceeding, say, sixty billion cycles per second; as amplifiers of infinitesimal currents by factors of many millions; as rectifiers of alternating currents, and for a host of other purposes. The development and design of such tubes preoccupies an increasing number of physicists and engineers. This is becoming one of the very important parts of the national industry.

To operate effectively for a given purpose, an electron or vacuum tube is associated with a circuit containing resistance, conductance, and capacitance. The design and production of these circuit elements comprise another vast division of the electrical industry.

Finally, the actual transfer of intelligence or information requires ttansmission lines that carry electromagnetic energy from one point to another, or antenna systems that facilitate its radiation into space. The design and development of these antennas, or radiating systems, constitute another very important part of the electronic industry.

I have endeavored to convey to you very quickly a first idea of what constitutes the field of electronics by indicating the functional problems with which one has to deal. Had I the time, I would elaborate by an historical account of the evolution of research and development in the field of electronics since the time of Edison.

I also wish that I had time to say a few words about the extraordinary accomplishments of electronics during the recent World War, particularly in connection with radar. But since we have to limit ourselves, I think it will be of more immediate interest to you and of more immediate benefit if I confine myself to a discussion of the current situation. It occurs to me that I can do this most easily by employing, as a means of classification, the pattern of the Research and Development Board itself. I am sure you are all familiar with the structure and the purpose of the original Joint Research and Development Board and the new form it is expected to take as an agency of the Secretary of Defense.

The board, as you know, does its work, in the first place, through an extraordinarily effective secretariat; that is really the thing that makes it work. And, second, through a group of technical committees. These technical committees, in the order of research and development funds for which they are responsible, are the Committee on Aeronautics, the Committee on Electronics, the Committee on Guided Missiles, and the Committee on Ordnance.

To give you some idea of the amounts of money I am going to talk about, let me say that the ceiling budget for the fiscal year 1949, for all military services--the Navy, the Army, and the Air Force--is about 477 million dollars. Although there is a supplemental budget, for purposes of comparison I shall refer to the ceiling.

Of this 477 million dollars, there was allocated to the Committee on Aeronautics approximately 126 million dollars; to the Committee on Electronics about 116 million dollars; the Committee on Guided Missiles is responsible for research and development to the extent of about 75 million; finally, the Committee on Ordnance, 67 million dollars, or a total of these four committees of 384 million out of a total of 477 million dollars.

The committees, in turn, in order to do their work, appoint technical panels and the panels proliferate subpanels and this process seems to continue, like an epidemic, until very shortly it appears that almost all civilian engineers, and most of the military, will be engaged in R&DB committee work. The situation in this regard in the Electronics Committee is particularly bad. There are now associated with the Committee on Electronics, for one reason or another, about three hundred people. This staggering multitude simply reflects the great complexity of the field of electronics, to which I referred at the outset.

Immediately after setting up the Committee on Electronics, we agreed among ourselves that we should reduce the number of panels to a bare minimum. The manpower problem was, and is, exceedingly serious. After all, people cannot spend their time going to meetings. So, the list of panels was reviewed with the utmost care. But, even so, as it now stands we operate ten technical panels. Let me name them for you--there is a panel on acoustics; a panel on basic research; a panel on communications; one on components; another on electronic countermeasures; a panel on electron tubes; one on infra-red; a panel on radar; a panel on radiating systems; and, finally, an exceedingly important panel on navigation.

Now the work of each of these ten panels, is, I think, from your point of view, not equally important in its relation to the industrial potential of the country or in its bearing on the problems of development, manufacture, and distribution. I should like, therefore, to comment in greater detail on the work of, say, three or four panels.

I mentioned first a panel on acoustics. It is questionable whether the Electronics Committee should be responsible for that field--if there is a question of jurisdiction. Acoustics involves, of course, the very important and vital field of submarine warfare; but it has also to do with all the so-called transducer equipment concerned with transforming electrical energy into acoustic energy, and the like; many acoustical problems related to telephone transmitters and receiver; and such problems as loudspeakers, which are transducers of a kind and related to the subject of electronics. And so, acoustics happen to fall in the Electronics Committee for no particularly good reason; but certainly we do have a very great interest in it. I shall pass quickly over that one.

Much as I should like to spend the remainder of the morning talking to you about the basic research panel, I also think I should pass rather quickly over the activities of that group. I am aware that earlier speakers in this series, particularly Admiral Lee, Dr. Bush, and Dr. Waterman, have impressed upon you the importance of pure research to our entire industrial effort.

I should, perhaps, pause to comment on the question of whether our present basic research effort is adequate. Frankly, that is a question I hardly know how to answer. I want to speak about this question of adequacy a little later.

My frank and purely personel feeling is that basic research in electronics at the present time is wholly adequate. I am sure that some of my scientific colleagues would be very unhappy to hear me say this. But I think there is a very effective amount of basic research being done throughout the country. It is also my feeling that although it is entirely adequate at the present time, inevitably that effort will be reduced in the coming years. That is the future we simply have to face.

At the present time, so far as we can determine, about sixteen percent of the total funds available for research and development in the field of electronics is being applied to projects that are termed "basic." Now you are familiar with this kind of statistics. You are aware of the great difficulty of saying, "This is basic and that is development," just as it becomes difficult or impossible to say, "This is development and this is design engineering." I am quite sure if we examined carefully to find out which are basic projects among the sixteen percent, we would find many of them might as well be called "development." There is really no sharp dividing line.

But that represents a fairly large effort expended by Service laboratories, and by industry and universities through contracts with the Services. I think this is going to be reduced, so far as the universities are concerned, for two reasons: One is that the effectiveness of the present program, at least in the universities, is thanks to sponsorship by the Navy, the Army and the Air Force.

I would like to testify at this point to the very enlightened policy, formulated perhaps under the leadership of the Office of Naval Research but backed by the Signal Corps and the Air Force, under which this sponsorship takes place at the present time. I cannot believe that we shall ever see from many points of view, a more satisfactory sponsorship of fundamental science than that which is now enjoyed by a number of our universities.

I feel that that almost inevitably will change. Probably there will be a science foundation; also I think that a science foundation must inevitably operate under a totally different policy.

In the second place, it is also inevitable that the budgets will be reduced and that under such circumstances research, however desirable, has to give way to the burden of maintaining operations.

In concluding my comments on basic research and its adequacy, I think I should say, with an avowed intention of being provocative, that I am not at all convinced that the support given to pure research by industry is sufficient or in any way adequate. Many industrial organizations carry on some amount of basic research; particularly have they done so in recent years. Companies like RCA, the Bell Laboratories, and GE do make splendid contributions, but I do not believe that the industry jointly does its share in the support of pure research throughout this country. If industry objects to military sponsorship, or federal sponsorship, perhaps the remedy lies very close to home. The fields of industry must be tilled and fertilized and cultivated as well as harvested.

Now to pass on to the panel on communications. It is on this one, perhaps, that I would like to dwell longest. It deals with the problem of transmitting command and information by wire and radio. Communications, in the restricted sense, is, by all odds, still the dominant interest of electronics. Historically, it was, of course, the first (going back to Morse's telegraph) and is now the greatest in terms of numbers and variety of components, volume of production, number of installations and operating personnel, both military and civil.

But in spite of all this--and this is, again, purely a personal view to provoke your interest--it lacks at the present time the imaginative effort in research and development that has been placed on other branches of electronics. Imaginative research and development--the breaking into entirely new concepts and new fields, particularly with regard to military communications--is wholly inadequate in terms of its importance.

It would be presumptuous for me, an amateur, to explain to you the importance of excellent communications in military operations. All history is full of illustrations of that fact. You may recall the celebrated pages of Tolstoy's account of the Battle of Smolensk, in "War and Peace," about 1812; how carefully the plans for each part of the operation were laid and how, once the enemy was engaged, the command lost all contact with what was going on and how the whole battle proceeded in quite a different fashion from what was ever contemplated.

And one cannot read again the accounts of the long campaigns across the river here some 80 years ago, between the Army of the Potomac and the Army of Northern Virginia, without being impressed time and time again with the fact that if a commander had received promptly information that was in the hands of his subordinates, or if his subordinates had known what action was to be carried out, the tide of battle would have been turned irrevocably. But too often the decision hinged upon a lucky guess, made without a true knowledge of the actual disposition of either force.

The recent war is too close to all of us for me to need to take many examples from it. I might just mention in passing, however, that if you were to examine the early days of the antisubmarine campaign, you would find its ineffectiveness was frequently due to lack of proper communications. The failures in this period were both technical and organizational.

Another example we might look at is the rather shaky efforts at air-ground support, particularly in the early part of the Italian campaign when tactical air was not used effectively because of the difficulties involved in transmitting back and forth the necessary information.

All of this is simply to say that in winning a war, communications is vitally important.

During the past war the effort we placed in the research and development of radar was tremendous. It was comparable only to that of the Manhattan District. I venture to suggest that if a sizable part of the effort that was placed on radar had been applied to communications, many lives would have been saved and much time spared.

Technically, the basic problem of communications is that of transmitting the maximum amount of information in the minimum time, using the smallest band of radio frequency. I am simply saying we have available only the radio frequency spectrum; in fact, we have available only certain parts of it because its use depends upon the distance of transmission and a number of other purely technical factors.

The radio frequency spectrum is becoming tremendously congested. The problem of obtaining channels is growing increasingly difficult. So our technical effort--and this, I should say, is the problem to which research in communications is now being applied--is that of determining limits to the transmission of intelligence. How can we send this maximum amount of information in a minimum of time, and with a degree of security; security not only that the enemy won't be able to decode it, but also that you will get it and get it in time to be of some importance.

I might suggest also in passing that in thinking about communications from, what I conceive to be, your point of view, it would be well to study our relations with and the dependence of military communications upon AT&T, the Bell System. We are extremely fortunate that the country possesses as one of its industrial resources such a splendid organization. It is proper, indeed, that we should draw upon it, probably more than we have in the past. But I should like to say also that I think perhaps there is a tendency sometimes to depend too much upon the techniques developed by Bell and its procedures. Let us use them to the very best advantage--and certainly no industrial organization has given more generously of its time and resources--but I believe there are problems in battle communications that are not normally encountered in the day-by-day operations of AT&T.

I think we should give a lot of attention to the problem of what is called "flash communications," or transmission prearrangement and by proper technique of a very large amount of information from one point to another securely and in a very short time. One example was the use by the Germans of "flash communications" to get information from their submarines.

In summary, I should like to place perhaps my maximum emphasis in this talk on the subject of communications because I believe that, although there has been a tremendous effort in terms of production, we really are somewhat in a rut; we have a long way to go and I recommend the problem to your attention.

Going on now to radar. This story of radar during the late war is such a fascinating one that I dare not dwell on it lest I be carried away by the magnificence of the whole achievement. To me, it represents an unparalleled example of technical development following closely upon the heels of rapidly evolving tactics and strategy.

Let me just pause to make that point clear. In talking about radar, one tends to discuss the various kinds of radar that were developed from a technical point of view. But the truth of it is that the evolution of radar during the war followed exceedingly close upon the changes, let us say, in the conduct of the war.

First of all, in 1939 the British constructed radar warning systems along the Channel Coast in view of the anticipated daylight attacks by German bombers. That was the beginning of air warning. We, of course, had corresponding projects in this country, perhaps even earlier. They, unfortunately, had to use them first.

After the Battle of Britain, daylight bombing failed and the Germans shifted over to night bombing. The British had foreseen that. They had developed their night fighters to a high point of efficiency. But in order to attack and shoot down German bombers at night it was necessary to have something more than ground-control radar; something more precise.

So the problem became one of developing airborne, search radar for night-fighter interception. As a matter of fact, the first microwave problem that came to this country was exactly that of airborne interception, put to the Radiation Laboratory in 1940.

No soon had progress been made in the mastery of night-fighter radar than the submarine warfare took on increasing importance, particularly for this country. So radar was developed for airborne purposes in the search for submarines.

You see, the time allowed for development of a totally new thing was exceedingly small. Then, with the submarine warfare beginning to be under control and the strategy changing, we began bombing at night and through the overcast. This really was simply an evolution of submarine-search radar. We found out that by adding a computer we could drop our bombs automatically on the target.

Finally, there were the invasions of Africa, Italy, and Normandy, and the period of the great land battles. Very quickly the importance of air-ground cooperation became evident. This was really the hardest problem, I think, that was ever put to radar; certainly it was not satisfactorily solved. But radar should become one of the most important adjuncts to the cooperation of a tactical air force and ground troops.

There was, then, this rapid evolution. I have omitted in passing some other developments, such as fire-control radar, navigation aids, and a few more. But all of this took place, you see, within a period of four, or at the maximum five, years. Laboratory Staffs were able to modify designs and to keep pace with the necessary development of equipment. But in order for this to be of any use, the engineering design had to follow and the equipment had to be manufactured. I haven't time to dwell on that point. But, there again, I would suggest that for some of your seminars you may, at one time or another, want to examine and review some of the production policies during the war in connection with radar.

One cannot help but have the feeling it might have been better, on certain occasions, to limit much of it to model-shop production. It seems after we had done the engineering development and design there elapsed always a period of eighteen months--no matter what you did there was always eighteen months--before the wheels of industry began to roll the stuff out in vast quantity. By that time, the character of the war had changed and you did not need fighter-intercepting radar, but something else. So this problem of gearing production in the proper quantities to rapidly evolving strategy is a very important one, indeed.

Our enthusiasm for the possibilities of radar was boundless, and the effort, of course, was stupendous. In 1944 alone it is estimated that about one billion dollars' worth of military radar was produced. I have heard that during the entire war the production amounted to something between two billion and four billion dollars.

In retrospect, it appears to many of us that some of this huge effort might better have been expended in giving life to other problems. For example, the Radiation Laboratory started out on Armistice Day 1940, with somewhere between eleven and fifteen people. When the laboratory was terminated 31 December 1945, there were just under four thousand on the staff, including technicians, senior staff members, and all the rest. A very large fraction of all the best minds in physics and electrical engineering of this country was concentrated there.

As I say, I think perhaps we would have been better off if some of that effort had been applied to communications and possibly to other weapons of war. There is a feeling among a number of people--and this is one that I share--that probably radar has been over-researched. The answer to this question applies directly to problems that you will meet, either in connection with your studies or in your own professional work. It bears on the question of whether we should allow present momentum to carry us forward with the further development of radar, or whether we would not do better to consolidate first the technical gains that were made during the war and capitalize on them through, essentially, the modification of present equipment rather than the development of entirely new radar.

Particularly there is the question of a radar warning network for national defense, a problem that I am sure many of you are, or will be, concerned with. You know that there are a number of such plans under discussion. Almost all, or any that I have ever seen, involve fantastic sums of money. It has been proposed that we erect a chain of early warning stations along the Arctic Ocean, or throughout northern Canada, or surrounding certain first-priority areas.

The question of what we do with such early warning if we had it is, of course, an extremely difficult one to answer. Radars have been suggested and preliminary designs made for warning of oncoming airplanes or perhaps even a large guided missile, at a distance of 800 to a thousand miles.

A recent report indicated that the only way of achieving the necessary security would be to stop all procurement of present equipment; to agree that present radar equipment was obsolete and to concentrate on the development of such very long-range radars. I think that such a conclusion would be exceedingly dangerous. If we take the radar that was developed at the end of the war in essentially its existing form and then apply many of the developments that were in "bread-board" form, in the laboratory at that time, I think we shall have equipment which is far more advanced than all the other weapons and aids we will use in conjunction with it.

It isn't that it would not be fine to have this long-range radar; it is simply the fact that we are not in a position to back it up with a complete defense system. It seems questionable whether we should expend that amount of money and manpower, placing a tremendous load on our national economy, if the result is only going to be a sort of Maginot Line. It is very well to talk about long-range radar on the Arctic Ocean or on the Canadian Border, but the real question is, when they have picked up the guided missile approaching at, say, three thousand miles an hour, what are you going to do with the information?

That means you have to consider the communication net and the whole problem of handling intelligence and dispatching fighter groups or homing missiles that really home, because the final desired result is to prevent that missile or that airplane from exploding over your own territory. This whole question on what to do about a national warning system is one of the most difficult and one of the most complex with which we have to deal.

I shall pass rapidly over the problems of countermeasures, but feel I should point out certain questions of principle. It seems to me that countermeasures fall very much into the category of life and fire insurance. You would be in a very bad situation, probably, if you carried no insurance. But, on the other hand, it is dead certain if you buy all the insurance that the salesman wants to sell you--for very good reasons, apparently--you will go bankrupt. The same thing applies, to some extent, to countermeasures. If you try to construct and have ready a countermeasure for every possible attack, or every possible electronic weapon, you will go bankrupt, at least industrially.

What we need very much, it seems to me, is a sound policy on countermeasures; a policy for determining to what extent we will continue in time of peace a countermeasure program--certainly we must do something to keep alive the countermeasure techniques learned during the war; and a clear plan on how and to what extent we shall suddenly expand this countermeasure program on the outbreak of war.

I am saying a number of things in this talk this morning that express only my own personal opinion, frankly to be provocative and cause you to challenge certain accepted ideas. Thus, I am inclined to say, although I am by no means a countermeasure expert, that we over-did some of our countermeasures during the war; we did not get an adequate return--I am speaking now of radar countermeasures--and should not have expected an adequate return for our effort. Whether we did too much or too little is a problem to be investigated. It is one, I think, that requires very great study because once we are committed to a plan of developing countermeasures and have on the shelf countermeasure equipment for all the things that the enemy is known to possess, or might develop, we are assuming an enormous industrial burden.

I see, with some dismay, that my time is getting rather short. But I would like to comment on another panel which is intimately related to the problem of communications and radar. It is one of the most important. I refer to the problem of navigation.

At the very outset of the formation of the Research and Development Board, consideration was given to the proper handling of the problem of air navigation; and in a broader sense navigation on both land and sea. It was rather difficult to say just how this should be approached. Some thought possibly a separate committee should be set up for the purpose of coordinating all research on aids to navigation and for that of proposing plans for a national system. Others suggested another panel to the Committee on Electronics.

There was finally agreement that this problem should be placed under the jurisdiction of the Committee on Electronics. The Committee accepted it with very great reluctance; first, because it is an old problem involving many difficulties that are political rather than technical; and, second, because it was felt that although electronics entered into the problem of air navigation very intimately, nevertheless there are involved many other vital aspects such as the airplane itself, landing fields, lights, and other aids of a nonelectronic character.

But the basic difficulty is that the Military Services cannot handle it as an independent problem. The Military Forces can take such a problem as fire control or guided missiles, and agree among themselves how it should be treated. They can also implement that agreement. No one else is concerned. No other civil agency is concerned with the manner in which your guided missile or ordnance program is organized.

But that is definitely not the case in connection with air navigation. Certainly the Research and Development Board, which is responsible for the national security, is obligated to deal with this problem of air navigation; but it cannot set up requirements or establish a research and development program without taking account of the interest of civil aviation.

Just how to do justice to this joint interest has been a very difficult matter, indeed. A first step has been taken by the appointment of an ad hoc committee, under the chairmanship of Mr. Ralph Damon, not for the purpose of establishing directly a navigation system, but for that of finding out the nature of the problem; how it relates to the national security; how it is related to civil aeronautics; also, to examine the present research and development program of the Military Services and to suggest how all these interests can be reconciled.

One point is fairly clear. On the one hand it is improbable that the Nation can support in time of peace an adequate air-warning system that serves no other function. It is also somewhat difficult to say whether the country, whether Congress, would subsidize an air-navigation system for the sole purpose of, let us say, civil aeronautics, or solely as a measure of security for use in time of war. But if it were possible to join these two together and to develop a complete system which could be converted in time of emergency to a warning and control system and employed in time of peace as a national air-navigation system, the cost of the investment might well be justfied.

I must needs skip over the activities of a number of the other panels. But I want to conclude by saying that because of the vast extent of electronic interest and the inherently diffuse nature of the subject, I have endeavored to sketch for you the broad scene in rather quick strokes. Many matters I have touched upon only lightly; many questions I have not answered at all. However, I trust I have conveyed to you a clearer view of the magnitude of the current effort in the field of electronics and the extent to which it is woven into our entire fabric of national defense.

I wish I might say to you that the Committee on Electronics of the Research and Development Board is convinced that the present research and development effort in this field is adequate. Frankly, the committee does not know that. I, for one, do not see how we, or any other technical body, can possibly answer such a question. The question of adequacy hinges on a delicate balance between the national economy and the threat of foreign aggression. Except in time of war, funds allocated to research and development must always fall short of what the planners ask for, unless the country is intellectually bankrupt. Engineers can always think of more things they would like to do than we can pay for. That is why we have ceiling budgets and supplemental budgets.

But if it isn't entirely within our province to determine the absolute expenditure, we can at least endeavor to apply the funds available for research and development in the wisest possible manner. This, I believe, the board is learning to do, and in so doing, I think it will perform an unprecedented service to the people of this Nation.

Thank you very much.

CAPTAIN ROWLEY: Are you ready for a few questions, Dr. Stratton?

DR. STRATTON: I'll do the best I can to answer them.

QUESTION: I have two questions: First, how can industry be induced to take on a greater research load than it does at present in view of the probability that the military budgets will need to be decreased in the future? And, second, how can the educational field induce industry to take on a greater load?

DR. STRATTON: I saw several questions that might be asked, just before this hour. That wasn't among them. (Laughter)

That is about one of the hardest you could possibly put to me. Frankly, I do not know. It seems to me I made some passing remark about the support of research by industry. One cannot expect industry to bear the entire burden, or even a large fraction of the burden of subsidizing pure research in the universities. They are doing a considerable amount in their own laboratories. I do not think, frankly, that the amount of basic research that is done by industry, even by Bell, RCA and GE, is quite so large as is sometimes believed. Inevitably it must be directed, to a considerable degree, to their end-product.

In the first place, if a particular company were asked by a university for support the answer might well be, "Well, we have available just so much money in this company for research. We have decided, as a matter of policy, that we shall operate a research laboratory. It only makes sense that that money be placed there rather than in some university."

That is perfectly true. Yet, the industry as a whole depends on research in the field of electronics for its future markets.

It seems to me that the organizations comprising the industry as a whole must meet this cost jointly--I should say on a cartel basis were it not for the Trust laws. Together they should assume the continuance of a healthy amount of pure research in their own best interests. Almost every manufacturing company in the field of electronics at the present time depends heavily upon military orders. They are in a better position now than ever to support research in view of lean times. What is going to happen, of course, is that as the Government budgets are cut back, our research effort is going to suffer accordingly.

I have not answered your question fully, I know, but that is about as close as I can come to it.

QUESTION: You indicated, I believe, that the transfer of sponsorship of the National Science Foundation would adversely affect the program. Would you expand on that?

DR. STRATTON: Did I say "adversely affect"? I said the administration and policies inevitably would be different.

(After a pause) This is one I naturally want to think over for just a moment.

My general feeling is this: The objectives of a national science foundation in the support of research must, I think, be different from those, let us say, of the Office of Naval Research. The Military Services were impressed with the important bearing of the research effort during the war upon the actual fulfillment of military requirements. As an interim measure, they stepped in to assure the continuance of that effort.

We well recognize that the Military Services should not be the ultimate sponsors of peacetime pure research. That is more properly the function of a science foundation.

It is, for example, in the opinion of O.N.R., exceedingly important to the Navy to have certain kinds of research done. O.N.R. at present is in a position to go out to try to buy that research, so to speak, in those places where it thinks it will get the job best done.

I do not think that immediate effectiveness of its projects is the primary object of the National Science Foundation. I believe inevitably it should and must spread its funds. It must encourage to a greater extent the development of research in many centers throughout the country.

Do I make my point clear? The Services are in a better position to administer their money more efficiently, in terms of immediate results. This may not be the most efficient manner from the national point of view. I think that that is the standpoint that the Science Foundation would take: that in the long term, it might be better to concentrate less money in the larger universities and industrial organizations and put more about the country.

CAPTAIN ROWLEY: Dr. Stratton, the officer asking the next question is chairman of the committee working on this subject.

QUESTION: To what extent do you consider that the availability of sufficiently qualified technical men in the field will act as a real limiting factor on the introduction of new and ever more increasingly complicated electronic equipment?

DR. STRATTON: A great deal has been made of the point that we are short of technical personnel. Much has been said--perhaps it was in your mind--to the effect that there won't be enough people to operate all these devices produced by industry.

I think that situation is being rectified very rapidly. The universities at the present time and the engineering schools are able to pour out men trained in the field of electronics in enormous numbers. I think it is a better bet that the market will be glutted three years from now than that there will be a scarcity, unless the pace of new development proceeds faster than at the present time. This is a point I would be very glad to examine further. I might be quite wrong. I don't feel the alarm that was current, I think, a year ago.

Let me take a case in point. There are at the present time in the Electrical Engineering Department at MIT just a thousand students; that is, freshmen to graduate students. That is a lot of people with electrical engineering training. I think the same thing probably applies all the way down, through technicians, and so on. So, I am not at all convinced that this is one of our most serious problems at the present time.

QUESTIONER: I was thinking more in terms of men on the lower level for maintaining the equipment on the ship and in the airplane.

DR. STRATTON: That is a little different point. The answer to that would depend a great deal upon our engineering design as well as upon the number of men available.

During the recent war production equipment was so close to the breadboard stage in the laboratory, that there was practically nobody who understood how the thing worked except the man who made it. The training problem was simply staggering. It seems to me the best way to meet this difficulty would be by miniaturization of components; the use of such things as printed circuits, and the like. If we continue the old manner of design, it will be out of the question to try to supply technical help necessary for maintenance. But it is perfectly possible, it seems to me, to make all your engineering design in such a way that almost anybody can be trained in a very short time to apply some simple test and to make replacements rather than repairs.

CAPTAIN ROWLEY: I think his question could be broadened into something along the lines of what you said: If either side is going to use electronics in a war to a much greater extent, multiplied maybe ten or twentyfold, how can we fight such a war? There won't be anybody to do anything but push the simplest buttons that are marked with A and B.

QUESTION: Doctor, would you mind elaborating on some of the problems that resulted in a delay in getting your equipment into production? In other words, you spoke of the eighteen months' time.

DR. STRATTON: I think that the principal reason was the rather restricted amount of model-shop facilities; also the rather American concept we must do everything in enormous quantities.

American industry is tooled, and our thinking oriented to production-line methods. Therefore, when you build a new car, you lay off everybody for several weeks or months and go through a process of tooling-up. When things finally start, they do pour off in tremendous numbers. But that tooling-up process is a very long one, particularly when you haven't any last year's model to follow.

Presumably, the prime contract went to Western Electric, GE, or an organization of which the whole philosophy is based on that of large-quantity production. Engineering drawings were made first in the laboratory. They must then be redone to meet the manufacturers standards. All this had then to be subcontracted out over the country to people who had never seen anything of this kind before. All those steps resulted in a tremendous consumption of time. I think it is remarkable, actually, that the period was only eighteen months.

But the idea I wish to suggest is that greater use of model shops, in which you would get out thirty or fifty pieces, or maybe 200, in a period of three or four months might have done a great deal more toward winning the war than to have gotten out 10,000 eighteen months afterward. Now I have no criticism of the eighteen months; in fact, I think it is a remarkable achievement, provided your objective is 10,000. I do question whether we were wise always in setting our scale.

Another factor was the frequent change in program. In radar, for example, we started out with 10-centimeter equipment. Then it became apparent that it was possible to accomplish a better result with 3-centimeter equipment. You could get greater resolving power using smaller antennas, which was very important. Without question this was a great advance in the art.

But then, the enthusiasm of physicists which followed the laboratory conquest of 1-centimeter techniques was such that a proposal was made to convert much of the radar program to this new wave length.

The problems of production arising from changing of plans were tremendous. A number of our people were reluctant to take into account the magnitude of the task. In other words, technical progress went ahead so fast that equipment appeared obsolete before manufacturing was complete. This certainly complicated the production problem.

QUESTION: Your statements tie in very closely to the question I had in mind, that in production you are interested in converting development to usable equipment in quantities.

But aren't we running the same danger in organizing our research, probably, on the same basis? You look back in history. Our greatest advances, actually, our greatest fundamental discoveries, have not been found in the universities or under organized reasearch, but in small places. Take, for example, men like Faraday, Boyle, and the rest, and their discoveries. Aren't we running into danger of regimenting our science too much in that sense?

DR. STRATTON: Yes, sir; I think we are.

But it is a little more complicated than that implies. There is a very real danger and in that I think concerns a great many people. The difficulty is that contemporary science is a very much more complex thing than it was in the days of Boyle, Faraday, and the rest. It is a very much more complex thing than it was, let us say, even in 1935. Even in the universities in that period (1930 to 1935), you could take a graduate student and you could give him a little bit of equipment, put him off somewhere, and he might do a very good job. In the same way, an instructor on your staff could be given minimum amount of technical help through machine-shops and technicians and be expected to conduct the research himself. If he needed some vacuum tubes, he possibly had to build them. He had to design his own circuits. He had in mind a certain object in carrying out the experiments and to this end he developed his own equipment.

Well, it is just about impossible to do that at the present time because of the complexity of electronic devices. Look at the back of a television set, or just an ordinary eleven-tube receiver, and you will find a mass of wire, coils, and condensers.

Now the time that this research man would have to put in, even if he was familiar with the whole business, to construct such a thing would absorb a very large fraction of his whole creative effort. That time is simply not available in the universities and it is not profitably available in any other laboratory. Yet, in order to carry out some of these crucial experiments, which perhaps are of the same character as those of Faraday, he needs just such complicated instruments.

This discussion could easily develop into a thesis on the function of electronics. It is not only important in time of war, but it has become an indispensable aid to almost all other physical research. The research worker, the Faraday of today, therefore, needs much greater facilities, in terms of equipment, technicians and machine shops, than he ever did in the past.

So our man has to become, to some extent, organized. He may become a member of a team. One of the things you will note in looking at the current physical periodicals is that papers will appear with, say, five or ten names on them instead of just one, as heretofore. This is disturbing. Many physicists feel, just as you implied, that this highly organized type of research may sap something that is individual or creative from the artist himself.

In conclusion, let me say that many people share your concern. But it is not easy to say how the problem is to be avoided.

QUESTION: My question is on the time factor involved in the testing, evaluation, and acceptance of prototypes.

Take the Navy for example: When the responsible bureau has the pilot model completed, it sends it to the operational development factory which puts it through the test. Then they make an evaluation report. That goes back to the bureau and it is either accepted, further developed, or suspended. Then it goes up to CNO where it is evaluated. The CNO gives the decision whether to go ahead or not.

Is our system the best we can use, or is there a better system, or can we reduce the time factor any way?

DR. STRATTON: Captain, I think you are in a much better position to answer that than I am.

It does seem to me that that ties in very closely with this question of mass production. That system is essential when you are dealing with a large number of items and cannot afford to risk an improperly developed product.

What was so exasperating during the war was to see that same succession of processes applied to devices which were to be used in very small quantities, and immediately. It was a pity to see a piece of equipment ready for battle going through a lot of tests at the various proving grounds throughout the country for a period of another eighteen months.

QUESTION: Doctor, in what fields do you consider research and development facilities inadequate?

DR. STRATTON: In electronics, you mean?

QUESTIONER: I am speaking of clear across the board.

DR. STRATTON: I don't believe I could answer that; sorry.

QUESTION: You commented on the acoustical panel. Would you care to comment on the infrared panel, their work?

DR. STRATTON: Yes; surely.

A little bit like the acoustical panel, the infrared panel needed a home. The Electronics Committee was set up before the Committee on Physical Sciences, within which it would more naturally fall.

Infrared was not developed very greatly in this country during the war--or at least not to the same extent it was in Germany. Nevertheless, it has been a field of interest for many years, even during World War I. There was the possibility of detecting an airplane by infrared emissions from the exhaust.

Then, of course, the use of infrared detectors for various purposes was current during our own war. In addition to these applications to detection, very probably infrared will be of great use for interfleet communications problems, where you wish to communicate with complete assurance you will not pass beyond a certain limit. I think that is one of the principal applications. Present infrared detectors are not altogether satisfactory, but progress is being made.

QUESTION: Dr. Stratton, you have indicated that you think research in electronics is probably adequate. What about development and design? Do you think our effort toward really effecting good design is adequate? In other words, are we making the best effort to keep from complicating our equipment needlessly?

DR. STRATTON: You realize how hard such questions are to answer.

QUESTIONER: You spoke of communications equipment particularly. That is also my field. You are the first person I have ever heard speak with whom I agreed 100 per cent. But our design of equipment during the war was not up to the standard that the article would have permitted. We needlessly complicated many devices and our repair and maintenance problem ran up terrifically.

Unless we impose much greater restrictions on design, if an engineer will design what he wants and lets his creative urge run away with him, we will have a heterogeneous mess nobody can make function efficiently.

DR. STRATTON: You make it easy for me to agree with you. Frankly, I do.

Coming back to my statement about the 3 centimeter radar. We would have been better off perhaps to stick with that and see how simply we could make it. We would have a lot more sets working instead of a vast number sitting on the side with no one knowing how to repair them.

Although, if you ask me whether the effort is adequate or not, that I do not know how to answer. But I can say without any hesitation that I believe a tremendous amount along those lines can be done. It is in the line of block replacement and simplification of maintenance and installations, and all that sort of thing. That is why I said, in connection with our radar program, I am somewhat concerned to see with what enthusiasm people talk about new kinds of radar when I think there are in the laboratories--at least there were at the end of the war--so many techniques that could be used and applied to existing radar to make them much more effective and usable pieces of equipment.

So I think your point is entirely well taken.

QUESTION: Many years ago I was impressed by the fact that statements were quite current--and very much so in the field of radio--that developments were going on so fast even the expert could not keep up with them.

Assuming there will be tremendous advances, could you comment for a moment on the problem facing the scientist in the field of getting information on what has already been done before so that when war comes again we can avoid duplicating some very expensive experiments and go on from what is then known?

DR. STRATTON: Yes I can do that. I will try to make it short. I do not think enough is being done along those lines. One of the suggestions that has been made very recently is that this country needs a sort of handbook, such as was put out in Germany prior to the war, whereby someone or several people made a review--not a bibliography, but a review--of the technical accomplishments during the past year, with sufficient references so one could pursue the subject further.

Now there are two implications: I think that the circuits of radio sets are indeed becoming complicated that only an expert technician can detect a failure and repair it. On the other hand, the development of principles does not proceed that fast. These are the things we should be kept up to date on. Then, having agreed to that, we must have at our disposal two things: One of them is a proper digest of the literature, which I do not think exists adequately in this country; and, second, the point I made earlier: you need technicians, whose job it is to know how to put those circuits together when you explain what it is that you desire.

CAPTAIN ROWLEY: Our time is about up, Dr. Stratton. I certainly want to thank you, on behalf of the Commandant and the student body, for bringing together and synthesizing all these studies into a very informative discussion.

(7 January 1948--450)S.