RESEARCH AND DEVELOPMENT IN THE ARMED FORCES

19 December 1958

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Brigadier General Austin W. Betts, USA, Military Executive Assistant to the Director of Guided Missiles of the Office of the Secretary of Defense was born in Baldwin, New York on 22 November 1912. He was graduated from the U. S. Military Academy at West Point in 1934. He was transferred to the Corps of Engineers in 1935, received his M. S. degree from MIT in 1938. His career in research and development began in 1945 when he became Associate Director of the Los Alamos Scientific Laboratory at Los Alamos, New Mexico. He has served in similar positions ever since. He commenced his present assignment in early 1956. He is a graduate of the Industrial College of the Armed Forces, Class of 1955. This is General Betts' second appearance at the Industrial College.

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GENERAL ZITZMAN: I think that everyone nowadays seems to agree on the importance of research and development in the defense effort. Everyone does not always agree, however, on just who should do what. During the last year or so, several organizations have been set up to conduct research in the sciences related to space travel and national security. These organizations do not always appear to have clear-cut responsibilities, and probably some duplication and overlap are inevitable. With sincere, capable, and conscientious people directing them, however, the differences can usually be worked out very reasonably.

It may be an oversimplification, but I think the situation is very similar to the one in which this very gentle farmer bought the most recalcitrant and stubborn mule in the area. His friends warned him about this mule and said he would never be able to do anything with it. But he said, "I am simply going to get his attention and speak very gently to him." He started to lead the mule away, and the mule did not budge. The friend said, "Now I want to see this gentle speaking." There was a 2 by 4 right handy. The farmer picked it up and tossed it to one side so that he could get a little closer to the mule. He said, "Now, look, you know that I am a Quaker and I'm dedicated to peace and nonviolence. You also know that, as a Quaker, I'll not even speak roughly or curse you, and, certainly, as a Quaker, you know that I am not going to strike you. What you may be overlooking is that I can sell you to a Methodist who will beat the hell out of you."

Last Friday, when Mr. Roy Johnson of ARPA was here, he listed some of that organization's projects and described what, in his opinion, was the ARPA role in the Defense Department.

Our speaker today, Brigadier General Austin W. Betts, will discuss the mission and the responsibility of the military services in the Nation's complex system of scientific research. And, just as an aside, and to give you a little more information about our speaker, when he was a cadet at West Point he was captain of the gym team, starred on the high bar, the side horse, and the parallel bars, and set what was the highest alltime individual score, the all-around individual score, up to that time. He now refers to those days as the sedentary and tranquil period of his life.

Today he is going to highlight the problem areas arising from the development and introduction of missiles into our military arsenal. He is well qualified for the task, of course. He has been associated with science in the military ever since 1945, when he was appointed Associate Director of the Los Alamos Scientific Laboratory. He has filled such positions as Chief of the Atomic Energy Branch of the Research and Development division of the Department of the Army, and executive to the Chief of Research and Development for the Army.

With that background, he was admitted to the Industrial College as a member of the Class of 1955. He went overseas immediately upon graduating, but was recalled early, specifically to take his present position.

It is a real honor and a pleasure to welcome back for a second appearance at ICAF the Military Executive to the Director of Guided Missiles, Office of the Secretary of Defense, my old friend, Brigadier General Austin W. Betts, U. S. Army. General Betts.

GENERAL BETTS: The basic problem is that I know Ken too well. General Mundy, Gentlemen: Before I begin my presentation I would like to say specifically that I have cleared this talk with no one, I have discussed with no one the question of what I am going to say. These are my opinions with respect to research and development. If you don't like them, you can't go back to Mr. Holladay or anyone else. You have to throw your brickbats at me personally.

I take it for granted that most of you have read the words that appeared in the book about the scope of this lecture, but, for those of you who have not, I would like to summarize briefly what I consider to be the task this morning. Incidentally, this summary will also serve to carry out the instructions I received as a student at this illustrious institution. The speech program was: "Always start in by telling them what it is you are going to tell them." So I have placed a chart over there which tells you what it is I am going to tell you.

As I see it, the job is to cover four areas: first, the military's responsibility in the Nation's system of scientific research; next, the military need for basic and applied research; then, the climate for research within the military, or, as the faculty would put it in the scope, "the extent to which the military can provide a sympathetic and inspired guidance for scientific undertakings;" and, finally, the effectiveness of the military organization for the conduct of research and

development. I have added one to this, sticking my neck out, and taking advantage of my opportunity this morning to say that I will try to hit on the solution of what I consider to be some of our toughest problems.

Before we go any further, I think we have to agree on definitions of words--what constitutes basic research, or applied research, or development, for example. Words don't always mean the same thing to everybody.

So that we will have our words agreed, now, I'd like to take the definitions which were recently put in a document in connection with cost principles, cost allowances on contracts with the military departments.

The first of these is basic research. Basic research is that type of research directed toward increase of knowledge of science where the primary aim of the investigator is a fuller knowledge or understanding of the subject under study, rather than any practical application thereof.

Applied research is that type of effort which normally follows basic research, but which may not be severable from the related basic research. Applied research represents efforts to determine and expand the potentialities of new scientific discoveries or improvements in technologies, materials, processes, methods, devices, and techniques. It may also be defined as an effort to advance the "state of the art."

Development is defined as the systematic use of scientific knowledge directed toward the production of, or improvement in, useful products to meet specific requirements, but exclusive of manufacturing and production engineering.

Here are three separate but very closely interrelated parts of the general business of research and development. One must recognize these for what they are and keep them in proper perspective one to the other in order to understand what makes research and development tick. It is not enough to understand their interrelationship, in particular, the question of broad areas of overlap. One must also understand and accept the fact that these three areas are fundamentally different. I would say categorically that much of the criticism of military research and development can be traced to the fact that the critic in question did not understand these differences.

So, with that brief introduction, let's tackle the four subdivisions of this discussion, starting with item one, the responsibility of the military in the Nation's system of scientific research.

Now, I don't like statistics any more than you do, but they can be useful if properly understood. With respect to military responsibility for research, the single statistic that impresses me most was used by Mr. Quarles in his statement before the Congress this past spring in the budget hearings. He said that the research and development program for FY 1959 is expected to reach about $6.2 billion. This is a 15 percent slice of the FY 1959 total Department of Defense budget. This certainly makes it an important item of business for all of us.

That $6.2 billion is made up of many things besides the direct research and development appropriation. It also includes over $3 billion of procurement money in support of development, evaluation and testing, as well as about a half-billion dollars for military personnel, construction, and industrial facilities. The research and development appropriation part of this is just about $2.5 billion. However, in thinking in terms of the Nation's scientific research effort, perhaps we should forget the additional dollars and just look at the research and development appropriation.

About this time last year, from this same platform, Dr. Furnas, a former Assistant Secretary of Defense for Research and Development, addressed himself to this same problem. At that time he estimated that about one-third of the total research and development dollars in the United States were being spent on military research and development, and that about 40 percent of all the engineering and scientific manpower in the country, working on research and development, were doing it under military auspices. He also stated at that time that he could not go back into precise statistics to support this, and, frankly, I have seen no more recent statistics that would change it one way or the other. As far as I am concerned, it is as good today as it was then. In his discussion, Dr. Furnas was referring only to the program supported by the research and development appropriation, which at that time was spending at the rate of about $1.6 billion a year. As he indicated then, if you add to that $1.6 billion the $3 billion plus of other appropriations going into military research and development, it is not hard to see that we in the military have a very considerable responsibility to the research community. If a realistic disarmament agreement or some other international event suddenly greatly reduced the military threat, and thence the military budget, the scientific and engineering community

across the country would be very seriously affected. The economic implications are obvious.

If we stop to think about this a little bit, it becomes apparent that a real understanding of research and development is an absolute must for the military manager. One way or another, every segment of military activity affects or is affected by the research and development program. With so much at stake, we must do the best possible job of research and development, and, frankly, this has, unfortunately, not always been the case. But more of that a little bit later.

I would like to leave the matter of responsibility right at this point. Certainly there is no question that we have a considerable responsibility for the continued support and direction of a major portion of the research and development effort in this country. And I don't think that we can abdicate this responsibility, although there are those who have argued very strongly to the contrary.

One of the most effective individuals who has argued along these lines is Dr. L. V. Berkner, president of Associated Universities, the organization which runs the Brookhaven National Laboratory for the Atomic Energy Commission. Dr. Berkner has consistently argued that we need another OSRD--Office of Scientific Research and Development. You will recall that this was the free-wheeling scientific group that carried the ball on new weapon research and development during World War II. Dr. Berkner apparently feels that the military are competent to manage weapon improvement and perfection, but he argues that research and development to produce radically new weapons is beyond our competence.

I say "apparently" because I have to admit that my references to Dr. Berkner's opinions on this subject date back to his appearance before the Riehlman committee, the House Government Operations Committee, which studied the research and development problem at that time. Knowing how strongly he felt then, I have no reason to believe that he would have changed his mind in the meantime.

I think this logically brings us to the second item on our schedule this morning--the need for research and development. As we explore this and the next two topics, I think we should certainly be able to answer the question raised by Dr. Berkner.

Now, I am virtually certain that no one here really questions seriously the military need for the results of either basic or applied research, or development effort. We certainly can't let our weapons remain static in effectiveness while the scientific world goes on around us in the face of a fast-moving Communist threat. The problem is rather whether or not such efforts should be supported and directed by the military, isn't it?

For this discussion, let's go back to our definition and break up the second item into the three areas concerned--basic research, applied, and development effort. I am going to take them in reverse order, if you will, because, by taking them in reverse order, I can take development first, and I think this is the easiest one to answer.

You will recall that we defined development as the systematic use of scientific knowledge directed toward the production of, or improvement in, useful products to meet specific performance requirements. There is no fundamental law which says that the military must direct the program for development of new weapons or military equipment. In fact, Dr. Berkner has argued to the contrary. Certainly there was a time when the military supported very little research and development, preferring, presumably, to let new weapons and new equipment appear as they might from work of individual inventors or applicable effort on commercial products. But, could we actually get out of this development business at this time? I am convinced that we could not.

What industry would pick up the tab for the development of jet engines, for example, or a jet aircraft? And what possible civilian or industrial application is there for rocket engines that could justify an industrial organization's gambling on the development of such a machine? Trucks? Of course, this is easy. There are industrial needs for trucks, and we can let them develop military trucks, to some extent. Military tanks? Of course not. The need for a military development program is so obvious that I don't think it is particularly useful to argue the point here. What can be argued is the question of how we organize to manage it. And I prefer to leave that point to the later discussion on organization.

The next area, the question of the need for military support and direction of applied research, is not so easily answered. The industrial research effort of some $7 or $8 billion a year is largely applied research. Remember our definition: efforts to determine and expand the

potentialities of new scientific discoveries or improvements in technology, materials, processes, methods, devices and techniques, or that great catchall, to advance the state of the art. Today, we in the military do support a considerable effort in applied research. Personally, I am sure it is important and warranted. But don't leave it to my opinion. Let's look at the major solid propellant rockets, for example. Certainly, industrial research in explosives and in synthetic rubber contributed very greatly to the development of the large, single grain castings that make possible these solid propellant rockets, like the Pershing, Sergeant, Polaris, or Minuteman. But it was applied research, specifically directed at the business of solid propellant rocket fuels, which brought the technology to the point where development of a military rocket was feasible.

Look at transistors. It took military support of applied research, particularly in the silicone area, to move transistors out of their early limitations into the area where high temperature and high power applications were possible. Industry might have accomplished this result in time, but it was the military requirement for lighter, more rugged, equipment that telescoped these advances into a relatively few years.

Could this effort have been directed by an agency like OSRD? I am sure it could, but, whether they would have gained as early military acceptance if they had come to the military from without is quite another question. More to the point is the question of how much dollar support would have been provided for the further development of transistors if it had come from an agency not so strongly motivated as was the military. But let's look at that further in terms of the research and development climate within the military for this kind of support as we get into this next item on the list.

Before we hit that, let's tackle the third area, the question of basic research. Again let's check back to the definition. We defined this as research directed toward the increase in knowledge and science. There was a move not too long ago to give the responsibility for all basic research in Government to the National Science Foundation. This was resisted by the military, and wisely so, as far as I am concerned. Not that I question in any way the competence of the NSF to run a scientific basic research program. Nor could I quarrel effectively with those who would argue that a better coordination of our total basic research effort would result if it were all managed by one agency.

No, I think my worry on this score is a little more fundamental. I would worry that too centralized a control would deny the military a very profitable two-way street for knowledge to travel. It is not enough that we gain scientific knowledge in the military as a result of support of basic research. The scientist has to have some understanding of just where we are hurting--where we in the military need new information.

This brings us, of course, to the neat question of where basic research ends and applied research begins. I think the best way to answer this is to take an example and see where it leads us. Not very long ago we asked an outstanding explosives expert, a topflight chemist in the country, Dr. George Kistiakowsky, to review the national effort in solid propellants and make appropriate recommendations. His report speaks to this very problem we are discussing this morning.

He concluded that the recent tremendous upsurge in the military support of development of solid propellant rockets was rapidly exhausting our store of basic knowledge in this area. He had certain recommendations to make with respect to the direction that should be pursued in propellants using combinations like boron hydrides with fluorine for an "oxydizer," as well as others, seeking to get very much higher specific impulses than any propellant combinations that have yet been used.

By specific impulse, I refer to the amount of rocket thrust which can be obtained from a given weight of rocket propellant. It is one very important measure of the efficiency of a propellant combination.

In order to determine whether or not these propellant combinations will in fact be effective, Dr. Kistiakowsky recommended that the applicable physical properties of certain of these combinations be studied very carefully. This I would term as applied research, and I don't think there is very much argument here. But he also pointed out that many combinations had been postulated, based on extrapolations from existing information on other materials, similar materials, perhaps, but, nevertheless, those for which these properties were not known. Basic thermochemical information on the materials involved was just not available. There is a critical need today for basic research on certain of these materials to fill these voids in our fundamental knowledge. I call it basic research because we don't even know if it is applicable. It is just a question of expanding our information on these materials on the off chance that they might be useful. If this research

turns up information which leads us to believe that these materials can be useful, then we would go into applied research directing them down the path of propellant studies.

Perhaps this is a fine distinction, but I think not. The point is that Dr. Kistiakowsky's review pointed up voids in our basic knowledge that need filling. If the scientists working in this general area have the knowledge of our military problems which is inherent in the close relationship between the military and the scientists as we support basic research, they will see these voids and they, themselves, will press to fill them. At the same time, the military will develop a much better understanding of the areas in which they should concentrate their development effort.

That, I believe, covers item 2, the need for military research and development.

Let's move to item 3, the question of the extent to which the military is able to provide sympathetic and inspired guidance to scientific undertakings. I have shortened this to climate. I think this is about the stickiest question on this agenda. There are as many views on this subject as there are people who have had research and development contracts with the military, or who have worked in military research and development laboratories.

Earlier this year the Congress took a look at some aspects of this problem in a report of the Committee on Government Operations, familiarly referred to as the Dawson report, after the name of its chairman. This report treated only the Office of the Secretary of Defense and was highly critical of our efforts to manage the research and development program.

According to the Dawson report, we are not in very good shape on the question of sympathetic and inspired guidance to scientific undertakings by any means. The report says we review projects too frequently; we are too negative in our management attitudes; our methods of contracting are wrong; we don't permit every new promising project that comes along to have carte blanche for funds; we have no policy that would prevent the sudden termination of projects that are making good progress; and we encourage the buildup of highly skilled technical teams and then cut off support and allow them to break up. Those are some of the most serious charges--there are others.

To discuss each of these in a thorough and objective way would take hours, literally, and certainly we haven't either the time or the inclination this morning. My own reaction to this report--and I mean it sincerely--is that the writers did not understand the research and development problem well enough to tackle the monumental task of putting together such a comprehensive treatment. They used the words "research project" to cover not only basic and applied research but also actual development, as well as even test and evaluation effort. This is much too big a tent to try to include all of these items.

Perhaps a key weakness could best be illustrated by referring to the report's very strong support of parallel efforts in the research and early development stages of these various projects. This support appears to be based on a statement in the discussion to the effect that development is cheap in relation to the cost of future procurement programs. Now, that has certainly been true in many instances in the past, and it is a canard which we have heard referred to many times, but, in the major new weapon systems developments that are going on at this time, it is no longer true. We must come to an understanding of this. For example, the cost of producing all of the operational inventory of both the Thor and the Jupiter missiles--two separate production lines--is only about one-sixth of the development cost of only one of these two missiles. Even if you add to the missile cost the cost of the total system, the ground support equipment and the construction of the bases on which these missiles are to be put, you come to the numbers that the missile development effort for only one missile was more costly by a factor of about three.

Certainly one should not use one lone example to try to discredit a report as comprehensive and as well done as the Dawson report in many respects. I did not intend that that be the import of my comments. I merely wished to indicate that I think the report made the fatal mistake of not very carefully differentiating between research--and the two kinds of research--and development in treating the general subject of research and development management. My point here is that this subject cannot be treated in generalities. It is fatal to do so whether in the interest of brevity or for whatever reason one might approach it in that way.

That leads me to an easy out in answering the question of climate within research and development. The outstanding results of some arsenal-type developments--the Sidewinder missile, for example, or industrial developments such as the FPS 17 radar, or combination arsenal-industrial efforts such as the Jupiter missile--all are clear

evidence that the military can carry out effective developments very quickly when the right approach is taken. There are equally outstanding examples of effective research effort that was supported by military research and development programs--the rapid progress of the last few years in solid propellants, which we have mentioned, the absolutely fantastic results of miniaturization and subminiaturization efforts in electronic equipment, and the tremendous strides in metallurgy to get high heat, high strength materials. Obviously, the climate for effective research was right in those programs.

But, just as obviously, there have been instances of equal importance where the climate was wrong, or one would not hear so much criticism of military research and development direction. The Dawson report says we must change our contract procedures for handling research and development. We recently had an all-day meeting in the Pentagon with industry representatives on the question of cost allowances on cost-plus-fixed-fee contracts--and most of our research and development contracts are of that type. This is a continuing problem in the allowance of research and development costs. The meeting was largely a gripe session.

When I last visited a Government research management establishment to ask about this problem, I heard similar complaints. For example, I was told about a property clerk going crazy trying to make a scientist account for $10,000 worth of a rare, odorless, colorless gas that was said to be somewhere in a maze of interconnecting glass tubes in his laboratory.

These are problems of people. We can write rules, we can draft procedures, train individuals, and all that sort of thing; but enlightenment doesn't come out of a book. The matter of climate is a problem of growing research and development management competence over the years. I think we have made progress, but there is a lot more to be made, I am sure, and much of it is in the area of the human question itself. You know about human nature. Like the story of the man who went all out to help a friend of his who was in serious trouble. When it was all over the friend came to him and said, "I am never going to forget you for this." And sure enough, he didn't. The next time he was in trouble he came right back.

Let's tackle item 4--organization. When I was a student at this organization or institution, I wrote a 30-page thesis on this same subject. The Dawson report devotes a considerable amount of attention

to the question of organization. The Riehlman Report in 1954 also did. There are other treatments too numerous to mention this morning.

I referred earlier to Dr. Furnas' lecture to this College last year. He met this problem head on with this statement: "I would say that the high degree of autonomy of the three services, at least as far as research and development are concerned, represents an obsolete organizational setup."

Since the time he made that statement, the law has established a new position--the Director of Defense Research and Engineering. There are words in the law which give that individual full authority to direct research and development. But, the funds are not to be appropriated to the Director of Defense Research and Engineering, and he has no direct control over the management teams. The research and development management teams are right at the day-by-day working level in this business. We are apparently trying to have our cake and eat it too. I just do not see how we can preserve the administrative autonomy of the military departments and at the same time talk about effective centralized direction of research and development.

No matter how you look at it, the people who know most about any research or development effort are those who are in everyday contact with the effort in question. At the OSD level, it is practically impossible to keep abreast of what is happening at the bench level in a development effort. The facts are very elusive, at best.

I would like to illustrate this by the story of the spinster who was sitting in her room in a downtown apartment. The window was open and there were a couple of telephone linesmen repairing the line outside. Something went wrong, and the air was slightly blue with invective. She happened to be a very large stockholder in this telephone company and she called the president and identified herself. She said she thought he really ought to be ashamed that his employees would act like this and give the telephone company such a bad name. Since she was such a large stockholder, he called in the superintendent, and the superintendent called the foreman and demanded that this matter be investigated and a reply furnished him the first thing in the morning. Sure enough, the foreman appeared before the superintendent the first thing in the morning and said, "Well, the story that I get goes like this. Harry was up on top of the pole working on a splice and Mike was down below. Harry had a crucible of hot lead and it kind of tipped and went down on Mike's back. Mike looked up and said, 'Harry, you've just got to be more careful!'"

Believe me, this is just about the way we get the facts from the working level. After it has gone through four levels of staff between the development team doing the job and the Director of Guided Missiles, in this case--and it will certainly be so in the case of the Director of Defense Research and Engineering--it will never be possible to do a real job of central direction under our present Department of Defense organizational structure. Policy, procedures, level of effort, broad program approvals, yes, you can have those, but direction in the full sense of the word, absolutely no.

If one takes the research and development program as an entity, a central direction eliminating intermediate staffs is possible. The law will permit the Director of Defense Research and Engineering to handle some effort by direct contracting, if he chooses to do so, with the same kind of authority which the Director of the Advanced Research Projects Agency now has. But you can't separate research and development from the military organization, as we are now organized, and with the basic responsibilities as now assigned. Under our present overall Department of Defense organization, I can see no better solution than that which has been provided, if we can make it work. But I question whether or not we can. The law was signed out some time last summer but we are still unable to find a competent individual who is knowledgeable and who will accept the job as Director of Defense Research and Engineering. And without the individual all these words about organization don't make very much sense. So I leave this as an unsolved problem. When I stuck my neck out and put solution at the bottom of that chart I ignored organization. That's not one of the items on which I am prepared to propose a solution.

No, the only solution that I would propose this morning is that to the question of the general approach to our overall research and development effort. I propose that we should break this down into four major segments: research, component development--or subsystem development, if you will--weapon system development, and commitment to production. And it must be recognized that these will not only overlap but that these segments can and undoubtedly will in many cases be proceeding simultaneously.

I would propose that we handle each area in a somewhat different way, with ever tighter, centralized control as we progress from the first to the last of these categories. Research, both basic and applied, as well as feasibility studies, could be carried on with a considerable decentralization of control. Obviously, reports should be made to some

central agency for technical review, to be sure that we don't drop the ball between the cracks, and to be sure that all areas are covered and that there is no unwarranted and wasteful duplication of effort. The central agency would be responsible to assure free and complete interchange of technical information.

Component or subsystem development could also proceed with very largely decentralized direction, but each project would require prior approval at various levels of Government above the operating level--and by operating level I mean the actual research and development team manager--based on the magnitude of the effort involved. Only major efforts would come up to the level of the Secretary of Defense for prior approval. But again, reports would flow to a central agency for technical review similar to that which would be given research. This component and subsystem development may be system directed or not, as is appropriate in any specific case.

Control of commitment to a specific weapon system development or to production would be very tightly controlled by the Office of the Secretary of Defense for all major projects. Minor efforts could be approved by delegated authority. The key principle to be followed would be this: that no weapon system development, or end item development in the case of equipment, would be authorized unless it could be demonstrated that no significant invention was still required to meet reasonable military requirements for the item in question.

Let's look at two examples, to see what I am driving at. First--the Polaris nozzle problem, the question of the hardware that goes with the high specific impulse propellants in the Polaris development. This missile was originally proposed as a 1,500-mile ballistic missile capability, to be available in 1963. To get to that range, a rocket propellant of very high specific impulse, higher than anything that had been fired in a large rocket motor, was known to be required. High specific impulse meant that one must expect very much higher flame temperatures in carrying out the nozzle development. In fact, the long development time scale had been planned to allow among other things for the development effort in hardware necessary to take care of these very high temperatures. Nozzles must be provided to withstand the high temperatures and still be of light enough weight not to penalize the range capability of the missile system.

The hardware solution that looked best permits a ring, which is called a jetavator, to get into the jet stream itself and deflect the jet stream enough to give some directional control to the missile for flight

control. A very early success with this jetavator idea, plus success in thrust termination, which was another problem they foresaw in the early days, led the Navy to revise its development schedule to plan an operational capability three years earlier--1960. But there is a joker here. They could schedule this early solution only if they could use a propellant of somewhat lower specific impulse than the propellant known to be necessary to get to the 1,500-mile range that was desired. The predicted range of first operational capability is now in excess of 1,200 miles but less than the longer range desired. Since this still permits a very considerable target coverage in the USSR, it is certainly worth going ahead on the basis that as the development efforts progress, later models will have the greater range capability.

Let's look at another example, the nose cone of both the early Atlas and the Thor missiles, which is a solid dome of copper. This nose cone provides a very considerable mass of copper to sop up the heat that is generated on reentry. There were many really competent people who predicted in the early days of the ICBM development, only as recently as two or three years ago, that this just would not solve the problem, that we might well find the reentry problem to be beyond our ability to solve with known materials. But the people who chose this copper heat-sink approach chose it as the most conservative approach they could think of at that time. It was assumed that one could always add more copper if it appeared that early designs had not provided enough heat-sink capacity. But, obviously, if one had to add more copper, this was more weight, and it would penalize the range capability of the missile itself. This problem was considered so serious that, in the very beginning of the crash ICBM program in 1955, it was decided to let two separate contracts for nose cone development.

Now, along behind that development they pursued a more radical approach to the problem. This was the ablation nose cone. The ablation nose cone works on the principle that the coating of the nose cone shape will boil away by the heat of reentry. This boiling away removes heat which would otherwise reach the inside of the nose cone. The success which they had in this later development effort now leads us to plan that the Atlas will ultimately use an ablation nose cone. This not only cuts the nose cone weight by a large amount but also cuts the cost. One can put this weight back in decoys or take it to increase the range of Atlas.

The point I want to make is this: missile hardware was available in the early 1950's to use as test vehicles to find solutions to this reentry

problem and the Polaris nozzle problem, if we had been bold enough at that time to spend the money to move into these areas and to shake out some of these basic unknowns, rather than wait until we were in a crash development effort, going for a specific weapon system, in order to solve the problem. As research and development managers we have been too insistent that everything that the developer wants to do must be tied to a specific weapon system development in order to get any kind of fiscal support.

Well, in these cases we were lucky. The unknowns turned out to be all in our favor. But they could just as readily have gone the other way, and, in the crash aspect of the program, could have left us with unusable hardware and with several hundred million dollars down the drain.

This brings me back to the proposal I made. We have to be bolder and more imaginative about supporting a broad research and development effort that does not have to be justified by specific weapon system developments. It can be weapon system oriented. That's perfectly all right, naturally, and is advisable in most cases. If we do that, we can hold a tighter rein on commitment to weapon system development based on proved technology. The result will be fewer abortive development efforts and a very much shorter development cycle.

I would ask you to watch the development of the Hound Dog air-to-surface guided missile. This is a development to provide the B-52 with an air-to-surface missile capability. It is a development which is being pursued in just the manner I described--no invention involved. All of the subsystems are current state of the art. They are neither obsolescent nor are they far out in the future. The basic airframe design was proved in the supersonic Navaho program, as was the guidance system. The engine is essentially off-the-shelf just now completing its development and test. The development schedule for the Hound Dog, therefore, can be brief, with no reason to expect any bad surprises either in time scale or cost. I see no reason why they can't meet the planned schedule.

I hate to rest my case on one program, but I am so convinced that this is the right approach that I'll stand pat on it. Just remember this. When the research and development enthusiasts promise you the moon in some new system, find out if there is any significant invention required for success of the system, for it to meet some reasonable operational requirement. You can schedule engineering in a development effort. But you cannot schedule invention. When you try to, more often than not you fall flat on your face.

In summary, let's quickly review our outline. Our very heavy responsibility to the scientific community is accepted and well-known. Our need to support military research and development is clear. The climate to carry out research and development in the military is spotty and certainly needs improvement. Our organization for research and development is not particularly good, but it is the best that we can get within the present Department of Defense organization. And the solution to all of these problems lies in a better understanding of how to do research and development; particularly, an understanding of the basic fact that you can schedule engineering but you can't schedule invention.

CAPTAIN STEVENS: Now we are ready for the questions.

QUESTION: General, the Air Force ballistic missile ICBM program went ahead with some parallel developments, certainly parallel in some of the subsystems complex. Under your concept of not proceeding with production until invention was pretty much assured, would it be possible to proceed with the kind of program that we did, even the ICBM program?

GENERAL BETTS: You have to make your choice at the time you kick off to a weapon system development. In this case, because we had not had broad, imaginative support in solving some of the problems that were in front of us, and thinking about the ICBM capability, we had to throw certain invention items into the program. The nose cone was one of them. But, on the other side of the fence, the existing state of the art was used in the engine program. The Atlas engines are essentially Navaho engines, upgraded from the Navaho to the Atlas. So that, where they had state-of-the-art components that would do the job, they were plowed into the system, this being a crash development. But, where there were unknowns, they were forced to spread their effort in a kind of a crash way to solve those unknowns. As it turned out, as I remarked, we were lucky. In retrospect, we weren't lucky. It was all there, if we just knew the right answers. But, at the time they started the ICBM development, there were a great many people who thought, and one person who said, that the micrometeorite particles in the sky, through which this nose cone would go, would just tear it to shreds, and it had no possibility of surviving. These things were bugaboos, and they disappeared. But these were good physicists who dreamed up some of these problems, and, presumably, it could just as well have gone in the other direction.

You can't go back with individual developments now and say, "Why didn't we do it this way?" At the time you kick off the development you've got to have your broad support so that you can pick up these things. You can't start from now and say, "Well, lacking broad support, we'll start a development which depends on broad support that it should have had earlier."

QUESTION: How do you fit the horrible problem of applications engineering into your program?

GENERAL BETTS: I think applications engineering as a term, as far as I am concerned, is something that was dreamed up to get a little bit more of the engineering types into the research and development game. It doesn't really mean much more than that. I think that you've got to think about applications engineering from your basic engineering design. If you don't have sense enough to think in terms of the use of an item, it is not going to be particularly well designed anyhow.

QUESTION: General, we notice that the Atlas missile has been getting some very fine results recently, and also we hear that there are plans of going ahead with a solid-propellant Minuteman. It seems a little unusual that we are continuing to develop the Titan. Would you care to comment on this problem?

GENERAL BETTS: Well, the problem of choice of systems in the missile business, as you are pretty well aware, is not too easy, because, in order to get enough test vehicles to do your development job properly, before you fly the first missile you have to have a rather significant production capability. This is where we are today with the Titan. As a matter of fact, the Titan is supposed to fly today, the first missile. This will be a one-stage flight as far as the developer is concerned. He is trying to check out his first stage.

But, from the point of view of the team--in this case the Air Force--that has poured several hundred million dollars into the Titan effort, it has a two-stage missile which has certainly greater growth capability than the Atlas one and one-half stage, by the basic fact that it was laid down a couple of years later in design.

So the Air Force, in looking at the problem of whether it should or should not continue the Titan--and believe me, they have been doing just this and considering what it would get from the greater growth capability of the Titan against what it would get if it took all the dollars that might

go there and plowed them into Atlas production, has come to the conclusion that the additional costs over the whole program for both missiles to have this second capability is worth the kind of increased capabilities one will get from Titan. That is the Air Force judgment in the matter.

QUESTION: General, would you comment on how we got this huge mass up in the satellite sphere yesterday and the significance of this, if any? Second, with the problems on reentry, will this disintegrate totally, or are we going to have it hit the land somewhere?

GENERAL BETTS: I think it will disintegrate, because we didn't intend to bring it back down. In other words, we were not worried about reentry. The thing will have accomplished its mission by going around the world for 20 days or however many is the best prediction. The significance from the missile developer's point of view is that we made this a maximum thrust effort, with maximum acceleration. We got this capability by taking the weight out of the nose of the missile. Here's where we got all the additional thrust that was necessary to put that thing into orbit. In getting that additional thrust, obviously, we subjected the missile to more structural stress and greater acceleration than would be normally expected in an operational missile. Consequently, it was a sort of an overdesign test.

QUESTION: General, you mentioned that something in the order of $6 billion will go in the research and development funds this year. How much of that is basic research, as you defined it? How does this compare with the National Science Foundation?

GENERAL BETTS: I don't have specific numbers on that, but I think our total basic research in all three services is something in the order of $25 to $30 million. I haven't checked back to look at the statistic. (Ed. note: The most recent DOD estimate is $115 million.) The National Science Foundation fund a couple of years ago was about $12 million. I think it is higher than that now. I think it is up to about $20 million. So we are still putting more Department of Defense support into basic research than is the National Science Foundation.

QUESTION: General, you remarked that in industry there is no comparable need for rockets and so on, so that industry would probably be of little help unless the military services were deeply involved. Actually, the critics--Dr. Berkner and some of his later followers--have pointed out that the incentive to industry is what pays off in industry

on its progress and that our military system of doing research and development and utilizing industry ignores the incentive motive in industry to bring out its better capabilities. They feel that some form of incentive in our research and development contractual arrangements should really pay off for us on these new, radical improvements. Will you comment on that?

GENERAL BETTS: I am sure that this is probably the single most critical item in the business of actually contracting for research and development, the fact that you have to allocate research and development contracts based on a whole gamut of factors which may or may not be real. Not the least of these is the question of the contractor's ability to sell a bill of goods to the military. I have heard very competent contractors in some of these major competitions say that this is not a question of competition among technical capabilities but is really a competition among salesmen types to see who can lie the worst and get away with it.

But there are some attempts under way to correct this. One of those which is farthest along, and one which we should watch, is an Air Force effort to actually put some dollar incentive into a cost-plus research and development contract. A lot of people have said this won't work. It is being tried and I am certain that all three services will jump on this bandwagon if it turns out to be an effective mechanism.

This business is not without competition, and that competition comes in the performance of individual contractors. If you look at the contractors who have had real, solid, continuing success in turning out systems, you will find that they manage to continue to get military support. This is a pretty long-range type of competition and one which is a little bit hard to assess. But, nevertheless, it is there.

QUESTION: General, in your opinion, is Defense support of basic research in consonance with the rate at which we are now using new knowledge and will require it in the future?

GENERAL BETTS: Well, as a research and development type, I am always inclined to say that we do shortchange the basic and applied research effort. It is awfully hard to cite chapter and verse on this, because those who are trying to squeeze every last dollar out to support some particular weapon system can always manage to find marginal research projects which some manager, at whatever level it may be, has decided to support.

By the same token, if you talk to the people who are research types, they will tell you of lots of things they would like to see explored.

Dr. Furnas last year went into this very briefly, I notice, in his lecture, and I think he said something which I have heard many times from really competent people in the scientific fraternity. That is that, if you made any major increase in your amount of allocation to the support of basic research, you would start picking up some pretty marginal capabilities, and you have to be awfully careful of this. Then you pay more money for no more particularly successful work.

QUESTION: There has been a lot of noise in the press of late, and, of course, for several years, regarding the fact that secrecy and security inhibit scientific research. There has been a lot of dust, I believe, which has not yet settled on this subject. Will you comment on it with relation to climate?

GENERAL BETTS: Well, one: I don't think there is any question that secrecy does stifle the exchange of information, and, when you stifle the exchange of information you are bound to create situations where people are working on something that has already been solved. At the same time, however, a rather massive effort goes on at the military management level to try to get a very considerable exchange of information. I can pick an example right out of the air. Dr. Tenney of Los Alamos was asked just recently to go to all of the solid propellant development people--these are the people making the big single-grain castings in the several different missile systems and rocket systems we have now being supported. He is an expert in the business of analysis of what is wrong with an explosive casting, since this is one of the techniques that are tremendously important in atomic weapons work. This has been his business for many years. So he really knows it. He came back in and said, "The word just isn't getting around." He said, "Some of these people just don't even know the latest techniques." The solution there was to add this as one more chore to the solid propellant central information agency, a contractor who is specifically designated by the three services as a single point--in this case it is the Applied Physics Laboratory--to which all of the technical reports flow and from which they would flow back to the other people who are in this business.

Let's look at it this way: I talked to Admiral Raburn just the other day about a discovery in his nozzle development program. It came up in a discussion about the support of the Polaris research and development program for the next couple of years. We had just had a thorough briefing

on Polaris only a few weeks before that, and the subject hadn't been mentioned. I am absolutely certain that the idea is relatively new and that it has not gotten around through the solid propellant industry. These things don't get down in reports five minutes after the man has the idea. Once he has the idea, he wants to go through some elementary hardware work, and perhaps an actual test, before he is prepared to say it is a good idea.

These things take time, and, after he goes through this business, he will write a report. It takes time for that to get circulated and takes time to get to the people in the business. We have technical symposia where people who are cleared are brought in, and we try to have the exchange of information. We try to encourage it through these technical advisory groups who are in the Pentagon and do meet and pull in together the various people in the industry.

But there is no ready solution to the exchange of information that is any better than putting it out into technical journals for broad distribution just as quickly as we come to this information. But, obviously, if we are to keep our best secrets to ourselves, this is a rather hazardous thing to do. You have a tough choice here. That's no answer.

QUESTION: General, how much change do you think there would be in our OSD research and development review process if we lost the services of a couple of uniquely qualified people like Dr. Quarles?

GENERAL BETTS: I think that Dr. Quarles has done a very effective job in all three of the major positions he has held in the recent past, as Assistant Secretary for Research and Development, as Secretary of the Air Force, and now as Deputy Secretary of Defense. I think that the Secretary of Defense today would have a hard time filling that job, because there is no question that Dr. Quarles is a really solid, dedicated individual who probably works more hours than anybody else in the Pentagon. He is really sharp and retains what information he gets in the various briefings. He is a tremendous help to the Secretary. But I don't think that you can say that the organization would fall flat on its face if we lost an individual. Somebody, somewhere, would rise into this position. I would say that Dr. Quarles' predecessor, Deputy Secretary of Defense Mr. Robertson, was a real tower of strength and very effective.

These people are available, but it is a question of cajoling, arm-twisting, and whatnot to get them to take these very difficult jobs that are subject to so much political pressure.

QUESTION: The Federal budget for FY 1959 contains the following statement: "For the past four years we have spent a total of $20 billion for research and development test of guided missiles." Would you care to comment, General, on whether you think that rate of $5 billion a year for research and development test of guided missiles should stay about the same or increase?

GENERAL BETTS: I think the inflationary factors that are concerned, plus the increasing service acceptance of guided missiles, will steadily increase that number. How fast it does go up will unquestionably be tied to just how good the missiles are that we get out of the development effort. The history has been a steady progression upward in the allocation of dollars to guided missiles, but you can't take much bigger a bite out of the defense procurement effort, and a large part of this bite comes out of that effort, unless you do in fact terminate some of the manned aircraft development effort. We have reached the point now where, in the FY 1960 budget review, there are manned aircraft developments which look awfully good for various reasons, which are not going to get the kind of support that had been planned for them, simply because the guided missiles are taking the bite which they are taking.

I would hesitate to predict anything more precise than that.

QUESTION: General, in your estimation, would there be anything gained if the three services established, say, a 10-year program of officer exchange in the research and development field?

GENERAL BETTS: I think joint organizations of this sort and joint staffing jobs are tremendously helpful in the business of getting all of us to understand one another's problems. I would say without question that, having moved into the Office of the Secretary of Defense, I know a whale of a lot more and understand a lot more about both the Navy and the Air Force missile problems than I had any idea about before I moved into that job. I don't know whether you would help as much as you might hurt if you actually put various men of one service in the actual management of another service's research and development effort. One puts these managers in primarily because, as military people, they have experience in the problems of their services and one would want to bring that experience to the direction of that development effort.

I would hesitate considerably if anybody asked me to get into the development of an aircraft that was to be used for the tactical support of

ground forces. My contribution to this would be nowhere near as much as would be the contribution of an officer who had actually flown tactical support missions and really knew what it was all about. You can't have it both ways.

CAPTAIN STEVENS: General, at this time it is the usual custom for the monitor to describe the speech with a lot of flattering adjectives. I am going to say, and I am sure everyone here agrees with me, you were great. Thank you very much.

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