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ORGANIZATION FOR RESEARCH AND DEVELOPMENT IN MAINTAINING
NATIONAL SECURITY

21 November 1947

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GENERAL MCKINLEY: Gentlemen, Dr. Bush lectured to us last year with such effect that he was kept overtime answering questions. We know that today he has an even more important message for us. You know his background, of course, from the biographical sketch that you have read. I will review some of his more important recent positions.

In 1940 he was made the Chairman of the National Defense Research Committee, and in 1941 Director of the Office of Scientific Research and Development. In October 1947 he was appointed Chairman of the Research and Development Board of the National Military Establishment. He has for some time been President of the Carnegie Institution of Washington, D. C. His subject today is "Organization for Research and Development in Maintaining National Security." I am highly honored to present to the Industrial College and assembled guests, Dr. Vannevar Bush.

DR. BUSH: It used to be said that an institution is the lenthoned shadow of one man. I suppose in earlier days that may have been true; it may still be true in simple affairs. In our complex times, however, we cannot usually think of institutions in quite such simple terms, nor can we hope to have a clear understanding of the complicated structures which we set up unless we come at them from the point of view of the ideas which they embody. Thus my subject this morning, "Organization for Research and Development," must be approached philosophically if we are to get anywhere. Organization, regardless of the care with which it is put together, regardless of its plan and balance, is meaningful only as it is the expression of a set of concepts of the relationships between men. Organization otherwise conceived is little more than a chart on a piece of paper, full of boxes and lines and labels, but empty otherwise. The primary fault of an organization chart is that it is sometimes mistaken for an organization.

In this era of canonization of science and scientists perhaps the best way of pointing up the problem of properly administering the research and development program is to ask the question, "In what directions should we not conduct research and development?" Literature abounds with stories of brilliant inventions occurring through happenstance, of great industries being founded upon the random curiosity of an eccentric individual, and of research which failed in its original purpose bearing huge dividends along the lines of the most unexpected by-product. He would be a bold man indeed who took it upon himself to say that some project would not be worth doing. Yet here is the crux of the matter. Our funds are limited; so are our facilities and our

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reservoir of talent. There is not a single successful laboratory director, nor in fact a single successful scientist, who has not shaped his career in terms of tempting avenues along which he elected not to travel.

We have reached for the military needs of this Nation a pattern of organization for research and development which gives promise of effective functioning, provided we can see clearly what it is supposed to do, and how it is supposed to do it. The experience of the specialized means used during the war years has been helpful in the development of our present structure. Similarly, we have been able to draw on the rich experience of the United States in university and industrial research. We have within the Services more logical structure than before the war, with some centralized co-ordination of programs, scientific advisory committees, analytical groups in OEG and Rand, and rather embryonic means for tying together users and suppliers, developers and strategic planners. Even before passage of the National Security Act of 1947, we had, thanks to the action of the Secretaries of War and Navy in 1946, also a valuable year's experience in integrating the programs of research and development for the entire military effort under the Joint Research and Development Board. The lessons learned from this history have been incorporated in the present pattern of the Research and Development Board of the National Military Establishment. The directive for this body, which will probably issue shortly, will elaborate on how it is to carry out its duties under the Act, define its relationship to budget making, and provide for close relations to the Joint Chiefs of Staff, in order that development planning and strategic planning may proceed with that interrelationship without which either would consist of planning in a vacuum. All of the charts and documents will be available to you, and I do not need to describe them here. Rather, let us concern ourselves with the larger aspects of the affair. Organization is of importance primarily as it expresses a philosophy and a basis for action. Let us examine what these are in the case of the research and development programs of the National Military Establishment.

The undertaking which is formalized in the creation of the Establishment is a new thing to this country, a new thing in the world except insofar as our British contemporaries took the lead in making certain comparable provisions. As it evolves, as we now conduct our programs and planning on a truly over-all national basis, as in other words we become modern in our military thinking, it is incumbent upon us to be sure that the core of future military strength, namely the development of the weapons of the future, is on a truly sound basis.

What is a sound basis?

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Let us first consider a group of general principles applying to the conduct of research and development for any purpose. We may regard these as essential elements of any sound program. I propose then to discuss principles peculiar to military research and development, which make the problem unique in many respects, and finally to attempt to draw some specific conclusions.

Analysis will disclose a clear pattern of evolution from the original basic research to the training in use of a particular embodiment or product which is generally the final step. It is important to have that pattern firmly in mind, for in it are involved the general principles or essential elements with which we are concerned. It falls into eight stages.

First, at the base of the pyramid, or the beginning of the story, must be an expanding body of science in general, a storehouse of fundamental ideas constantly being replenished through basic research, for which there must be an ample supply of well-trained men with proper facilities and above all with the creative ingenuity and initiative which are the hallmarks of the able fundamental investigator.

Second--and this is all too easily taken for granted rather than specifically recognized as it should be--there must be a constant alertness to discern in the growing body of basic knowledge those points whence practical applications may grow. This is not a matter to be left to chance; it is a matter that should be constantly under examination. It is decidedly under examination in industry, and it should be in military circles.

Third, the ideas thus discerned must be tested in order to determine whether they have merit--this is the stage of primary applied research. Well done, it results in the elimination of some possibilities, the recognition of others as offering major promise, the centering of major effort on them, and consequently, greater insurance of economy and effectiveness in the subsequent stages of the total program.

Fourth, we come to an operation which like the second may too readily be simply assumed instead of actively perceived. Just as at the second stage there must be alertness to the applicability of ideas, so at the fourth, there must be reasoned and deliberate examination of the possible outcome--in terms of technical performance--of development of ideas which have stood the test of the primary, or exploratory, applied research. It is absurd to try to predict in advance the full outcome of truly fundamental research. It is equally foolhardy to omit the attempt in the case when applied research is to lead to further steps. It is dangerously easy, in the enthusiasm of the effort or under the pressure of non-research consideration, whether industrial competition, economic necessity, or political agitation, to minimize,

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or even to omit entirely, this reasoned and deliberate advance appraisal that belongs at the fourth stage. If it is minimized or omitted, trouble is very likely to result at the next step.

The fifth step, in a sound and well-ordered program, is the setting up of programs of development for the purpose of producing prototypes. I want to emphasize here again the second and fourth steps. By one of them, we increase the number of possibilities to be explored; by the other, we cut down the number of actual physical results of that exploration. That is, we do not build a prototype of every idea, but we do consider all the ideas available before selecting those to be carried through. The point of this, and the reason why I stress it so often, will appear the moment one looks at cost sheets. To pin down an idea often takes only pencil and paper. To build prototypes may take millions. Obviously, then, the prototype should embody only ideas selected with the greatest of care from the largest possible stock. And immediately this fifth stage has established a development program from which a prototype results, there ensues a further analytical and evaluative operation--the sixth stage--that of testing the prototypes and re-examining them in the light of the test results.

A number of improved devices based on the prototype but incorporating the changes and adaptations indicated as desirable by the sixth-stage test results can now go into experimental production, and the seventh step in the process is now taken. The next, the eighth, is not, however, to begin manufacture and get ready to go onto the market or into final use. On the contrary, if the program is soundly conceived and properly conducted, this stage again is a process of analysis and evaluation, if possible through out-and-out field testing of the experimental devices, for the final ironing out of difficulties. Only after this has been done can we truly say that the research and development program has been fully carried through and the point of engineering for production, with subsequent manufacture for commercial purposes--or for procurement, if we are talking in military terms--has been reached. And it should be emphasized that after the actual program of research and development, strictly defined, has reached culmination in the program of engineering and production, the research attitude of mind will be called on again, both in the training in use which must follow manufacture or procurement and in the continuing examination of performance with a view to continued improvement in accepted devices or systems.

We have, then, an eight-stage process preceding the actual production of device or system. If any link in this process is missing, the program of research and development falters. In general, the costs rise steadily as each stage of the process is passed. This fact underlines the importance of the recurrence, at practically every second

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stage, of the procedure of analysis and evaluation before a decision to press further. In industry, under the compulsion of competition, and the need for timely results, there is sometimes a strong temptation to scant these analytical stages, sometimes even to omit them, but an industrial research and development program conducted in this fashion can easily grease the ways to bankruptcy, and the operating statement and the balance sheet are strong deterrents after one sad experience.

Now let us consider general principles that are of general import.

The upper limit on the amount of basic research is automatically set by the number of skilled researchers available and the facilities which they have to work with. In times of peace it is reasonable to push the program very close to its maximum rate. Basic research is our source of ideas, our supply of potential applications. When we no longer have need of new ideas will be seen enough to call a halt on basic research--not before. Nor should the range of basic research be limited in the fancied interest of an immediate and specific applicability. There is a good example of this in recent military history--in the long research that seemed to have little practical bearing but that led finally to the atomic bomb. There may indeed be parallel cases in the future. But the question of support for basic research is intimately tied up with the question of manpower. There is no way of wasting money more rapidly than to place research in mediocre hands.

Basic research flourishes only when free, when men are able to pursue their own interests in their own way, when support of the individual depends solely on his ability as estimated by his scientific peers. Given able men, given conditions of free action for them to work in, the results will be a safe bet. Money to support as full a program as the number of available men of great research ability will allow should be expected to flow to support their work, and this should be definitely assured, whether the support is private or governmental, military or nonmilitary, for there is no sounder national investment from every angle. But that money must not involve external controls breaking down the essential condition of freedom. I stress this in order to point out that there is no reason why basic research should not be supported by military funds, as indeed it is being very effectively supported at this moment, although in my opinion the ultimate future control of the major part of federal support of basic research should reside elsewhere. I also emphasize that the military need to guard against distorting their present attitude and procedure; they should rigorously continue the salutary attitude toward support and freedom which is characteristic of the present situation.

Basic research must be regarded as a broad common foundation for almost every conceivable type of new practical application, and we must religiously protect its independence from the pressure of practical demands and insure its freedom of conduct by the men whose lives

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are devoted to it. For this is the area of greatest unpredictability and most sweeping long-range possibilities. Fortunately, it is also by far the cheapest part of the program. Military funds devoted to basic research at present are less than ten percent of the total research and development budget, and one must look far to find a scientist today who is not busily occupied and well-equipped with facilities.

To appraise proposed fundamental research in advance, a number of questions may be asked. A primary one is: What do you expect to know, if successful, that is not now known, and is it worth knowing? This is a favorite question for research directors, and there is value in asking it if one does not expect explicit answers and recognizes the inherent intangibility that sometimes must characterize a search for the unknown. The playing of hunches has its place. But an overriding question is: Is a scientist of earned reputation for success willing to gamble his time on it? If the answer is yes, it is a bet. This single question would do, except that there must for every youngster be a first research on his own. In the case the question: Has he fire in his eye? is as good as any.

Primary applied research, to seek out new data and examine the possibilities and limitations of newly understood phenomena under conditions of actual embodiment, is, like basic research, relatively inexpensive. The questions to ask here are: Why do it now, why do it at all, why do it this way? Much of such research will be done by industry for its own purposes, and the military needs can be taken care of by maintaining contacts and alertness. Some will not be done adequately except by government. An example is in applied aerodynamics, where facilities are expensive, unduly so even for universities or industry.

We then come to the stage of developing prototypes. Here some big questions arise. What is a prototype? A prototype may be a model of a supposedly advanced and valuable device ready for engineering for production and use. It may also be merely a necessary intermediate step toward such an end. These distinctions are often confused, and when they are the result of the effort is neither hay nor grass.

For the intermediate prototype the question is simply: is it worth the effort? Will the knowledge gained by testing it warrant its cost? Is it an essential step in the whole effort, or could it be omitted? More specifically: Is it a sufficient extrapolation on existing knowledge and practice to be a reasonably hazardous undertaking, neither so safe as to be pedestrian nor so extreme as to be likely to throw the main effort off the rails? These questions are seldom asked and answered. If they were, there would be fewer prototypes.

The other type of prototype means that we stop there and freeze. Production, with its very large expense, will ensue if the effort is

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successful. For a prototype to be undertaken with this objective there must be an imperative reason for freezing. In industry, the pressure of competition or the opening up of a new market may provide the reason. From the military point of view, such a reason occurs if there is a substantial probability of need soon after the time the new device will emerge from procurement in moderate quantities. Another reason may be that currently used devices in the class are sufficiently obsolescent so that training suffers. The essence of the matter here is timing. There is no sense in developing a device for immediate use, if there is to be no immediate use. While progress toward an ultimate result often has to be taken in steps, there is no benefit in concentrating energy on things that are foredoomed to obsolescence. While we must be sure of being ahead of the enemy at the outbreak of a war, it is readily possible to be too far ahead in some areas in time of peace, with the result of doing some of the enemy's work for him and prematurely exhausting the possibilities of a specific advance.

It is in the field of highly programmatic research and of development that the most difficult decisions must be made. Much of our ablest and most essential talent is required for developments shaped around definite so-called "requirements." It is a costly and time-consuming operation to transfer such talent from one line of endeavor to another. Picking the choosing among the lines of endeavor is therefore where the highest art of technical administration comes into play. Examples can be cited. The British decided early in World War II to devote none of their effort to guided missiles. This was a carefully measured choice, based upon their estimates of the dividends that might be earned through guided missiles as compared to improved conventional aircraft. The wisdom of this decision is an interesting subject for further analysis, which might point up a number of identical questions which we ourselves face. Are all the various types of prototypes of guided missiles now under development for this country worth the effort? Do we expect all of them to represent necessary steps toward genuinely useful additions to our military strength? The art with which the ax of cancellation is wielded will do much to shape our future security.

There is opportunity enough for a whole lecture in comment on every one of the principles of research and development which we have been discussing. I wish there were time for all of them. As it is, let me remark on but one more in general before proceeding to discussion of primarily military programs. I should like to underline to you the importance in all research and development of provisions whereby sound ideas arising at any level in the organization will receive the attention they merit, will be sympathetically evaluated. After all, the primary business of research is to uncover ideas and the primary business of development is to make use of them. It follows logically enough that a program so set up that ideas run the risk of being perfunctorily examined, or of being weakened, stultified, and finally extinguished

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because of an inordinately long distance between the point of origin and the point of authority, is doomed to fail. I do not want to be misunderstood as arguing that we should let the enthusiasm of advocates go on a rampage at every turn. Unrestrained enthusiasm can waste money, personnel, and facilities, and produce nothing but confusion. In fact, it has often done just that.

On the other hand, suppressed enthusiasm creates stagnation. There must of course be evaluation and judgment, but that evaluation must be sympathetic, informed, competent, realistic. And it must be accessible to the new idea, regardless of its source. Any system in which the idea of the man in the laboratory or at the controls must go through a dozen echelons before anything can happen, each echelon less informed and more conservative when it comes to technical innovations than the preceding, will perpetuate the obsolete, produce discouragement and stultification, wreck morale, and invite disaster.

Leaving the broad subject of the general principles necessary to sound research and development programs regardless of field, let us now consider a number of specific conditions pertaining to research and development for military purposes. In the main, these are extreme forms of conditions intimately affecting any program, and as extremes they cause peculiar problems.

Primary among them is the matter of security. About this I might give a full lecture. We recognize in the abstract the necessity and responsibility of preserving in secrecy information essential to the defense of the country. We at least give expression to the principle that we do military research for our own benefit, and not for the benefit of a potential enemy. While we recognize the need of an informed public concerning matters on which it needs to be informed, we also recognize the principle that we do not conduct military research for the public entertainment, or to provide newspaper copy. Our principles and our practice in this country are unfortunately not now in line. But I do not have time to elaborate on this matter here, neither can I examine in detail the principle that security is a two-edged sword. It at times has done damage to our progress in the free flow of ideas--the free interchange of knowledge among those who should know--so highly important in basic stages. The problem of declassification hence is of the first urgency; few people carry a heavier responsibility than do those who, charged with the task of declassification, must seek to strike the safe balance between what can properly be made known and what must be retained. To refer in this connection to the security problem as the Atomic Energy Commission confronts it is sufficient.

It is a sound principle that clearly technical decisions should be made by technical men, and clearly military decisions by military men. The trouble is that nowadays we have fewer and fewer of such

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clearly defined decisions to make. Few problems are so neatly segregated as, for example, was the atomic bomb. Here military men had little trouble in appraising and certifying the potential military worth of the proposed development, and the decision whether to push it hinged primarily upon its scientific, engineering, and industrial feasibility. In most cases, scientific and technical solutions and military appraisals are not so neatly distinguished, will both be subject to controversy, and each will influence the other. Close interrelationship, with specific duties and responsibilities clearly defined, is evidently needed between developers and strategic planners.

The sheer magnitude of the effort required in military research and development dwarfs any program that could be undertaken by even the largest industrial, academic, or other unit. All the problems of personnel and facilities are hence intensified to the maximum. We hence cannot plan in isolation, but only in the light of the over-all facilities and manpower of the country.

The work of military research and development is done in fulfillment of a public trust. Administration of military research and development is not made any simpler by its public character, with all that that implies of the proper demand for public knowledge and control, as against the equally proper demand for security. Administration of military research is also difficult because financial control becomes exerted in strange and roundabout ways. In an industry there is usually one man who is carefully following the flow of dollars throughout, and alert to stop leaks or wastage. If there is not, that company is a good one to sell short. In government, by its very nature, the corresponding function is diffused. The pressure of budgets often acts erratically. Retention of an item in a decreasing budget sometimes depends upon decisions by those who are far from the facts. It is a wonder that we do as well as we do. But that is no reason we should not improve in the direction of rendering budget control more logical and intelligent.

Introduction into military use presents problems going beyond those which industry faces. To introduce a half-baked product industrially may mean passing a dividend. In military affairs it may lose a battle.

The breadth and uncertainty of the planning problem of military research and development present extreme difficulties. Granted that the analysis of markets, sales problems, and competition in industry and commerce is hardly free from uncertainty. Yet such matters seem trifling when they are compared to the strategic, tactical, and operational questions which must be faced in military planning.

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Industry, when it launches a new model, has the last year's model. The military have the experience of the last war, and that is soon too far in the past. Even the change to come gradually, to be under control, so that we do not get into a situation where we are outstripped before they cost heavily, with little fear that a strike out far ahead. The military, on the other hand, competitors who will make the radical advance and take a reveal occur at long intervals, and the testing of maneuvers is a substitute.

This leads to another principle. Military research and development will always be pedestrian in nature if the organizations are such as to frown on taking a chance. If service records are filled with successes and failures, and failures are always black marks, forward-looking virile research will not occur, for failures always happen and should be expected in any research program which is alive. If they are penalized, then in general men will play safe, development will be reduced to the sure thing, the net result will be a set of elaborately perfected weapons that are out of date. If our military leaders are men of such vision that they can provide an atmosphere in which those who control military research at the operating level know that their advancement depends on true accomplishment, and not merely on the avoidance of missteps, then we can indeed go places. Otherwise we will emulate the dodo bird that flies backward, for it is not interested in where it is going, merely in where it has been.

One evident principle flows clearly from the experience of the last war. Planning today--strategic planning or developmental planning--must be on a genuine over-all national basis. The interrelation of the arms and services demands it. Combined operations will be the rule rather than the exception in the future, and must be based on careful advance planning on strategy, tactics, and materiel. This requires that there be cadres of officers, with strong emotional loyalties to their early associations it is true, but capable of rising above these in intellectual efforts, and hence able to plan broadly and without distortion due to personal experience, attachment, or prejudice. The extent to which we accomplish this, as opposed to the solutions arrived at by trading between competitors, will be a measure of our military maturity. There is no need of sacrificing the valuable esprit de corps, pride in the uniform, or natural human loyalties in the process. But there is extreme need for rising above the partisan approach if we are to produce full national military strength.

One further point: In industry, the research laboratory and the sales department, which of course represents the user, are traditionally on opposite sides in philosophical wrangles. But the gap between them

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Industry is small compared to that which may be expected to exist in the military sphere between developers and users. At one extreme organizational practice we will have developing agencies that produce what they will, and pass it on willy-nilly for use. At the other extreme we have service agencies at the beck and call of the users, without inspiration or initiative. Neither makes sense. In industry the vice-president in charge of production, and the vice-president in charge of sales, report to a president or executive vice-president. If he understands both phases, and skillfully brings harmony and understanding, there is a prosperous company. In a military organization, in times of peace, a primary duty of the general staff, or its equivalent, is similarly to connect developers and users. If the staff officer charged with this duty truly understands both development and use, and can cause harmonious and active interrelations, well and good. The job has sometimes not been done well in our military history. Sometimes it has not been sufficiently grasped for an attempt to be made to do it at all. After all, there are not many officers who genuinely understand both development and use. The job is being done today better than ever before in this country, but, in my opinion, not yet well enough.

Now in all truth, the difficulties which are summarized in these special conditions I have mentioned are formidable. The task of military men has always been one of vast complexity, and military research and development could not be expected to be an exception. In point of fact, war is undoubtedly one of the least efficient of human activities. The victor usually wins by muddling through. Fortunately for him it is not necessary to be good in order to win a battle or a war; it is merely necessary to be better than the enemy. But war is bound to be confused and inefficient. Our task in peace is to substitute for the muddling the greatest amount of foresight, intelligent planning, and efficiency possible. In the sphere of organization for research and development, this demands the clear perception both of the basic philosophy of research in general and of the adaptations and modulations which match it specifically to the particular requirements suggested by the special conditions we have been surveying.

Let me, therefore, turn to a summation of certain things which the organization we consider should be set to accomplish. The elements of a complete program of research and development as generally understood are all present, at least in embryonic form, in the military undertaking. The problem is so to relate them that an integral program is the result. The objective is clear enough--this country should enter any future war with superior material and ample means for producing and improving it.

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Certain functions which must be carried out to achieve the objective are:

1. The projection of weapons systems into the future to the essential development of components for their realization and to predict future technical performance.
2. Analysis of the probable performance in the field of weapons systems thus projected, based on scientific analysis combined with judgment resulting from field experience with earlier systems.
3. Analysis of the status of comparable programs in other countries, especially potential enemies, by means of close relationship between intelligence and research.
4. Examination of the relation of projected systems of weapons to strategic planning, especially in connection with timing, in order to arrive at the relative military value of predicted results, by means of close relationship between the strategic planners and the research and development planners.
5. Evaluation of available resources in men, materials, facilities, and funds, to arrive at plans for the extension of these in essential particulars, and to distribute in the most effective manner what is available.
6. Preparation of master plans of research and development, based on all of these, giving the timetable and technical characteristics of the future weapons systems, as definite objectives or targets, each one of which can be fully substantiated at its assigned priority.
7. Creation, in the light of the master plan, of complete detailed programs of research and development, every item of which is necessary to the attainment of the adopted objectives, and containing every item which is thus needed, with relative priorities assigned.

I need only recite this set of functions, which seem to form a logical basis for over-all planning, in order to convince many of you that we are very far as yet from a completely ordered affair. As our organizational scheme within the Military Establishment evolves, it will be essential that it be made to provide explicitly for logical planning of research programs on this or some sounder basis. It is well that, today, we are in a state of organizational flux.

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I would like to place before you now our present organization in detail, and then to examine carefully whether every necessary function is provided for, whether sound principles apply, just what elements in the organization is expected to perform each necessary step, and how they are interrelated. I can by no means do this in the time allowable. But it should be done. When I started this talk I emphasized that the only sound way to create an organizational structure is to pay attention to its philosophy and principles first, and then to draw it up so that they become embodied in it. Fortunately we have some years of gradual organizational evolution during which such a process can occur, and you gentlemen will have an important part in it. Today I can barely get started on what I think should be done.

To me, it is a matter of never-failing interest and of reassurance that discussions of philosophy evolve almost imperceptibly, but very firmly, into discussions of the relationships between men. It has been so in this discussion, and properly, for the philosophy underlying organization for research and development for military purposes must depend ultimately for effectiveness upon men. This does not mean that sets of principles, or essential elements, are trivial, or lip service. Not a bit of it. For men who really want to achieve through collaboration, there must be clear understanding and agreement about the direction which their efforts shall take; there must be a frame of reference both for guidance and for encouragement in dark days; there must be known centers of responsibility and authority, and clear lines of connection among them; there must be provision for the definitive resolving of differences, for honest men must expect to disagree. We have some of these things, and they are meaningful to the extent that they express a philosophy. The organization is not fixed, not frozen; we are still developing it, for it is a relatively new affair, and the time has not been long. I have enjoyed this effort to analyze it, and look forward to seeing it grow in the time before us.

QUESTION: This question was brought up at one of our seminars at which representatives of your board and the Armed Services board were present, and there was not a definite answer to it. If we should pursue the next war as we did the last one, in which the emphasis was on destroying materials as well as people, the next war may leave us with a world which will be difficult, to say the least, to reconstruct. So the point was brought up that perhaps another form of war, should be prosecuted in which people should be destroyed, but the material things left more or less intact. If such decision were made under another form of war, then that would influence our thinking and work in research and development. My question is, which agency in the United States Government do you believe should be charged with the responsibility of such study and such decision?

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DR. BUSH: Yes. You might just incapacitate people for the proper length of time. There are so many parts to that that I do not think it should reside in any one place. I suspect that if we get into another war with the conditions that obtained the last time, we will probably go about winning it in any way we can, and we will not be too careful whom we knock over in the process.

But there is a possibility along the line mentioned, of course, if biological warfare develops along certain paths. And that takes me back to reemphasize for just a moment the fact that I believe the strongest provision we can make for such an eventuality is to have in this country a broad range of highly effective biological research, and in addition an alert government group of small size thinking of the practical application.

Biological warfare might indeed go along the line that you have indicated. But who should be thinking about that? Obviously from the standpoint of possible development of that form of warfare, the research and development organization is specifically the Research and Development Board, wherever they are located. But it would be utterly absurd to be thinking about that in a vacuum. Unless the Joint Chiefs of Staff and their planning organization are thinking of its possible implications, and unless there is close contact between those two as they plan, they will both be planning in a vacuum.

But the problem you mention goes further than that. We have to think of defense as well as attack, and I believe that the National Security Council is the only place today where the over-all picture from the national standpoint can be approached.

QUESTION: You mentioned a master plan. Would you mind elaborating on that just a little? Also there is one other point I would like to mention. Do you think it is necessary to have a large research and development organization under the Secretary of Defense?

DR. BUSH: The question is whether the Research and Development Board should become an operating board. It should not. The Research and Development Board is a part of the staff of the Secretary of Defense. Broadly conceived, its purpose is coordination, and its work is staff work. The conduct of research and development should be within the line organization. Any time you get a staff in operations you get into trouble, as you well know. I think we should religiously keep the Research and Development Board from operations.

Your other question was about the master plan. What is a master plan? I thought I had defined it. It seems to me that when a specific program comes up, we ought to be able to turn to the master plan of the

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departments of the Government to see whether it fits into the picture.

Let us be very specific. The Research and Development Board has before it a plan for AEDC, the Air Engineering and Development Center. That is a very broad plan for supersonic wind tunnels, and testing facilities, involving the Bureau of Aeronautics, the Air Force, and the National Advisory Committee for Aeronautics. The AEDC would test prototypes. Prototypes in turn are part of the plan for having military aircraft available for production at the proper intervals in the proper types, so far as the need can now be projected into the future, and so far as the technical situation will allow.

Then, if we are going to consider a plan for adding facilities, it seems to me we ought first to sit down and look at the master plan. What is it that we are going to develop? When is it going to be ready? What is it for? What are the objectives? What is its time schedule? Otherwise it seems that you may have one element that makes no sense.

Now, I do not believe that in the Military Establishment at the present time that concept has yet been developed to anywhere near the extent it must be developed if our programs of research and development are to bear the kind of close scrutiny that they are going to get in the days when budgets are being cut down, money is tight, and Congress is critical. I believe when that time comes, the man who goes before an alert committee and says, "We need this. It will cost X million dollars" will be asked: "Does it fit into your master plan as a necessary element?"

Who is the man who can testify to that now? That question must be answered. So far as I am personally concerned I do not know how to answer that specific question in regard to details unless I can look at the whole show. I think the main job we have before us at present is to produce such a master plan.

QUESTION: I believe the whole concept with respect to producing a master plan for research is that it will furnish guidance for the Research and Development Board in coordinating with the other agencies?

DR. BUSH: My concept is this: The Congress of the United States has instructed the Research and Development Board to produce a unitary program of military research and development for the country, which to my mind means a master plan for the country and the Military Establishment as well. It will be utterly impossible to do any such thing unless it is done by unifying the master plans of the three services. Then I think it can be done, if you make it an over-all affair.

QUESTION: How can we at the department level produce a plan other than based on the past unless we get some guidance from the higher level

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as to what the world will look like when the next emergency comes?

DR. BUSH: I hope you get plenty of guidance of that sort. I think that from the technical standpoint such guidance must come from the Research and Development Board and from the strategic standpoint from the Joint Chiefs of Staff.

QUESTION: You stressed the necessity for evaluating a project as it goes through all stages of development. You also mentioned the necessity of having a cadre of officers who could rise a little above their original association and take a bigger view. Third, you wanted to bridge the gap between developer and user. Do you consider that the officers of the combat arms as opposed to the technical services should receive scientific and/or engineering training.

DR. BUSH: I believe that cadre of officers will have to be made up of men both from the technical services and from the field army in order to have a balanced group. But my point of view there is that if we are to have genuine over-all war planning, if we are to have a war plan for this country for a particular set of premises, then that must be produced by very able men, decidedly able men, with all the facts before them, who can divest themselves of preconceived notions when they are about the planning table and approach the program sensibly.

Now, should they be technically concerned? It depends upon where they are going, what their job is going to be. Of course, for a man in the technical services the answer is obvious. I would say that in the future the General Staff officer, the strategic planner, who is completely outside of the scientific and technical picture, who simply takes it second hand, who consults the scientist and the technical man only and does not understand it, will be a fish out of water in the war to come.

QUESTION: Is there an agency in being, or proposed, to look over engineering developments in industry and the Armed Forces as well and coordinate them to fill the gap from a national security point of view?

DR. BUSH: The National Resources Board certainly comes the closest to that under the duties of its job as set down by Congress. This board has not been implemented as yet. They are concerned with national manpower facilities, industrial potential, and so on, from the war planning standpoint. I presume that when they get moving, that will be one of the areas in which they will operate.

You ask also whether there is any agency for over-all coordination of federal research. The Bureau of the Budget exercises a fair amount of coordination. Further, Mr. Steelman recently recommended an inter-departmental Committee to coordinate research; the matter is under consideration.

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QUESTION: You mentioned the need for coordination between the intelligence agency and the research and development agency. Does that mean that the Research and Development Board will have no formal or informal means of getting information on foreign scientific activities?

DR. BUSH: It means that the RDB has the closest connection possible with CIA, formerly CIG. They have had, ever since CIG was formed and the Joint Research and Development Board was formed. There is a very considerable tendency for CIA, when it has scientific or technical problems, to lean on the personnel of the Board for consultation in the technical field. The channel is certainly clear for the same thing to happen in every service at every level. No one can plan a broad program of research and development by ignoring what is being done overseas--for two reasons: one, to be sure to be ahead; the other, to be sure not to be too far ahead.

QUESTION: Is it necessary for you to go through the CIA? Is that your established channel?

DR. BUSH: Oh, no. There are plenty of channels. You realize that every committee of the Research and Development Board is made up of service personnel and particularly qualified civilians in that field. These men generally are very active in research and development planning in their services. They have all the contacts. These contacts include contacts with intelligence groups.

QUESTION: You just made a statement that we had to be sure to be ahead but not too far ahead. In that connection would you care to say anything on the organization of science in Russia, its advantages or disadvantages as compared with ours?

DR. BUSH: It would have to be pretty brief, because I could give you a whole lecture on that subject with no difficulty at all. Russia is exceedingly alert on science. That is not a new thing there. I was in Russia twenty years ago and the interest in science in Russia at that time was really white hat. They were putting all the pressure they could, even then, on finding youngsters of promise and giving them a chance. Any youngster in the Soviet Union who has great skill, genius, and ability in the scientific field is sought out and given all the education he can possibly absorb. They have been doing that for a long time.

We provided for the same thing under the Science Foundation Bill. We have been a little slow on the uptake here. Our facilities are not such that a youngster who happens to have no means but has a good head can move right through to the top of the educational ladder if he is able. There is some opportunity now through the military training programs such as the one administered by the Navy, but these are limited.

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The Russians are alert. They also have good scientists and they have always had them. That is one side of the picture. They have plenty of conviction that therein lies their salvation.

The other side of the picture is this: I, personally, do not believe that in the development of science and its application we can proceed under regimentation at anywhere near the speed that we can without it. The Russian scientist is regimented. The red tape of bureaucracy flows all through their institutions and hampers their efforts. If a scientist has a sound program, but he cannot convince his commissar of it, or if he is unsuccessful in some way, out he goes. So I don't believe that, in competition between regimented science and its application and free science and industry as we know them in this country, we need have any fear unless we lose sight of our goal.

QUESTION: My question concerns science legislation, specifically the bill for the National Foundation of Science and also the Technical Information and Services Act, involving the Department of Commerce. Would you comment briefly on the implications of these bills and the possibility of passage in the near future?

DR. BUSH: I think the foundation bill will come up after January. I rather expect it will be passed this time. The reason that it got into a tangle in the last session was more casual than anything else. It was quite unnecessary, in my opinion. The conference committee threw out a couple of amendments that certainly should have been in there. That they threw them out at the last minute I regard as a specific affront to the President, and he apparently regarded it as such, because he vetoed the bill. But I do not think that Congress and the President are very far apart today, and I believe that that bill will probably be enacted into law before we are through with the next session.

I opposed that Fulbright Bill, and I opposed it rather vigorously, because I thought it involved a good deal of boondoggling. The objective was to aid small business in this country by some sort of paternal scheme by which the Department of Commerce would do a lot of things for it.

Now, I got my industrial experience in small companies. Some of them are still alive. Some of them paid dividends. So I know something about small industry and its vicissitudes in this country. That it needs is not paternal meddling by some group in the Department of Commerce. So far as facilities for information are concerned, we do not need a new bill for that. The Department of Commerce can provide it if it has the money. What small industry in this country needs is a chance. That is all. It will go ahead fast enough if it has a reasonable chance of surviving.

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QUESTION: You pointed out the disadvantages of decentralized control of appropriated funds. Is any organization or group giving any study to preparing a recommendation with respect to more effective budget control of appropriated money?

DR. BUSH: I do not know of any system that, over-all, would be better than the financial budgetary control system that we have in this country. I certainly would not advocate that we go back to some of the old days before the Bureau of the Budget was formed under Mr. Dawos. My criticism is on the details, rather than on an over-all basis. But I believe in such details we have quite a way to go in the manner in which we handle our research and development budget.

We have made a step in the direction you suggest, and I see another step in that direction coming. The directive to the Research and Development Board has not yet been issued; and, until the Secretary gives his approval, I do not think I ought to describe to you how the directive will read. But it seems to me that in one way or another there should be a consideration of the research and development budget as a whole, as a unit, in horizontal cross section, before the Bureau of the Budget considers it. It is an error for that to be done in the Bureau of the Budget. That analysis should be done before they get it.

GENERAL MCKINLEY: Do I understand you, in that regard, to say that, if you should have an integrated program, you would agree to financing it in the various appropriations where it would be carried out?

DR. BUSH: Yes. That goes right along with my previous statement-- that the Research and Development Board should not operate. The preparation of the budget, the support of the budget, and advocacy, should reside altogether in the services. They have the responsibility for doing the job. But because operations are going well at the moment, there is no reason why you should not have staff work going on, bringing these things together that ought to be brought together for consideration as a unit as the plans are made.

QUESTION: Going back to this master plan, do you propose that the national policy would be set up in the Air Force from research to user, for example, from electronic research to user, and also in other major items of national importance?

DR. BUSH: No. You would not want me to try to tell the services how to prepare their master plans, would you? It seems to me that they would differ in all three services. The problems are different, the approach is different. The Air Force has not been on its own way very long. I believe that it has had a program for development of aircraft for specific purposes, for specific timing, in the war plan. But the program has not become focussed and fully defined.

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The job in the Army is vastly different and more complex, because of the varied objectives of the technical services, much of which is supplementary to what the others are doing. It is much harder to do it in the Army alone. The Navy has long had such a plan as a part of its general strategic plan.

What I am advocating is this: That we bring out into the open, into the clear--not to publish them, but to get them on paper, so we can look at them--these master plans which represent the best thinking in the three services as to what we should develop to meet the problems of a possible war. What schedules should we put them on? What things should go ahead of what in order to meet the possible emergency? And how do the various parts of this program fit in and support this picture?

If I was going to build an automobile plant, and I got one group of production engineers and said to them, "Your job is to see that cylinders are cast. Get a foundry going," and to another group, "You make crank shafts" and should just say, "Here is the building, boys. We have so much money unless Congress takes it away," what would you get? You would not get automobiles. There has to be a plan for the whole operation, a very explicit plan, because it depends upon an assembly system, in which the conveyor structure is planned and around which every assembly operation is planned; so that when you get through, things get where they ought to be when they are needed.

Now, if that kind of planning were done by production people, you would be sunk. In any operation involving complex procedures of large magnitude, particularly one involving engineering operations, it is not sound to proceed by simply joining together the particulars without putting together the general purpose first and then looking at it.

QUESTION: If we had three separate plans, would that not tend to delay a mobilization in time of emergency?

DR. BUSH: We are talking about research and development. If we have an emergency, the amount of research and development we do thereafter is likely to be rather small unless the emergency is rather long. No. I do not see how it could delay the mobilization.

QUESTION: Do you contemplate that the Research and Development Board will consider those projects of the Army, Navy, and Air Force which are of sole interest to the Army, Navy, and Air Force?

DR. BUSH: Yes, I do. Read the law. The law does not say that the Research and Development Board creates a national unitary program of research and development from those projects which are of joint interest, only the responsibility of one service.

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QUESTION: I asked that question because of the previous conception that under the Joint Research and Development Board only projects of joint interest would be considered.

DR. BUSH: That is right. The Joint Research and Development Board was created by an agreement between the Secretaries of War and Navy specifically for the purpose of ironing out some differences that had arisen. It has concentrated almost entirely on that job. As a result of its first year's effort we were able to say this year that in the specific areas in which we were working, and not otherwise, duplications had been eliminated except where duplication was definitely planned as a necessary part of the process. Incidentally, the Bureau of the Budget was quite happy to accept that sort of statement; and that question is not up.

Now, the Research and Development Board is established by statute and its functions are much broader. Now we will approach those functions I can tell you better when the directive is issued.

QUESTION: Possibly my second question involves the directive of the Research and Development Board. I was wondering if it is contemplated that the personnel of the Research and Development Board and its panels and committees will be limited to research and development people only or will include operational people also.

DR. BUSH: Does that make sense? One of the members of the Board last year was General Devers. He was a very valuable member of the Board. Unless that element enters, we will not get the broad view.

QUESTION: In your eight-tiered pyramid of essential elements and principles or steps you left out standard end-item production.

DR. BUSH: I left out all production.

QUESTION: I am wondering why you did that and what should be the relation between procurement or production on standard end-items to research and development. Should it be friendship, courtship, or marriage?

DR. BUSH: I left out purposely everything concerned with procurement. That is, I tried to take the thing right through the research and development stage to engineering for production. I considered that a part of procurement. The question of standardization must be thought of as development proceeds or you will find all your conditions frozen when you get to procurement.

I do not know how to answer that question at the moment. We have considered it quite a bit. What is needed, of course, is this: If

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items are under development and are likely to result in acceptance for procurement, then early in the game there should be such an interrelation. But standardization will not be impeded by reason of these things necessarily having gone on diverse paths. I think perhaps we can do something on that during this coming year. We did not get far on it this past one.

GENERAL MCKINLEY: Dr. Bush, I want to thank you very much for giving so generously of your time in this stimulating talk.

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