RESEARCH AND DEVELOPMENT IN INDUSTRY

21 January 1958

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Dr. Augustus Braun Kinzel, Vice President, Research, Union Carbide Corporation, was born on 26 July 1900, in New York City, New York. He received a Bachelor of Arts degree from Columbia University in 1919. In 1921 the Massachusetts Institute of Technology awarded him a Bachelor of Science degree. He obtained a Doctor of Metallurgy from the University of Nancy (France) in 1921 and this was followed by the Doctor of Science. He was employed by the General Electric Company and the Henry Disston Company during the years 1919-1920, 1922-1923 and 1923-1926, respectively. Since 1926 he has been associated with the Union Carbide Corporation in various technical and executive capacities. He has been and is a consultant for many Government agencies, such as the Atomic Energy Commission, and is presently a member of the Defense Science Board and Naval Research Advisory Committee. He is president elect of the American Institute of Mining, Metalurgical, and Petroleum Engineers, Inc. He has written many articles of technical interest in the metallurgic and engineering fields and he is the author of Alloys of Iron and Chromium, volumes 1 and 2, published in 1937 and 1940. He holds many patents in the fields of metallurgy and engineering. This is his first lecture at the College.

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CAPTAIN STEIGELMAN: Admiral Clark, Students, Faculty, and Guests: Since the Materiel Management Course began a few weeks ago, you have heard various members of industry talk on certain functions, such as production, profit, price, and so forth.

Today we are to hear about "Research and Development In Industry." Our speaker is Dr. A. B. Kinzel. He is Vice President, Research, of Union Carbide Corporation. You have all read his biography and realize that we are indeed fortunate to have a man so eminently qualified in his field to talk to us this morning.

Dr. Kinzel, it is a pleasure for me to introduce you to this class. Gentlemen, Dr. Kinzel.

DR. KINZEL: Thank you Captain. The request to give this talk carried with it a list of items to be covered. The first of these, very properly, is motivation. Motivation is extremely simple in industry. It is to make money. The big problem is, How do you handle your research in such a way that you get the greatest return for the dollar spent? This leads directly into the question of organization, administration, and the like.

We can talk about ways of setting up laboratories. We can talk about ways of administering them. This is always important, and is a factor in the total picture. But there are two items that are absolutely paramount when it comes to research and development, and I am not sure but that this is true of every other aspect of industry or the military. These are programs and people. I don't care how much money you have. I don't care how you are organized. If you don't have a program that meets the purpose, and if you don't have people to implement this program--and by people I don't mean just people, I mean the right kind of people, the uncommon men--you are not going to have successful research, whether it be industrial or any other kind.

Keeping these two things always in mind, let's take a look at how we go about this sort of thing. In industry you will find that there are two general approaches to research and development: the centralized

and the decentralized. Each of them has advantages. General Electric and Bell Telephone are illustrations of centralized research. Carbide is an illustration of decentralized research.

The advantages of centralized research are that you have a series of batches of talent geographically concentrated, generally under one roof or very close together, so that whatever the problem may be it is a relatively simple matter to call on the expert who can help you. This is the way it is spelled out in theory.

The decentralized laboratory has the great advantage that the leader of a given laboratory knows his people intimately. You may say that in the centralized laboratory you can have a group leader, or a section leader, who also knows his people intimately. But the big difference is that the section leader is not autonomous. Because of the geography, because of the location, there is somebody over him. He doesn't make decisions without conferring on up, and up, and up. So you get this echelon of conferences and so on. The laboratory leader, let's say a man who has 125 technical people under his jurisdiction, can know each of these 125 people well. Our experience is that this is just about the limit--125 to 150 people. The leader knows what these people are doing, so that when somebody's work falls off he knows it is because the man is having trouble with his wife, or has a bad heart, or something else. By having laboratories decentralized, each director of research does have autonomy and exercises it. This is a great advantage.

Now, you say there is a disadvantage in not having the experts where you want them when you want them. If you have a group as large as 125--or I'd say anything over about 80--you can have a broad range of science well covered. You may not have the detailed expert in any one given instance, but you will have your broad fields pretty well covered, and the problem of communication is one of which you are conscious. It is our own belief that, in a decentralized setup, where you are conscious of the problem of communication among your laboratories, you get at least as good communication as you do when you have them in the same building, but where John Jones has never heard of Jim Smith on the second floor, and it is just taken for granted that he has.

So here we have the difference between theory and practice in the way things work. The answer, of course, is that there is no clean-cut argument for or against either of these methods, and that in each case what you do is try to have your cake and eat it, too. You correct what you know to be the shortcomings of the system you use.

Once you are set up either way, the first question is programing. In a brand new laboratory, this does present a problem. It requires a lot of thought and a lot of coordination in the working out of an initial program. But it isn't very often that you have new laboratories. By and large, when you do there is a specific reason for it; there is a specific series of problems; and you can arrive at a program very quickly. But, in a going laboratory, the program problem is a little different. Here we have not only the problem of what we shall put into the program but the all-important problems of what we shall take out of the program and on what we shall stop putting effort. I still have to meet a scientist who is willing to agree that the particular project on which he is working is not about to be successful the day after tomorrow. Of course, with this attitude it would be perfectly silly--I mean if it were so--ever to stop any research program.

So, what do you do? You realize that in any industrial research effort, coming back to motivation--to make money--you have a series of types of projects. Much of the work has to be what we now call learning work. This is a little better term, we think, than the old terms, basic or fundamental. It is work in which you have selected a field, a scientific area, and you have said to yourselves, "Now, we are putting work in this area into our program because we believe that in this area we will find things which can be put to industrial use by our production and other people." You take the area, then. For example, amino acids might be a limited area; polyurethanes for fuel--solid fuel--might be a limited area. We may start out and say, "All right; now, we don't have any bright idea at this minute, but we do know that the general subject of polyurethanes is very important to our corporation, so we are going to learn everything we can about it." We start research--learning research--in this field. Sometimes we will start it in a broader field. The general field of bonding, for example, is very important to anyone working with basic materials--to know what makes the atoms of materials hang together. This is all involved with strength, ductility, conductivity, and the other properties that are of interest. So we might set up a learning research on the nature of bonding.

You may say, "How are all these things going to bring any money into the corporation?" Well, we have learned by experience--and I speak not only for Carbide but for industrial research in general--that it is important to have a certain percentage of our research in this learning area. It will vary in different outfits, but a good round figure is somewhere around 30 to 40 percent. From the learning research come clues and leads to applications.

Now, you've got a group learning, finding out everything they can about, let's say, polyurethanes. What happens? They note, for example, that, if the foaming is suppressed in a certain way, or accelerated in a certain way, the cellular structure and the lining up of the long polymers are altered. One of the group who observes this says, "Ah! If we could do this regularly, this would make a wonderful pincushion." It may be that we are interested in pincushions. It may be that we are not. If we are not interested in pincushions, we say, "That's very nice, it would make a wonderful pincushion, but let's not worry about that. Suppose, instead of its being a wonderful pincushion, it were a wonderful material with respect to its ability to retain its shape at high temperatures under compression, by virtue of gas expansion in the holes and the deformation that you get." We say, "Ah, this is what we are looking for in a solid fuel."

Then what happens? We take the particular individual who thought of this--if he's the right type--because nobody will push a project like the man who thinks it is his baby. If he isn't capable of it, and doesn't have enough practical sense, we will take another group and say, "Those fellows over there are going to keep on learning more and more about polyurethanes, but over here we want you to start out specifically to find out everything that has to be found out in order to make a practical solid fuel out of this material."

This is applied research. When we do this, we are embarking on a research venture that, broadly, takes 10 times as much money per project as the learning end of it did. In the learning work, we can be very loose in controls. We can give the researchers maximum freedom. In fact, at Carbide we try to give them complete freedom. The big problem is that too often they don't take it. You hear a lot of talk among young people these days about the freedom of the researcher in industry. When you recruit these youngsters, the first thing they always ask is, "Are we free to work on anything we want to work on?" We tell them, "Yes, We only hope that you will think of something that you want to work on."

Now, in this learning research, let them go. This is a general philosophy. This isn't unique to Carbide, although it is unique to, shall I say, the bigger and older research establishments. It is something that isn't understood as well as it might be by some of the newer ones.

At any rate, when you make the decision that you are going to take a project and put it into your program in the applied research area,

you are making the first major decision with respect to what you are going to get for your money on research. Now you are going to begin to spend relatively large sums--I am still not over in development, mind you--on the applied research. True, this is still work that goes on at a laboratory bench or in a small furnace, or something that could be encompassed on this platform. You proceed then to make this decision consciously.

A lot of research labs have gotten into trouble, and a lot of Government research is in trouble, because at this junction they didn't stop and analyze the problem. For example, let's stick to polyurethane a minute, and suppose, at this stage, we've got to make up our minds whether we are going to go into applied research to get a solid fuel. Then we say, "All right. What does it mean if we do? Suppose we are successful?" That's the first question we ask. What will happen? Well, we put up a plant. If we put up a plant, there is no reason to believe that the capital cost of this plant should be extraordinary. It should be in the general range of $1.25 per dollar of annual output. That's all right. There will be danger, hazard, and the like. Yes, there are going to be problems. We think we will be able to work them out. This is a part of what the research is going to do--work this out for us.

Then we take a look at the economies. We find out that everything is fine; it works out exactly as we have planned it. We will have a beautiful product that will cost us about $200 a pound. At this stage we are faced with a decision. Is this product useful at $200 a pound? Is it sufficiently useful that we ought to go into the business, and put up a plant, and so on, assuming that the research is successful? Or, had we better say, "Well, now, this is wonderful. It is very interesting, but we are not going to spend the money on making an applied research project out of this at the present time. We are going to keep this project in the learning area to see if we can't, in that area, come up with something that will make this material for $2 a pound, instead of $200." This is the decision we have to make at that point. And this is a decision that is so often not made at that point. However, there is nothing to stop up from reviewing and reviewing. We should and must.

Let us assume, for example, that everything worked out the way I have just said, except that, instead of $200 a pound, the product cost $5 a pound. All right take $5 a pound. We know that, by applying technology and by modifying processes slightly, we can cut cost in

half and maybe again in half. This brings it down to the realm where, let's say, it is useful for the price. So we go ahead. We do the bench-scale applied research. As it goes along, we keep on reviewing, to see whether new troubles have reared their ugly heads and are vitiating some of the assumptions that were made at the time we came to a decision. If they do, that doesn't mean that we drop the project, but it does mean that we concentrate on clearing up that particular aspect of it before we spend too much money on the rest.

This is all programing, gentlemen. It comes back to what I said--that the most important thing in research is your program. This programing must be done essentially by the man on the job, the group leader or the head of the local section of the laboratory that is running about 100-odd people. It can be done in conference as you get into bigger projects and go further, and it can be done in conferences with vice presidents and all the rest. But this programing must be done.

Now, you get through the applied research, and we assume you are successful. Then you come into the next stage, which is development. Where are you going to put up a pilot plant--not something that will go on this platform, but something that will fill this whole room? Again, you are multiplying your expenditure by 10 per project. When you do this, you must again make a detailed analysis of the same kind you made before. This time, however, it can be a lot better and sounder because you have learned so much and you know so much more about the project by virture of your applied research.

Again there is the temptation to go ahead and forget the differentiation between a research success and a potential economic success. A research success is where all the technology looks good. An economic success requires that not only all the technology look good but that the economics and its various aspects in industry--marketing, application, use, and production--look good as well.

Here is the place where you have to stop and redo your analysis once more to be sure that you are still on firm ground. The temptation to go ahead with anything that can be labeled a research success is extremely great. The goals, of course, are great. In general, at Carbide--and I think this holds pretty well for most of the larger industrial outfits that have had research for some time--we find that in any given year the new sales from products of research are, roughly, five times the

research and development charges of seven years before. That is when these products reach a point where they begin to be interesting. It takes about seven years. We have an old saying, "Seven years from test tube to tank car."

This isn't the end, of course, because in the next year and the following year the sales mount still further, and finally you get into an obsolescence period. It is not unusual in companies with sound research programs to see the kind of a statement we had in the 1956 Annual Report of Carbide, that 40 percent of our sales and 60 percent of our profits came from products and processes unknown 15 years before.

So much for the programing and the importance thereof. The way it is actually done, of course, is that each year, generally in October, each group in the laboratory sets forth what it thinks it would like to do in the subsequent year. This is reviewed by the people in the local laboratory, and their proposals are all put together with everyone else's and finally reviewed at headquarters.

There is another great temptation that we have to look out for in industrial research. It applies, I think, even more in the military. You have a limited number of people; you don't have an unlimited number of dollars. I think that is true even in the military. So you can't do everything. Now you come along with your learning research and your applied research. Remember I said that your applied research takes about 10 times as many dollars per project as your learning, and your development takes about 10 times as much per project as your applied. You have a limited number of dollars and you see these things which ought to be brought along to ultimate use. So you take Project A, polyethylene; Project B, the amino acids; Project C, blood plasma; Project D, high temperature metals--all of which have been successful in research--and you put them all in and say, "We are going to go right ahead with these now."

Then you find you haven't enough people and money to do them all. So there is the temptation to say, "Well, we have those fellows over there that are learning, and, after all, what they are doing isn't immediately applicable; and here we have something to apply right away." The temptation to cut back on the learning and put more money into the applied is extremely great. In any given year you will probably make a little more money for your corporation by doing this, but the year after that you probably won't; and three or four years after that you'll be sunk because you have shut off the fountain.

When you realize that you can do 10 times as much in coverage in this learning area for the same dollar, and that this will open up new fountains and new streams, you see how foolish and shortsighted it is to do this. This you have to fight all the time; and the strange thing is that your own researchers won't help you. Jim Jones has a research success in polyurethanes and Bill Smith has one in blood plasma, and each is pushing to get his stuff out. Each is willing to stop the learning and go ahead and get this project rolling.

The director of the lab is the first echelon high enough to appreciate the full significance of all this, by and large, and he will fight this tendency. He has to fight his own people. In Carbide, I spend a good deal of my time being sure that this balance of learning research and applied research and development is maintained. Of course, then you get into definitions and you find such things as "bootlegging." You'll have "bootlegged" applied research under the guise of learning, and so on. You gentlemen in the military I am sure are familiar with the process.

Now, what do you do about it? Well, we have a device at Carbide whereby we break our program into parts. I don't know that it is used in quite this way in any other organizations, although I understand two or three have started it, too. As I said, we are divisionalized and we are decentralized. Each of our divisions is responsible for making money. Each of our divisions has a vice president for research. He gets his research money on the basis of the program presented, and that is paid for by the earnings of the division in which he is operating, his division--let's say Electromet, Chemicals, Bakelite, Linde, or whichever it may be. We have 11 of these divisions that are actually doing research. The research vice president gets most of his money from his division president for this research. We have a research committee responsible for overall coordination. The members of this committee look over the programs to see that there is a reasonable balance among learning research, applied research, and development.

But this is not enough, because of the various factors that I have already mentioned. So, in addition to this, we have a block of corporation money. We call it a corporation research budget. This money is distributed out of the central office to these laboratories for learning research only on projects that are approved in the central office. The division doesn't pay for them. In this way we manage to put some greater emphasis and greater control on the learning research and to keep it in balance.

Actually we are going a little further than that at Carbide now. We find that, even with the procedure we use, the tendency of the individual researcher--and, mind you, the researcher is always criticizing management for wanting to go into applied research and not doing enough basic research--is to go ahead. He's the fellow that we have to watch, not only in industrial research but in research of any kind.

While the device I have just described works pretty well, it works less well in time. So in addition to all this, now, we are setting up a relatively small, again decentralized, laboratory where rule number one is that nothing shall be done in this laboratory but learning research. If something should turn up in this learning research that would be useful, we propose that it immediately be moved over to one of our division laboratories. Or, if it is of such a nature that our division laboratories are not interested, we will put up another laboratory and work it out. We are not going to have any applied research at all in this one. This will supplement all the others.

I think it can be worked out. I am sure that many of you, as I talk, are thinking of some of the recent doings and conversations on research in Washington, on special agencies to do research, and so forth. While on paper this kind of thing may look clumsy in some ways, it will work if properly handled. The difference between what things should be as they are presented on paper and the way they are when handled by human beings can be great.

So this is the organizational administrative setup that we use at Carbide. All of industry uses, in one form or another, some device of this kind to maintain the situation and to keep this learning balance where it should be.

The next item that I would like to talk about is people. I think that the average research fellow--and I can speak freely because I came up from the research bench myself, and spent lots of years actually experimenting with my own hands trying to use my head at the same time--is not even second to an opera star when it comes to being a prima donna. He is dedicated. If he is any good, he has to be because he can't do this kind of job on a 5-day, 8-hour-a-day week. He has to live it, eat it, and sleep it, so that it is in his subconscious. Otherwise he will not be creative, and creativity is the essence of it all. Skilled people? Yes, you can get them. Schools are turning them out. There is a lot of hullabaloo about their not turning out enough. That's probably

true, and far be it from me to belittle this effort. I think it is wonderful that we have this effort. But the real problem is to find the fellow with a spark, to find the one with creativity. Once you have found him, nurture him.

Many laboratories today--and this is true throughout the Carbide system--have two ladders of advancement. You can go up the administrative route and become a group leader, a section leader, a branch leader, a division leader, a director of a laboratory, or a vice president. This is the organization route. But along with it we have the so-called professional ladder. Here a man can reach a high status, without ever having anybody work for him. Generally, of course, he likes to have one assistant to turn the stopcock or to hold his watch. In the past, the professional scientist, the dedicated scientist, who wants to and should devote himself strictly to science and technology as distinct from administration, had no place to go. Today we have these ladders of advancement whereby you start out as an assistant metallurgist, for example, then you are a metallurgist, and then you are a research metallurgist--and each time you get a raise in pay. Then you are a junior research metallurgist and then you are a senior research metallurgist, a junior consultant and a senior consultant, a general consultant, and finally an associate director of the laboratory. We actually have this case in point in several of our laboratories. We have associate directors who have no administrative duties whatever. I can say this truthfully, and I emphasize the fact because it seems so strange to a lot of people. In our laboratories the top scientist, who has no administrative duties, can be paid more than the director of the laboratory. We have no trouble with it at all. The director of the laboratory understands it.

I could go on and talk about a lot of other facets having to do with industrial research. Patents, I think, was one of the things on the list that I was supposed to cover. Sure; the patent system of the United States is essential to the research system, because you are always hoping, and management is hoping, that you will be able to get a 17-year monopoly without violating the antitrust laws. The patent system provides this. In order to fully take advantage of it, we find that we would have to have at least one man for every 100 scientists or technical people in a laboratory do nothing but follow patent matters. You can't count on the scientist and the researcher himself to write out a patent memo. He's too busy on his job; he's too busy working, thinking about what he is trying to do. Patents and all the rest of it are on the side. What we do is to have patent liaison man, at least

one in each of our laboratories, sometimes two, depending on the size of the lab, and so on. He keeps in touch with what is going on. If he sees something patentable, he goes around and sees Jim Jones, and says, "Look, Jim, this looks patentable. Please let us get it down on paper and send it into the patent office in New York." This works. It is the only thing we have found that does work. The men themselves let it go too long.

Occasionally, of course, you have what we call a patent hound. That will be some fellow who hasn't been around long enough to find out quite what it's all about. He's writing a patent memo every week about trivia. But this corrects itself very quickly.

So far as communication and dissemination of information, and the like, are concerned, again, one of the things that every youngster asks when he comes for a job--the Ph.D. fellows in particular, and today you pretty nearly need Ph.D.'s to do the research, because of the complexity of the problem--is, "Do I have freedom to publish?" We say, "Yes, you do, within these limits. There are certain know-how areas where you may publish only after the process or the article in question has been in use for a couple of years. There are certain patent areas where you may publish only when the patent has been issued. With these two limitations and they are not serious limitations, gentlemen, I can assure you we want you to publish." All right, what happens? Generally in the first year or two that the fellow is with us, he will write a paper. He will be very proud of it. But try to get him to write another one. It's a time-consuming task; it must be very carefully done and very carefully thought out, because any paper published by an industrial organization must be reviewed. At Carbide a paper must have a very intensive review by both scientists and engineers. When you go into print with something over the Carbide name--or General Electric or Bell or anybody else--you've got to maintain the reputation that has been developed. So every sentence gets a going over. It is a job; there's no question about it. The net result is that the men lose their ambition to write papers. We'd like to get more papers written. We are always after our people to write them.

On the other hand, they will go to a symposium, such as the Gordon Conference, meetings here in Washington, technical society meetings, and so on. They will sit around and chin, and talk about these things to their confreres in other industries. They will learn a lot themselves, too. We send our people to meetings. The only reservation we have on our people when they go to meetings is that we want at least one man back

in the laboratory at any given moment. There are so many meetings that they could be out all the time. This has to be handled with judgment.

As to what all this means to the national welfare, it is true that in industrial research the motivation is to make money for the company; but, by the very nature of things, serendipity prevails. I like to use that word because it's one of the big ones that can be easily explained. It means a lucky accident. In other words, the interlock of desirable properties, desirable performance, and the like, that which we are looking for in industry, and that which you are looking for in the military, is so close that pretty nearly everything that we find in industry can be applied to the military or will have an impact on what finally happens in the military.

I see that I have talked just about the length of time I was requested to talk. As you can gather, in a field of this kind I could go on for quite a while. I am not quite sure whether I covered all the aspects that were given to me in this very beautiful statement that I received from your officers here. It was a good statement. I didn't have to stop and think of what I was going to talk about. I had it made for me. If I have skipped anything I think we can take care of it in the discussion period. I shall be very happy to try to answer your questions.

CAPTAIN STEIGELMAN: Gentlemen, Dr. Kinzel is ready for your questions.

QUESTION: Doctor, when you come across an item which may be of benefit to the public welfare but is beyond the capabilities of Union Carbide, or which is of defense interest but not within your developmental point of view from a profit point of view, to whom do you turn it over? Or do you turn it over? To the National Science Foundation, or the military R&D people? Or what would you do with a project of that kind, sir?

DR. KINZEL: We have had several such projects and have turned them over either to the Office of Naval Research or to Army Ordnance. Those are the ones I can think of. There was one given to the Western Air Command. Also I think we turned one over to Wright Field. We are in relatively close touch with the military people, so it does not present a problem to us.

Incidentally, before you ask another question, I would like to make a comment. I made one statement in my talk that one of the gentlemen

asked me to clarify for him, and so I want to clarify it for all of you. I said that our learning research was about 40 percent of the research we do. This is 40 percent of research only and not of research plus development. The relation of research and development charges will vary greatly in various companies. But broadly it is in the order of magnitude of two to one, total.

QUESTION: Doctor Kinzel, I was interested in your two different routes of advancement. One was the researcher or scientist professional route, and the other was the administrative route. In the Government the supervisor of a laboratory under Civil Service is expected to go the route that seems to be the administrative route of empire building. I was wondering where we fall down on this business of supervisors under Civil Service. At what point in the career of your learning researcher do you determine whether he is actually studying research or whether he is thinking about a vacation at the Thousand Islands? How do you finally start him over the professional route?

DR. KINZEL: That's a very good question. In answering it, I hope I don't offend anybody. I think one of the advantages that industry has over Government is that we make our own rules; therefore we can break them, or change them. We select our own people and have confidence in them and rely on personal judgment rather than on some numerical evaluation of some sort. We can do this to a much greater degree than you can in the Government. Actually, going up the professional ladder is strictly a matter of what the man's colleagues think of him, as interpreted by the director of that laboratory. There's no number system; there's no evaluation system that has been devised, to the best of my knowledge. So, with your Civil Service, where you have a greater need to do things by number, as it were, you have a different problem and a more difficult one.

QUESTION: Sir, with 11 separate divisions conducting learning research, and then with this top company learning research pool making 12, what kind of coordination do you use to be sure there is no duplication? Our three services are conducting research in missiles, and we are in for a lot of criticism, as you know. Would the same type of criticism apply to your 12 units conducting learning research?

DR. KINZEL: I think that first I want to emphasize that you have directed this question to learning research. Now, a duplication in learning research is not expensive, and I have yet to see any duplication, because no two men will go about learning the same thing in the same way.

In the learning area, therefore, the danger of duplication is really very small. Actually, we do have a laboratory symposia, and inter-laboratory committees that are composed of groups of a half-dozen fellows working in the same general field. They visit each other regularly and we maintain communication this way. These boys don't want to duplicate, anyway. There's no point to it. So in the learning area there is not too much of a problem of duplication.

In the applied area we begin to get into a real problem. This is costing 10 times as much as any given project in learning. So we want to be sure not to duplicate. There is no great advantage in knowing how to make the same thing in two different ways, unless the last one is very much better than the first.

In our applications research, suppose we have a program set up that we can put our teeth in. But suppose there are two routes that we can take. We look at one way and evaluate the possible results; then we look at the other and evaluate the possible results. Then we say, "All right; we are going to go down this route. You, Jim Jones, over there, you cancel out." In development, of course, it becomes extremely easy, because there we look at the project sizewise. As I said, we take a very good, direct look at it. But there has been a lot of talk about duplication in learning research. This doesn't bother me.

QUESTION: Sir, as you spoke you said there is very little danger of duplication in learning research. You made a statement before, and I couldn't decide which side of the fence you were on.

DR. KINZEL: Good, I didn't know I was that clever.

STUDENT: I felt that you had an inclination to say something that you didn't want to say. I would rather have the criticism on our research in the military departments come from a professional here than have it politically inspired; that is, criticism on what we should do to change it. What is your opinion of our administration of both our research efforts and the performance of our laboratories in the military? Since Union Carbide does not have a centralized system, don't you think there is great danger in the loss of this personal-touch business in our whole research program if we go to a central committee here? Because we are going to have 117 intermediate bosses. Can you open up your heart and let us know professionally what you think of it, sir?

DR. KINZEL: Professionally I don't think it makes too much difference if you have 117 intermediate bosses or 98, which is more or less what you have now. So from that angle I don't get particularly excited. My own view is--and again, let's distinguish between the learning and the application research--that in the learning area it is perfectly proper to have the Army, Navy, and Air Force spend reasonably; and even though you triple the sums or multiply them by five, they are still very moderate for learning research.

I think that is in order to have other organizations in the Government devote effort to learning. I do think that it would be helpful if you could get a little better coordination among these groups, particularly from the standpoint of being sure that what is learned over here is known over there. This is a question of communication. So far as the learning phase of this is concerned, I am not alarmed one way or another. I can see quite a lot of good in having more laboratories doing learning research, even though some of these may be in a fourth agency. I think that could be good, rather than bad. I think in balance it is on the good side.

But the minute you jump from the learning to the applied research, here is what is bound to happen unless it is handled with a very strong hand and unless you have someone in a position of authority who is both strong and knowing. You go from learning to application and on to your development, and this is really where the criticism has been of the three services in your failure to cooperate, if the criticism is valid. I think it has been caused by the fact that you have gone into development areas without sufficient conference as to the merits of any one of your findings, and there you do hit duplication--not exact duplication, but close enough so that there is warranty in the criticism that you duplicate.

So, keep the original differential in mind--that is, in respect to learning, applied research, development--and then also bear in mind that in learning research you should not worry much about duplication; in applied you should watch it very closely; and in development you just can't afford it, for there is just not enough money; and even if there were it would not be wisely spent.

If another agency is set up that is finally going to wind up doing development work without sufficient coordination with the three that now exist--and I agree, although most of you may not, that there is insufficient coordination among the three that now exist--then the situation will be very bad. It will be much worse than it is now.

QUESTION: Dr. Kinzel, in connection with projects which are sponsored by the Government, one of the principal difficulties apparently has been the pressure to move this project, not from basic research to applied but almost from basic research on into production, if you will. Would you care to comment on your experience in this matter, and on what pressures might exist in your company?

DR. KINZEL: Well, the enthusiast for a process always wants to do just that. I am reminded of the reactor situation, the atomic reactor situation. The very best reactors, without question, are those that we know the least about. This is axiomatic. So, when you are in the early phase of the applied stage, or even through it, the tendency and the wish to jump development and get right into production are very great and the pressure is great.

The way you answer this sort of thing is to ask, first, What is the need for the urgency? In business the need is to make money sooner, and in the military it is to keep ahead of the enemy. So you have to take a look at that. The next thing you look at, and this is something that I think is done quite well in industry and I suspect--in fact, I have more than a suspicion--that it has not been done very well in the military, is what elements are involved. If you have a chemical process on a bench--applied research--in which the various steps are a standard filtration, a standard distillation, and a standard reaction at usual temperatures--all of which can be done in unit equipment of a kind that we are running all the time--sure, you can jump the development stage. In so doing, you will run into some bugs and it will be expensive to remove them. Nevertheless, in the end you will probably be ahead of the game by having done so. But, suppose in this complex there is a pressure precipitation at, let's say, 100 degrees higher than what is normally operated. Also, there is a filtration with a slime, or something that might slime (you are not quite sure whether it will or not but, if you are not sure, you can be sure it will) and you don't know whether you can handle it. There also is a timing involved between the functioning of this unit and that unit. Then you can look at the project and say, "Now, the chances of bugs developing, which we are going to have to work out in the new product development, are so great that it would be absolutely silly to skip the development stage because of the terrific amounts of money involved; and in the end we would lose time as well." This is the way you have to make these decisions.

QUESTION: Sir, you mentioned a while ago the pressure that is going on today for some sort of Federal or national organization for research either in the Defense Department or in some other department. I think that most of us agree, in regard to the dollars spent, that probably more would come out of the same amounts of money spent by industry for their research. Has any thought or any discussion been given to some sort of rebate or tax benefit for that part of research which is pure, basic, and not development, and not of any primary concern to any one company, but which would be of national interest and could be financed by a rebate on income taxes?

DR. KINZEL: That has been mentioned. I have heard it before. I don't know that anybody is doing anything about it or taking it very seriously. But there is another problem in industry research for the Government. When we do it, we are paid for it under the rules set down by the Comptroller, whereby we have certain items allowed as overhead. But remember that what we are shooting for in industry is sales. As I said, seven years later we expect sales five times the total research and development cost. We don't just want to get our money back. When we take a Government contract, we take the capital investment and the people investment that we have in the project and simply turn it over without net profit. So it is a sacrifice, by and large. This has never been fully appreciated. So some sort of extra benefit would be in order, whether by tax rebate or something else.

STUDENT: I didn't mean on a contract basis--I mean just like any other tax reduction.

DR. KINZEL: Yes, but whether we do it in the public interest or on contract, that has been mentioned. But, as I said, I don't know that anybody is doing anything about it.

QUESTION: Sir, in the area of exchange of information, how much do your people review foreign publications--French, Russian, German? How useful do you find the information obtained in foreign scientific publications?

DR. KINZEL: That's a toughie. Today, of course, there is so much being written that it is a very difficult problem to keep up with the literature. We are using all sorts of devices in the way of special library service, abstract service, and what not. In the last analysis, the individual researcher has to take what aids he can get and use them. Actually, what happens is that if an article has any importance in any

given area, within three months of foreign publication, the man who is working on a project in that area will spot the article and get it. This works out pretty well. I don't know of any case where we have missed a foreign aritcle three months after it was available in this country. But we are all scared that we are going to miss them.

QUESTION: Sir, suppose one of your researchers requires an expensive piece of apparatus or environmental equipment. What is the decision process before this is approved, and where does the money come from?

DR. KINZEL: The decision process is pretty simple. The vice president in charge of that laboratory and the division will decide either that he would like to have this equipment for his laboratory or that he wouldn't. Then, the decision having been made that this is a proper request, the money comes out of a capital budget which is set up annually and which has a contingency fund in it, as well as money for things that can be foreseen. For example, they wanted a Van de Graaff accelerator a few years ago at our plastics division. At that time it sounded like an odd thing for a plastics division to have, but we had no trouble whatsoever in getting approval for purchase of the Van de Graaff. We were all agreed that we ought to have it. This comes out of the capital budget tied in with research. Of course, it comes out of the earnings of the corporation.

QUESTION: What do you find are the most productive years for the scientists who are working for you, and what do you do with a man who doesn't produce? That is, maybe he just hasn't had a breakthrough, and it might take 20 or 25 years before he comes through with something. What do you do in a case like that?

DR. KINZEL: This, again, is a matter of personal judgment on the individual. If he has not had a breakthrough but his work has been such that we think it is as good as some of the good boys could have done, we will just let him keep on going. If, on the other hand, we feel that he has lost his punch and is not a researcher any more, that he has lost his curiosity and has settled down, and that he is more interested in television than in reading a magazine in his own area when he goes home, we transfer him. We are not slave drivers; we are willing for him to look at television, but we want him to be more interested in things concerned with his work. When he gets to that stage--and it frequently happens--we transfer him--perhaps to engineering. That is not funny. It doesn't mean that he is less useful. It means

that his work will be more tangible and laid out for him. He is not quite the master of his own time to the same degree that he was in research. Some men will go into sales. We place them wherever their aptitudes are. We do quite a lot of transferring from research to other operations of the corporation.

QUESTION: Sir, I understand that your company is doing much work in the field of atomic research. Do you feel that our present security laws unnecessarily impede your progress? If you do, how would you change them?

DR. KINZEL: Let me answer you a little indirectly on that one. I don't think that the present security laws are impeding our progress particularly. There is no trouble in getting our people cleared. We operate at Oak Ridge. I am presently chief consultant in metallurgy at Los Alamos. So there is no great problem so far as the immediate and specific points are concerned. But I do believe that we could do with a great deal of declassification in the basic and learning areas in the atomic field. How much further we would go if a great deal of this information were more readily available is a moot question; but I think we would go further.

QUESTION: In the area of people, sir, how do you handle the problem that is posed when you, or management, in its widsom, decide that a particular applied research project is not worthy of being carried on further and the dedicated personalities are convinced that the day after tomorrow they can come up with something.

DR. KINZEL: We just get hold of them and explain the story to them, and say that this is it. At the same time, we are very careful to have another project. Then we show them what a terrifically important project this other one is and tell them they are just the people for it.

QUESTION: Sir, we have heard from previous speakers that most of the outstanding basic research people have come from Europe. I am wondering what your observations are concerning this, and your feelings concerning the stimulation of curiosity in graduate studies that come to young Ph.D.'s via our educational system.

DR. KINZEL: On your statement that most of the people come from Europe, I am not sure that it is true. Certainly, some of those that have made the headlines have come from Europe, and it undoubtedly was true in the period between the two wars or even earlier. It was certainly true earlier.

Remember, at the time I got out of school (Columbia 1919) if you wanted to do graduate work in many fields you practically had to go to Europe, and you acquired a reputation if you did go. I went to Europe. A lot of us did. Today the reverse has happened. Europeans are coming over here. We are in a state of flux. So far as inventions and creativity and so on are concerned, I think our boys are right up there with them. As to what the universities are doing to encourge this and bring it out, I think that we have creative fellows in spite of the training that they are getting. I am talking about the average school now. There are exceptions, certainly. But I think the average school is apt to put a Ph.D. on a research project that is the pet of some professor, and he just grinds it out. When he comes out the influence has not been such as to make him more curious or to stimulate him particularly. Depending on the man, he can correct that after he comes out, or he may not just be that kind of fellow.

QUESTION: Dr. Kinzel, how does creativity in the scientific area vary with age, if it does? Can you generalize on that?

DR. KINZEL: Yes. But again let me say that there are many exceptions. It is an interesting thing that most of the really great concepts have been generated by people under 30. This goes back in history. This does not mean, however, that people stop generating after 30. Some of them have generated later. James Clerk Maxwell kept on generating first-class concepts until he died. So did Einstein. They were great when they were young, too, you see. In general, though, in research, on the scientific ladder that we were talking about, we find that there are a lot of people who remain highly creative (maybe not quite so creative as when they were younger) right up to the time when we put them on consultantship and they finally pass away.

CAPTAIN STEIGELMAN: Doctor, I wish we had more time so that you would answer more questions, but the time has run out.

DR. KINZEL: The spot was getting hotter.

CAPTAIN STEIGELMAN: On behalf of the students and the Commandant I want to thank you for a most stimulating talk and for your frankness in answering questions. Thank you very much.

(15 May 1958--4, 100)O/msr: sgh