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RESEARCH AND DEVELOPMENT IN INDUSTRY

Dr. August B. Kinzel

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INDUSTRIAL COLLEGE OF THE ARMED FORCES

WASHINGTON, D. C.

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RESEARCH AND DEVELOPMENT IN INDUSTRY

12 January 1960

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GENERAL HOUSEMAN: It's almost trite for me to say that we are interested in research and development in industry, but I think it doesn't hurt to mention that we are interested in it for a variety of reasons, one of which is that research and development has become one of the big businesses of industry and one of the major segments of industry, particularly since the war. And also some knowledge of how research and development is done in industry is an aid to us who are interested in research and development within the various facets of the Government service.

Our speaker this morning is vice president for research and development of Union Carbide. This is one of the larger integrated organizations and in the forefront in research and development. He's a very busy man, a scientist and an engineer. We've had to drag him away from his very busy session this week. In fact, we have taken him away from a morning session with the National Research Council. He told me a few moments ago that he is involved in the highway research board activities of that council.

We are particularly happy to have Dr. Augustus Kinzel to talk to us this morning. This is his second visit to the College.

DR. KINZEL: Thank you very much General.

In addressing a group of this kind, who are students on the one hand but mature individuals on the other, with a whole set of personal experiences, one has to be particularly careful, because it's very easy in a talk of this kind to speak in generalities, some of which don't apply in specific cases. So I'm very happy to hear that we have a long and free discussion period; and if some of the generalities that I may set forth don't fit the special cases in which you are interested, I'll be glad to go into them further in the discussion period.

Now, the whole question of research and development has changed radically in recent years. It wasn't so long ago when you could count on the fingers of two hands, maybe even one hand, those industrial establishments that truly fostered research rather than development. And

even today you hear a great deal about the amounts of money that are being spent on research and development; but only in the larger and more sophisticated industrial companies do you have research as it is normally thought of by the academic people. This is the acid test as to whether it's research or development.

Now, when you talk about research in the industrial world, the first question, of course, is, Why? The objective of industry is to make money. In so doing, it serves the public. If it doesn't serve the public well, it isn't going to make money and it isn't going to last. But this is the way in which you make money--by serving the public well. So when I say I'm going to talk about research, you immediately ask the question. How does this contribute to making money?

It's pretty easy to understand development. You've got an idea. You've got something that is wanted, that is required, or you think it is. So you go ahead and bring it to the stage where it's a finished product suitable for general use. It's easy to see how you make money that way.

But research is a little more difficult. For a while people thought of research in industry as sort of set aside. It wasn't really part of the main activity. It was something that some companies did because they thought it was a good thing.

Well, any time this happens you can be sure that that particular company and that particular research, if this is the way it's handled, is ineffective. Research has to be an integral part of the total industrial operation. It cannot be a thing set off on the side. When that happens, there is lack of communication. The technological successes do not result, in reasonable time or reasonable degree, in exploitable products, and I use the word "exploitable" in its good and true sense.

So you start out with the point of view that the purpose of research in industry, is to make money. Now, if you glue your eye to that alone, you also get into trouble, because this leads to very short-range thinking--if your eyes are too closely glued to this. But you must never forget it when you're running research in industry. When you forget it is when you go down the blue sky route, and after a while you find that there's a new research director in that company, or they've cut their budget, or something else.

Now, when I say you must keep it in mind without focussing on it too intensely, what I mean is that the way in which research makes money for a corporation is, of course, by providing new products and processes. But these things don't come into being by themselves. Somebody has to have an idea. Where do these ideas come from? They come from new facts or new assemblages of facts.

Where do they come from? They come from research. They come as the result of experimentation, whether the experimentation be a mental exercise that finally ends up on a computer, or whether it be test tubes where you mix chemicals or metals and come up with a new plastic; or whether it be something that you first do on the computer and then put in the test tubes and show that you've got a new plastic, having actually produced it on paper before you went into the laboratory. But this is research, and it's long-range research; and you don't know where you're going. It's intangible. This is what makes it difficult.

Now, in most corporations, large ones, you have a vice president for research and development. You will not have a general vice president for sales in the large corporations. You have a vice president in each division for selling. The same with production, and so on. Now, why do you have a vice president for research of a corporation and don't have a vice president for production and so forth and so on? I am distinguishing now between a single company and a large corporation.

The reason is very simple. In sales management can be given figures, tangible figures, tangible results, immediate in character, by which to judge performance. You know the company is making money. The balance sheet shows it. You know what your costs are. You know what your productivity is. You know what your sales are. You may have nice curves to prove that a certain sales campaign has done a certain thing. In other words, these various factors and elements of a business can be measured with a certain degree of precision, true, not perfectly, but sufficiently well so that a president or an executive vice president or a management official of some kind can judge what is happening.

In research you've got a different situation. In the first place, you've got a time delay. We say "Seven years from test tube to tank car." Maybe it's seven, maybe it's five, maybe it's ten; but you've got a time delay. I can tell my boss, and prove to him conclusively, that the research that was finished or that was done seven years ago is today

paying off fivefold, tenfold, whatever it may happen to be in any given part of the corporation or all told. That's fine. But what can I tell him about the research that is going on today?

I can only reason by analogy that because it always has in the past, seven years later, paid off on a certain basis, that which we are doing today will also pay off seven years from now. Maybe it will and maybe it won't. There is no way to tell. It's a matter of judgment.

So why do you have a vice president for research? Because this is an area in which judgment must be applied, with relatively few tangible facts. And this is a type of judgment that management itself can apply only by virtue of special training of the type that is required in the research and technical work. So the vice president for research in a corporation is really the president's alter ego in this area. And this brings us over toward the whole management problem with respect to research and development.

To start with, you must set up in such a way that your research people have at one and the same time freedom of action, freedom to explore, and yet discipline and goals. Industrial research differs from the academic, not in the fundamental nature of the work--the research which is done in industry is every bit as fundamental as that which is done in the academic world--but it differs in that there is a goal, and there is a certain discipline in the approach to that goal.

Now, combine this with freedom of action of the individual. It sounds as if it's contradictory. Actually it's fairly easy to do, in that the greatest problem is not to allow freedom of action on the part of the individual researcher, but to get him to take it.

We always have the problem with the youngster coming out of school. "Will you let me work on anything I want to? Will I have freedom of action?" We tell him: "Sure. What do you want to work on?" Well, it so happens that Carbide is big enough that there isn't very much that a fellow could want to work on that we aren't interested in in some degree. So we don't have too much of a problem in this way. So, fine. He starts out working on that which he is interested in.

Presumably he's a bright young fellow or we wouldn't have hired him. He goes along on this, and all of a sudden in his findings there is

something that fits into applied research, something which can be developed later, something which leads into an exploitable product or process.

He sees this. He concentrates on it. We begin to put this into our evaluation machinery. It's his baby. If we evaluate it in such a way that we are going to carry on with it, he will go on with it. And try and get that fellow to think for himself in any other area or to start anything else or to use the freedom that he's got from that minute on. He's had it, as it were. And this is typical. The big problem is to get these young fellows to continue to think along original lines by using their freedom.

This leads into another matter having to do with organization and management. In research--and again I am talking about larger corporations--you will find that some groups go in for centralized research and others for decentralized. There is much to be said for both of these systems. But whichever system you have, you have to remember that you are dealing with people, and that creativity is involved--I gave this kind of a talk once before some production and sales people and when I got all through, they said: "Gus, this is fine. You are emphasizing the importance of the personal touch with people and the difficulty of keeping people personally interested and knowing the individuals characteristics so as to put them to best use. It's no different in engineering and production and sales." Well, it probably isn't different, but I think the degree is different. If you want to keep a man creative, he must have personal encouragement.

Researchers have been called prima donnas. I started out as a researcher in the lab myself, and maybe I am maligning myself when I say this, but as a group I think they qualify about 10 percent higher than opera singers as prima donnas. They are very sensitive and they need the personal touch.

Now, you can get this personal touch in either a centralized or a decentralized system. We think that it's easier to get in a decentralized system, where you have a series of laboratories--in Carbide, for example, we have 26 different laboratories right now, in which we make a valiant attempt--and so far it has been successful--to have no more than about 125--well, we set up a limit of 150 and try to hold it to 125 technical people. By "technical" I mean B.S. equivalent or better,

although today we are getting a very high percentage of these people in Ph.D's or the equivalent.

Now, a research director of such a laboratory, we find by experience, can get to know about 125 people. And by getting to know, what I mean is that if a man's work is extra good, if it has an extra spark in it, the research director will know it. If it's getting to be a little bit mundane, the research director will know it.

And, what's more, we want him to know why. With 125 people he can know why. He can know whether this fellow is worried because his son has polio or his wife left him, if his work is suffering. He can know what his personal problems are outside the work as well as inside the work.

In a decentralized system the local director has to have, because he's too far from his boss geographically, autonomy; so that he can do something about it, and do something about it right then and there. He doesn't have to ask anybody else.

In the centralized system you can have divisionization in the laboratory. You can have groups with, let's say, 100 people in a group; and the head of that group can also know his 100 people. But if he's in a big laboratory, and his boss is in the next office, and his boss's boss is in the office next to that, and his boss's boss's boss is in the third office, autonomy vanishes. It may be there on paper, but it actually vanishes.

So that knowing the problem and doing something about it are two different things. We feel that the decentralized system has a great advantage in that the local director can know his people and act on his knowledge directly and immediately.

Of course there's a disadvantage, in that you don't have right at hand in one group quite the expertness and in the number of disciplines that you will have in a centralized laboratory. Obviously, in a group of 150 you will not have the same expertness in as many disciplines as you do in a group of, let's say, 2,000. This is something of a disadvantage.

It can be overcome by arranging for proper communication. And the communication problem, I think, in the decentralized system is no worse, no more difficult, than it is in the centralized system, in that you recognize it in the decentralized system. When you've got 26

laboratories, or X number of laboratories, separated by hundreds of miles, or thousands, you realize that there's a communication problem and you've got to do something about it, and you do, whereas when you're all under one roof, it's too easy to assume that Jim Jones knows John Smith because they're only a few hundred yards away and one floor up. But, as all of you know, I am sure, this does not make for automatic communication. You've all had the experience of having introduced to each other two men who were within a few hundred yards apart in a certain office for 10 years without knowing each other. The communication problem, then, is a very real one even in the centralized laboratory.

Now, when you put all these things together, as I say, what you are trying to do in each case is to take the major advantage of the system you're using, centralized or decentralized, and then put in corrective factors so as to overcome the disadvantages so as to get most of the advantages of the other system if you can.

And this is done in both cases so that no one today can say that one system or the other is better in the overall. The results are the thing that count. And certainly in such centralized outfits as General Electric and Bell Telephone you have excellent results. In a demicentralized, that is, a combination deal, such as DuPont, you have excellent results. And we're not apologizing to anybody for the results at Carbide, in which research is decentralized.

Now, how to get your money's worth out of research? Well, the most important single thing besides people--people, of course, apply to everything--is the program. In a going laboratory a program is not a very difficult thing to handle on a routine basis. And it's frequently so handled; and when it's so handled, that laboratory and its results frequently slide downhill. In other words, doing some more of the same is very profitable for a while--and when I talk about profitability I am talking about research results which will then go on to development and production--but after a while, like so many things, it doesn't grow; it becomes less productive. Yes, the program is the important thing.

I have yet to meet the researcher who isn't completely confident that the project he's working on will be successful if you give him just a little more time to work it out. And the roughest job in the world for a research director is to stop a research project. On the other hand, if you'll set up your concepts properly, you can make this a lot easier

and a lot more effective. If not you put a lot of money down the drain of unsuccessful projects.

How do you do this? How do you set up a live program? Well, the first thing is, you look at the overall picture, and take any one of the successful projects, or a number of them so as to get something of a statistical look at the situation; and you find that the fundamental research, basic, call it what you will, learning research, on that project, has cost dollars, let's say it cost $1, and then the project goes over to the applied phase where your goal is now much more succinctly formulated--you know pretty well what you are trying to do; you aren't just learning. You've got your teeth into something that you want to push on through--and in the applied research it will cost about $10.

Now, the applied research goes along and now you're beginning not only to get your teeth into something, but you're beginning to learn quite a lot about it, how you're going to do it, quite a lot of the details of what it's going to be like; and you go on to development. Now, at this stage you can make a pretty fair evaluation, within rather broad limits, of course, of what you've got and what you might have as the result of this work; and as you go on then, you decide to go into development with this project. If the fundamental research work cost you $1 and the applied cost you $10, the development will cost you $100. This is a very broad and rough order of magnitude.

Then you go through development for $100, and you put up a factory and go into production in a big way, and that will cost you $1,000.

Now, if you look at it that way, you realize that, as far as the $1 phase of things is concerned, you can afford to work on quite a number of projects that will not be successful, because, if they are not successful, all you've done is partly lost you dollar. You've spent a dollar. You've gotten information. You've gotten things that may stand you in good stead later in other areas. You always have the chance of a lucky find, of a lucky accident. So that you can actually afford to do quite a good deal of this basic and fundamental work, that costs you a dollar a unit, that is not going to be "successful." So that chopping off this kind of work is not a very great problem, because there is no great need to chop it off.

But you get over to the applied, where each unit costs you $10, and here, it's at this transfer point that you should make your first economic

study. You people in the Armed Forces have been criticized for duplication, for ventures that have gone a long way and then been cancelled out, and so on. And I venture to say that some of this criticism--not all of it--is justified. I think it tends to show a failure to follow the system or some similar approach that I am now talking about.

You get over into applied research, and here you can define the product or the process that you're trying to work out. And the first thing that you ask is: If this is completely successful, are we going to have something that anybody wants? And how, if it's completely successful, is it going to stack up with that which now exists and that which in all probability will result from the improvement of that which now exists?

In other words, you take that which now exists and project the improvements for seven years. Then you take a look at what you think you've got--this is just still almost in the idea stage--and you postulate all the favorable things about it. And you would be surprised in how many cases, even if you postulate all of the most favorable things about it, this will be a project that is not worth being put into applied research. And here you have a good way of cutting off.

An illustration that I used here a couple of years ago, if I remember correctly, had to do with a plastic. Somebody had come up with a new plastic, or with the concept of a new plastic. Somebody had made a few grams in the fundamental research laboratory and measured some of its properties. But if the sum total of all those properties is such that it's not very much better, if at all, just a little bit different from, let's say, a vinyl or a polyethylene, unless there is something unique and truly different, there would be no point in working the thing through. If, on the other hand, it has some unique properties, therefore it will have a place and use, it will serve a function and a need that is not now being fulfilled, then we certainly should go ahead.

Now, at this stage what you are thinking about is primarily the technological phases, the technological properties, and so on, as to the needs to be filled. Sure, economics are beginning to play a little role. If, for example, this will be a wonderful high-temperature material, but at that time your methods, and the best assumptions you can make indicate that it's going to $2,500 a pound, maybe you'd better go back and do some more fundamental research on methods, rather than do

applied research on how to make this stuff at $2,500 a pound, because somebody else is going to make it for $5 and the third guy is going to come along and make it for a few cents some day. So, broadly, economics begin to come into the picture at this stage.

All right. Let's assume that on the evaluation that is made at this stage, you do go into applied research on this particular product. You go along with this applied research, and after a while you begin to get a whole series of facts, enough so that you could actually sit down and, without having all the numbers, but broadly, figure out about the kind of plant you would have to have to make this in and about what it would cost.

You also learn at this stage a lot more about the product itself. And now you begin to really bring the economic factor in. It isn't a case of "Does this fulfill a need?" any more. Now the question is "Will it fulfill a need at a price?" with emphasis on the "at a price" because you've gotten to the stage now where you can begin to make some pretty fair guesses as to the order of magnitude of the price.

And here again let's say that in this plastic that we're talking about you've been able at this stage to work it out so that it's not going to cost $2,500 a pound, but it's going to cost $5 a pound, as we foresee the thing at that time. Well, depending upon what it is, it may or may not be worth while to go ahead along that line. But this is the time--when you've got that much information--to reevaluate the total project from the economic point of view.

And in the military you have exact parallels. You reevaluate the project from the point of view of what it will do for you in a given system, and how this compares with the weaponry, firepower, and so forth that you have otherwise.

All right. Let's say that at this stage we know that it's going to cost, if we go along that line, about $5 a pound. Shall we go to development then or not? This is the stage where we're going to jump from a $10 to a $100 project. And here you bring in your marketing people, your sales people, as well as your engineering and production and so on.

You may find out that even though it costs $5 a pound, there's going to be a pretty good market for it; that your capital investment for your plant, instead of being about a dollar and a quarter or a dollar and a

half per dollar's worth of sales, which is more or less the average in the chemical industry, will only be fifty cents or something of this kind, your economic factor on the right side overbalances this price business; and you will render a judgment. You may say: "Yes. Even though it's going to cost $5 a pound, because of the low capital investment or because of the market or because of the fact that it fulfills a need that nothing else can fill, we're going to go into this business" and you go ahead.

On the other hand, you may look it over at this stage and say: "No. We're not going to put it into this hundred-dollars-a-unit category. We're going to send it back and say, 'We've got to figure out some other approach to this problem of price. There must be sone other technological way of arriving at this point.'"

A good illustration of this kind of thing would be the epoxyresins, which are today coming into their own. Many of us believe that the epoxyresins are going to fulfill a bigger need and have as big a place in the total picture as the polyethylenes, which are so well known, or the vinyls, which have been with us for a long time. The epoxyresins depend on a peroxide function. And, broadly speaking, they are made--they're not brand new; they've been known for a long time--broadly speaking, they were made via hydrogen peroxide, which all of you know from the bottle of it on your shelf. But this is pretty expensive stuff and nobody has been able to figure out a cheap way to make it, and so the epoxys were limited in their usefulness by their price, the price depending on the cost, of course.

All right. Now we've got this picture. So that in our chemical work on the epoxys we never went very heavily. Neither did anybody else, because of this limitation.

Over on the other side we had some fundamental research going along on the nature of oxidation, organic oxidation. And the fellow who did that, while working on it--this was in the one dollar phase of the deal--and this was some years ago--while working on it found that peracetic acid would provide that per oxygen to the epoxys just as well as peroxide would; and at the same time he devised a new way, very much cheaper, of making peracetic acid.

Immediately we did two things. We put that into applied research, and into development, and built plants. This was over a period of some

years. These plants now are constructed and operating. We're going to put up some more. We didn't put up enough. We did that on the one hand.

On the other hand we said: "Ah, this removes the block from the peroxide, from the epoxys." So we started a very much more intense, greater in magnitude, applied research program on the epoxys. That has gone ahead simultaneously; and, as I say, I think that today we are just beginning to see enough to know that we are probably right in thinking that the epoxys are going to be a factor in the total plastics, total use, and total service to the civilian and the military at least as great as some of the more classical things.

Had we spent a great deal of money on applied research on the epoxys before that time, and had this peracetic acid thing not developed, that money would not have been well spent.

What I am talking about is a continuous yardstick measurement as you go along, first strictly technical, then technical-economic; and finally you say: "Well, its technology is all settled. We don't need to worry about it. It's total economics we're interested in. Do we go ahead?"

So I maintain that there is almost no excuse for a development project not to go on into production and final use; that the decision to go from applied research to development--that at that time you know enough that to go into a development project which you then decide cannot go into production, shows a lack of judgment at that time.

Now, there is a little excuse for a mistake there and bad judgment, but not much. One factor that could exist without your knowing it is that somebody else has beaten you to it and done better than you have. This risk you always have. This is the standard business risk in development. But, aside from that, there really isn't much excuse, it's just a case of bad judgment, bad system, bad method, to spend $100 on development without winding up and going through to production.

You take a look at total development costs and total research costs in the various corporations, and you'll see that it will vary--I'm talking about the large ones again--from something like two to one--that is, twice as much for development--these are the $100 projects--to, 6, 7, or 8 to 1.

In general you will find that the growth companies--which means that their development projects have gone on into production and been successful--have the ratio on the lower side, because more of their projects in development are successful and go on through, and they spend less money on unsuccessful development projects, which is your $100 deal, as against your $1 deal in the fundamental research.

Actually there is only one other point that I want to make before we get into general discussion here, and that is that the role of industry with respect to the military where R&D is concerned has also changed. There is a move on foot now to have all of your contracts so written that there will be an automatic 5 percent allowable overhead for research in any hardware contract. Whether this finally comes to be or not is in the laps of a good many people.

But it has elicited a point, namely, that you require research in connection with your hardware; but at the time, for growth, for new systems, and the like you require research not connected with your hardware. And you can get this in the academic world; sure--a certain amount of it, a certain approach, a certain point of view. But if you are talking about specialized areas--materials, as in Carbide, for example--electronics, as in Bell and a whole slew of others--when you're talking about specialties, you will find greater background, more knowledge, and greater experience in industry than you will anywhere else. And you've got to tap that. This should be put at the disposition of the military. And it's easy to do with the right type of contract; and by the right type of contract what I'm talking about is one in which the industrial researcher can go at the thing and use his own yardstick on the one hand, and on the other, he gets certain benefits in the civilian area. In other words, you cannot expect an industrialist to use his capital to do Government research without having some of the benefits, because the fee that you get on Government research is relatively unimportant and minor. The benefit is the knowledge that you get, and the use to which you can put it, first, for the military, of course--that's the purpose--then for the civilian. But the minute you say it's exclusively for the military, or that that which is not for the military is completely in the public domain, you have taken away from industry the incentive to do research for you.

I see time is getting on. I hope I have given you some bit of the spirit and the philosophy that we use in trying to have industrial research

result in those things that make for better living on the one hand, and, speaking for the military, better fighting on the other.

COLONEL FORBES: Dr. Kinzel is ready for your questions.

QUESTION: Sir, you touched on this problem of creativity in your organization. Could you say a little more about how many of your people are really creative, who get these new ideas and start working on them? About what percentage do they run? And how do you spark this?

DR. KINZEL: I would say that, averaging out in our laboratories, which will run around 125 technical people each, we may have as many as three creative people. You've got to recognize them, and you treat them with silk gloves, cotton batting; give them all the freedom they can have, pat them on the back, work with them. Don't be negative. Encourage every one of their ideas, even though you think it's no good, because here's the interesting thing on that:

This fellow comes in with an idea. You think it's no good and you tell him so. This is very discouraging. So the next time he has some idea, before he thinks it through, he'll decide it's probably no good. Pretty soon he'll dry up. But if you tell him: "Well, now, this certainly has a lot of good features. Here they are. Now, we don't know about these other features. Why don't we do a little experimentation and find out!" remember, this is back in the one dollar class "do a little experimentation and find out" what happens is that in about three weeks he comes to you and says: "You know, I don't think this is so hot" and then you agree with him.

But we have all been through the so-called brainstorming type of thing and so on; and you can do certain things that way to help a little bit in stimulating creativity. But there is nothing to match, first, the guy who does think; and, second, the guy who at the same time so steeps himself in his subject that his subconscious is working for him.

Now, your subconscious won't work for you--unless you soaked in your subject and this is where the new ideas come from. You don't get a new idea by thinking it through rationally, consciously. The ideas come to you. It's a trite thing to say: "You know, while I was shaving this morning, I just thought of something" or "while I was in the shower." This is a reaction. It happens because it happened during the night. Your subconscious worked. And if you're sufficiently steeped in the subject, and you live it and you eat it, and have the right kind of mind at the same time, then the ideas will come.

So much for that. I could go on on that all night and you wouldn't know any more when I got through than you know now.

QUESTION: You covered centralized and decentralized research thoroughly, but back in this dollar class what is your opinion on individual versus committee or group research?

DR. KINZEL: Well, in the fundamental area there is need for a group only insofar as a bright, aggressive, hard working, thinking individual can keep a half dozen assistants pretty busy. If you want to call that a group, fine; but otherwise there's no need for a group.

Now, you get over into the applied end of things and here you begin to require solutions to specific things that are not directly part of the problem but they are ancillary to it, but you have to have them. In other words, you need more disciplines. You need some physicists, some metallurgists, some mathematicians, some chemists, and so on. Here you have what you might call a team approach. It's not a committee approach; it's a team approach. And this is highly useful. But again you need one important ingredient in every case, and that's the spark plug. If one of that team isn't a real spark plug, you might just as well send them all home.

As far as committees are concerned, they're fine. They get things aired. They let people express their views. They're wonderful for everything except making decisions.

QUESTION: I was interested in the problem of getting one of these idea men unlatched of the project so he can go on and have other ideas while the thing goes on into production. How much effect has the economics business of the sales director, for instance, of, say, a nylon plant getting probably a good bit more than the fellow who thought out this--how much is that affecting this tendency to stick with a project?

DR. KINZEL: Well, certainly not so long ago it affected him very seriously. But most of us have come to the so-called two-ladder system. And actually, in our own research laboratories you can go up the administrative ladder or you can go up the strictly scientific, technological ladder. We have titles of prestige that go along in the scientific ladder as well as dollars. And we have what seems strange to some people, but doesn't seem strange to us at all because it works, but in more than one of our laboratories we have a fellow whom you might call a chief scientist--we happen to call them consultant metallurgists or consultant chemists--making more money than the director of the laboratory.

The day when the scientist was sorely underpaid is fast vanishing. Ph.D's out of school start in at around $10,000 a year. It wasn't so many years ago when I said that, roughly, in strictly scientific work in industry you've got a top of $20,000. If you want to make more money than that, you've got to go over to administrative work. That day is gone too. The world changes.

QUESTION: Aside from the three hot shots out of every 125 in your laboratory, the problem of the advantage of turnover of the other 122 gets felt. Would you want to speak to that a bit, as to what you get out of the other 122?

DR. KINZEL: Oh, sure. Let me put it this way: In your other 122 you have people who know how to apply scientific knowledge rationally and bring the thing up to a point where it's an engineering job. And this is absolutely necessary and highly important. It's highly important to be able to take any problem that you've got and turn it into a practical reality.

Now, these people, as they get older--this also requires imagination, but not quite of the type that I was talking about as creativity in answering the other question--it also requires imagination. But gradually, in many cases, these people lose their imagination, in other words, it lessens; and they become more interested in other things. And then we switch them. It generally happens between 35 and 40 years of age. And, again, there are plenty of exceptions to the rule, but broadly. We are continually looking at our people, and at this time we'll frequently encourage them to switch to engineering, to production, to sales, to some other activity in the corporation.

It's an interesting thing that today at Carbide one of our two executive vice presidents started in research. In our various companies at the vice presidential division level, there are so many fellows who started in research that I can't tell you how many there are. But these fellows have good brains. They think rationally and clearly. They are hard workers. They have sense of balance. They've got an economic sense. And they can do better by switching from research. And we want them to.

QUESTION: Dr. Kinzel, you have in Union Carbide about $80 million a year to put on research, don't you?

DR. KINZEL: Right. R&D.

QUESTION: I wonder if at your level you must count on these 26 laboratory chiefs to get something out of it. Do you have some measure, say, new products per year or something else, that you go by to tell whether or not they are being profitable, and worth the amount of money that they're getting? Specifically what do you do at your level to tell what they are going?

DR. KINZEL: Well, this goes to what I was saying earlier about why you have a vice president for research. You are asking me, How do I judge their performance? Well, obviously, by results in being. But you can't judge them by what they did seven years ago. You've got to look at the major projects they're working on and see the progress they're making. You walk into the laboratory and catch the spirit of the place. If you've been around a lot, you can walk into a place and it doesn't take long to know whether this is an enthusiastic, dedicated bunch and knows what it's doing or not.

So you judge by a whole series of intangibles. This is the very point that I was trying to make. I can't give you any yardstick to tell you whether a lab crowd or a lab organization is a good one.

QUESTION: Can you give me any idea of the number of new products a year that come out of your research?

DR. KINZEL: The last number that I had, which was about a year and a half ago, was two a month. But I know it's greater than that now.

QUESTION: We hear a lot these days about the deficiencies of our educational system vis-a-vis the Russian. I was just wondering, What kind of characteristics to these individuals have to have to be creative? Are they young? Are they married? Do they have all these domestic problems when they go home? Are they personally a little bit peculiar? Are they people that we are turning out in greater numbers than we used to? Can you address yourself to that?

DR. KINZEL: I can address myself to it. I might have the wrong address, though.

In the first place, there's no pattern. It includes what you normally think of as screwballs and very sane married men. But from the angle that you touched on there, there's no pattern at all. That's item one.

Item two, as to how you get them. In the first place, a surprising number of them have a high proportion of self-education. In other words, I think there's something in the educational system which trims them off, gets them in a habit of thought; everything is too logical. It's as they grow older too. One of the big troubles with most of us is that we know what can't be done.

There are a few of you here who are old enough to remember the big joke in the early days of radio. Fellows were boasting about how good their ham radios were. That was 1920 or thereabouts. One fellow says: "Well, I've got one that I put together that's so darn good that I can turn a dial and tune out the saxophone." Well, this was so impossible that we knew it couldn't be done and everybody laughed. It was the biggest joke of the year. Of course, today with a filter you can practically tune out a saxophone. And the business of knowing what can't be done I think is drummed into the boys a little too heavily in the educational system.

As far as the Russians and we are concerned, it's a case of turning out enough people to do the 122 jobs, and I don't think "education" has much to do with how many of these three guys of the 125 are going to be turned out. But I do think that the more people, that statistically it will work, that if instead of turning out 125 guys at a university, at two universities you turn out 250, the chances of having six guys instead of three that are this way are pretty good.

QUESTION: Sir, would you identify a little more specifically the level in your company that makes this rather important decision to go from the $10 to the $100 phase?

DR. KINZEL: Yes. In each division we have a vice president for research. We have 14 divisions. He may have one or more laboratories with local laboratory directors.

Now, when the program is put together--annually the program is reviewed budgetwise, and that has been recorded--but some time during the year when the time is right on a given project, the local director of the laboratory will say: "This should go on" or "It shouldn't." And the vice president for research and the local director and the guy who is doing the job will then sit down and put all the facts together. And as far as going from fundamental to applied, the decision rests there.

Now, when you go from there to development, you then bring in your engineering and your sales people in the discussion with respect

to decision. But the decision is made by the vice president. And, of course, to go from development to manufacture requires a presidential decision in the division, and, if it's a lot of money, maybe the corporation.

But then you get other things where, for example, out of my own office I'll just start things by giving them money to do it. For example, a few years ago we decided that biochemistry was something that we ought to be in quite a lot deeper than we are and we set up a few more groups. One of these came up with this business of how to freeze whole blood. The Navy is now very much interested in that. In fact, the whole country is. You freeze blood and store it forever or until you want it and then you unfreeze it and use it. This started in fundamental research, came into applied, and then it stopped because we said: "We don't know how we're going to do anything businesswise with it. We don't want to "exploit" it." We're not bloodsuckers, you see. So we dedicated it to the public and the Navy has picked it up.

QUESTION: Doctor, would you be willing to either criticize or comment on the management of military research programs in the light of the standards that you set for your own company? I might say that this captive audience usually maintains good security.

DR. KINZEL: I'm not worried about that. Having been on the Defense Science Board for some time and on the Naval Research Advisory Committee--I've been chairman of that and been on it for a long time--I have spoken my piece quite often.

My own feeling is that, as far as the fundamental work is concerned, there is not enough of it in relation to the applied. As far as the applied research is concerned, there is not enough of it with respect to development. And that the shift from basic to applied to development too often is done without enough thought, consideration, and evaluation, and that this is one of the reasons that we are in the situation where the military is subject to some of the criticism that it is getting.

I think the biggest single place where you can do yourselves good is an evaluation between the applied research and the development. There are too many projects that you get into because you have spent $1 million on applied research, or $500,000 or $300,000, over a period of X years, because you spent that and it's technologically successful, you go on to the next step whether or not it's going to fit into one of your systems or compete with one of your systems or be any good later. There is not enough evaluation at that stage in the military.

QUESTION: Sir, I wonder if you could comment a little more on formal control, for example, technical reports which may come to your laboratory directors, to yourself, or to the chairman of the board--budget. To what extent do you use these cost reports and how they may help or hinder research.

DR. KINZEL: The budget and the program go hand in hand. This can either help or hinder quite a lot.

The budget has to be approved, in our case by our Appropriations Committee-and I'm their chief advisor on it--and here it's a case of taking a look at your total program, seeing the balance of fundamental to applied and development, looking at specific projects and their goals, taking a re-look at the evaluation and the decisions that were made in the evaluation. So that budget can be very important.

Now, the way the budget is put together is very simple. It starts right in with the group leader, who tells the division leader in the laboratory: "This is what I want to work on. I've got this many people and this is going to be all right" or "I need some more and so on next year, and here's what I want." You put that all together and finally it comes up to the local director of the laboratory, who then takes all of these requests, which generally total four or five times as much money as he's ever spent in his life. He then gets the boys in and they decide what their requests will be.

It then goes to the vice president of the division, who again goes over it with these fellows in the light of other laboratories that he has, and finally comes in with a division request to the Appropriations Committee. And at this stage we are looking at dollars, goals, and scope. It's very helpful.

As far as individual reports are concerned, we insist that when a job is wrapped up either way, by being transferred from fundamental to applied or from applied to development, we get a complete report on it. In between they can issue interim reports if they want to. We generally have a monthly or a quarterly report at the same time on all projects, just to be able to take a look at them quickly.

I think the main good of the monthly and quarterly reports is to make a man stop and take stock of himself. The fellow who writes it gets more out of it than the fellow who reads it.

QUESTION: My question refers to the reply that you gave to Captain Schneider a moment ago where you commented on your views on research and development in the Military Establishment. Do your comments apply equally to the research that is done on the civilian side of the Government? I am thinking of my own agency, the Bureau of Mines, and that which goes on in the Bureau of Standards, the Department of Agriculture, the Weather Bureau, and so forth.

DR. KINZEL: No. I think they are in a little different category in that it isn't too often that they get over into the heavy applied and only rarely into development. Industry and others, if they are alert--your material is all public information--watch it pretty closely; and should it get to the stage where it should get into development, it's that good, you generally find somebody else doing it for you.

So I would say that, the way the money is spent in the agencies you have mentioned--and I don't want to make this blanket, because you have to be specific--but right now in the Department of Commerce I'm a member of a small group with Kelly and a couple of others that are making a series of recommendations with respect to those bureaus and how the moneys are being spent and so on. And I know that in the business of going from the 1 to the 10 you've got the same problem, but it isn't too serious, and its generally looked at fairly well. But it's this 10 to 100, that is, applied research to development, that is the place that I think could be greatly improved. And that doesn't exist to a great degree in these other bureaus.

QUESTION: Would you address yourself a little bit to the individual who is the director of one of these laboratories? You were talking about these three guys who were creative as being sparkplugs. It seems to me that another guy who's pretty important in terms of the overall program is the director of the laboratory. What qualifications should he have? How do you find him? Are they extra creative? Are they people who get outside the laboratory in their profession or something like that?

DR. KINZEL: That's a very good question, and a good last question. To start with, if you think of an individual research lab for a minute as an individual business, then the local director can be thought of as the general manager of that business. Now, what do you need in the general manager of any business? Well, first you need a really intimate working knowledge of the business. So rule No. 1 is, Never put an accountant in as head of a laboratory. I'll go further and say, Don't put a production man in or a development man in. The director

of a research laboratory has to come up through research, as far as I'm concerned. That's No. 1.

But, as for the general manager of anything, in addition to knowing the intimate details of the business and how it works, how people down below think and how they work, and knowing every job on the way up pretty much--in addition to that he has to have some business sense. He has to know how to deal with people. He may even have to deal with a union. So he has to have those same characteristics, in other words, an ability to handle people.

The general manager of any organization, in addition to knowledge and the ability to handle people, has to have a logical, clear, organized mind.

All right. I could go on. I don't have to enumerate the rest of them. But the analogy holds completely. So the local director of a research lab has to be a fellow who's a good researcher, not necessarily one of the three that I'm talking about, in fact, generally not--he has to be a good researcher, who knows the ropes, knows how it's done, knows what's there. He has to be open minded. And, while he may not be super-imaginative himself, he has to respect imagination and understand that that which can't be done isn't always so. He has to be able to handle his people. He has to be able to run an organization. And he has to be able to do what any general manager has to do, what any administrator has to do--please his boss. You put those things all together.

Where do these people come from? From the other 122, by and large. You just watch them as they grow up. They become group leaders. The next thing you know they are division heads. Next thing you know they're head of the laboratory. I think that's the best way I can answer that.

COLONEL FORBES: Dr. Kinzel, it's almost unnecessary to note that you've been both stimulating and informative. We realize the heavy demands on your time and how active you are in scientific and engineering circles. On behalf of the Commandant, the faculty, and the student body, we thank you very much.

(18 May 1960)B/de:pc