INDUSTRIAL RESEARCH

A RETROSPECT AND A PROSPECT OF SCIENTIFIC INVESTIGATION IN INDUSTRY

FRANK B JEWETT
President, Bell Telephone Laboratories

Professor Agg, members of the Highway Research Board and guests, Mr Upham talked with me some months ago about the possibility of meeting with you this morning and saying a few words. We discussed and agreed what subject matter might be most helpful in the problems that confront you, and finally chose the topic which the chairman has just announced I propose to give you, for what it is worth, my personal picture of the situation with regard to research as it concerns industry

I haven't prepared any formal paper I thought it would be easier for me and possibly more pleasant for you if what I had to say were done in a personal and intimate way. In order that you may have a fair basis on which to appraise anything I say, it might be well to know something of my background, my history, because we are all tinged by our antecedents and experiences

I started mature life as a graduate of an engineering school, as an engineer trained in the more or less stereotyped manner in which engineers were trained twenty-five or thirty years ago. I looked upon physics and chemistry and other fundamental sciences as most engineering graduates did at that time—as obscure things, but essentially fixed and achieved sciences, the facts of which were to be used by engineers as the blocks with which to build up their structures. It happened purely by chance that I was thrown into an atmosphere of scientific research in physics, and I learned, much to my surprise, that what I had looked upon as rather a fixed achievement of the past was not that at all, but was a living, growing, expanding thing. In fact, I came to the conclusion that it was living and growing faster than engineering itself Later on it became my lot to apply the knowledge that I had of engineering and of science, pure science, in the industrial field. I presume that I was one of the earlier industrial research workers of the period which we are now in, where scientific research has come to be a recognized and a very necessary part of modern industry. So my experience runs the gamut of the engineer of the old school, of the research worker of the university, and of the industrial research executive So much for the background.

I imagine that a large part of the population who give any thought to it at all think of research in the physical sciences as something new in man's activities As a matter of fact, it is not at all new. Research work in its broadest, biggest sense has been going on since time began. Every engineering problem that was ever undertaken, which required the planning of something new, involved some form of research work However, one thing which has come into the field of man's activities in the last two hundred years has been a little different point of view with regard to the machinery and mechanism of research. Quite naturally, it first found its application among scholarly people, people who were, if you will, rather of the cloister type. What came in was a realization that to get real and lasting results you had to perform experiments based on a well-considered plan, worked out from the experience of the past; and to produce valid results, your experiments had to be so controlled that you could interpret the results in terms of the real factors of the problem. One of the hardest things we research people or engineers or anybody else have to contend with is the temptation to draw conclusions from complex experiments or experience in terms of part of the factors of the problem only Even in our modern institutions and industrial research laboratories, equipped with the best means and men available, we find that inevitable tendency continually cropping up; results are obtained with a complete knowledge of some of the factors only, and sooner or later we realize that there was no valid reason for drawing these conclusions, because there were, in addition, a lot of other factors which played a greater or lesser part in the results which we had obtained

I am afraid that engineers as a group are, in the main, pretty prone to do that sort of thing; but even our scientific people do it—not consciously, but unconsciously. To get a result that means anything, in most cases you have to so control your investigation that but one factor only is a variable in the problem If you have two, it is very difficult to interpret your result as between the two factors. If you have three or more, it is practically impossible, and yet we do it every day, and the only way that we make progress when we have such a condition is by main strength and the correctiveness of continually doing it over and over again, until by statistical methods we just naturally sift out the results in terms of the individual factors.

There is another failing which we are very prone to, and that is to try our initial experiments on too large a scale I have seen—and I am afraid I have been guilty myself at times—experiments started on a supposition that certain things can be done, and started on essentially a commercial or large-scale engineering basis If you can do a thing in a test tube in a laboratory under controlled conditions, you may be able to do it on a large scale, but not necessarily If you perform your initial experiment on a large scale, whether the results justify your expectations or whether they don't, you may not, and probably cannot, definitely account for the results Suppose you have an idea and perform an experiment on a large scale, and the result does not come out as you thought it was going to You do not know why it didn't, you do not know whether your assumption was wrong or that the experiment was impossible, nor do you know but that the cause was a failure of the technique applied to a thing which was inherently possible Having decided what we want to do, and having made an assumption as to the correct method of doing it, we have to try it out on what is essentially a laboratory scale It has to be tried out under conditions that, so far as it is humanly possible, are precisely controlled to provide only a single variable factor in a given experiment If at the conclusion of that experiment we obtain results which are in line with our presupposed ideas, we may fairly assume to have proved that what we have started out to do can be done—at least, experimentally We may want to repeat the experiment once or twice under other conditions to make sure of our conclusions We are then ready to start the next part of the experiment, namely, to see if we can transfer this laboratory process into a large-scale operation We all know that when we try to do things on a large commercial scale, we inevitably bring in a great many factors which tend to lessen the control had in the smaller laboratory experiment, and may, in fact, find it impossible or impracticable to avail ourselves of the new thing

Just as an illustration of what I mean, I might cite the process which has been followed for twenty years in our research work in the telephone industry Let us assume that the problem in hand has for its end the design of a complicated piece of apparatus The first experiments are primarily to check up certain fundamental physical or chemical characteristics or reactions We have succeeded, let us assume, in checking our fundamentals The next step in the process is to make a piece of apparatus which, as nearly as possible, conforms to the ideal requirements of this piece of apparatus. It is the best we can do regardless of cost We use the best machinery and the best men and the best materials and everything we can to produce an ideal piece of apparatus Any results we obtain with it are a test not of the fundamental principles, since these have already been checked, but are a test of our ability, with the tools at present at our disposal, to make a physical thing giving certain desired results Tested under conditions under which this piece of apparatus will have to operate, nine times out of ten that step in the process develops certain difficulties which our earlier and more fundamental tests did not disclose It means we have to make modifications if we are going to use our fundamental principles commercially

Finally, let us suppose we have an operating piece of apparatus made under this ideal condition, but which is not at all commercial For example, this particular piece of apparatus may have cost a thousand times what we know we can afford to pay for a large number of these pieces of apparatus used commercially We have shown, however, that the thing can be done

The next step in the process is to build what we call tool-made models These are models which conform as nearly as possible to the ideal, but which are made under commercial processes applicable to large-scale production and with ultimate cost considered Tests on tool-made models are not tests of the fundamental thing, nor of our ability to make one thing to conform to the ideal, but are tests of our ability to make in a physical, economical fashion a large number of things capable of producing a desired result

It is only after we have gone through all three of these steps and tried the results under service conditions and found them satisfactory that we feel safe in going into production on a huge scale and using the apparatus as a part of our standard equipment

This general process must be gone through with in one way or another in practically every line of industry where we are trying to apply science to engineering for practical purposes

Now, another matter We have learned, particularly in the past thirty or forty years, that it is not always safe to assume that very small admixtures of foreign substances may not produce perfectly stupendous results in the characteristics of a material This is particularly true with regard to things chemical There was a time not so long ago when a thousandth of a per cent or a hundredth of a per cent of a foreign body in a chemical mixture was looked upon merely as an incidental inclusion which could have no possible effect on the characteristics of the substance. We have learned differently. We have learned in recent years that this is an absolutely erroneous idea.

Merely as an illustration, let me recite one experience in my own field of work Some years ago we came to the conclusion that if we could get magnetic materials which were very much more magnetic than the best magnetic materials then existing, we could do many things which were theoretically possible, but which we were stopped from doing by the limitations of substances then known We started an investigation to see if it were not possible to develop a better magnetic material We felt there must be an alloy which would have the characteristics which we sought To make a long story short, we found it. We found it in an alloy of iron and nickel, roughly 78 per cent nickel and 22 per cent iron, which when perfectly made and perfectly treated and worked had, under the conditions of our use, magnetic characteristics many times greater than the magnetic characteristics of either iron or nickel or any other known substance or alloy. This material has come to be known as permalloy

Our extended series of experiments taught us conclusively that almost infinitesimal admixtures of certain things would reduce the magnetic characteristics of the alloy from their high level to practically the level of either iron or nickel I don't carry the exact figures in mind, but my recollection is that between a thousandth and a hundredth of a per cent of certain other materials in this mixture of 78 per cent nickel and 22 per cent iron will cut the magnetic properties down to 10 per cent or 15 per cent of what they are with the pure alloy So all of us who have had very much experience in dealing with complex substances and complicated aggregations of things have come to be very cautious about drawing conclusions as to what can and can't be done when there is any chance of these so-called "poisonous" things being present.

More nearly approaching conditions of the problem which confronts you gentlemen, is a circumstance which developed in another undertaking of the National Research Council in its Engineering Division some years ago It was in connection with the problem of studying the effect of marine borers, teredo and the like, which are so deleterious in some sections where wooden structures are put into sea water. In the course of the experiment, industry all over the United States and Canada and abroad became involved A certain amount of work was done on substitutes for wooden structures, particularly the use of concrete. It developed from this rather preliminary investigation, which was not then carried to any final conclusion, that concrete —good concrete, or what people thought was good concrete—when immersed in sea water was extremely variable. Records showed that some concrete structures had apparently stood up for an almost indefinite time, with very little deterioration Yet right alongside of them other concrete structures, presumably made in exactly the same way, had gone to pieces in a very short time

The little investigation that was made seemed to point to the conclusion that this variation in result was not so much due to variations in the mechanical processes of mixing the concrete as it was due to the fact that the cement, sand and stone employed, each one in its own proper place, were chemical substances which when in contact with other chemical substances reacted slightly differently under the conditions which were to be met in the sea-water structure. The trouble, where trouble occurred, really went back to the raw materials used in making the concrete and their chemical composition. It seemed clear that no satisfactory answer to the problem of concrete structures in sea water could ever be hoped for until men had studied further and knew more about the individual constituents of the materials.

Slight consideration will show what I have in mind

Suppose in a given concrete mixture—a given mixture of sand, stone and cement—the chemical reaction which goes on (with any one of the three constituents) under the influence of warm sea water tends to produce a substance which is slightly greater in volume than the original substance Even a slight expansion will tend to disintegration.

All of this I have cited merely to emphasize the necessity which experience seems to have taught us of extreme caution We must, to be sure, base our conclusions on good engineering practice derived from controlled experiments where one variable at a time is involved, but back of this we must seek to find out all we can about the constitution of the things which are the blocks with which we build our engineering structures

After twenty-five years or more of experience in this field of so-called pure and applied science in engineering and industry, I am absolutely convinced that in no other way can we make substantial progress as fast as we can by adhering closely to this simple fundamental procedure—avoid in every case drawing final conclusions from the type of experiment in which two variables in a single equation appear We all know in our mathematics that we have to have at least as many equations as we have variables if we are to obtain unique solutions We should find out, so far as we can through a controlled experiment, all that there is to be known about the individual components of the things which we aggregate together A and B in association produce a certain result, B and C in association produce another result, A and C produce a third result, but it is not safe, in most cases, to assume that the result of combining A, B and C together in varying proportion can necessarily be derived from our knowledge of those things in pairs

Now, with regard to the future Taking the situation as a whole, it seems to me that our progress in pure science and our progress in applied science are each day-producing for use a rapidly increasing amount of substantial knowledge—that is, knowledge which can be used safely by engineers in the planning of their work

The universities are turning out more and more really fundamental information, and it is only the universities, so far as I know, who are organized to turn out that kind of information That is one of their two primary jobs The other is to turn out trained human beings as workers in pure or applied science

Further, the industrial research organizations, whether they act for a single industry or whether they do the work of associated groups, are applying this vast store of rapidly increasing knowledge more and more to the making of things which are purely of substantial commercial value

So it seems to me quite clear that whatever may have been our progress during the past couple of decades—and, by the way, it has been a very substantial progress all along the line—we have every reason to anticipate that there will be a much more rapid and greater progress in the decades just ahead

Now, how are we making all of this knowledge available to society? Through the period of the last twenty-five years there hasn't been, I take it, a very great change in the fundamental set-up of the universities so far as research is concerned The general set-up is the same The activity along research lines and the facilities placed at the disposal of the staffs of the universities have, of course, been very greatly increased On the other hand, there has been a vast change in the set-up of industry to use the material at its disposal The period of the last quarter century in that field has been a period of intense experimentation in the means for applying knowledge. We have tried in various parts of industry all kinds of schemes and arrangements. In the early stages of the game we were handicapped not only by a lack of actual physical knowledge in certain directions, but we were more handicapped by a lack of trained scientific workers who had an appreciation of what was required fundamentally

To a large extent that has been gradually overcome In the organization of our groups to carry on industrial research work, we have tried all kinds of experiments not only in this country, but in the world as a whole, until the thing has practically come down at the present time to two types of industrial research organization One, the type which is conducted by an industry of such size that it can afford to maintain a completely equipped engineering research organization—and by " completely equipped " I mean not only to do work in physics and chemistry, backed up by mathematics, but to carry the work through the whole period of trial to the final commercial product This is the type of organization of which the laboratories of the great electrical industries are examples

Taken in its totality, the field for this type of research laboratory probably is not as great as the remainder of the field The world still carries on the bulk of its work through relatively small organizations If these organizations are to do their part in advancing the application of knowledge, they must supplement their own individual efforts by some form of cooperative undertaking They may do it by cooperation with the governmental organizations—the Bureau of Standards or the Bureau of Mines—or with some of the commercial research organizations that are beginning to spring up, or they may do it by cooperation among themselves, as is the case with the tanners, who established a joint research undertaking for the conduct of research in those fields which are of common interest to everybody in the tanning industry Such a cooperation does not vitiate the element of competition between individual concerns It intensifies competition because it puts everybody in possession of increased knowledge

Finally, coming back to the problem which I think confronts you gentlemen more than almost anybody else, there is the situation which is so vast in its entirety and so diverse in its ramifications that only by some form of general cooperative attack can we hope to make the progress which the world has the right to hope we should make It seems to me—and here I find myself in the same situation as Dr Kellogg, namely, that of knowing very little specifically about roads or road-building or the things involved in roads—that sooner or later, under the auspices of a group like this, we must work out a scheme of cooperative research involving all of the elements required in the whole problem of road-planning and road-building which will give to all of us, and through us to the population as a whole, the maximum benefits of the application of science to the art of road-building and maintenance We will then have set the stage for a perfectly enormous improvement in what I presume to call low-cost roads, roads where and for which we must use the materials at hand, since we cannot afford to import any considerable quantity of materials such as those we employ for higher-priced roads Even in the case of high-priced roads, much of the material for which obviously comes from great distances, I feel confident we can look for great improvements and cost reductions when we are in position to know more about the fundamental properties of our aggregate.

Now, just a forward word in closing, and I will step aside for the more important matters which have brought you together. That word is merely a reiteration of my belief that if you are going to make the most progress in this whole art of road-building—which has become one of our largest concerns, one of the biggest factors of importance to the population of this country—you must, I believe, find some mechanism by which you can bring actively into the research picture not only the highway engineer and the scientific man in the universities and laboratories, but you must bring in all of the other commercial factors which, whether you know it or not, are factors in your problem of attempting to solve the question of better roads It seems to me that the particular kind of a problem with which you are dealing is one which calls clearly for a cooperative attack on the fundamentals of the problem If this is so, I know of no better agency under which to bring about such cooperation than this very body of which you are a part, the National Research Council, which is known to be a national body without an ulterior motive of self-interest—a body designed for service along scientific lines for the population as a whole