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RESEARCH[1]

MAN'S search for information about his surroundings and about himself is as old as the race. He has been conscious of it since thought began. Only in degree, in precision, has our search changed in all the ages; now, in designating a very careful, very logical, extremely critical phase of this feature of our intellectual life we have come to use the word research.

A dog searches for a bone led by his senses and experience, influenced little, if at all, by what he has of reasoning ability. The morphologist searches for the reasons underlying the shape of the bone; the physiologist examines into its functions—both search with the aid of their highly developed reasoning powers, and their work we call research.

Whatever the details of the special case, research is a mental process superimposed upon the observation of facts. It is mechanical as well as rational, the two functions being equally important. Because it is a human activity it may be judged in terms of its usefulness as against its cost, cost being interpreted as human effort rather than as mere money expended. However, being wide enough to embrace the infinite multitude of observable facts, whether these are found under natural conditions or as the result of the artificial conditions we call experiment, and also being a product of the trained imagination whose every guess is legitimate if in harmony with the facts, research is not easily reduced to analysis. How is it possible to place a value on a product as intangible as a work of art? How can we say whether the effort that went to the making of it is justified or wasted? It is no easier to judge the value of the products of the play of the imagination on the facts of existence. It is perhaps still harder to judge the value of the effort that goes to the collection of a mass of minute facts, each trivial as the hammering of a nail into a plank, yet each contributing to knowledge. It is much easier to judge of visible products, thus, when the architect and the artisan are finished, the result of the interplay of imagination and detail stands before us and we can judge it according to our likes and dislikes, of our feeling of its fitness to fulfill its purpose as measured against its cost.

Because of its illusiveness, because of the enormous prizes it has brought to mankind, because of its value

[1] Presented at a joint meeting of the Rhode Island Sections of the American Chemical Society and the American Association of Textile Chemists and Colorists.

as a mental training, there is a tendency to be slipshod in criticizing research. Research is such an honest effort to achieve something of value that we are apt to condone the futility because of the good intention, or we may err the other way and condemn what is intangible because we can not measure its use.

I propose to you that together we examine the modes and the cost of research and see if we can not reach some conclusion regarding the enterprise as a function of human society. Research for our purpose is organized effort to acquire knowledge regarding natural phenomena. If we consider the research going forward in this country, we can divide it roughly into four groups; that carried out as a part of the intellectual program of institutes of learning, which we may call academic research simply because of its home, not because of its character. Next we may place together the activities of institutions founded especially to advance our knowledge by research. The work done in the industries we shall take for our third group and, finally, we shall make a fourth classification which we shall call professional research.

In all this work the effort can be classified in this way: materials and energy used, which includes the cost of the surroundings in which the research is carried out, of the equipment and of the energy consumed in the shape of heat, light and power. The man power—this involves not only the thinking brain conducting the research, but also the manipulative functions of the research man or those assisting him in the research. This would include what the industrialist calls executive overhead as well as labor assistants. Time, as a function of research, is really important, especially in the industrial field and should be considered in connection with all research.

Academic research has been the fountain from which the most important knowledge regarding our surroundings has come. There was a time when research was unknown outside the walls of the institute of learning or the private home of the learned man. Now-a-days the quantity of academic work is greater than ever before, and I think that we are not critical enough of it. Let us consider first of all the cost of research, which is obviously the most easily measured factor. In industry, where the cost of doing anything is checked very closely, it has been found that a trained research man whose salary is about $300 per month requires materials and man-power assistants costing $450 per month at least. That is to say, the research cost per hour, assuming a 200-hour month, is $4.00. The work done in the university costs less because, in view of the fact that the student doing his research gains by learning how he should approach a problem, he is paid nothing for the time he puts in, although he may work his way through by teaching part time for which he is paid. Assuming, and I think this is not assuming too much, that the incidentals cost just as much in academic work as they do in the industries, that is, the working space properly furnished, heated and lighted, the laboratory facilities, the administrative expenses, the time of the man assisting the student, we reach a figure of approximately $2.25 per hour, plus the effort of the worker. If a doctor's thesis takes 2 years of 9 months a year and the research occupies the candidate 8 hours a day during the academic year, then we reach a figure of $6,480 as the cost of a doctor's thesis. Multiply this by the number of researches of this kind being carried out and we find that the people of the country are spending millions of dollars on academic research of this type alone. Any industrial firm spending such an enormous sum would be highly critical of the results obtained.

I suggest that academic research is not properly scrutinized. We regard it so much as one of the steps in the course perfecting a student that we are apt to ignore its intrinsic value. Now I am not going to argue that the training the student gets is not of primary consideration, although I think that it is less important than it is sometimes thought. I am also willing to concede that the student is doing his first research work and is therefore much less competent than the older research worker of the industry, yet when everything is taken into account it seems to me that we could get very much more from the work than we do.

A destructive criticism is easy. On the constructive side I suggest that research problems can only be chosen by men who have a research instinct; by men who are following up a lead which may mean a real advance in our knowledge. Such men are rare and therefore my first change would be in limiting the number of academic institutions in which research is done for advanced degrees. This calls for a great unselfishness, while I am afraid that inevitably selfishness is characteristic of the attitude of the academic body to its students, perhaps unconscious, but arising out of the situation. Thus if a senior has shown great promise it is natural for the graduate school to try to keep him when they should send him to another university where he will find the man best able to lead him on in the lines which he has chosen. Of course, if the graduate school of his own university can conscientiously hold him because they believe they can give him the best that there is in the country, then they are justified in doing so, but they should be extremely critical.

In furthering this improvement I should like to see the undergraduates acquire some critical faculty of their own, just as they do in Europe. This, I believe, can be brought about only by getting away from the idea that the university is merely a finishing up of an ordinary education and by adopting the European belief that it is a great advantage to move from one university to another, which can be done there without loss of effort.. In that way the student encounters different presentations of the same subject, and he learns to acquire a certain discrimination which seems totally lacking in the student's attitude to research in this country. When it comes time for him to do his doctor's work, he should be quite clear in mind that such and such a university, because of the research ability of the professor in charge of a single branch, is the only place in the country for him to go. Since he can work his way about as easily in one place as another, there seems no reason why we should not be able to foster this procedure.

I should like to see a definite stand taken against the point of view that because a conscientious young instructor has been promoted on account of his teaching ability to an assistant professorship, he is therefore entitled to experiment on graduate students. Unless the young man has, by his own work, established the intrinsic merit of his attitude to research and his capacity for initiating research, he should not be allowed to act without the advice and direction of a maturer research man. On the other hand, it should be recognized much more quickly than is often the case when the young man is a more brilliant research man than the head of the department, and then the head of the department should be honest enough to turn over his best students to the assistant professor.

From my experience of industry and of academic research, I do not believe it possible for a man to function at the same time both as an executive of a large establishment, as a teacher and as a director of research. I do not believe it can be done, except by a great sacrifice of the highest attribute of the man, that most delicately balanced function of the mind which is the guiding spirit of research. Therefore, I should like to reiterate what has been said so often, both by myself and others, that we should not reward research by executive responsibility and that we should relieve the true research man from the round of ordinary teaching and let him build up a research school fed by students from the countrywide, sent by his colleagues and the well-informed opinion of the student body.

I know that we argue for the complete independence of the research man and yet I should like to see a more collective effort made to unify the research effort of the country. A young teacher, having carried out a small research problem which may have been a very secondary feature of a larger problem given him by his research master, starts in to do research for himself. His mind is led, very naturally, to some little detail arising from the work he has done. That is good for him, but his view is too narrow and he is quite remarkable if he does not overestimate what is really quite trivial. If we carried our national effort further and placed before our research men problems which might appear worth attacking because of their relation to still larger problems of importance in the opinion of the great research men in the field, I believe that these men would be tolerant enough to avoid the danger of stifling a new line thought out by a young man and yet we should have the advantage of far less wasted effort than we have at present, because I think we stretch much too far our sympathy with the piece of research which is just one more little pebble in the palace of knowledge. We are too apt to encourage the collection of pebbles to put around the flower borders and grounds instead of hewn stone to build into new wings.

Even assuming an excellent subject for a doctor's thesis it still may remain true that there is a waste. In industrial work the importance of time is stressed constantly, perhaps it is over-emphasized. In academic work we have accepted the doctrine that accuracy is so much in danger of being sacrificed if an effort is made to speed up the work that we lean the other way. I think we should remember that the real leaders of research, while they may have taken a great deal of time before they felt their results sure enough to present them to their colleagues, did actually work very fast in getting the evidence together. I remember that when his friends urged my old teacher Wislicenus to hurry up his publications for fear of losing his priority, he paid no attention whatsoever but went along gathering the data necessary. At last he would publish and his paper would, perhaps, fill a whole volume of the *Annalen*. His contribution was so finished, was so profound, that the matter of priority ceased to bother anybody, but from this it would be quite unfair to argue that he wasted time. Actually he worked very rapidly and proceeded from step to step with a certainty that was most economical of effort.

Of course the young research man on his first problem is bound to waste a lot of time, but let us show him how to economize his effort and let us, above all things, point out to him that his loss of time is something which marks his immaturity rather than being the proper attitude toward the work he is doing. A young man may be able to observe only one reaction at a time, whereas a man with experience and a higher critical faculty may undertake six parallel experiments with success, but let the young man do as much as possible and do not encourage him to think that it is the essence of research to watch the pot boil. In this connection I think that it would be wisdom to devote a little more of the money available for research work to amplifying the apparatus available and to furnishing manual help so that dish washing might not be a necessary part of the research worker's time consumption. I always remember the contrast that was presented in the attack on water analysis by the regular class in this subject at one of the universities at which I taught and by the government analyst on the same work. The students carried out about two complete analyses in a semester, giving six hours a week of laboratory time to it. The expert carried out, as I remember it, something between 10 and 15 analyses in parallel and was busy every instant of the time. It would be impossible to expect such a high technique from students, but it is reasonable to demand the best they can give.

In substance, then, my criticism of academic research is that it fails of being what it should, because the subjects chosen are not well selected, the time spent is out of all proportion to the results, and the effort is not sufficiently coordinate.

My criticism I want to be taken as constructive, because I am, in reality, thoroughly in accord with the belief that academic research is fundamental to the success of the race. It is on this account that I am glad to think that owing to the realization by those directly engaged in industry of the importance of the work done by their own research men, we shall see eventually a very great encouragement of basic research by men of wealth. Already these men have found it possible to express their interest by giving magnificent laboratories, but they have not yet found a means of doing that which they realize is still more important, fostering the man of genius. Obviously, it would only be the part of stupidity to feel that magnificent walls are a more permanent contribution and a fitter monument than a share in deeply significant work. The rich man knows well that were he to succeed in raising the status of the scientist he would be wonderfully rewarded. At present there is no very obvious way of doing this. Unquestionably our great research men should be able to look forward to earning salaries of $25,000 and over. We must admit that our society is built up on success as measured in terms of money, and therefore a great man should have the satisfaction of independence, together with the stimulation of feeling that society has awarded him a position of success.

It is true that industrial scientists are not paid any such munificent salaries, yet they are placed in a position to participate in industrial success to an extent unknown in academic circles. If a university were making 25 per cent. clear on its investment and the merit of this financial success were traced in large part to special men on the teaching staff whose outstanding genius drew the large student body, it would be only fair to share the profit with them to some extent. Actually this is practically what was done in Germany and it has resulted in the social status of the professor being all that he could ask and has made him relatively rich. It also introduced the factor of competition among professors, which is most excellent though practically unknown in this country. How can we make it possible that our great scientists should realize such success? I do not believe in tying up large endowments with particular chairs, because frequently the surroundings may be such that they prevent the best man available from being secured or the move essential to accepting the new chair may tear him from friendly surroundings which contribute largely to his success as a scientist. Again I say I do believe in extremely handsome salaries as a reward of enlarging the domain of our basic knowledge and this irrespective of the apparent utility of the discoveries. Surely we may count on enlightened response to this problem by the wealthy who have before them the admirable example set by Nobel, who, you will remember, stipulated that his prizes should not be awarded were there no worthy recipient found.

Industrial Research

Industrial research I define broadly as all research paid for by the owners of our industries, research being taken to mean anything out of the ordinary in the way of control work, as well as such utilization of basic knowledge as is necessary for greater economy in production. I am willing to call research even that type of work which is no more than intelligent works control.

Research is just as imperative to the existence of a large producing corporation as it is to the growth of science. That fact is generally recognized in this country. It is not so taken for granted, but it is no less true, that research is equally as important to the small concern. Unfortunately the small industrial unit does not feel that it can afford research. Actually the stockholders would find it a most profitable investment were they to plow back some of their profits into research. However, there is every need for caution in this matter. Usually it is true that the more isolated the research chemist the maturer he ought to be. I am a thorough believer in the genius of youth, but as an independent research worker in surroundings which limit him strictly and force a great deal of routine upon him as well as a necessity for impressing owner-managers, the very young chemist can not compare with one of many years' experience. It follows, then, that the smaller the company the more experienced should be their chemists, but good experienced men are necessarily more expensive than young ones, and the small company on deciding to choose a research chemist usually picks one unsuited to its needs. They will pay a man $175 a month when they need the experience that makes a man worth $125 more than that. The extra salary would make the whole outlay a very much better investment if they could but be brought to see it; an investment that would carry itself easily. Another cause of trouble and unproductive expense is that a man hired to do research by directors or owners having but little knowledge of the real meaning of such work is put by them into a position calling for a great deal of routine, so much that real research is out of the question. This discourages the chemist and disappoints his employers who consider research a failure because they have had only bills to pay and can see no return.

In the case of large corporations conditions are very different. They can afford research on a very generous scale. Such research has to pay its way and to say this does not mean that directors are blind to the value of research even though there is no immediate return from it. It means that the money of the stockholders must be spent profitably under the conditions existing at the moment. It means that the research must have a direct purpose in view; it is not enough that it should extend our knowledge even though in that field of enterprise in which the industry grows.

It is evident that herein lies the very most difficult problem of the research director. In a large establishment he must delegate much of the detail to the senior men under him, but the choice of what is done must rest with him. He must decide, in cooperation with his maturest men, whether a problem should be taken up fundamentally or empirically. Often a piece of work will be productive much sooner and will cost much less if it is treated from basic principles. For example, it may be that the establishment of the relation between certain fundamental physical constants and observed results when once correlated will enable the research man to formulate a general statement covering all cases in point and enable him to go ahead much more quickly than were he to try to find the most suitable material for his purpose by trial. On the other hand, it may be very much cheaper, though essentially less satisfactory, to learn by trial. It all depends on the problem in relation to the industrial requirement. This is the very striking difference between academic and industrial research. It is one the young man, fresh from the university where he has been drilled in the importance of fundamentals, is very apt to ignore, with the consequence that he thinks ill of research done as he finds he is told to do it, he grows careless or he insists on going his own way and gets into trouble in either case.

In a sense research that can not go to fundamentals and yet must bring results is more of an art than a science. It is just on that account that it calls for a very high order of scientific training, otherwise it will degenerate into unproductive pottering, that worst curse of the research laboratory. It takes patience and a good training to carry out work with the utmost elaboration, but it takes patience, training, experience and a certain type of sagacity if a man is to use his faculties as a guide and follow the scent without stopping to make sure of anything.

I think that on the whole the work done by the industries, as far as subject-matter is concerned, is fairly satisfactory, more so, perhaps, than academic research. Every now and then we run across some absurdity carried out by a research worker who has forgotten his elementary chemistry and physics and decides to prove or disprove some fact by experiment when he should be able to reason it out with pen, paper and a text-book. I think also that on the whole industry utilizes its man power satisfactorily. I think that the rewards coming to the research worker in industry are on the whole adequate. If I have any criticism it is that the industrial concern does not reward sufficiently the acquisition of experience that is invaluable; by that I mean that the knowledge acquired by a good man, after ten years as a specialist, can not be replaced unless the firm is lucky enough to have a subordinate growing up to fill the position. Such a man should be made comfortable, should be kept satisfied, in order that he may do his best. I am sure that the employers of research men are not sufficiently alert to the importance of preventing occupational stagnation if good research is to be done. Every research man should have a chance to get away from his working surroundings and meet other scientific men at least once a year, quite without jeopardizing his vacation. He should be encouraged to develop as a man of culture as well as a specialist and on this account should he desire an extension of the two weeks' vacation in order to travel, such a request should be given sympathetic hearing. Unless a research worker is growing in all directions mentally he is not fully efficient.

Turning to the time factor I think it fair to say on the whole that the industry is apt to force research to be more hurried than is wise, but I am rather slow to make this statement because after a considerable experience I realize that there are a great many factors other than the mere acquisition of the information which must be taken into account. Let us say that a research laboratory has developed a method on a small scale for making a product which promises to be very profitable. The directors of the company believe that by an expenditure of a million dollars and provided they can begin production in six months they can obtain a handsome return on their investment. Plans for the plant must be rushed, but plans depend, in the case of chemical plants, on the equipment to be housed and the equipment depends on the details of the process as carried out on a large scale. Naturally the research necessary to put the process into shape for large scale production is rushed to an unhealthy extent and in consequence it is not unusual to find that a good deal of the equipment has to be scrapped later. But this does not mean that the hurry was unwise. It may be that in the long run the stockholders benefit more by the speed with which the work goes forward, even counting the loss of the equipment, than they would if time were taken to put the process into excellent shape. One must remember that to do a thing perfectly from one point of view is not necessarily the best commercial procedure.

Of that work which is done in the research institutions of the country I am unable to say much because I have no direct experience. I believe that an institute like that founded by Rockefeller is one of the nation's greatest assets, largely because its contributions to medicine are international and tend to draw together the peoples of the world. As long as such institutes are in the hands of scientists of genius they will be of the utmost importance to us. They are expensive only when looked at from the narrow point of view of dollars and cents.

The professional men of this country are doing a great deal of good research. They do this by observing carefully during their contacts with conditions as they find them. They are taking the place, to some extent, of the rich amateur who at one time was the large contributor to scientific progress. The difference is that the work of these men of to-day is more directly related to the practical use of science than was the work of men like Cavendish. Work of this kind carried out by physicians may flower into research work as richly endowed as that carried out at the Mayo Foundation, which has become a center of biological chemical research.

In looking over the whole field of research in this country we should be satisfied that we are putting so much effort into so useful an endeavor. We should not, however, be satisfied with things as they are and research, like everything else, must grow, must develop, if it is to mean all that it should to us. What we should strive to bear in mind is the truth of the statement made by John Milton:

> Our greatness will appear
> Then most conspicuous, when
> great things of small,
> Useful of hurtful, prosperous of adverse,
> We can create.

R. E. ROSE

WILMINGTON, DELAWARE

A LITER AND A HALF OF BRAINS[1]

A LITER and a half is our portion: so much of it water that Hippocrates called the brain the metropolis of humidity and Sir Thomas Browne noted that, in consequence, skulls are less consumed by fire than other bones.[2] Doubtless there are other functions of the brain for us to consider than that of rescuing the skull from a fiery oblivion.

How greatly we extol ourselves above the brutes which perish. Each of us, lords of creation, maintains within his skull as much brains as would fill the heads of three gorillas. And very gravely we are told that for our bulk one third of all this mass would suffice; the rest is sheer intellect "which some suppose the soul's frail dwelling house."[3] But this is mere convention and with the times conventions change. In Aristotle's day the brain was not, as some have said, the seat of sensation and of thought: it is required rather to cool the blood and by thus tempering its heat to make sensation possible. Because man is the hottest of animals therefore he has the largest brain.[4]

> I talk of dreams,
> Which are but children of an idle brain,
> Begot of nothing but vain fantasy,
> Which is as thin of substance as the air,
> And more inconstant than the wind.[5]

But enough. Aristotle was born about 384 B. C., when Plato was already forty-three and Socrates had been dead fifteen years. The foundations of a study

[1] An address delivered at the annual banquet of the American College of Physicians, Cleveland, February 24, 1927.

[2] Sir Thomas Browne, "Hydriotaphia." D. Lloyd Roberts, ed. London, 1898, p. 277.

[3] King John, V. 7. 3.

[4] Stocks, J. L., "Aristotelianism." London and New York, 1927, p. 76.

[5] "Romeo and Juliet," I, 4, 95.