SOME ASPECTS OF SCIENTIFIC RESEARCH[1]

So much has been said of late on the subject of scientific research, its value to science, to the industries of the country and to the War Department, that it would seem fitting to use the first meeting of our club for a discussion of certain phases of this subject. Furthermore, research is an appropriate topic for discussion in our meetings since the Faculty Science Club was organized for the purpose of bringing each member into closer touch with the recent advances and research of the different fields of scientific endeavor.

Scientific research is, by the general public, one of the least understood and therefore least appreciated departments of science. The American people have been comparatively slow to recognize the value of the deeper and more fundamental researches in science. The national trait of desiring quick returns with a minimum expenditure of time and money has led to a certain superficial empiricism, which has gone under the garb of research. This empirical testing is even now the predominant principle in most of the so-called research laboratories of our factories and industrial plants. Even in our agricultural experiment stations, I venture to say that the major portion of the work is either routine or of the cut-and-dry type, without reference to fundamental principles.

Scientific research has perhaps as varied a meaning among scientists as ethics has in the field of law and jurisprudence. To illustrate, I might cite a paragraph from a recommendation of an applicant for the position now vacant in my department. The employer states concerning Mr. X that

> His particular work, aside from some analytical work, has been the care and conduct of the State Food Exhibit, and he has done good research work in the taking of ice-cream samples, which has been of great value to the department.

To some the term research is so comprehensive that it might properly be applied to a man's search for the best trail leading to the summit of a mountain peak, or to the prospector's investigation of the slopes of Mount Shasta. To me research means something entirely different. The personal contact with some of the leading research men of the day and an acquaintance with the writings and views of others of their type, have moulded a definite concept in my mind of the meaning of this term. To another this concept might seem erroneous, and consequently it behooves me to exercise a little charitable tolerance until I or he will have new light thrown upon the subject.

Scientific research is the slow, laborious process of laying bare, one by one, the facts and truths of nature, which have a definite bearing upon the fundamental and general principles involved in the problem. The isolation of a new chemical compound, the invention of a machine or piece of apparatus, or the discovery of a new force in nature would not necessarily be research. Only as these are units in the larger and more fundamental problem could they be included under that head. An illustration may make this point a little clearer: Some fifteen years ago Professor H. N. Morse, of Johns Hopkins University, undertook the problem of determining whether the osmotic pressure of solutions obeys the laws of Boyle, Gay Lussac and Avogadro for gases. He assumed that this could be demonstrated inside of two or three years. He learned, however, that he was unable to make or procure one satisfactory osmotic pressure cell the first year, but during that period worked out the proper clay mixtures for such cells and the methods for purifying the clays and moulding and burning the

[1] Address by the president of the Faculty Science Club of the University of Nevada, read at the opening session, September 23, 1915.

cells. The second year was occupied largely in working out and depositing a satisfactory semipermeable membrane in the cells, the third year, in making a sufficiently strong and accurate manometer and the means of joining the manometer to the cells. The following two years were required for constructing constant temperature rooms and baths where osmotic-pressure readings could be taken without temperature fluctuations in the cells. It was then five or six years from the time he started the work until he could make the first reliable set of osmotic pressure readings. During all this time Professor Morse was assisted by one or two men besides the three or four graduate students who worked with him each year. The following eight or ten years he made osmotic-pressure observations with glucose and cane sugar in water solutions at temperatures from 0 to 80 degrees.

He has finally established that the osmotic pressure of dilute solutions obeys the gas laws. Each individual unit of this great work, upon which the score or more candidates received their doctor's degree, could only be called research work when considered as a part of the general problem. Each man's contribution was of course a separate piece of original investigation.

The German army, more than any other agency, is now forcing upon the world the value of chemical research. It is the German chemists who have won the battles in Russia, Belgium and France, and the United States is now sitting up and taking notice, with the result, we hope, of finally getting the recognition that this branch of science deserves. Secretary Daniels is becoming aware of this, for he is recently reported to have said:

> The time was, that when we thought of battles, we thought of men. We were told by great leaders who had not looked into the future that the nations with the most men would win. Now it is not men, it is munitions and inventions, and to-morrow it will be neither—it will be chemistry.

The reason why the chemist has not received popular recognition like the physician, the engineer, the physicist or the geologist is that the activity of the chemist is outside the realm of comprehension of the average individual and he sees nothing imposing or spectacular with which to associate the chemist. He may admire the delicate shades of his wife's costume without considering what part the chemist had in producing them, and every day of his life he comes in contact with something or other upon which the chemist has left his finger prints.

The greatest problem confronting the profession to-day is that of getting recognition and support from the public, through its legislative bodies, but, as I mentioned before, the European war has done more than any one thing to secure the desired recognition.

The question has doubtless arisen in your minds, why it is that scientific research in Germany and other European countries occupies a higher plane than in America. To me the reason is obvious, and it is this: In Germany there is popular recognition for the research man. Everybody knows his worth to the state and to civilization. The manufacturer is especially appreciative of his work and is glad to cooperate even to his own disadvantage and loss in trying out certain processes or marketing given chemical substances whose demand may be very limited. Four out of five such ventures may be a loss to the manufacturer, but the fifth proves such a success that it overbalances the other four, and therefore if the manufacturer had not accredited the research work of the one who made the propositions, not one of the ventures would have been tried. This popular confidence in the value of chemical research has led to al-

most complete autonomy in the departments concerned.

In Germany the chief chemist is his own boss. He engages his assistants, fixes their compensation, engages and discharges janitors and laboratory helpers. He makes changes in the building and the laboratory with but nominal supervision. He apportions his funds according to his own ideas and is virtually his own administrator.

What the research man in this country needs more than anything else in order to make his work efficient, is freedom from restraint and petty annoyances. He should be made to feel that he has at least a part in the general organization and progress of the institution. Oftentimes he feels, and with a considerable degree of justification, that his department exists only by the gracious magnanimity of the administrators, whose knowledge of his work may be very limited.

Professor W. H. Walker at a joint meeting of three chemical societies in New York last winter made the following statement:

> My plea at this time is not so much for greater generosity on the part of the employer in matters of laboratory facilities, special equipment or a good library, however important these are, but rather for a larger appreciation of the conditions which make for ultimate success in research work.

In the same vein, Professor Arthur D. Little, speaking before the United States Chamber of Commerce, says:

> The plain underlying reason why we have been unable during thirty years of tariff protection to develop in this country an independent and self-contained coal tar color industry while during the same period the Germans have magnificently succeeded is to be found in the failure of our manufacturers and capitalists to realize the creative power and earning capacity of industrial research. This power and this capacity have been recognized by Germany and on them as corner stones her industries are based.

Aside from the question of recognition and support for the research man, another factor enters in, upon which the effectiveness of his work largely depends, and that factor is the time allotted to his work.

In this country there are very few independent research institutions and for that reason the major portion of scientific research is carried out in the universities and agricultural experiment stations. Everybody recognizes that teaching and research should go hand in hand and that no university professor fulfils his obligations unless he is doing some original investigation tending to advance human knowledge. This is all well and good, but are the colleges and smaller universities of the country allowing sufficient time to their professors for such work? How much creditable research could a professor carry out in the course of a year who is obliged to teach twelve to eighteen hours per week with an additional twelve or more hours in the preparation for his work? Young, enthusiastic professors have tried it over and over again, but with the same result—a stupendous failure—as far as the research goes.

The professor who has spent his energies in the classroom during the day is in no way fitted to continue his research problem in the evening, as many of them do. A neglect to observe the proper requirements for rest and relaxation will immediately tell upon the quality as well as the quantity of work produced. Consequently the college or small university can never hope to produce but an insignificant amount of research work, and this fact is recognized by President Woodward, of the Carnegie Institution at Washington, and by other administrators of research funds. It is very rare that a college professor gets a grant from such a fund, and for the very reason mentioned above.

The productive research workers in the country to-day are those who are devoting their whole time or practically their whole

time to that work. As a rule, the head professors in the larger universities are not giving more than one to five hours of lectures during the week, the rest of their time being devoted to research, while a large number of them have one or two private research assistants besides the candidates for degrees doing research work. The same is true in the European universities.

There are many other activities besides teaching that may seriously interfere with a man's productive capacity in research. The public demand for something spectacular that may be flaunted in the daily press sometimes prevails upon the scientist to forsake modest but meritorious investigation. The bid for popularity may even carry a man so far away from his department that no time at all remains for research. Furthermore, numerous cases are on record where good research men have been spoiled by promotions to official positions, so that their energies become dissipated in a mass of official detail instead of concentrated upon some one problem for solution.

Professor W. E. Castle in speaking of research establishments and the universities says:

> The attempt to combine teaching with research has another indirect but evil consequence. The periods which the professor can himself devote to research are intermittent and fragmentary. This affects disadvantageously the topics selected for investigation. They too must be minor and fragmentary. Great fundamental questions requiring long continued and uninterrupted investigation can not be attacked with any hope of success by one who has only an occasional day or a summer vacation to devote to research.

Also quoting a paragraph from Professor Woodward's report of the Carnegie Institution at Washington for last year. As regards the conditions favorable to research he says

> that fruitful research entails, in general, prolonged and arduous, if not exhaustive labor for which all of the investigator's time is none too much. Little productive work in this line may be expected from those who are absorbingly preoccupied with other affairs. Herein, as well as in other vocations, it is difficult to serve two or more exacting masters.

Another serious impediment to scientific research may be found in a too perfect organization for the handling of routine affairs connected with such work. In common parlance this perfect organization has been nicknamed "red tape." Now it sometimes happens that the red tape reels off smoothly and rapidly, but dare I say that more often it is thrown into kinks and snarls when the reeling stops. The phenomenon has doubtless been experienced to a greater or less degree by every one, but to conjure up pleasant memories, let me hypothesize as follows: *A* is a research chemist. He has discovered a new chemical compound which is rather unstable. He requires a certain chemical that will combine with the new compound and render it stable so that it can be investigated further. The requisition for the purchase of the chemical goes to *B*-check, then to *C*-check, then to *D*-check and finally to *E*-check, whereupon the chemical is ordered and within a short time delivered to *A*, greatly to his delight.

Next let us suppose that *A* is a research biologist who has just discovered a new form of marine life. He makes out a requisition for the purchase of a suitable stain or preservative and sends it to *B*. He learns, however, that *B* has gone fishing and the requisition rests. *B* returns in the course of time, checks the document and sends it to *C*. *C* has been unavoidably called away by the death of a close relative and the requisition is deposited to bide its time. Once released and checked by *C*, it is also checked at *D*, but for a good and valid reason is pigeon-holed at *E* for a few

days. The order finally goes, but when the material arrives the little stranger for whom it was intended has been dead and buried eight weeks and the discoverer *A*, whose fame might have been noised abroad in this connection, goes down to his grave untoasted, unhonored and unsung.

Any system, however perfect, that fails to provide for an emergency is worse than no system at all. As soon as a research man becomes tied down by arbitrary rules, whether they be called systems, organizations or what not, that soon his creative powers and effectiveness will be diminished. In this connection I perhaps could do no better in emphasizing my conviction than to quote a paragraph or two from Professor R. S. Lillie's Founder's Day Address at Clark University last winter.

> When we look at our universities we are impressed with certain obvious peculiarities—their size, their wealth, the variety and complexity of their activities and of their organization. We may agree that size and wealth with the resources that they bring are all very well—in themselves desirable—but complexity of organization, and the practises and tendencies that go with it? Are these conducive to the intellectual life? This, in my opinion, is the critical question. So far from our taking this for granted there is good reason to believe that beyond a certain limit dependence on system and organization in institutions of learning is directly injurious to good work, and this for the simple reason that it makes for the stereotyping of activities, and hence interferes with freedom and its expression, which is originality. Such restriction, in fact, is the general purpose of organization: it aims at diminishing variation from an accepted norm. Now the more stereotyped certain things are the better; thus a railway service or a department store can not be too regular and dependable; but if our aim is not simply to repeat things that have already been done, but to discover new truth, the conditions that surround us, as well as our own temper of mind, should so far as possible encourage independent activity, and not simply that carried out in accordance with a program. In brief, purely routine activities should be subordinated in an institution of higher learning: all needless machinery should be disposed of, and the rest should be relegated to its proper place. This is a practical suggestion, and it is one of the first that I should make.

Lastly, I would like to consider a little more in detail the status of the research work in our agricultural experiment stations. Scientific work is sometimes very incorrectly and superficially judged by individuals or small groups of individuals. A meritorious piece of work may not receive immediate recognition, but will hibernate in the archives of some musty library for decades before it bounces forth in its full splendor. Nevertheless its status will soon be known, after having received due consideration by the scientists of the world.

From time to time various attempts are made to segregate and classify worthy and illustrious individuals in science, and it would be interesting to see what place the experiment station worker occupies in such segregations. From Professor Pickering's tabulation of eminent scientists (*The Popular Science Monthly*, February, 1915), it will be seen, that among the ten Americans who have been accorded the distinction of being elected foreign associates of two or more of the leading scientific societies of the world, there is no one who has been connected with an agricultural experiment station, but they are all research men who have devoted little or no time to teaching. It also appears that Norway and Sweden, with a combined population of less than eight million have produced nine scientists of the same distinction.

The Nobel Institute at Stockholm, Sweden, awards five prizes each year, three of which are for the most meritorious accomplishments in physics, chemistry and medicine. Among the forty or more who have received this recognition are two Americans, eminent research men, but neither of whom is a station man, and not

even a chemist. It should be said that only the chemists of the experiment stations would be eligible to these awards, but there are several hundred of them and a large number have now had ample time in which to establish the character of their work.

In this connection I wish to state, very emphatically, that I am not decrying all the scientific work of the stations. By no means. A great deal of it is of a high character and is becoming more and more so as time goes on. Some stations have manifested a marked improvement in the character of their work during the past four or five years, and it is to be hoped that our station will not be obliged to take a backward step. Why do you suppose our men of science in the agricultural experiment stations are not found among the fellows of the Royal Society of London or among the foreign members of the French Academy? Why is it that they are not in evidence in the Berlin Academy, or even in our own National Academy of Sciences?

The twenty-two men recently chosen on the Naval Advisory Board are essentially research men and inventors. The two men chosen by the American Chemical Society, Dr. Whitney and Dr. Baekeland are research chemists in the true sense of the word. It is obvious that the experiment station men have not yet risen to the rank entitling them to places in the notable segregations of the truly scientific men of the world. Exceptions of course must be made to the representations in "Who's Who?" and to pay-as-you-enter classifications.

There are now nearly seventeen hundred agricultural experiment-station workers in this country and their combined productivity is something enormous when quantity is considered. Their opportunities for producing genuine and fundamental research could be as great as those of the professors of the larger universities if they had the training and the concept of the deeper problems of nature and were not led astray by the tyro and dilettante who is invariably imbued with the get-rich-quick idea, although he uses the much more elegant and suggestive term "practical." The cry in the experiment stations is for something practical, not realizing that the most fundamental is the most practical in the long run.

Our stations are organized to benefit the farmer, but when we accord to the farmer the privilege of deciding what work is practical and what is not, and what problems should be undertaken and what ones should be dropped, we are committing a grave error. The pedestrian journeying along the road may properly express his opinion about the desirability of having a bridge built to span the stream but when he proposes to direct the engineer regarding the location or type of the bridge or even regarding the feasibility of having a bridge built at all, he is overstepping his bounds; but no more so than when the farmer frames the problem for the research man. Valuable suggestions regarding desired results may often be obtained from the layman, but the trained expert is the best judge whether or not the "practical" problem is practicable.

The fact that the experiment-station worker must cater to public sentiment is one of the main reasons for his failure to occupy the respected places in truly scientific circles. It is to be hoped that the farmer will acquire a greater degree of tolerance for the technical, the obscure and to him unintelligible, and when that day comes we hope that this admirable branch of science will be elevated to its proper place. Then, truly scientific men who have had the misfortune to become enmeshed in the agricultural experiment stations will

not need to be buried alive, but will stand an equal chance with their fellows in other departments of science.

The matter of training for research work has only been alluded to indirectly but I have assumed as self-evident that only the best kind of training suffices for the highest type of research. Charlatans are found in every department of science and administrators of research funds must be on their guard against their plausible but evanescent schemes. Men of this class are usually much better talkers than experimenters.

Having pointed out some of the weaknesses in research work of our agricultural experiment stations, I would like to offer a suggestion that might prove beneficial. In my opinion there is not enough expert counsel and supervision over the departmental work that is being carried on. The work in almost every field of industry is inspected and criticized, some time or other by experts efficiently trained in the departments concerned. No large engineering or construction work could proceed efficiently without expert supervision and oftentimes outside counsel and advice, and I venture to say that if this were secured for the departments of the agricultural experiment stations it would do much toward improving the character of the work and giving directors more reliable information regarding the work of their departments, in which they themselves are untrained.

It seems to me that a chemist should inspect the chemical work of a station, a botanist the botanical work, an entomologist the entomological work, and so on. The cost of inspection would thus be considerably higher than at present, but I am inclined to believe that the value to the stations would greatly overbalance this and bring departmental work up to the standard required of them. Incidentally, favorable reports from technically trained inspectors would greatly influence popular opinion concerning a given piece of work and act as a bulwark to the director in meeting outside criticism. Such inspections would also materially aid in ridding the stations of the superannuated, derelicts and driftwood which are such impediments to progress.

Another suggestion I should make is that research projects of whatever nature should be passed upon or suggested by a committee of men technically trained in the fields in which the projects are to be launched. It is not to be expected that directors and officials at Washington are competent to judge of the feasibility of a given project for research, especially when it lies outside the circumference of their own training. In my opinion such a system would result in much good to the stations and to the people at large.

To sum up, let me say that the scientific research of this country and especially of the agricultural experiment stations, has not yet reached the high standard that is possible of attainment, and that the reasons would seem to be the following: a popular disregard or lack of appreciation for research; the encroachment upon the time of the research man by teaching, outside and official work; annoyances and distractions through the business and administrative organizations; the popular demand for practical and control work rather than for the fundamentally scientific.

These unfavorable conditions could easily be remedied; and then by requiring of the research man a more thorough training, and giving him some reliable counsel, the character of his research work would unquestionably advance.

Let me close by quoting Director A. C. True's advice, which as seed, I hope, will not fall by the way side, or among thorns, or upon stony ground, but in rich black

loam where it may grow and bring forth the desired harvest. He says:

Words of friendly criticism may be as silver, but far better are golden words of encouragement.

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THE U. S. FISHERIES BIOLOGICAL STATION AT WOODS HOLE

THE laboratory of the U. S. Fisheries Biological Station at Woods Hole, Mass., was open from June 21 to September 15 during the past summer. P. H. Mitchell, of Brown University, was director. Investigators appointed by the Bureau of Fisheries conducted the following researches bearing on the economics of the fishing industries: I. A. Field, of Clark College, the anatomy of the circulatory and nervous systems and the embryology of the edible muscle; C. W. Hahn, of the High School of Commerce, the mode of infection by, and the life history of several parasites of herring, alewives and some other food fishes; A. Kuntz, of the Washington University Medical School, with L. Radcliffe, of the U. S. Bureau of Fisheries, the identification and study of the embryological and larval stages of twelve species of common fishes; E. Linton, of Washington and Jefferson College, investigations of various fish parasites with special study of helminth and nematode parasites of butter fish, also a study of the food of winter flounders; P. H. Mitchell and W. W. Browne, of the College of City of New York, nutrition of oysters with special reference to conditions of glycogen formation; S. Morgulis, of the College of Physicians and Surgeons, the digestive enzymes of Teleosts, the changes in weight and composition of starving lobsters, a critical analysis of Moore's investigations on the metabolism of marine organisms, and a colorimetric method for approximate oxygen determinations in sea-water; G. G. Scott, of the College of the City of New York, the oxygen consumption of developing fishes at various stages, the oxygen consumption of 42 marine forms for comparison of rates of metabolism, the efficiency of various means of aerating aquaria, conditions affecting the oxygen requirements of fishes, the oxygen consumption of regenerating tissues, and the dry method of shipping live fishes; A. Thomas, of Clark University, the toxic effect of heavy metals on fishes; G. F. White, of Clark University, methods of preparing dried dogfish for human food, the distribution of nitrogen in dog fish muscle, the phosphatides of dogfish egg-oil, the collagenous matter of dogfish skulls and of tilefish swim-bladders; W. W. Browne, of the College of the City of New York, the possibilities for fish to act as carriers of pollution bacteria and the time required to rid fish of such bacteria when put in unpolluted water; B. H. Gross, the conditions affecting the occurrence of color in "green oysters"; K. S. Rice, of Brown University, the behavior of oyster spat under artificial conditions, and the methods of ridding oysters of the colored copper-containing compound found in "green oysters."

Besides the work of employees of the bureau, a number of investigations were conducted by table applicants to whom the facilities of the laboratory were extended. Such researches were as follows: R. P. Bigelow, of the Massachusetts Institute of Technology, an examination and study of 27 species of Crustacea collected by the *Albatross* during the Philippine expedition; S. R. Clemence, of the American Museum of Natural History, a survey of the reptilian and batrachian fauna of the Elizabeth Island; G. A. MacCallum, observations on fish parasites; G. H. Parker, of Harvard University, a study of reflexes and other nerve reactions of Cœlenterates; A. C. Redfield, of Harvard University, the control of chromatophores in *Fundulus* embryos, in flounders and in horned toads; E. A. Redfield, of Harvard University, the movements of shell and mantle in Lammellibranchs and the relation of such movements to respiration; I. L. Shaw, studies of diatoms; J. M. Thorrington and F. P. Reagan, of Princeton University, the development of hybrids with especial reference to the vascular system; H. C. Tracy, of Marquette University, the relation of the swim-bladder to the ear and the eighth nerve in *clupeidæ*; G. B. Wislocki, of Johns Hopkins University, the internal secretions of fishes.