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SPECIALIZATION AND COOPERATION IN SCIENTIFIC RESEARCH[1]

THE three great functions of a university are to train young people in the art of living, to guide them in the search for truth and actually to engage in the pursuit of truth. The first two of these are universally agreed upon and endorsed, but not so universal and whole-hearted is the recognition of the third—the research function of an institution of higher learning. In some quarters the research aspirations of a university meet with distinct disapproval as encroaching upon the supposedly more serious business of the institution. In other quarters they are viewed with grudging or amused tolerance as harmless little idiosyncrasies in which scholarly men must be indulged in order to keep them contented and out of mischief. In the more enlightened quarters, however, it is realized that a university can not best perform any of its functions or measure up to its opportunities unless full and ungrudging support is given to its attempts to advance human knowledge.

In such a gathering as this I need make no defense of research as a valuable and legitimate activity of a university. It may not be out of place, however, briefly to review some of the considerations which are involved. These naturally fall under three heads, of which the first is

I. *Advantage to the student.* If ability to think clearly and independently, to organize all his available mental resources, to direct his best efforts for the purpose of solving problems and meeting difficult situations is a thing worth gaining, then research is an important part of a student's training. For the carrying on of research and its success depend on the degree to which these qualities of the mind can be focused on the problem. And what qualities of the mind are more important to develop than these? They can not be developed by that woefully overdone method of teaching by the easier process of fact-cramming. To learn a thing one must do it. To develop these mental traits they must be exercised. And the exercise of these mental traits, focused on some problem, *is* research.

Another advantage of research to the student is to be found in its stimulation of his interest. Nowadays people are not interested in things static, they

[1] Address delivered at the Founder's Day Exercises, Lehigh University, on October 5, by Professor K. T. Compton, Princeton University.

must be moving ahead. Our age is looking forward, not backward. For this reason we teachers find our students lukewarm or rebellious if confronted with the prospect of a long, long study of facts, laws and events for their own sake, without any immediate objective. But if we can put the student on his mettle by arousing his curiosity or interest in something into which he feels that he can put his creative and critical personality, it is rare indeed that he fails to respond.

I was at one time on the faculty of a small new college in the far west. Here not only was every graduating senior supposed to present a thesis embodying some original research in the field of his major subject of study, but class study itself was largely built around the idea of original investigation. I have never been in such an atmosphere of undergraduate intellectual activity. Real study was not neglected but stimulated. The objects of this undergraduate research were, of course, of various values. But whether it was sleeping, eating and talking with the army of the unemployed in the old Billy Sunday tabernacle in order to investigate the causes of their economic troubles or whether it was attempting to predict the weather from observations of wind, barometer and length of a horse hair, it *did* lead to eager study and independent judgment, and it led a very large proportion of the students into post-graduate study.

A few days ago I was talking with a chemist who told me how he happened to take up the study of sugars and other carbohydrates. During his senior year he decided to go on with post-graduate study, but did not know exactly what he wanted to do. He was just generally interested in science. His professor of physics had asked him to assist in some research work on the solubility of a certain kind of sugar by testing its purity by the well-known method of measuring its power to rotate the plane of polarization of light. In doing so he noticed that he obtained a variable result depending on whether he measured this effect immediately after dissolving the sugar or waited some time before making the measurements. The professor then suggested that the further investigation of this phenomenon might be a good thing to follow up. His professor of chemistry, on the other hand, pooh-poohed such a project as trivial and almost certain to be principally a waste of time. He urged this student to take a good advanced text-book of organic chemistry and, starting at its beginning, go right through it and actually make a sample of every organic substance whose method of preparation was described. In this way, correctly argued the professor, he would become a skillful and learned chemist.

Now the investigation proposed by the physics professor *might* have been of trivial importance, but the idea of doing something new appealed so strongly to the student that he chose to try it. The result of this work was a fundamental discovery regarding the nature of sugar which has opened up a life's work for this man, made him one of the three world's authorities in this field, and shows promise of throwing light on some of the obscure phenomena of digestion and metabolism.

The second consideration to which I would call attention is

II. *Advantage to the teacher*. Of course anything which benefits the student is at the same time an advantage to the teacher. The preceding remarks may therefore be considered as applying also under this heading. In addition I would call attention to the pedagogical value of research in maintaining the enthusiasm, confidence and prestige of the teacher. Concerning his enthusiasm and confidence nothing need be said, for it is obvious that a man possesses these qualities especially in regard to a subject to which he himself is contributing. If you wish to learn something about houses, talk to a man who is building one; if you wish to learn something about textiles, talk to a textile manufacturer, or his shop foreman, not to a clerk who is paid to sell textiles over the counter and who knows just enough to answer the questions usually asked by customers; if you wish to know some field of knowledge, go to a man who is contributing to it.

As regards the prestige of a teacher, his general standing in the community and the regard in which he is held by his students much might be said, but I shall make only one suggestion. It is well known that the general prestige of a university professor in Europe is far above that in this country. Der Herr Professor Geheimrat is a power in the country and an ideal to his students, most of whom would give anything to become such as he. How many of our students envy us our positions? Not so many, I fear. To what is the difference due? I would suggest two elements of the situation which appear to me to deserve consideration.

In the tremendously rapid development of our country we have always had room for expansion and had at our disposal vast untouched natural resources from which to draw. Consequently great industries have been based upon exploitation of these resources and the whole tenor of our business life is permeated with the idea of success to the pusher—to the aggressive, quick-witted and perhaps not too scrupulous fellow who knows a good thing when he sees it. The American idol has been success, meaning usually business success. There are thousands who have attained this success by our so-called keen business methods to one who has attained it through scholarly intellectual effort—and our country is still young and undeveloped enough to stand it.

In the older countries, however, equilibrium has been reached. The natural resources are limited and their conservation is a momentous question. Competition is keener. It is universally recognized that the economic outlook is dependent on the wisest (not the fastest) utilization of resources. Hence the whole industrial structure is far more dependent on research than is the case in this country, and the value of research is correspondingly more universally recognized. So the university professor, who there is preeminently a research man and trainer of research men, is recognized as an invaluable asset to the community.

I believe that a second element in the situation is the very rapid growth of the schools in this country. It has been impossible to train teachers or increase budgets rapidly enough to keep pace with the demand. Consequently the average fitness of both our teachers and our methods leaves much to be desired. I do not wish to decry our school system, for it has developed magnificently considering the magnitude of the difficulties to be overcome. But I believe that the consensus of opinion among those who have studied the situation (as opposed to the popular conception) is undoubtedly that, as regards *quality,* the education in a number of the European countries is distinctly superior to ours. And if we fail to demand and bring out the best in our students, we fail to the same degree to earn their respect and that of the community.

Now, in the field of higher education, the fundamental difference between our usual methods of education and those in vogue in the European countries to which I have referred is found in the degree of independence and responsibility expected of the student. Here we have too largely carried over the secondary school methods of mass production, involving frequent tests, marks and examinations, careful assignments of blocked out lessons, adherence to textbooks, supervision of attendance and study. There the student is set to master a subject. How or when he does it, whether through the aid of the professor's lectures or not, is not of much consequence. The big requirement is that he pass a comprehensive examination at the end. He knows that if he fails it is his own fault, and also that the choice positions in professional and government service will go to those who pass with highest distinction. He has therefore the incentive to exert every effort, and he must do it to a large extent independently. This independence of study, which culminates in research, inevitably leads to respect for the men who have contributed to the subject which he studies. The professor is thus a guide and example rather than a taskmaster or tutor.

As evidence of the truth of these remarks and as a hopeful sign for the future I may call attention to the increasing tendency in the colleges of this country to increase both the excellence of achievement expected of the student and the amount of freedom and responsibility accorded him in his endeavor to attain it. This movement is usually undertaken somewhat timorously and in small steps, but I have yet to hear of a place where it has been tried and has not met with marked success and approval both by students and faculty. I am therefore firmly convinced that, in the field of higher education, we have greatly overestimated the importance of pedagogical systems and have greatly underestimated the possibilities inherent in a mutual relation of student and professor as common searchers for the truth.

In this brief analysis of the importance of research, my third heading is

III. *Advantage to the community.* Inventors in this country have always been popular idols. We tell young school children about the inventions of Robert Fulton, Eli Whitney and Thomas Edison. We have been blessed by a number of men who had the spark of genius to conceive of a steamboat, a cotton gin, a dynamo or an incandescent lamp and numerous other machines and processes on which so much of life to-day depends. Nothing in the world is so potent with possibilities as a new idea, and really new ideas are rare and the product of genius. (Not all inventions are of this class. Though I occasionally enjoy Life Saver candies, I do not believe that the invention of the hole in the candy deserved the reputed reward of a million dollars, or that the invention of a blue stripe on kitchen utensils should establish a man in either Wall Street or the Hall of Fame.)

As I have said, we have always lauded the inventor. But both behind and in front of the inventor is the true research worker. The research worker first makes a fundamental discovery; then he proceeds to investigate it in all its aspects and attempts to explain it in its relations with other known phenomena. Next the inventor sees some way of turning this discovery to some practical account—and *this* is the step ordinarily called invention. Finally other research men investigate ways in which this practical application of the discovery may be made most efficient and effective. Who, in this chain of activities, deserves the credit? The patent and the right to financial rewards go to the inventor, and I should be the last to try to belittle the value of his work. My plea, however, is for greater realization of the fundamental and indispensable character of real scientific research, both as prerequisite to inventions and as essential to their perfection.

Permit me to give one or two illustrations, which I choose from my own field of science, though I realize that equally striking illustrations could be drawn from other fields by those familiar with them.

Back in the seventeenth century a number of French, English and Italian scientists investigated a newly-discovered phenomenon, which was that air in the neighborhood of hot objects was not a good insulator but could conduct feeble electric currents. The phenomenon appears to have attracted no further interest until Guthrie, in 1853, showed that a red hot iron ball, suspended in air, could retain a charge of positive electricity but not one of negative electricity. About thirty years later two Germans, Elster and Geitel, again took up a study of this phenomenon and with characteristic German thoroughness, investigated the electric currents obtained in all kinds of gases in the neighborhood of a variety of heated metal wires. Then Edison found similar currents of electricity flowing from the filaments of his newly invented carbon filament lamps into the surrounding partially vacuous space. These and numerous similar investigations led to no satisfactory explanation of the phenomenon.

But, about this time, the discovery of X-rays by Roentgen, of radioactivity by Becquerel and the researches of Sir Wm. Crookes led to the discovery of electrons and largely at the hands of Sir J. J. Thomson and his pupils there was developed a coherent theory capable of explaining the known facts of electric discharges in gases and of predicting many new phenomena hitherto unsuspected. Among other things, Thomson showed that the electrical conductivity of gases in the neighorhood of hot wires is due to electrons which are emitted from the wires, and that this emission occurs even in the best vacuum. Then one of Thomson's most brilliant pupils, O. W. Richardson, developed a theory to explain this emission of electrons and, in a remarkable series of investigations carried on partly in England and partly at Princeton University, he arrived at a very good understanding of the various factors which control this emission of electricity from hot bodies. As far as I have been able to verify the matter, every investigator who had made an important contribution to the discovery and study of the phenomenon up to this point had been a university professor or his pupil, with the single exception of Edison.

Now the story of how this was put to practical use has been the subject of much patent litigation and there were doubtless a number of nearly simultaneous steps in the matter. As I heard the story it is as follows, which, if not exactly true in detail, is certainly true in principle. Richardson delivered a lecture before an Electrical Engineering Society. At the close of the lecture an electrical engineer came up to him with a pencil sketch on the back of an envelope and asked whether the filament and metal electrodes in a glass bulb, there shown, would not function as an electric valve, to permit electric current to flow in only one direction. This suggestion was an invention, the forerunner of all our radio tubes, certain types of battery chargers, important links in long distance telephony, special furnaces for careful metallurgical processes, and modern X-ray apparatus. Now there are literally thousands of men engaged in research on this subject, which has drawn on the engineer, physicist, mathematician, chemist, geologist, biologist and physician. The importance to our civilization of the modern means of communication made possible by this work can scarcely be conceived.

I have gone into this example in some detail. Some other illustrations I have time only to suggest. Their development has gone through the same general stages as in the case just discussed.

The entire industry of electrochemistry is built upon the discoveries of Faraday, a professor at the Royal Institution.

Joseph Henry, first a teacher in a boys' school, then professor of physics at Princeton, constructed the first real electromagnet, the first telegraph and printing telegraph, had a wireless set with which his family used to call him from the laboratory to his meals and, most important of all, discovered, jointly with Faraday, the laws of electromagnetic induction which underlie all electric power machinery. And when urged by his friends to press his claims for patent rights he answered that his scientific work was too important to be hampered by attending to such trivial matters.

In the electric light industry, the single discovery of a method to make tungsten wire, which involved some years of painstaking and disheartening research, is conservatively estimated to save the American public $1,000,000,000 per year in electric light bills.

Of course not all our industrial life is dependent to the same extent on research. At the head of the list I would place such industries as the Bell Telephone Company, the radio companies and great chemical firms such as Du Ponts. At the bottom of the list would come those based on exploitation of natural resources, such as coal, steel, lumber and farming. Yet in all these the time is rapidly approaching when research will become of predominating importance in enabling the industries to meet the demands made upon them. An interesting example of this is found in the oil industry. Just after the war two young university instructors conceived the idea of utilizing certain well-known laboratory instruments and methods for the location of oil fields. They made some tests to prove its feasibility and talked about taking out a patent, but, being inexperienced, decided first to consult a prominent oil geologist. He expressed interest and approval of their method, but discouraged them by saying that oil geologists now know all that this method could give in regard to oil location, so that such a device would be quite superfluous. So they dropped the matter. Now, less than ten years later, this very method is in use together with several other applications of laboratory devices. The oil companies are obtaining an astonishing record of "strikes" by their use and are vieing with each other to secure properly trained men and equipment to beat their competitors in the survey of new fields.

If we turn to biological and medical science, we come to a field which is almost free from industrial exploitation, but which is even more fundamentally important than are the physical sciences to human welfare. In these fields the only possible advance comes through research, and research is done almost exclusively in such altruistic institutions as universities, hospitals and foundations such as the Rockefeller Institute.

These illustrations bring out with striking emphasis three facts: (1) Inventions were preceded and followed by a very great amount of research work. (2) The research work which paved the way for the inventions was carried out almost exclusively in universities. (3) The entire body of work has resulted in the establishment of large industries and in inestimable opportunity and profit to humanity.

Let me in another way emphasize our debt to research. Sociologists have pointed out the important part played by slavery in the development of civilization. It was only when the ablest men's routine work was done by slaves, giving them leisure time to think, that those ideas were evolved which resulted in the physical and spiritual uplift of the race. To-day it is estimated that our practical applications of scientific discoveries have a producing power equal to sixty slaves for every man, woman and child. Compare, then, our present life with that of our primitive ancestors, and you have a vision of our direct and indirect debt to research.

Let us turn now to some practical aspects of the situation which have a bearing on our attitude toward research in the future. I wish very briefly to call attention to four conclusions which seem to be certainly justified by past experience. The first of these is that:

I. *Research must be more generally encouraged and supported.* You may have thought, in my illustrations of the advantages which have been derived from research, that I gave undue emphasis to invention and commercial development. I did so purposely, for I wished to bring out a contrast. Where immediate financial returns are in sight, the keen search for profits which spurs our business life brings quick support and reward. Thus industrial research and development are coming more and more to be looked upon as shrewd business policy. Purely scientific research, which is absolutely prerequisite and basic to invention and development, is, on the other hand, generally carried on at a personal sacrifice and cramped for facilities. The average scientist has to battle against the odds of other supposedly prior duties in order to get time for concentrated thought and sustained experimental effort. How short-sighted is this policy, which starves the roots of our future progress! Yet it is easily understood. The results of purely scientific inquiry are uncertain. There are many trivial steps for one great stride in advance, and there is no foretelling in which direction this stride will be. No ordinary business organization which has its vision fixed on the profits of the next few years can, as a selfish business proposition, support pure and independent scientific inquiry, because the chances of a return within this time and in the range of its interests are too remote. A few only of the big industries do support pure research from altruistic motives because they are able and willing to return to pure science a little in return for the benefits derived from it; or they may support it because their interests are so varied that they can justify taking a chance on some discovery that can be turned to profit; or they may support it for reasons of publicity and personnel. Whence, then, is support for scientific research to be obtained?

In the first place this support should come from those same funds which support the other activities of our universities, on the ground that research is an essential element and method of education. In the second place it will come from altruistic citizens and organizations who possess the wisdom and imagination to visualize the possibilities which research will uncover for the future. In the third place, as I have suggested, it should come from industrial and business organizations. I should like to see all such enterprises which profit directly from the work of science taxed to support research. Probably this can never be done by legal means, but its equivalent may be brought about by publicity and the force of enlightened public opinion. If, for example, a large proportion of such industries could be induced to share the burden of supporting research and this were done on a large scale, no one industry would be handicapped in respect to the others and from the widespread nature of the research thus made possible, every industry would have a reasonable expectation of obtaining something to its direct advantage in addition to the indirect benefit of general business stimulation. Such is the justification of the "Hoover Fund" which the National Academy of Sciences is attempting to raise from large industries with the strong backing of Secretary Hoover. Finally, support may come from the government, if this can rise above the pressure of political struggle and popular appeal for tax reduction, and look to the welfare of the future. True, the government is now supporting a few research agencies, but on a very niggardly scale, in very restricted fields, and with such pressure for immediate practical returns as to drive out those scientists who might do big things and almost to kill the possibility of that type of research which might have great consequences.

It has been said that civilization is measured by the degree to which the people will sacrifice present desires for the sake of future benefits. On this basis the degree to which we support research should count heavily in estimating our degree of civilization.

The second conclusion which I would draw is that:

II. *The universities must be the chief agents and mainsprings of research.* There is no organization other than the universities where there are gathered together the men of scholarly training capable of carrying on research in the whole field of scientific inquiry. If there *were* some other suitable organization, it is there, rather than to the universities, that young men would go for research training, and this organization would at once *become* a university. Such work is undoubtedly the function of the universities.

Furthermore, it is a well-known paradox that the discoveries which have led to the greatest practical results have not been made by the men who were seeking the practical results. The reason for this is plain. The man who is seeking a certain end is mentally circumscribed by the methods which are already known for attaining this end. He may perfect some previous process, or he may see how to apply some phenomenon or principle which has not hitherto been applied, but his mental state handicaps him in discovering anything fundamentally new. The basic research must, therefore, be a free and unfettered search for truth. It is the universities alone which can offer any considerable opportunity for such endeavor.

Besides this, it is to the universities that the industrial laboratories and government bureaus must turn for their trained men. The head of one of the largest electrical companies recently stated that the only limitation to the development and extension of his industry lay in the dearth of trained men fitted to take charge of the developments which they had in sight.

The third lesson I would draw from the experience of the past is that:

III. *Research must proceed by specialization.* Research has always required concentration, but it did not formerly require intense specialization. Look, for instance, at Benjamin Franklin, at once philosopher, publisher, scientist and diplomat. Even in science he did not specialize. To quote from an admirable address by Dr. Little before the Franklin Institute:

> Among all the activities with which his busy life was crowded, Franklin undoubtedly found his greatest interest in the pursuit of science. . . . At a time when nearly everything was awaiting explanation, his focused attention ranged like a searchlight over many fields. He observed the movement of winds and developed a theory of storms. He considered ventilation and the causes of smoky chimneys and proceeded to invent new stoves. He introduced the Gulf Stream to Falmouth skippers and demonstrated the calming effect of oil on turbulent seas to officers of the British Navy at Portsmouth. From earthquakes he turned to the heat absorption of colored cloths and the fertilizing properties of gypsum. He wrote on sun spots and meteors; waterspouts, tides and sound. The kite, which for centuries had been the toy of boys, became in Franklin's hands a scientific instrument, the means to a great discovery. That its significance is, even now, not universally appreciated is shown by the recent answer of a school boy, "Lightning differs from electricity because you don't have to pay for lightning." To Franklin . . . we owe our initial conceptions of positive and negative electricity, and he was the first to suggest that the aurora is an electrical phenomenon.

But to-day each individual branch of science is larger than the entire body of science of Franklin's day. The philosopher may still contemplate the entire field of knowledge and consider its interrelationships, but no man can do creative work in the entire field. Some especially gifted men can leave the imprint of their genius in a larger variety of directions than the majority of us, but the bulk of the research of the future seems destined more and more to be carried out by people who are highly trained in specialized fields.

With this tendency comes the necessity of a balancing movement, which is my last conclusion:

IV. *Research must become more and more cooperative.* An obvious handicap and danger of specialization is that a man may not know, and in fact now-a-days can not know, even all the aspects and relationships of his own special field, however small. Hence, unaided, he becomes helpless through the very specialization which he hoped would give him power. The remedy for this danger lies in cooperation between workers in closely related branches of study.

Just as in the process of organic evolution we find increasing specialization accompanied by increasing interdependence, just as the growth of highly specialized industries has necessitated careful attention to their coordination, just so the increasing specialization in research makes necessary an increasing degree of contact and mutual assistance between scientists.

To take just one example: Though physics is the most mathematical of the sciences, the average research physicist and the average productive mathematician speak languages unknown to each other. To make advances in one field available for progress in the other there has arisen a chain of connecting links. We have the experimental physicist, the theoretical physicist, the mathematical physicist, the applied mathematician and the pure mathematician. I do not believe there is a single living scientist who could at the same time classify under all six headings. There is possibly one, named Einstein, who might classify under five, and there are very few who could classify in four of these groups.

How can this essential cooperation best be brought about? Certainly no single formula will serve to solve the whole problem, and no solution will be easy because research, to be effective, must be independent as well as coordinated, and these are two almost antagonistic features. Several solutions may, however, be suggested as of proven value and worthy of encouragement.

One of these is the encouragement of research in the so-called border-line fields such as mathematical physics, physical chemistry, biophysics, biochemistry, etc. Not only are such coordinating studies necessary, but they are, in my opinion, the most fruitful fields of investigation. Nature herself is not divided into a physical world, a chemical world, a biological world; she is a unit. These artificial distinctions have been introduced for convenience and because of our inability to see the whole field at once. They have resulted in rapid development in the particular direction and by the particular method of each of the sciences, whereas work in the border-line fields has lagged behind. It is as if the whole field of knowledge were originally a desert. When the rain fell on this desert, the water flowed off in streams and rivulets, each digging its channel deeper and deeper but leaving the intervening space relatively untouched. The best place to dig is now in these intervening spaces.

The necessity of coordinating border-line work is recognized by such far-sighted organizations as the General and International Education Boards and the Rockefeller Foundation which have, for example, supported the great system of National and International Research Fellowships, one of whose guiding principles is the stimulation of research in the border-line fields. In our universities further facility and encouragement should be given to men to prepare for work in these directions.

Another solution can advantageously be advanced by wise administration of the universities. There seems to be a wide-spread, but ill founded, feeling that all departments of a university should be developed together and kept closely abreast. Perhaps this relieves the administration from embarrassment, but I venture to suggest (though the suggestion is not new) that this is not sound educational policy except for an ideal institution which has unlimited resources. Such a policy dissipates effort, and if every institution followed it we should have the spectacle of a great many universities all very much alike and all with struggling, mediocre departments. Much more effective in advancing knowledge as well as in bringing distinction to the university is the policy of supporting to the available limit certain departments selected because of their already outstanding character, or because of the traditions and purposes of the university, or for any other reason. If these favored departments are chosen in a coordinated group, then the university becomes an active center for the development of that field and the promotion of cooperative effort. For example, one institution may choose to give *particular* facilities for advanced work in classics and languages, another in historical, economic and social sciences, another to physical and biological sciences, etc. If we were to examine the record of those universities of limited endowment which have nevertheless been preeminent in the life of the country, we should find that they attained this preeminence through concentration of effort. The words "To him that hath shall be given" apply here as well as elsewhere.

Through concentration of effort in a coordinated group of departments, a university has the opportunity not only to correct the dangers of over-specialization, but also to take a strategic position in fulfilling its obligations to society.

Much can also be done to promote cooperation and coordination through actual methods of organization. This has been strikingly demonstrated in some of the big industrial research laboratories, from which the output has greatly exceeded the individual capacities of the research workers and has been achieved only by coordination of effort. Such organization requires a very wise and far-visioned director who can visualize the big objectives and steer through the mass of petty details which must be worked out in order to attain them.

In a university, where the number of workers is much smaller than in a big industrial laboratory, such army-like organization does not appear feasible or probably desirable. Much is being done voluntarily by scientists themselves in dividing up particular fields for cooperative investigation, and this is being fostered by research committees of organizations like the National Research Council. However, there is another direction in which more effective organization is possible within the universities themselves!

Departments of a somewhat more flexible nature than those to which we are accustomed and which could, more than now, be built around one or two outstanding men in the department, could give these men an opportunity for organization and concentration of effort which is now rarely possible. This would, of course, require careful selection of men. In this matter of organization of departments around the most productive and outstanding men, of taking for granted that they will have research assistants to increase the efficiency of their labors, of selection and recognition of men on the basis of merit and promise rather than seniority, and of wise procedure in the selection of men to fill important posts, America is far more backward and bound by tradition than are those European countries in which scientific achievements have been most rapid. It may surprise you, for instance, as it did me, to learn that in America, the land of wealth and opportunity, there is no university which is able to offer a salary equal dollar for dollar to salaries which universities even in war-ridden Germany will offer to secure the outstanding men. As is the tendency in other things American, our tendency to standardize, which is so useful in some directions, is interfering with our ability to recognize, secure and do our best. This situation in our universities is, I believe, a grave one if we set as our ideal the best possible achievement.

In these remarks I have attempted to suggest some of the accomplishments and opportunities of research and to indicate some of the directions in which we may hope to bring about even more fruitful service of science to society in the future.

This, gentlemen, is the situation. It is a situation that calls for serious thought and constructive action. The things which I have been able to say are not new, but I sincerely hope that you may find in them reasons sufficient to enlist your sympathy and active support of any movement which has for its purpose the better service of science to our country and to humanity.

KARL T. COMPTON

PRINCETON UNIVERSITY

SOME PROBLEMS IN BOTANICAL CLASSIFICATION[1]

THE systematic botanist (or taxonomist as he is often called) has a double duty. First, he must give each apparently new plant a name which other workers in plant science may use in describing their experiments with the plant or in drawing conclusions regarding plant distribution and so on. This name must designate the plant's relationships with approximate accuracy and is based on a close study of gross morphological characters. By that I mean such characteristics as shape of leaves, color of flower, number of flower parts and their arrangement—in fact such physical properties, to use a chemical phrase, as can be determined fairly readily with the naked eye or by aid of a small lens. The degree of resemblance of the total of these characters, and especially as regards those of the flower and fruit, between plants has served as the criterion in judging the degree of relationship of the plants. This type of taxonomic work will probably always be based on the same method. It is worth noting here that there still are enormous areas of the earth's surface, the vegetation of which remains comparatively unknown despite the flood of taxonomic publications during nearly two centuries, and these areas offer an attractive field for what I may call this "preliminary naming."

Second, he must revise his conceptions of plant groups in accordance with the progress of work in other botanical fields and such discoveries as are constantly being made that tend to show more clearly the exact relationships of the plants involved.

Of chief interest to us is this second phase—namely, the correlation of taxonomic work with other botanical work and the revision of opinions that necessarily results from many-sided, intensive investigations. (The preliminary naming of the cultivated fruits and vegetables of the north temperate zone has long since been completed.) Taxonomists have perhaps been a little slow in the utilization of other workers' results; but the last decade has seen started a considerable number of new lines of systematic investigation based on the work of plant physiologists and chemists, geneticists and cytologists.

It is, I believe, the correlation between the work of the geneticist and cytologist and that of the systematic botanist that offers the most promising line of attack on the age-old problem of clearing up the relationships of cultivated plants. This problem is of vital interest to us at this station.

In the Division of Horticulture, we have been particularly interested in fruits and vegetables, and, at present, some typical questions of considerable moment are: What are the true species of apple—or, in other words: What are the fairly stable natural groups of the genus Malus? What species are we working with in our attempt to breed better fruits? What are the characteristics of each of these entities? As you may readily perceive, the answers to these

[1] Extract of address before the June staff meeting of the New York State Agricultural Experiment Station, Geneva, N. Y., June 6, 1927.