

continue the collaboration between the scientific and industrial elements of the country and the exchange of proposals and the united action for scientific and moral progress between the allied countries, specially what concerns the United States and Italy, who, during this war, have had opportunity to know and appreciate each other more thoroughly than before.

The institution, which, in the United States, is parallel to our *Ufficio Invenzioni e Ricerche* and the analogous institutions in France and England, is the National Research Council founded by Dr. G. E. Hale, which works as a scientific and research office acting as a department of the Council of National Defense.

In regard to the cooperation of the United States with the Allied countries we remember that on the entrance of the United States into the present struggle the following telegram was sent by Dr. Hale, foreign secretary of the American National Academy of the Sciences to the Royal Society of London, the *Académie des Sciences*, to the *Accademia dei Lincei* of Rome, and to the Russian Academy of Sciences, namely, to all the important scientific units with which the American Academy has cooperated for many years in the International Association of Academies:

The entrance of the United States into war unites our men of science with yours in a common cause. The National Academy of Sciences acting through the National Research Council, which has been designated by President Wilson and the Council of National Defense to mobilize the research facilities of the country, would gladly cooperate in any scientific research still underlying the solution of military or industrial problems.

As a result of this invitation missions composed of men of science were exchanged by the various allied nations and a research information committee was established in the National Research Council, represented at Rome, Paris and London by scientific attachés at those embassies. Exchange of ideas and progress in their common work have been and are still continuous and constant, and, speaking especially of Italy, they have brought their results, although greater progress is expected

with the increase in exchange of persons, ideas and facts.

The extended program of Dr. Hale for an interallied research council also plans for after the war an interallied institution that provides the means of reaching a common agreement as to what researches are most vital and ought to be begun because of the pressure of economic necessity or in light of recent progress, and has the privilege of selecting the countries or institutions best adapted to undertake certain researches and of finding the best methods to coordinate the work of the investigators of the different countries. This project was presented by Dr. Hale with complete success at the meeting of the academies in London. Thus were laid the foundations for a cooperation which will surely be fruitful of results, especially in the long work of readjustment and reconstruction which is going to begin with the desired conclusion of peace in the world.

GIORGIO ABETTI

ITALIAN WAR MISSION,
WASHINGTON, D. C.

THE PERSONAL RELATION OF THE INVESTIGATOR TO HIS PROBLEM¹

As president of the local chapter it is my privilege to welcome you to membership in Sigma Xi and to say a few words concerning the main purpose of the society, the furthering of original investigation in science. I shall confine myself to the question of the personal relation of the investigator to his problem.

As a preliminary consideration it is well to emphasize the unlimited possibilities of scientific investigation. I have been asked by a member of our faculty, not in scientific branches, what scientists will do when they have discovered everything that is to be known about natural phenomena. The obvious answer is of course that instead of approaching such a consummation we are rapidly moving away from it. The number of things to be

¹An address to newly initiated members of Sigma Xi at the University of Illinois, May 21, 1919.

investigated with profit is rapidly increasing and there is no reason to believe that there will ever be a change in this condition. The solution of any problem merely opens up new problems and the number of problems that can be effectively attacked is increasing in a geometrical progression. The formulation of new problems is therefore as much a function of research as the solution of problems already formulated.

Considering these facts we must realize that what we call the known is merely an infinitesimal part of what is to be known. Our systems of classifying facts, our so-called laws, are based upon partial views of the universe. At any time they may need revision. We should have no sympathy with the point of view expressed to me by one of my teachers when I was beginning graduate work in zoology. He said it was too bad that we were born too late, that we were living at a time when the great laws of biology had been discovered. He was referring to Darwin's theory of natural selection and went on to add that all we could hope to do was to elaborate upon Darwin's work and find additional examples of the law he had discovered. This remark of his has since reminded me of my boyhood regret that I had been born after all the great earthquakes and volcanic eruptions and great wars were over and I saw nothing ahead but a calm and dreadfully uninteresting future.

To my mind if there is anything that is clear it is that the great discoveries in science are still to be made, that while there has been great progress it is just a beginning. Nothing can be more fatal to our careers as investigators than the view that we are simply to elaborate the principles laid down by our predecessors. The whole spirit of research should be a continual revolt against this view, a continual attempt to find new and better modes of interpretation of phenomena. I feel safe in making this appeal because there will always be plenty of people who will follow the path of least resistance and they can be depended upon to pave the beaten paths and keep them smooth by rolling back and forth their truck loads of confirmatory observations.

Let me illustrate the value of the scientific imagination by an example from the biological field. One of the central problems in the field has been the question of the manner in which children obtain their hereditary qualities. The problem has been studied from two sides. On the one hand, an analysis of the adult characters of the children as compared with those of their parents and grandparents has disclosed certain so-called laws of heredity. On the other hand, a careful study of the minute structures and activities of cells, particularly of the sex-cells, has been made to see if the mechanism of heredity could be discovered. Each of these fields has yielded and continues to yield results of the greatest importance. It is interesting to find out that following crosses between certain kinds of individuals we may expect to get in the second hybrid generation certain kinds of individuals in definite ratio, but interest soon flags in the elaboration of the system and in the carrying out of the idea in endless variety. Likewise studies without end have been made of the microscopical structure of nucleus and cytoplasm, of chromosomes and centrosomes, of chromomeres and chondriosomes. The determination of a system by which we might describe the endless variety and relations of these structures is a task for numerous lifetimes.

The problems in these two fields are set. How easy to follow the beaten path, to polish off a paving stone here and there, to gain merited attention by developing a technique which will bring out some structures better than they had ever been seen before. But, whose soul is now fired by a new Mendelian ratio or by a new chromosomal number? Research in these lines became more and more a matter of routine, less and less inspiring, and there was danger that in these fields as in some others the investigators' main satisfaction would come from having faithfully put in his hours from eight to five. Why does he want to work overtime? Certainly not for the pleasures of contemplation of the beaten path. The inspiration comes from a vision of untrodden fields.

Returning to the example under consideration we find that the great spurts in the advance are due to the stimulation of the scientific imagination by hypotheses which aim to bridge the gulf between the two lines of research. The idea of a connection between the fact of the general equality of male and female parents in heredity, and the fact of equality in the nuclei of the male and female sex-cells was of epoch-making importance. Later the association of the precise though complicated behavior of the nuclear bodies called chromosomes with the possibility that these bodies are the bearers of the hereditary factors again caused a tremendous advance along both of the lines we have mentioned. More recently still, the hypothesis has been made that certain peculiarities in linkage of characters may be associated with peculiarities in the behavior of the parts within single chromosomes. If you will pardon my digression into this field I will elaborate the last step. According to the view that the chromosomes are the bearers of the hereditary factors it seemed clear that the factors located in a single chromosome must always go together and a corresponding linkage of adult characters in four groups corresponding to the number of chromosomes was made out for the fruit fly, *Drosophila*. But it was then discovered that the linkage was not complete. Occasional separation took place and the percentage of such separations was found to be definite for any two characters of such a linked group. Still further it was shown that knowing the percentage of such breakings of the linkage between characters A and B and between B and C the percentage of breaking of linkage between A and C could be calculated. It was either the sum or the difference between the first two cases. On this basis the factors in a linked group were arrayed in a linear series in which the actual breaking of linkage between any two points closely followed the expectation. Such a series was extremely interesting and the elaboration and perfection of the series in the various groups of plants and animals in itself would have furnished endless opportunity for research. But the in-

terest in the series was immediately greatly stimulated by the suggestion that these phenomena of inheritance of adult characters could be explained by certain peculiarities in the behavior of the chromosomes. By this suggestion the fields of experimental breeding and of cytology were again brought into connection and there is gradually developing a fruitful hypothesis which in some respects bears the same relation to heredity that the atomic theory does to chemistry. Even if the direct hypothesis proves to be premature it will have been the means of opening up lines of research which otherwise would have remained untouched.

I have taken the time to give this example from the field of biology in order to emphasize my point in urging you to encourage any tendency you may have toward visions of relationship between things that at first seem to be wholly different. Young investigators are often inclined to think when an idea comes to them that it can not be of value because no one else has ever followed it up. Besides I have heard it said that it is one of the functions of a teacher of graduate students to see that they do not waste their time following up unprofitable ideas of their own. This would be very well but for the unfortunate consideration that to many, only well-established ideas are profitable, and therefore new ideas are always considered as unprofitable. This is stand-patism in science and the stand-patter in this field should have even less sympathy than the stand-patter in politics. Our salvation as original investigators depends upon the development of our own ideas. The question may be raised whether it is ever unprofitable for a person to follow out his own ideas. Is it not of more value to us personally to follow up an idea of our own even if it be wrong than it is to prove the truth of a dozen ideas suggested to us by others? But, *your* idea is not necessarily wrong and the fact that it is your own makes it extremely valuable. As your own it enables you to draw on the fund of extra energy which we all possess but which only a personal, living interest can draw out. Only such ideas, to use the words of our

constitution, can make us true "companions in *zealous* research." But beware lest you become so zealous as to forget that theses must be typewritten in a perfectly definite manner in order to be acceptable.

As to procedure in discovering what we call truth, I wish to describe two opposing methods, overdrawing the point at first in order to make the notion clear.

We may decide to investigate some problem that has been called to our attention and may start out to collect all the ideas of previous workers on the subject, to study completely all the facts which others have collected and to study also their attempts at explanation. Then follows a critical analysis of the facts and ideas to see if they are satisfactory and if not satisfactory the lines along which further evidence is desired. Then we proceed to the investigation of those points which seem to us to be necessary for completing the ideas disclosed in the literature of the subject. This method is one generally taught to young investigators but it has certain defects which are not so generally recognized. It puts the investigator in the position of patching up a structure that some one else has built. Nothing is more certain than that in building up a system by which to explain phenomena we at once focus our attention upon certain phases of those phenomena to the exclusion of others. Following this procedure a student of human embryology a hundred and fifty years ago, studying carefully what others had written on the subject, would have had his mind focused on the attempt to see in the egg or spermatozoon the miniature individual with all the mature parts in proper proportion; for according to the view then prevailing either the egg or the spermatozoon must contain such a miniature man. The chances are that he would believe that the problem to be solved is, whether the miniature man is in one or the other, and judging by the experience we have in our beginning biology classes he would have seen it. We have some marvelous figures in the publications of the seventeenth and eighteenth centuries of miniature men all nicely coiled up in the head of the spermatozoon and

on the basis of such a figure who could believe that the egg is other than a medium for the proper nourishment and protection of the unfolding man. Especially was this likely to be the case in an age when all things feminine were considered inferior to things masculine. But even in those days there were feminists, for one group of investigators held that the miniature man was in the ovum and that the spermatozoon merely filled the comparatively unimportant rôle of a stimulating agent which started the unfolding process of the ovum.

One danger then in this procedure is that it concentrates our attention upon certain phases of phenomena and upon certain ideas concerning them. Even the procedure of definition of terms, while necessary for progress in science has the very evil effect of concentrating the attention upon certain features of a process to the exclusion of others.

It has become the custom in certain graduate schools to put the student through a preliminary training for research on the theory that he ought to obtain a certain familiarity with the ideas of others before he begins to have any of his own. He is supposed to be unfit for any original investigation until he has had his fill of the prevailing explanations. I am inclined to object very strenuously to this program. To my mind it is all important that the personal relation between the investigator and the phenomena he is to study should remain unbroken. There should be allowance in any program of study for some fraction of the time when there may be companionship between the investigator and the phenomena of nature without the presence of a chaperon. It is not at the beginning of an investigation that a person needs to know everything that has been thought by others concerning the problem. Of course, it is all important that before imposing his ideas upon the world he should have tested them fully by comparison with all that others have done. It is to the neglect of the factor of personal reaction that we owe so much of the dead timber among would-be investigators. It is for this reason that we so rarely tap the fires of enthusiasm, that we do not unloose the reserves of energy,

which are waiting to be put into action. Enthusiasm is lost before there is a fair start in investigation. When enthusiasm is lost everything is lost, and the graduate schools are all too full of persons who are carrying out researches as a matter of duty and not as a matter of personal inward necessity.

No one can be a "zealous" investigator unless the investigation of some particular problem is absolutely necessary for his comfort, unless he is unhappy if not at work on it, unless there is an inner flame which will not let him rest. Such an attitude of mind can be obtained only by continued contact with natural phenomena, by a realization of the kinship with nature which makes us carry the joy of companionship from the realm of human beings to all nature whether living or non-living. We know the man who is interested in his fellow-men because he wishes to use them for financial gain. We do not wish to follow his example. The same type of man is to be found in science, the man who sees in nature only a means for obtaining material gain. The true type of investigator, however, is he who delights in the existence of a universe which yields secrets to his tender regard. I remember when I was a boy our neighbors used to bring their sickly house plants to my mother to keep for them until they regained full vigor. When asked why they did so well for her she always said, "They grow so well for me because I love them."

So let me urge upon you the cultivation of a relationship with nature and its problems based upon direct and personal intimate contact with it. The problems you are engaged in solving then become your own problems, their solution becomes necessary for your happiness. Your soul can not have peace until they are solved.

CHARLES ZELENY

SCIENTIFIC EVENTS

AMERICAN ASTRONOMY¹

IN the year 1840 the Dana House Observatory of Harvard College was established by the aid of public funds and private subscription, with William Cranch Bond as director. It

¹ From *Nature*.

was not the first college observatory in America, and other eminent American astronomers had lived earlier in the century, but the date may be taken as the beginning of systematic astronomical observation in the western continent. The U. S. Naval Observatory was established in 1844, and the present Harvard Observatory founded, largely by generous help from private benefactors, in 1846. Other institutions of the period might be named where the science of astronomy of position was pursued, and this, with the splendid work on planets, satellites, comets, asteroids, nebulae and the astronomy of the solar system generally done at Harvard by W. C. Bond and G. P. Bond, and afterwards by Winlock, is to be considered representative of the astronomy of the United States in the succeeding forty years. The accession of the late Professor E. C. Pickering to the directorate of the Harvard Observatory in 1877 marks the beginning of the astronomical era in which we now live. Spectroscopy, stellar physics, and stellar statistics are the principal features. Professor Pickering's work was stellar photometry on a wholesale scale. Stellar spectroscopy and the determination of the radial velocity of stars by its means had been begun by Huggins in 1864; the photographic plate came into general use as an adjunct to the astronomer's equipment in the decade 1880-90, and these three items have formed the basis of the work of the American observatories of recent creation. The Lick Observatory, with the 36-inch telescope, was completed in 1887 at the expense, as every one knows, of an American business man. The Yerkes Observatory came into existence in 1897, and the observatory at Mount Wilson in 1904. These things are recalled at this moment because, during the past week, English astronomers have been gratified by a visit from a delegation of astronomers from across the Atlantic who were on their way to take part in the establishment of an International Astronomical Union at a conference now being held in Brussels (July 18-28).

At a meeting of the Royal Astronomical Society on July 11, specially arranged for the purpose, the visitors spoke in turn of the work