Experiment Station, Geneva, N. Y., have been appointed official delegates of the U. S. Government to the eighth International Seed-Testing Congress at Zurich. The congress will hold its sessions from June 29 to July 4.

DR. Z. KOZMINSKI, of the Wigry Biological Station at Suwalki, Poland, is carrying on research work in limnology at the University of Wisconsin during the month of June. During July and August he will be in residence at the Trout Lake Limnological Laboratory, where he will study the photosynthesis of the phytoplankton and the copepod fauna of the lakes of that district.

DR. HAROLD ST. JOHN, botanist at the Bishop Museum, Honolulu, will sail on June 28 for Fiji to continue the exploration sponsored by the museum of the South Sea Islands. He expects to collect plants for two months, principally on the interior plateau of Viti Levu, which is relatively unknown botanically.

Professor J. G. FITZGERALD, director of the School of Hygiene at the University of Toronto, has a year's leave of absence to study the teaching of preventive medicine in medical schools in the United States, Canada, the British Isles and other countries of Europe for the Rockefeller Foundation. Professor FitzGerald gave a Chadwick Public Lecture in London on May 26. His subject was "Preventive Medicine—an Avenue of Good Will."

DR. C. H. ROBERTSON recently completed a lecture tour of eight chapters of Sigma Pi Sigma, physics honor society, at Miami University, the University of Kentucky, Berea College, the University of Chattanooga, the College of William and Mary, the University of Richmond, West Virginia University and the Ohio State University. The subject of the series was "Gyroscopes and Boomerangs."

SIR ARTHUR EDDINGTON, director of the observatory and Plumian professor of astronomy at the University of Cambridge, gave the eighth annual Haldane Memorial Lecture at Birkbeck College, London, on May 26. His lecture was entitled "The Reign of Relativity, 1915–1937."

THE seventh annual research conference of the department of chemistry of the Johns Hopkins University is being held from June 7 to June 25, at the Cavalier Hotel, Virginia Beach. The subjects are "Enzymes" during the first week, "Phenanthrene Chemistry" during the second week and "The Mechanism of Some Homogeneous Organic Reactions," including oxidation, chlorination and polymerization, during the third week. These conferences provide an opportunity for discussion among a group of specialists in a particular field of chemistry. The meetings are kept as informal as possible, there are no printed papers, and no formal record of the proceedings is kept. It is preferred that attendance be limited to those working in the field under discussion or in closely allied fields, so that the groups may be small enough for each person to take part in the discussion. The conference is under the direction of Professor F. O. Rice, department of chemistry, the Johns Hopkins University, Baltimore, Md.

SIGMA PI SIGMA, physics honor society, installed its thirty-first chapter on May 10 at John B. Stetson University, DeLand, Florida. Dr. Marsh W. White, the Pennsylvania State College, was the installing officer and spoke at the first open meeting of the chapter, following the installation, on "Modern Alchemy."

THE installation of the Illinois Alpha Chapter of the Alpha Epsilon Delta Honorary Premedical Fraternity at Illinois Wesleyan University, Bloomington, was held on May 21. Dr. Emmett B. Carmichael, professor of physiological chemistry at the School of Medicine of the University of Alabama and grand president of the fraternity, conducted the ceremonies, which marked the installation of the eighteenth chapter since the establishment of the fraternity at the University of Alabama in 1926.

DISCUSSION

THE EXCESSIVE MEEKNESS OF AMERICAN BOTANISTS

THE meekness of American botanists has been so long and so generally recognized that no comment was offered and certainly no surprise was occasioned when some years ago Seifriz[1] quoted a visiting Swiss botanist as calling attention to the fact that it is easy in most American universities to recognize the botany building, because it is the "oldest building to be seen anywhere." We have, however, taken a certain satisfaction in the feeling that our zoological friends were ready to assert themselves, to think and to act independently, and that thus a fair balance would be maintained, and the general field of biology adequately, if not evenly, cultivated.

Rather recently, however, there has appeared in various quarters the more disturbing suggestion that

[1] *The Scientific Monthly*, May, 1928.

the apparent forcefulness of zoologists is relative only to their botanical associates and that biologists as a group are inclined to be meek and to accept their basic theories and even their methods ready-made and second-hand; moreover, that these hand-me-downs do not really fit. For example, Whitehead[2] says, ". . . at the present moment, the prestige of the more perfect scientific form belongs to the physical sciences. Accordingly, biology apes the manners of physics. It is orthodox to hold that there is nothing in biology but what is physical mechanism under somewhat complex circumstances." To which Russell[3] adds, ". . . Biology, impressed by the success of physical concepts in their own sphere at the time of the great development of the classical mechanics, took over to itself concepts and methods which were clearly inappropriate and inadequate."

Now criticism of this sort is disturbing enough, but after all, we have been believing that most of the workers in any field need not concern themselves with fundamental theory—provided, of course, they are doing their daily duty of accumulating "facts" with proper zeal and appropriate methods. Even this haven of refuge seems now endangered from two distinct angles, one, that the very volume and variety of the accumulated facts make real comprehension more difficult; and the other, that our present methods are as badly suited to our needs as our present concepts. On the first point, witness Crowther,[4] "But the neglect of comprehensive synthesis by which all the facts could be ordered led to intellectual chaos, just as the blind drive to increase production of goods, without working out any comprehensive system of distribution, led to chaos in social life," or Lyon,[5] who says the same thing in medical rather than economic figures. "There is a serious side to this unabsorbed gorge of science. It has given our people a bad indigestion. It lies in the public stomach and troubles their dreams. They do not know enough to know good science from bad." And on the second, Sullivan[6] says: ". . . Discrimination is fatiguing; also it makes appeal to sensibilities which many earnest 'scientific workers' do not possess. It is much easier to make measurements than to know exactly what you are measuring. To give up the ideal of measurability would be the equivalent, to many people, of abandoning 'science' altogether. 'Science is measurement,' we are told. . . . In their eagerness to measure something, our researchers seem to lose their ordinary common sense, whereas their subject really requires the subtlety and sympathy of a very good novelist."

[2] "Science and the Modern World," p. 144.
[3] "The Interpretation of Development and Heredity," p. 163.
[4] *Soviet Science.*
[5] *Sigma Xi Quarterly,* December, 1936, p. 208.
[6] "Gallio—or the Tyranny of Science," p. 50.

This last challenge is particularly disturbing to our complacency, since it at least suggests that the real foundation of our recently acquired faith in supposedly exact measurement may be found in mental laziness. For botanists—American botanists at least—have at last discovered mathematics and appear cheerfully ready to abandon any form of inquiry or information gathering which does not readily lend itself to measurement and statistical analysis. Like all good converts, we are trying to be more orthodox than the Pope, for the mathematicians are quite ready to concede that there are in biology important fields of inquiry where mathematics can play little part. To quote Carmichael[7]: "It must also be remembered that there are important Chapters of Science which do not come readily under the domain of number. Witness much of biology and in particular the theories of phylogenetic development." Some would go even further. Whitehead[8] agrees with Henri Poincaré in insisting "that instruments of precision, used unseasonably, may hinder the advance of science."

Certainly, within the writer's field of study (plant diseases) there occur phenomena which appear to defy accurate measurement by present methods, yet which seem important and abundantly worthy of record. Reference is here made to the fluctuations in plant disease which are of very great biological interest and economic importance, but which are most inadequately recorded, largely because they do not appear to be readily measurable. That large differences occur no one denies. That they are hard to measure is granted. The essential difficulty of the undertaking is emphasized whenever the accurate measurement of losses from a single plant disease is undertaken. One of the most interesting recent attempts[9] is based on a comparison of the yields of adjacent smutted and smut-free plants of dent corn. In spite of the care used in the field work and in the analysis of results, this work is still open to the criticism that the diseased plants may have become infected because they were different in the first place.

However, a discussion of the methods of measuring disease losses does not belong in this paper. There is no present possibility, even if we had developed the technique, of making measurements of losses due to even the important known diseases of our agricultural crops. Largely on this account, there is a decided, possibly an increasing, reluctance on the part of plant pathologists to record these differences at all. This willingness largely to ignore for purposes of record anything which can not be measured and set down in mathematical terms is, of course, just one more mani-

[7] *The Scientific Monthly,* December, 1935, p. 495.
[8] "Adventures of Ideas," p. 311.
[9] I. J. Johnson and J. J. Christensen, *Phytopathology,* 25: 223–233, 1935.

festation of the excessive meekness of American botanists. There may well be biological phenomena, the *record* of whose occurrence is more important than their measurement and which should be recorded even if they can not be measured. For example, only a few years ago, 1931, eel grass (*Zostera marina*) was common in the shallow waters of the Atlantic seaboard, from North Carolina to Nova Scotia—now it is rare. Its diminution was so sudden that no opportunity was given for statistical study, even by the quadrat method. Yet obviously the biologists of the future are entitled to the information that a striking phenomenon occurred in our coastal waters at this period, even if we are not able to furnish figures.

This is, of course, an extreme case, but somewhat similar situations arise over and over again in our consideration of the variations in the incidence of plant diseases. For example, it is biologically and economically important to know that bacterial wilt was exceedingly abundant on sweet corn in the Hudson Valley of New York in 1932 and 1933 and very rare in 1934 and 1935, but whether the loss occasioned in the earlier years was 20 or 40 per cent. and whether the loss in the two later years was one half or three fourths of 1 per cent. is of merely academic interest. In 1932 and 1933 the losses were disastrous, and in 1934 and 1935 negligible.

Nothing could be further from my thoughts than to suggest any radical reform such as would be needed to alter our general professional attitude or develop new concepts and methods particularly suited to the study of living things. I certainly cherish no illusions as to the possibility of securing some slowing down of the rate of accumulation of observations or even a little breathing spell during which we might consider what, if anything, these accumulated facts signify. Quite the contrary, I propose merely that we students of living things shall not restrict ourselves to the type of observation or record prescribed by devotees of other branches of science, but shall record as clearly as we may whatever phenomena seem interesting to us, even though we can not measure them with great accuracy. For such unrestrained self-expression, Dr. Sarton has recently furnished an adequate slogan in his book, "The Study of the History of Science"—"No scientist worth his salt has ever abandoned an investigation simply because the attainable precision was too low."

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"RACES" AND "HOMING" OF SALMON

IN support of the theory of the "homing" of salmon from distant places in the sea, Dr. Willis H. Rich[1] puts forward the argument that the local "races" of the Pacific salmon could not exist if the fish did not return to their own rivers, seeing that large numbers of them travel hundreds of miles in the sea before entering streams. It would appear, however, that the theory of "races" is in somewhat comparable condition to that of "homing" from distant places, in that adequate proof is lacking.

The characters that have been used to distinguish "races" in species of marine fishes, such as herring and cod, are being demonstrated to result from the action of the environment on the individual during its lifetime, so that it seems doubtful whether there are heritable differences between the populations of different districts. Without such differences the use of the term "race" would seem valueless. It would be interesting to know whether the "races" of any species of Pacific salmon have been shown by rigid experiment to have differences that are heritable rather than the effect of the environment.

It has been maintained for the Atlantic salmon not only that the different rivers have more or less peculiar "races," but also that the same river may contain two different "races," one entering early and the other late in the season, although not spawning at different times. This theory has been causing the Canadian Government to spend considerable money in securing the early running fish and in keeping them till spawning time for breeding purposes, since both anglers and commercial fishermen desire the early fish not only because they are available in the fishing season, but also because they tend to be larger than the late-running fish.

As crucial a test as possible[2] was made of this theory of "races" by taking the fry of Restigouche salmon, which characteristically run early and large (ordinary salmon and big salmon) and planting them in a salmonless branch of Apple River at the head of the Bay of Fundy in the middle of a district characterized by the salmon entering the streams only late in the season and almost wholly as grilse (small salmon). The transplantation was made in 1932 by Mr. H. C. White after studying the behavior of the local fish. He followed the result during the following years, marking the Restigouche smolt when they descended to the sea in 1934 and trapping the adults during the seasons of return. The experiment was concluded in 1936. He was unable to detect any difference between the Restigouche fish and the local fish in size (year of return), in season of return or in any other character except rapidity of growth in the stream, for which the conditions were not comparable. While such a result is no proof that races do not exist elsewhere, it is evidently desirable that local populations should not be considered to be racially

[1] SCIENCE, 85: 477–478.

[2] Ann. Rep. Biol. Bd. Can., 1932, 1933, 1934, 1935 and 1936: 43, 43, 10, 8–9, and 10–11, 1933, 1934, 1935, 1936 and 1937.