BIOLOGY AT THE UNIVERSITY OF MAINE

IN view of the importance of questions of academic tenure and of administrative attitude toward teaching and research, recent events at the University of Maine concerning the department of biology may be of interest. On November 23, 1926, Associate Professor Alvalyn E. Woodward and I were dismissed with no charges of neglect of duty, inefficiency or misconduct. When the administration objected to the display of the letters of dismissal to numerous biologists it was asked for a full statement of the true causes of dismissal. No reasons were given with reference to Dr. Woodward, and she has since been reinstated. With reference to my own case the correspondence printed below is self-explanatory.

On account of the plans of the administration for the second semester of the present year, an instructor and a full-time assistant resigned and left at the close of the first semester. The professor of botany has likewise resigned and it appears that the department will be entirely reorganized for the coming year.

P. W. WHITING

BUSSEY INSTITUTION

November 23, 1926

College of Arts and Sciences
Dean's Office
DR. P. W. WHITING,
Campus

Dear Dr. Whiting:

After consultation with the president of the university it has been decided that the department of biology is not efficiently organized for carrying on the necessary work here. As you know, Dr. Little had visions of a strong research department and you and others were brought here with that end in view. While we expect to emphasize research work to a considerable extent we feel that we can not give it the predominance which Dr. Little expected. We are, therefore, planning to reduce the staff somewhat, and this letter is to inform you that the chair which you now hold will be discontinued at the close of the academic year. I wish to make it perfectly clear to you that your scholarship and ability as a biologist are highly appreciated and that your going is a result of a situation which has arisen rather than from any criticism which we have to make upon your work. Both President Boardman and myself will be glad to make this situation clear to the department officials of any institution where you may care to seek a position.

Very truly yours,
(*Signed*) JAMES S. STEVENS.

January 18, 1927

The President
PROFESSOR P. W. WHITING,
Campus

Dear Professor Whiting:

Replying to your letter of the eighth instant, in which you suggest that I give you further information regarding our request that you sever your connection with the institution, I am giving you the following statement.

During the year and a half in which I have been in a position to judge, the department of biology has not functioned properly under your direction. It is not necessary for me to cite instances or discuss reasons. It is sufficient to say that the department has not shown signs of any recovery and a reorganization is necessary. It does not appear that you are primarily interested in the administration of a department, especially of one which from its position must render all the different kinds of service which in this institution we are obligated to give. The head of the department must in addition to other qualifications be able to guide it with a firm hand and so correlate its activities that it will render efficient service in *all* of its objectives. This you have not done, and this fact, together with your own statements, in which you have shown that your chief interest lies along other lines, show that you do not fit the position you now hold. That this is not against you as a scholar or a biologist is conceded, and the letter written by Dean Stevens, which evidently created a misunderstanding, attempted to make that clear without going into further details.

Permit me to say that it is far from the truth that you are severing your connection with this institution because you are a research man. It is because you lack additional qualifications which are vitally necessary for the head of any department. Whether this lack is due to inability or to lack of interest does not enter the discussion.

For your information I will say that it is my intention to foster the research work of the department of biology. I should not consider that the department could flourish without it. It is also my intention to see that a strong teaching department is developed so that the demands made upon it from all sources may be met.

Very truly yours,
(*Signed*) H. S. BOARDMAN,
President

February 1, 1927

PRESIDENT H. S. BOARDMAN,
Campus

Dear President Boardman:

Your letter of January eighteenth has been received. I assure you that, in accordance with your wishes, I shall take means to bring this letter to the attention of numerous biologists and to any who may be interested in affairs at Maine.

It is evident that my method of running a department is very different from that of which you approve. My policy is to have responsible members only, dismissing the irresponsible The "firm-hand" policy with all its time-wasting watchfulness would not then be necessary. It is well known that the latter method has recently been employed in another department with the approval of the administration and with results not conducive to the best type of scholarship.

I can not admit any lack of attention to management of the department or to details connected with teaching. Moreover, it should be recognized that, in addition to my

own efforts, Mrs. Whiting has devoted considerable time and attention in preparation of material for courses and in instructing individual students, thus relieving other members of the department.

My "Program for Teaching and Research in Biological Science at the University of Maine," which was submitted to you after President Little's departure, was worked out with much care and approved by several eminent biologists at the university. It was, however, negatived by the administration in points essential both to efficient teaching and to research, despite the fact that the present budget allowance was ample to cover all expenses involved.

I assure you that I am leaving Maine with no feeling of resentment, but with the hope that a more constructive policy will be pursued in the future.

Very truly yours,

(*Signed*) P. W. WHITING

SCIENTIFIC BOOKS

Textbook of Comparative Physiology. By CHARLES GARDNER ROGERS. McGraw-Hill Book Co., New York. 1927. List price, $5.50.

To state that Professor Rogers's book is new and different from others tells little; yet even this feeble remark may attract attention to the work among the books of the year. The present writer is not a physiologist, but he feels moved to say something about the book for the benefit of his non-physiological brethren. Professional physiologists will soon be familiar with it; zoologists and botanists working in other fields need to have it brought to their notice.

A few of the chapter headings may be listed: II. Solutions, III. Diffusions and Osmosis, V. Properties of Protoplasm, VII. General Phenomena of Life, X. Blood as an Oxygen Carrier, XIII. Circulatory Mechanisms, XVIII. Catalytic Actions of Animals, XXII. Nutrition of Different Animal Groups, XXVI. Physiology of Movement. These rather familiar titles, most of which are found in all physiologies, suggest the general scope of the work and yet they do not give any intimation of the freshness of treatment and the breadth of outlook which our author brings to us. In his hands, physiology becomes functional biology, the real science of life.

Dr. Rogers compassionately spares us the multitude of algebraic formulae and soul-corrupting graphs now so popular. He is teaching physiology, not mathematics, physics and chemistry. He gives clear pictures of life processes in general and offers a wealth of information about the physiology of invertebrates not obtainable in our usual books of reference. He presents his material in logical and interesting form with no apparent bias for pet theories. If an outsider might presume a suggestion, it would be that in future editions the introductory part to each chapter be somewhat amplified or that a rather "popular" summary be placed at the close of each chapter.

Every young zoologist and botanist (and some of the older ones, too) could profit by knowing the book and making use of it.

FRANCIS RAMALEY

UNIVERSITY OF COLORADO

Ice Ages, Recent and Ancient. By A. P. COLEMAN. New York: The Macmillan Co., 1926. pp. 296, 51 figs., 8 maps.

IT is the stroke of the master pen. Only mastery could produce so complete, frank, simple and obviously trustworthy an account of the Ice Ages of the earth. Many accounts of personal experience enliven the style of the book. As an introduction to the work of ancient glaciers, the activities of living glaciers are sketched with a few well-chosen examples. The Pleistocene glaciation is treated only briefly, since "the work is not intended to take up the Pleistocene in great detail, but rather to outline its extent, to describe its mode of operation and to study particularly such features as will throw light on more ancient and therefore less completely recorded glaciations." The drift, the extent, the centers of radiation and the interglacial periods are discussed both for North America and abroad. One interesting point in North America is that the Cordilleran sheet was formed first and was followed in succession by the Keewatin and then the Labradorian sheets. It is inferred by the reviewer that this applies to the last of the Pleistocene sheets. It would be of great interest to know whether this succession holds for the earlier of the Pleistocene invasions. Doubtless data are not available to answer this question, for the author makes no mention of it. Four interglacial periods are recognized near the drift-margin in North America, while at least one is distinguished in Canada near Toronto and Moose River. Likewise three warm interglacial periods are accepted in the Alps and in Denmark, while studies of the interglacial climates have led to the conclusion that the ice in Europe, as well as in North America, was completely removed at least once during the Pleistocene.

Ancient ice ages are beautifully described. Eocene and Jurassic tillites in North America are discussed. In all the periods of the Paleozoic era, glaciation is either strongly inferred or is proved. Of these the world-wide Permo-Carboniferous glaciation, the greatest in the history of the earth, receives its due consideration. Chapters on the Talchir tillite of India, the Dwyka of South Africa, the Squantum of North America and the tillites and interglacial deposits of