of the cell and to stratify the cytoplasmic and the nuclear contents.[6] The results were that the cells so mutilated or disturbed were capable of further growth, division and differentiation.

(2) *The cells without nuclei of the Cyanophyceae.* According to modern plant cytologists there is a large number of Cyanophyceae (blue-green algae) of which the cells are without nuclei. This conclusion, it is true, has been reached only after a long period of hesitation, but the reason of the hesitation is to be found in the influence of the cell theory. The adventures of some investigators who wanted to find a nucleus in the cells of these plants would be amusing, if they were not a bit tragical. The Cyanophyceae were, for many years, considered as consisting of anucleated cells. Then it was found that they possess chromatin. This could do for a nucleus except that it was not aggregated in one mass, as in ordinary nuclei. So the notion of "diffuse nuclei" was created and readily accepted. But, a real nucleus should divide by mitosis. Sure enough, mitotic figures, although strange ones, having but a vague resemblance to typical mitotic figures, were observed. Finally, it was shown[7] that the chromatin of the Cyanophyceae has nothing whatever to do with the cellular division and that it appears to be an accidental and insignificant part of the cell. So now, as 50 years ago, the more plausible conclusion is that the Cyanophyceae consist of cells without nuclei.

In the case just mentioned the significant fact concerning the cell theory is the existence of cells without nuclei. That some investigators have been victims of the theory should not be brought as a charge against it, since it is not its content which is responsible for the mischief but the mere fact that it is a theory and, therefore, as we said, a dangerous tool.

The history of the development of the cell theory and its present status can be summarized as follows. One hundred years ago, Schleiden, a botanist, and Schwann, a zoologist, were struck by the fact that most plants and animals are built on the same plan, that is, by the juxtaposition of cells.[8] This interesting similarity led these two pioneers to generalize the observed fact. So they formulated the cell theory. During the one hundred years which have since elapsed, a number of new observations have been made which are not in good agreement with the theory: in particular, the existence of the coenocytes, their biological features, the similarity of differentiation in acellulars, monocellulars and pluricellulars, and the biological properties of infracellular units. Consequently, the theory of Schleiden and Schwann appears to the modern unbiased observer as a rather daring and unjustified generalization.

The body of biological knowledge which grew and developed so enormously during this century finds itself now enclosed in a tight carapace which hinders its further development. But our contention is that the carapace has cracked and that, after an instar period of one hundred years, the molting time has come. We invite the biologists, on the occasion of this centenary, to band together for removing the carapace.

A new theory will necessarily replace the old one. We expect that, in the elaboration of this new theory, consideration will be given to the fundamental fact that in the higher animals, when the developing embryo has reached a certain size, it undergoes a division into metameres, superimposed to the division into cells, while the less differentiated and usually smaller organisms (as the hydra) do not divide into metameres but have ordinarily their body partitioned into cells, and the smallest and least differentiated forms (as the bacteria) do not present any known partition. According to a new theory based on these principles, the cell would then cease to be the "fundamental structural and functional unit" of any living matter and the cellular structure would become, in the last analysis, simply one of the various methods used by nature to partition a large mass of protoplasm.

OBITUARY

ROYAL NORTON CHAPMAN

ON December 2, 1939, died Royal Norton Chapman in Minneapolis, after a short illness, and thereby was removed from active service in the science of animal ecology, one of its most distinguished servants.

Royal Norton Chapman was born in Morristown, Minnesota, on September 17, 1889, and after preparatory work at Pillsbury Academy in Owatonna, he entered the University of Minnesota, taking the degree of B.A. in 1914 and M.A. in 1915. Following this he was Schuyler fellow in entomology at Cornell University, taking his doctorate there in 1917. His University of Minnesota appointments began as a scholar in animal biology, in 1912, and ended as dean of the graduate school and professor of ecology, beginning July 1, 1939. In the interim he was a Guggenheim Memorial Foundation fellow, and traveling professor of the International Education Board of the Rockefeller Foundation, 1926–1927, and for nine years, from 1930 to 1939, the director of the experiment

[6] H. W. Beams and R. L. King, SCIENCE, 84: 138, 1936.
[7] G. Poljansky and G. Petruschewsky, *Arch. Protistenk.*, 67: 11, 1929.
[8] *Cf.* the title of Schwann's mémoire.

station of the Pineapple Producers Cooperative Association at the University of Hawaii, Honolulu. He was a starred member of the American Association for the Advancement of Science and a member of the Society of Naturalists, the Entomological Society, the American Association of Economic Entomologists, the American Society of Zoologists, the Ecological Society of America, the British Ecological Society, the Hawaiian Entomological Society, the Hawaiian Academy of Science, the American Association of University Professors, Phi Kappa Phi, Sigma Xi, Gamma Alpha and Alpha Zeta. He is survived by his widow, Helen S. Chapman, and three children, Frances, Joyce and Kent.

Dr. Chapman's career had just entered its third phase when death overtook him. The first phase might be said to have begun in 1917 when he started his lecture course in animal ecology, and ended when he left the University of Minnesota to assume the directorship of the Pineapple Experiment Station in 1930. When the writer first met Chapman, it was in this lecture course, in 1923, and by that time it was evident that a new and fundamental approach was being developed, and a new "school" founded. Most of the students at that time were men of rather mature years, many of them with field experience behind them. On these men, Chapman's brilliant exposition of the field, his insistence on an objective, quantitative approach and his marshalling of what had previously been a wide array of scattered facts, left a permanent impression; and on their work, the unmistakable imprint of a master.

In the climate of Minnesota it was inevitable that attention would center on the environmental responses of laboratory animals and thus was begun the long series of studies by Dr. Chapman, and a devoted group of students of animal environments, on *Tribolium confusum* Duval. Whether Tribolium will be to ecologists what Drosophila is to geneticists remains to be seen; whether or not "biotic potential" and "environmental resistance" are but old concepts in new dress is not particularly important. What is important, is the indubitable fact that for the old natural history approach has been substituted a method comparable with that of the exact sciences, whereby the environmental responses of insects can be studied in an objective, quantitative manner. Both concept and method have since been widely used and will continue to influence students in that field for a long time to come.

The second phase of Chapman's career began in 1930 when he left Minnesota to become director of the Pineapple Experiment Station in Honolulu. From the standpoint of the continuation of *Tribolium* studies, this move was disastrous, but to the then newly expanded station devoted entirely to pineapple problems, Chapman brought a rich academic tradition and the bold initiative of a man fresh from recent successes. Then followed the golden age of the Pineapple Experiment Station and as a result the pineapple industry passed from a stage wherein biological problems in the field were the limiting factors to one wherein the problem is essentially that of disposing of a stabilized production. To the Graduate School of Tropical Agriculture, Chapman also brought to Hawaii a vision which some day, when the United States realizes more fully the enormous possibilities for research in tropical agriculture in the Islands, will bear fruit. But even though developments along these lines have been slow, they have been real, nevertheless.

It was during this nine-year stay in the Islands that Chapman's astonishing versatility made itself evident. As a local newspaper said on his leaving, "He has taken his place among men who give themselves and their energies and talents to the progress of Hawaii in cultural as well as in national affairs." He was in turn, president of the Institute of Pacific Relations, and the Y. M. C. A., both of which posts made large demands on his time. In addition to these, there was scarcely a single phase of public service to which he did not at one time or another make useful contributions. But although he revelled in the active and extremely varied life in the Islands, it was inevitable, to those who knew him, that when the opportunity came to return to his academic life, he would take it. For there was where his heart was. It was quite true that his life in the Islands had admirably prepared him for the public relations aspects of his new post as dean of the Graduate School in Minnesota but the everlasting conflict between the academic point of view and the requirements of a privately supported experiment station were extremely distasteful to him. He had, for nine years, privately financed and conducted his experiments on Tribolium populations, under many difficulties, and he left Hawaii in the summer of 1939 to return to his real field of research.

We in Hawaii were spared the intimate contact with the grief of his passing and the writer is glad that his last personal recollection of him is as he saw him in September of last year, full of enthusiasm for the graduate school and for the rehabilitation of his personal research program. Chapman's first appointment at Minnesota was as "scholar in animal biology." His last appointment, though called by a different name, meant just that.

WALTER CARTER

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