Theodore Lyman, a Pioneer in Far Ultraviolet Spectroscopy

THEODORE LYMAN, professor emeritus of physics in Harvard University, whose death occurred on 11 October in his 80th year, came of an old well-to-do family distinguished for philanthrophy and public service. His father was a marine biologist, a student under Louis Agassiz, once a member of Congress, and always a devoted friend of Harvard. Born in 1874 in Boston, Lyman attended various schools before entering Harvard in 1893. In some notes which he wrote he said that the selection of his schools was "governed more by the degree to which they suited my feeble health than by any considerations of the quality of instruction which they afforded." His uncertain health was a handicap from which he was never entirely free. This prevented him from taking part in any athletic pursuits, and may have produced in him a certain shyness or reserve. He once playfully said in connection with Radcliffe College that he approved of the education of women but wished it to take place at some distance from where he was. Under the same heading, perhaps, belongs the fact that he never married. He had, however, many friends and he was a member of several clubs. He was always a delightful guest and a most gracious host.

To those who worked with him Lyman showed a warm and friendly personality. The staff of the Jefferson Physical Laboratory of which he was the director were devoted to him. He was respected for his judgment, honored for his achievements, and loved for his kindness and generosity. Few knew of his many benefactions. On a larger scale he was a donor to hospitals and to Harvard itself. When it came to planning the new research laboratory that now bears his name, it was he who worked for years to raise the necessary funds and who contributed largely in money, time, and strength.

During his teens Lyman had an interest in chemical experiments and mechanical devices, but it was not until his years in Harvard that he began to devote himself to physics. There he fell under the influence of Wallace C. Sabine, especially in his optics course, which interested Lyman greatly. He must have done well, for he was invited to return as a graduate student, and then took on a research subject, suggested by Sabine, that occupied him for the rest of his active life—the exploration of the far ultraviolet spectrum.

V. Schumann had already discovered a spectrum of hydrogen so far out that the rays would not pass through air. Schumann had used a prism vacuum spectrograph but could not measure wavelengths. Sabine suggested that Lyman make a vacuum spectrograph with a grating in it. Lyman made an ingenious mounting furnished with two slits, with which he could photograph simultaneously different parts of the spectra of different sources on a plate. One spectrum was displaced by the distance between the slits, and when this distance was measured the wavelengths of the unknown spectrum could be found from those of the known one. Lyman's first paper was about the false lines produced by this grating, and their origin in irregularities in the ruling of the grating. He then measured the hydrogen spectrum, including the lines of the atomic series now often known by his name, and the lines that are given by the molecule of hydrogen. Later on he measured a great many other spectra, and found the strongest lines that form the simplest series for helium. His early work was limited to a range of wavelengths from 2000 to nearly 1000 A. After the war he pushed the outer limit down below 500 A. He also examined the transmission of gases and solids in this region, and the reflection of many solids. He discovered that for the shortest wavelengths a glass grating was better than a metal one, provided that the rays reflected from its surface were used. His list of 33 published papers shows a very extensive study of his field. There is also one paper on the sterilizing action of these rays, and others on diffraction and the luminescence of various substances exposed to them. His book entitled *Spectroscopy of the Extreme Ultra-violet* appeared in 1914, with a second edition ten years later.

While carrying on his research he climbed the academic ladder to the top, becoming Hollis professor of mathematics and natural philosophy in 1921. He was chairman of the physics department for about 15 years, and director of the Jefferson Physical Laboratory for 30 years. He won the Rumford medal of the American Academy of Arts and Sciences, and served as president of that academy for one term. He was also president of the American Physical Society, and won the Elliott Cresson medal of the American Philosophic Society, and the Frederick Ives medal of the Optical Society of America. He became a fellow of the National Academy of Sciences in 1917. He was also a fellow of the Royal Institution and of the Royal Geographical Society.

In the first World War he attended two voluntary military camps, was adjutant of the Harvard Training Corps, and went to France in 1917 as a captain in the Signal Corps to develop flash and sound ranging. There he soon had charge of a training school in this technique, and then, from April 1918 until the end of the war he was in charge of a flash ranging unit at the front, serving in three battles, including the critical operations on the Marne in June and July. He returned in March 1919 as a major in the Second Army in charge of a battalion of more than 1000 officers and men. He spoke of this miltary service as one of the "durable satisfactions" of his life, but he placed his experimental research ahead of it.

For many years he took long trips during his summer holidays. These took him to Japan, Korea, British East Africa, the Altai Mountains in Siberia, and Alaska. He also hunted in our Northwest and in British Columbia, and fished in the Gaspé Peninsula. Some of these trips had a scientific tinge; one, in particular, had as its goal the collecting of ticks from the bodies of mountain goats, which acted as vectors of Rocky Mountain Spotted Fever. Each of these expeditions served to renew his health for the next academic year.

In May 1930 while on a boat to England he was stricken with a ruptured appendix and spent the summer in a hospital. On his return he had to submit to a corrective operation. The whole experience was so severe that he never completely recovered from it. Nevertheless he continued research at a reduced pace, and still carried administrative responsibilities for several years.

His ability to drive himself against the drag of bodily ailments was realized by only a few of his friends. In the notes on his life that have been used for this article he says that he is struck by the number of hours of hard work he has been able to get out of a very imperfect system—that is, himself. The driving power came from within, and is the more impressive when we consider that he never had the spur of being compelled to earn money. Not many men in his circumstances would have labored so devotedly for the advance of pure science as he did.

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News and Notes

A Return to Reason

A very significant article has been published in the July-August issue of *Transactions* (Izvestia) *of the Academy of Sciences of U.S.S.R* [No. 4, Biological Series (1954), pp. 97–120]. Nothing like it has appeared in the U.S.S.R. since August of 1948. It is a review by V. I. Kremiansky on "Certain problems of general biology in modern Western literature. On the status of Morganism." The sense of the article is that Morganism (alias genetics) has made important advances in recent years. Although still not quite acceptable, genetics has evolved in the right direction.

The bibliography of Kremiansky's review contains more than 250 references to recent genetic literature, including some work of authors whose names were not mentioned in the U.S.S.R. for several years. Much attention is devoted to the research on the genetics of microorganisms, adaptive enzymes, physical and chemical mutagens, the problem of crossing-over, chromosome chemistry, cytoplasmic inheritance, heterosis, polyploidy, and chromosomal variations in natural populations. Another review of the modern Western literature concerning evolution is promised to follow.

The review covers so wide a field that Kremiansky's descriptions and comments are of necessity terse but usually to the point. He obviously knows what he is writing about. This does not mean that modern genetics has for him much validity. He is quite skeptical about the theory of the linear arrangement of genes in chromosomes. The concepts of genotype and of norm of reaction are not even mentioned, and the observations of Monod and others on adaptive enzymes are interpreted (though not without hesitation!) as showing inheritance of acquired traits. The high frequency of inversions in populations of many species of *Drosophila* is, curiously enough, taken to contradict the continuity of the chromosomal organization. However, Kremiansky's disapproval of these fundamentals of genetics is based on arguments other than that they disagree with the dicta of Michurin or Lysenko or that they were invented by a wrong sort of people.

Most geneticists will agree that the concepts of this science have changed a great deal during the last 20 years or so. But this is a doubtful warrant for the statement:

> It is no exaggeration to say that if anybody wanted now to return to the theoretical views of 1936–1937, this would be impossible because in place of the old 'fortress' (of genetics) there remain only scattered building blocks—facts without a general theory.

Few geneticists ever thought in terms of "fortresses," and many of them, certainly including the late T. H. Morgan, found a great pleasure in changing "general theories." Similarly overstated is the supposed contrast between the "old" and the "new" genetics given by Kremiansky in a tabular form at the end of his review. But however much one may disagree with Kremiansky about these and other points, his opinions could be profitably discussed and argued with. His closing sentences are worth quoting in full:

> The present state of Morganism is characterized by negation of the old rather than by affirmation of the new. This is an earmark of crisis of a scientific theory. Nevertheless, the direction of change in this field is already clear. Essentially, this is toward the rejection of the theoretical bases of the Morganism proper. This has been unavoidable. Science cannot reconcile itself to distortions of what is objectively valid.

To this last the Morganists will say "Amen."

I am indebted to I. L. Kosin, Washington State College, Pullman, for having called my attention to