analogous to the appearance of a point of light which appears through clear glass as a point but through slightly ground glass as somewhat diffused owing to the small irregularities caused by the grinding. Whether these variations will be amenable to such successful treatment as, for example, the statistical study of travel-time curves by Jeffreys, must be answered according to our states of mind, ranging, in Gutenberg's facetious phrase, from his own optimism to Macelwane's pessimism!

The whole subject of seismology is complex, somewhat as the field of economic or sociological phenomena; it grows out of and depends on a variety of superposed causes and elements and is therefore especially difficult of analysis. In its applications to seismology, mathematics must examine not merely its validity but its sufficiency, for in this field its sufficiency *is* the measure of its validity. In closing, we can only join with the Countess in "All's Well that Ends Well":

> Will your answer serve fit to all questions?
>
> . . . .
>
> It must be an answer of most monstrous size that must fit all demands.

OBITUARY

CALVIN BLACKMAN BRIDGES

THE death of Calvin Blackman Bridges on December 27, 1938, is a serious less to genetics and also a personal loss to his many friends. Taking part from the beginning in the *Drosophila* investigations that started at Columbia University about 1910, he became, after obtaining his doctorate, a member of the small group supported by a grant from the Carnegie Institution of Washington. He was still a member of the staff at the time of his death. During these twenty-five years Bridges made a long series of contributions that won him wide recognition as an outstanding genetic investigator.

He was born in 1889, at Schuyler Falls, New York State, and his early years were passed near Plattsburg, N. Y. Beginning as an undergraduate at Columbia he was my private assistant from 1910 to 1915, and fellow 1915–16, taking his Ph.D. in 1916. As stated above he was a member of the "Carnegie Group" 1915–38. In 1936 he was elected to the National Academy of Sciences.

A bare list of the titles of his papers from 1913 to 1938 would give some idea of the nature of the many contributions he has made. His paper on non-disjunction has become a classic; it adduced convincing evidence that chromosome movements furnish the mechanism of heredity. This evidence rested both on observational work and genetic experiment. What seemed at first an exception to accepted genetic interpretations turned out a brilliant confirmation of them —the exception that proved the rule.

Bridges' early discovery (1917) that certain genetic data could be interpreted as due to deficiencies in the chromosome-construction has led in recent years to a factual demonstration of such deficiencies. In some of his latest work (1937–38) he made use of this discovery in the interpretation of overlapping deficiencies to demonstrate the characteristics of certain mutant types. It would be hard to find in the history of genetic research a more convincing demonstration of the combination of factual evidence and masterly interpretation of it. As early as 1919 Bridges described "duplication" as a chromosomal aberration, and here, as in his other work, his conclusions rested not on guessing or vague hypotheses but on experimental proof. Much later he also reported the occurrence of "repeats" in the normal chromosome which will have to be seriously considered in future interpretations of certain types of genetic behavior.

His work on sex determination was a brilliant venture into a more theoretical field, although here, too, it is important to observe that there was no idle flight of speculation but an adherence to actual evidence based on his own thoroughgoing observations. His interpretation of the effects of tetraploidy, triploidy, haploidy on the constitution of the individual is an outstanding contribution to the theory of sex determination in such forms as *Drosophila,* where the outcome is not complicated by the presence of sex hormones in the conventional use of this expression. This work led him to a theory of gene balance that applies not only to problems of sex determination but more broadly to gene balance involving the physiology of phenotypic expression. His interpretations of balance in sex determination in particular inclined him to believe that it is unwise, *i.e.,* not in accord with the evidence at hand, to look for a male-producing and a female-producing gene, this being too naïve a way of expressing the facts, which are more probably due to balance of many kinds of genes more or less widely distributed in the chromosomes. This does not mean that some genes may not be more influential than others in regulating the development of one or the other sex, which may well be the case, but the search for genes concerned only with sex has up to the present not been successful.

In recent years Bridges has spent much time in revising the genetic maps which are the standard ones wherever *Drosophila* is used. His work here was more than a routine job, for he devised ingenious methods to meet some of the statistical problems involved. The discovery of the large striated chromosomes of the cells of the Malpighian tubes and of the salivary glands of Dipteran larvae by Heitz and Bauer and by Painter in 1933 opened the way for demonstrating some of the earlier conclusions reached by genetic analysis. They pointed out the constancy in the seriation of the banding along these chromosomes, and Painter emphasized the point to point apposition of the two homologous strands. He also went further and demonstrated the identity of particular sections of the salivary chromosomes with particular sections of the genetic map by utilizing the available materials for translocations, deficiencies and inversions. Bridges (1935) then made an elaborate study of the salivary chromosomes, and his more recent work has more than doubled the number of visible bands. These maps bid fair to become the standard ones for *D. melanogaster*. It should be pointed out that the identification of the salivary bands with the genetic map would not have been possible were it not that during the preceding twenty-three years the genetic maps had been built up to a point where such comparisons had a real, demonstrable basis. While many workers had contributed to bring the genetic maps to their status in 1933 it was Bridges in particular who had made a more detailed and critical study of the maps than had any single one of his contemporaries. It is generally recognized that the building up of stocks, containing efficient combinations of genes suitable for special genetic problems, was carried out by Bridges. Any one who is familiar with the labor and ingenuity involved in making such combinations will realize what a very great assistance Bridges has given to the workers in this field.

In the course of the 25 years that the map-making has been going on, more than 900 stocks have been constructed that are invaluable for the pursuit of many genetic problems. There is no other material comparable with this, and to-day the "Carnegie Group" is faced with the responsibility of maintaining these cultures, each of which is carried in three-fold for safety. This work involves most careful supervision to insure the purity of the material; for experience has only too well shown that if not carefully watched the stocks may deteriorate. These stocks are available to-day for research work anywhere in the world and have been widely used.

Since 1934 Bridges and Demerec have printed for private distribution (under the auspices of the Carnegie Institution of Washington) nine large volumes called "Drosophila Information Service" that bring together the vast amount of work in this field up to date. This undertaking was arduous in the extreme, and I am afraid it overtaxed Bridges and diverted him to some extent from his more important pioneering work. He has left behind a very large amount of unpublished data. Fortunately the requirements of the Carnegie grant were such that each year a report of progress had to be made (see Reports Nos. 15–37). In consequence the twenty-three reports give in briefest summary the results that Bridges had obtained. Whether the elaborate data, that are on file, on which these reports rest, can ever be fully utilized is questionable; but Bridges accomplished so much other work they will not be needed to place him amongst the leading geneticists of his time.

T. H. MORGAN

HENRY VAN PETERS WILSON

HENRY VAN PETERS WILSON, Kenan professor of zoology in the University of North Carolina, died in Duke Hospital, Durham, N. C., on January 4, 1939, and was buried at Chapel Hill on January 6, a few weeks prior to his seventy-sixth birthday. He was born in Baltimore, Maryland, on February 16, 1863, and was a son of the Reverend Samuel A. Wilson and Sophia Anne Stansbury Wilson.

Professor Wilson was educated in the schools of Baltimore and Johns Hopkins University. Following graduation from Hopkins in 1883, he was registered for a short time in the Medical School of the University of Maryland, but soon found that his interests were primarily in biological science rather than in clinical medicine. He transferred to the graduate school of Johns Hopkins and began work in zoology under Professor W. K. Brooks, who at that time was drawing into his laboratory a number of able young men. Under the inspiring tutelage of Professor Brooks, and in company with these eager fellow students, many of whom have since added luster to American science, H. V. Wilson worked for a number of years. He received the degree of doctor of philosophy in 1888 and continued at Hopkins as Bruce fellow until 1889. From 1889 to 1901 he worked at Woods Hole in the laboratory of the U. S. Fish Commission.

In 1891 Dr. Wilson, then a young man of twenty-eight, went to the University of North Carolina as professor of biology. With the separation of the departments of botany and zoology in 1904 he became professor of zoology, and he continued as head of that department until 1936. He became Kenan professor of zoology in 1917. At Chapel Hill Professor Wilson soon came to be recognized as a critical and inspiring teacher. Severe discipline and rigorous thinking became outstanding characteristics of his department. His insistence on thorough scholarship and his enthusiasm for research, shared by a number of his young colleagues, were important influences in laying the foundation for a tradition of creative scholarship in what was then a small isolated institution with an honorable history but suffering from the post-war poverty of the South. This enthusiasm for research and for building up the facilities for research caused