genetic basis of shape differences in the fruits of the Cucurbitaceae. These characters can be described by the patterns and shape indices of the mature fruits, but such tell only part of the story. It is essential to learn the developmental history of each type if we are to find what the genes actually control here. When length and width are measured at successive stages from ovary primordium to ripe fruit it is found that they grow at different rates, so that the fruit changes in shape somewhat during its development. The relative growth rate is consistently different in different races. In the Hercules club, length grows faster than width, so that the fruit becomes progressively more elongate. In the bottle gourd, on the other hand, width grows faster than length. Within a given race, however, this relationship is so unvarying that it may be expressed by a simple value or constant and thus used to describe very precisely the most important aspect of a fruit-shape difference. This constant relative growth rate segregates in inheritance and seems to be what the genes governing shape primarily control. It thus constitutes an important step into that unknown territory between the gene and the visible shape which this determines. The existence of such a constant relationship as this in the midst of developmental diversity and change could not have been recognized without a careful descriptive study of the entire history of the growing fruit, expressing its results not only in words but in measurements.

Such examples could be multiplied almost indefinitely, and from work with animals as well as with plants. The whole domain of developmental morphology, illuminated by the ideas and view-point of morphogenetic research and attacked by quantitative as well as qualitative methods thus offers a wide field for fruitful investigation. Let no one disparage such studies as "merely descriptive." Description must precede explanation, and in the combined attack on the problem of organization the morphologist should be a leader, not a follower. His is the task of the pioneer entering a wilderness of facts, which must be explored and cleared up before those who follow in his steps can practice their arts of greater refinement and precision.

For the welfare of biology as a whole, therefore, it is my plea that those who have been trained in the rigorous disciplines of morphology may turn in increasing numbers to the more dynamic aspects of their subject. Especially let us hope that those younger botanists and zoologists who choose to devote themselves to the problems of organic form may realize that these can not be set apart as a static compartment of biological thought but must touch and illuminate the whole. May they help to resolve for us this fundamental paradox: that protoplasm, itself liquid, formless and flowing, inevitably builds those formed and coordinated structures of cell, organ and body in which it is housed. If dynamic morphology can come to the center of this problem, it will have brought us close to the ultimate secret of life itself.

OBITUARY

STANLEY R. BENEDICT

THE death of Stanley Rossiter Benedict on the night of December 21 was a grievous shock to his friends and colleagues. He was only fifty-two years of age, and while he had suffered some physical disabilities in recent years he seemed to his friends to be in the prime of useful life until about a week before his untimely end.

Benedict's claims to distinction are of a very substantial order. As professor of biological chemistry in the Cornell University Medical College, he was a teacher of wide repute, who added much to the dignity of a young and growing department, where many workers of both sexes obtained not only knowledge but standards of scientific integrity which served them well in later life. His collaborators make up a lengthy list, and in addition to the younger workers his long association with Emil Osterberg, who survives him, is happily commemorated in many joint publications. Like all generous men Benedict was only intensely pleased with the successes that came to his former pupils. His early training had been in part at New Haven, and in most respects he was a true disciple of the Chittenden-Mendel tradition of physiological or metabolic chemistry. He possessed in addition masterly skill in analytical chemistry, a sound appreciation of physiology and considerable knowledge and ready understanding of the problems of structure that organic chemistry was presenting to the developing science of biochemistry. His attitude to the purely physical side of his subject may probably be described as receptive and sympathetic rather than enthusiastic.

His skill as an analyst can only be compared with that of Folin, with whom it must be confessed he was frequently in spirited argument, which only served to cement the underlying friendship of the two men, who really had much in common. Benedict's researches on the estimation of sugars, creatine, creatinine, purines, uric acid, phenols, sulfur, glutathione, ergothioneine and many other substances, by both macro and micro methods have become part of every biochemist's training. But he was not content with analysis for its own

sake and not infrequently made important discoveries of new substances whose presence had been indicated by the use of his precise analytical methods. Thus, for example, he isolated the interesting sulfur-containing compound "thiasine" from blood corpuscles and later identified this substance with ergothioneine, which had hitherto only been encountered in ergot. In similar fashion he was led to the isolation from blood of a beautifully crystalline compound of uric acid and ribose, and the guess may be hazarded that this totally novel discovery gave him as much personal satisfaction as any of his other investigations. Benedict's work in the field of metabolism covered an extensive range. Many fruitful investigations were carried out on glycosurias of various types and on the creatine-creatinine problems, while his work on the relation of the kidney to ammonia formation and excretion was stimulating and distinctly upsetting to the currently accepted doctrines. For a long time Benedict was associated with the Memorial Hospital in New York City and in conjunction with his old pupil Sugiura was responsible for a vast amount of useful information concerning the influence of various chemical and other agents on the growth of tumors.

In 1920 it became necessary for various reasons to find a new home and new managing editor for the *Journal of Biological Chemistry*. The home was provided through the generosity of Cornell University Medical College, and in spite of considerable hesitation, Stanley Benedict, who had long been one of the journal's most distinguished contributors, was at last persuaded to accept the managing editorship. The personal sacrifice involved was immense, but until the day of his death he gave of the very best that was in him to further the interests of the journal and the science that it represented. In this labor of love he was ably supported by Miss Smalley and her devoted associates. Probably few people except editors know much of the never-ending grind and human difficulties entailed in the successful editing of a scientific journal. The *Journal of Biological Chemistry* has indeed been fortunate in this respect, and Benedict has set a standard that will not easily be surpassed. He was an editor who really edited and was not content to pass for publication indifferent material simply because it happened to originate from individuals or institutions of standing. To some extent he had Samuel Johnson's dislike of impairing the clarity of expression of his views or judgments by surrounding them with a sugar coating of innocuous words. He was direct, forceful, tenacious in argument, but absolutely unswayed in his judgments by any consideration other than the facts as he saw them. His intimates knew that under a somewhat stern exterior he was the kindliest and friendliest of men, with a keen sense of humor and a very charming smile. Indeed, Benedict was always susceptible to a little innocent raillery and would go more than half way to meet a joke. On one occasion in early days when as editor he had decided, against the views of at least one of his colleagues, to amputate a good many of the final "e's" that terminate the names of so many biochemical compounds, a solemn request as to whether he proposed similarly to abolish the final "e" in the name of his patronymic liqueur "Benedictine" brought an immediate suspension of operations.

Few men had less desire for honors than Benedict. He was a member of the National Academy of Sciences, a past president of the Society of Biological Chemists and received many other notable distinctions, all of which he bore with a refreshing lightness.

He was born in Cincinnati on March 17, 1884, son of Professor Wayland Richardson and Anne Kendrick Benedict. His father was professor of philosophy and psychology at the University of Cincinnati. His maternal grandfather was A. C. Kendrick, professor of Greek, Hebrew and Sanskrit at the University of Rochester and a member of the American committee for the revision of the King James version of the Bible. He graduated from the Universities of Cincinnati and of Yale, and taught at Syracuse University and Columbia University before going to Cornell University. In 1913 he married Ruth Fulton, of Norwich, N. Y., a well-known ethnologist, who survives him. He had also three sisters, each of whom has achieved professional distinction.

Benedict's memory will long be cherished by his university, by the journal he loved and served so devotedly, and by his many colleagues and friends, who found in him a source of both stimulation and good fellowship.

H. D. DAKIN

GRAFTON ELLIOT SMITH

SIR GRAFTON ELLIOT SMITH was so well known and had so many friends and colleagues in this country that some comment on his life and personality and his contributions to science may be acceptable, even though SCIENCE does not usually print obituary notices of foreign men of science.

With regard to his childhood and youth, not long ago he told one of his recent students that when he was a very young boy he began to collect fossil ferns, which were found near his birthplace at Grafton, New South Wales, Australia. When about fourteen years old he attended an evening lecture on the brain, in which the lecturer described the complexity of the convolutions of the human brain and added that many of these convolutions did not even have names or definite boun-