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Dr. H. A. Gleason, Distinguished Ecologist

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H. A. GLEASON

(See Front Cover)

Dr. H. A. Gleason, Life Member of the Ecological Society of America, is retired from the New York Botanical Garden and is now living in Greenwich, Connecticut. Although he has never been honored by presidency of our Society, he is generally recognized as one of the outstanding ecologists of the first half of the twentieth century.

The following quotation is from a letter written by Dr. Gleason for the younger generation of ecologists. In it one may find the contributions he made for which he is well known and respected as an ecologist.

"Cowles' paper on physiographic ecology appeared in 1901. For five years before that time I had wandered over Illinois, collecting plants and making botanical observations. I had read his earlier paper on sand-dunes, but I had seen no dunes. His later paper on succession in the forests around Chicago intrigued me, and I found that his conclusions held in the forests near the University of Illinois, 120 miles away. There I had an additional problem which Cowles had slighted, on the successions in prairie and between forest and prairie. To answer this I had to get to work myself. I had the advantage of the personal opinions of two keen observers, my Father and Professor T. J. Burrill, who in the early days had seen and marveled at the relation between prairie and forest, and of a great collection of books on the early history and geography of Illinois in the Champaign Public Library.

"For nine years I collected evidence and made observations of my own, and found indisputable evidence that forests succeeded prairie and that prairie had succeeded forest. This was awful. It was directly contrary to the teachings of Clements that succession is an orderly process, proceeding directly and inevitably to the climax. In 1908 I found a remarkable succession on a sand-dune which led to the development of an actual pond with submersed hydrophytes and all the trimmings, and then I knew that I could be right about the successional relation of forest and prairie and that Clements' dogma of irreversible direction of succession could be wrong. Cowles had told me that a knowledge of succession enabled us to look ahead and predict the future of vegetation on any given spot. It now occurred to me that with equal accuracy we could look back and deduce the past of the vegetation. Cowles had never said that succession is a form of migration; Clements had never said or at least never emphasized that migration is impossible without succession. Both of these statements now appeared clear to me. Cowles' causes of rapid succession were just two, physiographic change and plant control. It seemed obvious to me that fire was also a potent cause of succession, that a slower type of succession might be caused by climatic change, and that the relic plants advocated as indicators of succession by Cowles could be supplemented by relic colonies to indicate broader or slower successions. It took nine years to develop these ideas. I finally wrote them up and presented them to a botanical magazine for publication. It was refused and I was told in thinly veiled terms that the paper was just so much tommyrot. Backed by extraordinary recommendations by Cowles and Transeau and I believe also by Adams, the paper was finally published in 1923 by the Association of American Geographers.

"This was not the only work which I did in these nine years. In 1903 I needed some means of indicating the abundance of plants and came up with a very crude but numerical method. Du Rietz, the Swedish ecologist, says this was the very first application of statistics of plant ecology, antedating the classical work of Raunkiaer but not antedating his publication. In 1911 I began to study the relation between the frequency index and random distribution and arrived at a formula which has sometimes been called Gleason's equation. Still later I studied the relation between species and area and again developed a formula which gives surprisingly accurate results. It is nevertheless wrong, as my mathematical son proved, but it is so nearly correct that its errors are far less than the usual personal errors of observation. If these errors are eliminated, the formula is usually accurate to less than one per cent. These two equations are, so far as I know, the historical basis for all modern work on statistical ecology, but they have been so extended and expanded that I am no longer able to read a paper on the subject intelligently, or to understand what it means or how its conclusions were derived.

"About 1905 an epidemic of porous cup atmometers broke out in America. No ecologist was well dressed unless he carried one in his hip pocket and no field work was complete unless a whole battery of them were set up and read at regular intervals. That is all very good, but the claim was made that succession was caused by change in the rate of evaporation. Frank Gates and I published a paper denying this about 1911 and to the best of my knowledge no one has since continued this claim.

"When I first began field work, the association was an organism. It had to be: Clements said so, and so did Tansley. As time went on and I became better acquainted with more kinds of vegetation, it became ever clearer that an association and an organism had essentially nothing in common. Next it became evident, from actual field observation, that two separate patches of the same association were never exactly alike, either in component species or in relative number of individuals of any species, and that the degree of likeness was roughly inversely proportional to their distance apart. I examined the floodplain forests of the Mississippi over four hundred miles; I examined the beech-maple forest at many stations from Lake Superior almost to the Ohio River. Each one of them formed a continuum, as Curtis would say today, and in each the unimportant and scarcely appreciable differences from one mile to the next cumulated into profound differences as miles were measured by hundreds. From that it was only a short step to the conclusion that the plant life on any area of soil is the resultant of two factors, the local environment and the available species, and that every variation of the environment, **whether in space or in time,** and every variation in the local flora produce a corresponding variation in the structure of vegetation.

"This was published in the spring of 1926. That summer the Ecological Section of the International Congress at Ithaca devoted a full half day to it, and George Nichols pulverized my theory. Worse than that, he ridiculed it. The room was crowded with botanists, including all the ecologists, of course, and about half of the taxonomists. After the session, many of the latter assured me that I was correct, but to ecologists I was anathema. Not one believed my ideas; not one would even argue the matter. In Europe things went a trifle better, for there I had two able supporters, Pavillard in France and Palmgren in Sweden. For ten years, or thereabout, I was an ecological outlaw, sometimes referred to as " a good man gone wrong."

"It was about 1936 that Stanley Cain stopped at the New York Botanical Garden to talk with me about the matter. We spent a whole day in the argument and he left absolutely certain that I was wrong. Just a year later he stopped off again on his way to the Cold Spring Harbor Laboratory and admitted that I was right. At the Boston meeting in 1946 Herbert Mason said he had discovered the same thing and, having never heard of my paper, thought it was an original idea. In 1950 or sooner, Crocker worked out the same principle in Australia. Now Curtis and his colleagues at Wisconsin are getting much better supporting evidence and will undoubtedly revise and extend my ideas in many ways but without changing their essential nature.

"Just one other point. The past history of our vegetation could be learned with much greater accuracy if we had plenty of evidence from fossils. It seemed that we might find them in peat bogs. So in 1911 Fred A. Loew, then a graduate student at Michigan, was set to work with a soil borer to dig up the fossibls in a bog at the Michigan Biological Station. He got peat from all depths, washed it thoroughly, and in it we found just one recognizable fossil, a seed of **Nuphar advena.** Neither of us ever imagined that we had washed away many thousands of perfectly fossilized pollen grains."