[Aliso: A Journal of Systematic and Floristic Botany](https://scholarship.claremont.edu/aliso)

Volume 12 | Issue 1 Article 2

1988

# Plant Taxonomy in My Time

Lincoln Constance
*University of California, Berkeley*

ALISO
12(1), 1988, pp. 1–5

# PLANT TAXONOMY IN MY TIME[1]

LINCOLN CONSTANCE

*Department of Botany, University of California*
*Berkeley, California 94720*

*You ask me what life is. That's like asking what a carrot is. A carrot is a carrot, and there's nothing more to know.—Anton Chekhov.*

I have a troublesome habit of thinking up what I hope will be a challenging title for a talk, and then being stuck with it and having to think up something that will fit comfortably under it. In this case, I think I shall juggle the title slightly to read, "My Time in Plant Taxonomy," since this can to some extent be quantified and also gives more license to reminisce. At this point I should insert my customary warning about the recollections of old men, botanists no less than others, that they tend to be inaccurate, self-serving, and interminable.

Like most botanists of my generation, I was born into a rural setting, in my case in western Oregon's Willamette Valley. My parents felt that my elder brother and I could best compensate for the lack of urban culture by exploiting an interest in natural history. I think my earliest goal was to become a taxidermist, but losing struggles with the skins of a decidedly dead squirrel and a few ill-fated birds put an early end to that ambition. A more satisfactory affair with butterflies and moths engrossed my attention for several years. But plants somehow emerged from the background to become a persistently dominating interest, which was furthered by the officials of various YMCA summer camps who were delighted to see at least one of their charges utilizing his excess energy in something that was not conspicuously destructive.

The Willamette Valley had a fairly distinctive flora that began with *Dentaria* about the middle of February, followed closely by such things as *Erythronium, Ranunculus, Dodecatheon, Calypso, Camassia, Lithophragma, Cynoglossum, Calochortus,* and *Sidalcea.* The first scientific name I learned was *Osmaronia cerasiformis,* which unfortunately has had to give way to *Oemleria,* a word that evokes no nostalgia whatever. Reportedly, odor is the most potent stimulus of memory: For me, the red-flowered *Ribes* yield the true smell of spring. The flowering season ended with the resinous-asphaltic odors of *Wyethia, Grindelia,* and the tarweeds, and the sharp tang of vinegar weed (*Trichostema*), but long before these appeared in the annual flowering cycle, my botanical enthusiasm usually had waned and my mind was on other things.

While still in high school, I discovered the University of Oregon's Botany Department and became acquainted with its staff: Albert Sweetser, who had a Master's degree from Harvard and wrote articles on wildflowers for the local newspaper; Ethel Sanborn, plant anatomist, paleobotanist, and sister of my high school biology teacher; and Louis F. Henderson, botanist turned orchardist and back to botanist, who became my mentor and role model. Henderson, who must have been about seventy and who had come out of retirement to take over management of the small herbarium, was the recipient of all plants sent in for identification, and I was indulgently allowed to participate in this activity. Al-

though I really knew nothing about taxonomy, I had learned to know a good many plants by sight and I must have developed some sense of pattern recognition. Sweetser and Kent's, "Key and flora. Some of the common flowers of Oregon" (1908), bore the warning under the families Umbelliferae and Compositae, "Species are too difficult for the beginner." Gramineae, Cyperaceae, Polygonaceae, and Chenopodiaceae evidently had neither genera nor species, since none were mentioned. But users of this admittedly compiled flora were invited to send specimens to the Herbarium with the assurance that they would be identified without expense to the sender. Thus, I was already a part of the botanical establishment.

It was preordained that when I entered college I should declare a Botany major, although that Department was swallowed up in Biology midway through my freshman year. It was in the summer of 1928, before my junior year, that I turned professional. Henderson, rejoicing in the rare miracle of a small private grant for field work but incapacitated by a painful hernia, dispatched Le Roy Detling to collect in the Paulina Mountains of central Oregon and me to spend two months botanizing throughout Klamath and Lake counties. It was a rugged and sweaty summer, traveling between widely spaced towns by stage (usually ancient Buicks) or by thumbing rides, and reaching the beckoning but distant hills where the best collecting always was to be found the *hard* way, by foot. Despite the heat, the voracious mosquitoes, and the ominous rattlesnakes, it was distinctly a "learning experience." When Henderson (1930) published in *Rhodora* a presumed new species of *Silene,* "Collected by *L. Constance,* a student at the University of Oregon . . . ," I was irretrievably hooked.

An interest in something as esoteric as plants was scarcely acceptable to the peer groups of that time, so Botany became a closet activity until, with the frightening prospect of graduation into the Depression World of 1930, it was necessary to think about the future and some visible means of support. Several former teachers suggested or supported an application to Berkeley, which was initially rejected there but was later accepted somewhat grudgingly when one or more more promising candidates failed to materialize. Berkeley *being* Berkeley, I had to take my course work all over again, but in my case this proved advantageous; indeed, it is too bad I did not have the wit to broaden my background by, for example, taking a course in genetics. When I look back on my graduate training, I think the high points were introduction to the concept of plant evolution, which I owe largely to Herbert Mason, and the ideas of plants occupying space (phytogeography from Setchell) and time (Setchell, again, and Ralph Chaney). Jepson's influence was important but more difficult to define; perhaps it consisted largely of taxonomic conservatism. And, as always, the graduate students taught one another.

The pattern of life for taxonomists in the early decades of this century at least in the West, as well illustrated in the recent biographies of Aven Nelson and Marcus E. Jones, was to collect omnivorously in spring, summer, and, if possible, early fall, and then to spend the winter, usually interspersed with teaching, scanning the haul for possibly undescribed species. Different collectors favored different plant families or groups, but in general each taxonomist played the whole field. The focus was strongly floristic because most were preparing a flora of some area, and few had the time, material, or facilities to undertake serious monographic study. The young learned to do research the same way they learned to teach—by imitating their elders.

Jepson used to assign each student a mountain as a Ph. D. problem. I escaped that fate but wound up with the composite genus *Eriophyllum* (Constance 1937), probably because it was the only generic name I recognized on the short list I was handed. "The Umbelliferae of Oregon" was also on the same list, but I had already been warned that the species were too difficult for beginners! We were instructed to begin our studies with a clean slate, to abjure consulting any previous treatments, and to let the material speak for itself into our virginal ears. Needless to say, we did not do anything of the sort. I was particularly fortunate that Rydberg had recently monographed the Tribe Helenieae for *North American Flora* and that Harvey Monroe Hall had attempted to apply his treatment to the Berkeley specimens, with copious annotations and comments. These illuminated my path.

This led me to "The Phylogenetic Method in Taxonomy" by Hall and Clements (1923), which smote the taxonomic "splitters" hip and thigh to my great satisfaction, and held out the promise of a strictly scientific, phylogenetic classification based on Linnean species that even a dummy could recognize unfailingly in the field. The knowledge that Clements did not "believe in" genetics but was enamored of LaMarckian transformations only came much later. My thesis, too, showed all the species arranged like bubbles on a forking string—surprisingly similar to some recent cladograms, I believe they are called.

My first and half-time professional teaching position, at Washington State College, involved one elementary botany course (for pharmacy students), beginning and advanced taxonomy, ecology (which I had never studied), and managing a sizable but badly neglected herbarium. It also involved mastering another flora, engaging in extensive correspondence with specialists in various parts of the country, and my resultant emergence into the national taxonomic fraternity by parcel post, so to speak. I was extraordinarily fortunate to acquire as my first graduate student Reed Rollins; I have partially chronicled this period elsewhere (Constance 1982).

A return to Berkeley three years later more or less coincided with establishment of the Biosystematists, a development catalyzed by Clausen, Keck, and Hiesey of the Carnegie Institution and Babcock and Stebbins of the Berkeley Genetics department, and joined in by most of the active systematists of the Bay Area. Although doubtless each of us defined it differently, we were all convinced that we were pursuing the evolutionary explanation of variation in living organisms by all the means available to us. Our passionate search for the perfect all-purpose species definition and our deference to the biological definition of taxa now seem naive, but my ideological clock remains synchronized to the Biosystematics of those years and I continue to contemplate the plant world from that bias.

I now had an opportunity to embark upon sustained taxonomic investigation and I chose the small and predominantly West American family Hydrophyllaceae as a suitable subject to explore the relatively new application of chromosome number to systematics. It occurred to me that the phenomenon of polyploidy just might help to explain the bewildering diversity of flower color in *Nemophila*; it did not, but it did later shed some light on *Phacelia*. Marion Cave volunteered to collaborate with me as the cytological expert, an association that continued for more than twenty years. Shortly thereafter, I began to kibitz on the revision of *Lomatium* that Mildred Mathias was completing at Berkeley. This indiscretion catapulted me into a joint attack on Umbelliferae that has lasted until almost the present day. I do not know whether these two associations with remarkably tal-

ented women were examples of continued exploitation or liberation, but they were of inestimable value to me.

An invitation to spend the academic year 1947–48 assisting in the reorganization of Harvard's complicated botanical establishment provided me with the opportunity to proselyte for the new California systematics in such hitherto sacrosanct preserves as the Gray Herbarium and the New England Botanical Club. I remember also giving a light-hearted spoof of over-zealous exponents of the same religion under the title of "Some foibles of biosystematists" to what was supposed to be a meeting of an informal student group, but that turned out to be attended by a formidable representation of the Biological Faculty.

My premier national think-piece was "The versatile taxonomist" (Constance 1951), given in 1950 as president of the American Society of Plant Taxonomists. "Red" Camp, who preceded me in that office, had taken the position in his address of the previous year that traditional taxonomy was so bankrupt that we should start all over again with "Biosystematy." I had neither heard nor read his paper, which was then in press, but proceeded to give what he must have thought was intended as a point-by-point refutation. My theme was that "Newer Systematics, Experimental Taxonomy, or Biosystematics" were simply "*normal, twentieth-century accretions* to the ever-growing body of taxonomic technique and knowledge." However, he never blinked, although he *did* introduce me as "Rogers McVaugh."

As outgoing vice-president of Section G of the A.A.A.S. two years later, I was faced with giving an address at just about the time botanists were shifting their allegiance from the A.A.A.S. to the A.I.B.S., and I feared I should have a cold Christmas in St. Louis with no audience whatever. So I hit upon the device of sending a postcard listing several topics on which I thought I might possibly be able to talk and dispatched them to about twenty friends located in the surrounding area. (At least, I ought to be able to shame these friends into attending!) The preponderance of votes was for two topics, so I put them together as "The role of plant ecology in biosystematics." I then proceeded to drub plant ecologists for not realizing that the transplant program of the Carnegie people was really "Experimental Ecology" rather than "Experimental Taxonomy," a fact to which almost all ecologists had been oblivious. This proclamation was published in *Ecology* (Constance 1953) over some dissent, and alienated me from an entire generation of ecologists.

Five years later at Storrs, I found myself offering a talk on "Plant taxonomy in an age of experiment" (Constance 1957) as part of the fifty-year anniversary of the Botanical Society of America. I concluded that, "Plant taxonomy has not outlived its usefulness; it is just getting under way on an attractively infinite task." Less prophetically, I also observed that, "there seems to be rather general agreement that sufficient evidence to formulate a really new, thorough-going, and generally satisfactory phylogenetic arrangement of flowering plants is not yet available." The same meetings witnessed the first presentations, I believe, of new systems of Angiosperm classification by Herbert Copeland, Arthur Cronquist, and Robert Thorne. You can't expect to get everything right. . . .

Meanwhile, in real life I spent most of my efforts teaching elementary and systematic botany, and in carrying on research on Hydrophyllaceae and Umbelliferae. Insofar as possible, I tried to combine field work with cultivation in the Botanical Garden, until field work became a casualty of a decade spent in uni-

versity administration. One Guggenheim fellowship and two sabbatical leaves permitted one collecting season each in Chile/Peru and Patagonia, and a third sabbatical permitted a half-year's exploration of American material in European herbaria.

What I trust will be my final major pontification, a "summation" of a three-day symposium at the Edinburgh Congress in 1964, under the title of "Systematic botany—an unending synthesis" (Constance 1964) pursued the same theme as "The versatile taxonomist" of 1950, that is the continuity and persistent need for ever-improving and -expanding research. This was the heyday of "phenetic classification," and perhaps my most significant contribution was a footnote: "A phylogenetic classification . . . means quite different things to some British and some American botanists. The British, doubtless as a consequence of childhood efforts to trace the lineage of the English kings supplemented by a later interest in the pedigrees of dogs and horses, tend to interpret phylogeny in terms of strict genealogy. Many Americans, on the contrary, seek a self-reinforcing classification based upon maximum correlation of characters, and believe that its explanation can only be an evolutionary or phylogenetic one. An awareness of this difference in viewpoint can alleviate considerable mutual irritation." This is the only one of my efforts to draw a rebuttal. Hermann Merxmüller of Munich gave a lecture to the 1971 annual general meeting of the Systematics Associaton, entitled "Systematic botany—an unachieved synthesis" (Merxmüller 1972), in which he misconstrued my use of the term "unending" as implying that "we are in the middle of this synthesis." The separate of his lecture bears the inscription, "To Lincoln, the venerable optimist, with respectful friendship from Hermann the pessimist." Of course, I implied no such thing! And besides, why *should* I want to bring to an end the fulfilling preoccupation that has sustained me for a very satisfying lifetime, and which I hope will do the same for many of you?

## LITERATURE CITED

Constance, L. 1937. A systematic study of the genus *Eriophyllum* Lag. Univ. Calif. Publ. Bot. 18(5): 69–136.

———. 1951. The versatile taxonomist. Brittonia 7:225–231.

———. 1953. The role of plant ecology in biosystematics. Ecology 34:642–649.

———. 1957. Plant taxonomy in an age of experiment. Amer. J. Bot. 44:88–92.

———. 1964. Systematic botany—an unending synthesis. Taxon 13:257–273.

———. 1982. [Reed Rollins] The years of preparation, 1911–1948. Taxon 31:401–404.

Hall, H. M., and F. E. Clements. 1923. The phylogenetic method in taxonomy. The North American species of *Artemisia, Chrysothamnus*, and *Atriplex*. Carnegie Inst. Wash. Publ. 326. 355 p.

Henderson, L. F. 1930. Some new species and varieties from Oregon. Rhodora 32:20–28.

Merxmüller, H. 1972. Systematic botany—an unachieved synthesis. Biol. J. Linn. Soc. 4:311–321.

Sweetser, A. R., and M. E. Kent. 1908. Key and flora. Some of the common flowers of Oregon. Ginn & Co., Boston-New York-Chicago-London. 151 p.

## FOOTNOTE

[1] Lecture presented at the *Third Annual Southwest Botanical Symposium*, Rancho Santa Ana Botanic Garden, Claremont, California, May 22–23, 1987.