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About the Institute

The Hunt Institute for Botanical Documentation, a research division of Carnegie Mellon University, specializes in the history of botany and all aspects of plant science and serves the international scientific community through research and documentation. To this end, the Institute acquires and maintains authoritative collections of books, plant images, manuscripts, portraits and data files, and provides publications and other modes of information service. The Institute meets the reference needs of botanists, biologists, historians, conservationists, librarians, bibliographers and the public at large, especially those concerned with any aspect of the North American flora.

Hunt Institute was dedicated in 1961 as the Rachel McMasters Miller Hunt Botanical Library, an international center for bibliographical research and service in the interests of botany and horticulture, as well as a center for the study of all aspects of the history of the plant sciences. By 1971 the Library's activities had so diversified that the name was changed to Hunt Institute for Botanical Documentation. Growth in collections and research projects led to the establishment of four programmatic departments: Archives, Art, Bibliography and the Library.

By Carleton R. Ball

I have just examined Part III of this work, which contains *Salix* (Lunds Universitets Arsskrift, N.F., Avd. 2, Bl. 39, Nr. 1., 1943.). It is very revealing, especially the treatment of *Salix*. It tragedy of errors shows up the lazy, careless, and selfish taxonomist at his worst. Note the following points.

1. Can a work honestly be called a Flora (or Manual) unless it presents descriptions of the plants included? In this work, only allegedly new entities are described. If he had written descriptions of all of the plants he talks so glibly about, he would know ten times as much about them, and would have been able to publish only about one-tenth as much. And his readers would have a chance to learn something. And other would not have to do all of the real work.

2. When no descriptions are given, there is added obligation to make the keys full and accurate. This certainly has not been done for the genus *Salix*. How can any one make accurate keys for a large genus of plastic (variable) ~~plant~~ species without first having carefully described them? This he had not done. "Let George do it".

3. Hulten obviously is "hipped" on the subject of hybrids in *Salix*, as shown by his discussion on pp. 498-99, and publication of numerous hybrids later on, with recurring suggestions that other forms probably are hybrids. This is an unfeeling resource of Old World botanists who have run out of new things to name at home and so tackle American plants, especially willows. Floderus, of Sweden, got the willows from the Natl Herbarium of Canada and renamed practically all of them as hybrids, not only of two supposed parents, but as hybrids of three, four, and even five species. That was sheer foolishness. Every geneticist knows that it is absolutely impossible to determine such degrees of hybridity from living plants, artificially crossed, let alone from dried fragments of allegedly hybrid wild plants, the actual parents of which were not there or collected. But calling many of them hybrids relieves such workers from hard study to find out what such plants really are, and gives them the desired new combinations to publish. Also, they ignore the well-known fact that most hybrids are sterile and do not reproduce themselves at all.

4. Hulten also has adopted the subterfuge of pretending that most variations are subspecies rather than varieties, which gives them opportunity to make many new subspecies or new combinations of previous varietal names. Such authors do not explain the criterions by which one determines whether a variation is a subspecies, a variety, or a form. Some authors now are giving Latin names to 8 categories: species, subspecies, variety, subvariety, form, subform, sub-subform, and sub-sub-subform. I collected five of their sub-forms from one tree. In this one paper, Hulten has 13 of his own subspecies (9 new), and two by others, and only 3 of his own varieties (2 new) by others. Most of the well-marked and long-established varieties of Alaska *Salix* he ignores.

5. The treatment of *Salix* is a tragedy of errors. The inaccurate keys have been mentioned. Two species so closely related that separation of extremes of poorly developed specimens is very difficult, are put in two different Sections: *S. retundifolia* in Herbaceae and *S. phlebophylla* in Myrtosaxil, along with *S. polaris* and *S. Chamissonis*. This last clearly belongs with *S. Barclayi*, *S. commutata*, etc., having all their characters but an abnormal creeping habit. There is an old gag: "If you found kittens in an oven, would that make them biscuits?". *S. Setchelliana*, whose nearest relative is *S. retusa* of Europe, is put in a new Section *Alienae* Floderus. Which reminds me that Floderus, receiving specimens of the male and female paratypes of my *S. Setchelliana* in Sweden, immediately sat down, described it as *Salix aliena* Floderus, rushed the paper thro into print ahead of accumulated papers and tried to beat me out of my species (nearly succeeded, because my paper took its turn).

6. An example of care in statements. Under *Salix Farrae* (p. 532), he says that Coville and Ball published subsp. *Walpolei* in 1921. But our combination is clearly printed: "n. var.". He also credits Schneider with publishing *Salix Walpolei* and gives 3 citations. Schneider did not publish such a combination anywhere. (ad infinitum).