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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.

OK of spare

I confess in looking over this very elaborate and exhaustive work by Miss Mathias that I <sup>do not like it.</sup> ~~am up against the hardest task in my life.~~ I would like to go <sup>critically</sup> through it ~~with a buzz saw~~ and show up all its shortcomings, but personal reasons prevent. I must content myself in saying that she seems to have no sense of generic proportion, but follows in the <sup>ml</sup> ~~main~~ wanderings of Rose in his treatment of the Umbelliferae, and in keeping up on trivial grounds various genera that never had any standing. The author probably never had much field experience, but bases the work on herbarium material. A very small amount of field work would show her the absolute untenability of many genera if she has any generic sense of proportion. Her sense of specific limitation is also weak, since she keeps up as species many varieties that any field botanist would <sup>recognize</sup> know at once. ~~I must stop here or tear the work to pieces.~~

Where there is such widespread opposition to Rose's treatment of the Umbelliferae, one expects a philosophical discussion of the basis of every genus, but Miss Mathias seems <sup>too timid</sup> ~~to lack courage~~ to attempt it, or discuss it. ~~We do not expect much courage in women, and here we do not find it.~~ I am amazed at her identification of Cymopterus lapidosus with C. longipes. It is quite a different thing, more like Cogswellia. C. longipes is a peculiar species, growing flat on the ground in early spring with a rosette of leaves and sessile inflorescence. As it matures the foot elongates, and the peduncle also so that the flowers or fruit are a foot above the ground. The root is tuberous and fleshy, while that of lapidosus is quite different. So is the habit of the two. I notice a similar absence of critical judgment in E. Wendleri and Newberryi. I discussed them in one of my early papers and showed the complete intergradation.

I do not favor the habit of recent monographers in citing a great mass of specimens. Doubtless this is done to give the appearance of profundity of study but to me it is simply ~~cumbering up~~ one's work with a useless mass of junk. I am not assuming that her work contains any junk, but its chief lack is judicial treatment. <sup>One must know ecology before being a monographer.</sup> From private conversation with the author I find that her position in

recognition of species is that if the variation is a marked one it should be considered a species irrespective of field conditions. This ignores ecology and <sup>gives</sup> a premium on freaks. The great herbaria are rapidly filling up with freaks, sports accidentally collected, that have no standing in nature. A species <sup>ought</sup> ~~ought~~ to mean the name applied to a well established group of self-perpetuating forms. Freaks have no standing in genetics, except as indices of the trend of developing forms. When a freak is shown to be a freak the name should be suppressed. Cases in point are *Astragalus grallator* Watson, *Astragalus oocal-*? *ycis* Jones, *Astragalus hyalinus* Jones. There is much duplication of forms in the Umbelliferae because of the varying development of wings according to the time of fruiting of certain species. It should be easy to give a good description of a well established species, and this description should be based not on the characters shown by the type specimen so much as on the plant as it is known to grow. Our nomenclature practices are at fault in many cases in the matter of priority. It is the practise to recognize the first name applied to a species, and if variations of it are later on found <sup>later on</sup> those go as varieties of the earlier species. But this practise is biologically wrong, for the reason that often the first name applied to a species was given not to the plant as it grows but to a variation, good enough as a variety, but not prevalent enough to warrant the specific name being used for it. An example of this is *Astragalus lentiginosus*, a name applied to a very local form of a cosmopolitan species. So *Astragalus diphyus*, a far more representative form must go as a variety of *lentiginosus*, a case where the tail wags the dog.

It was ~~impracticably~~ necessary that a monographer of *Cymopterus* should take up my animadversions on the work of Coulter and Rose and show that my criticisms were incorrect, See Contributions No. 13, <sup>12:96 + 2:17</sup> before <sup>accepting</sup> those spurious genera. But Miss Mathias does not even mention the fact that any criticism of those genera ever was made, though she had access to all my Contributions, and knew of all my criticisms. This is why I assume timidity on her part to go into that phase of the matter. I would invite any attempt to discuss the matter, but <sup>unless</sup> ~~is~~ <sup>is disproved</sup> ~~is~~ <sup>is</sup> ~~unable to disprove~~ my position there is no escape from rejecting the genera.

This makes her work inadequate in its most vital part.

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Cactaceae. In Vol. 39 part 237 Bulletin of the So. Cal. Academy of Science, Ray  
Fosberg makes a number of new genera out of various Cactaceae published in  
Britton and Rose's Cactaceae. After the fiasco of <sup>was a fiasco</sup> that publication which has  
made the authors justly ~~excruciating~~ <sup>It is a pity Fosberg does not</sup> ~~for a young and inexperienced person~~ <sup>make</sup>  
~~to come out with a lot of new genera, and who shows he knows nothing about~~  
~~generic limitations, makes the public laugh.~~ What the Cactaceae need is not  
expansion but drastic contraction, and nothing but that kind of treatment  
will ever go with western botanists. A person who tackles the Cactaceae, at  
once invites contrast with the magnificent work on the family by Engelmann,  
the greatest of American botanists, and he must be a very great person to  
measure up into anything but a pygmy in contrast. It is not a question of  
getting into print with one's ~~own~~ <sup>his ideas</sup> ideas. Anyone can do that. But the question  
is are ~~his~~ <sup>his ideas</sup> ideas worth the printing? <sup>His</sup> ~~his~~ Latin is rather raw, and his familiarity  
with Greek is nil, but he does not hesitate to try to coin a lot of Greek  
~~names~~ <sup>names</sup> as though he knew what ~~and~~ they mean and what they are. This shows the  
foolishness of insisting that all new descriptions be in Latin. ~~Let the kids~~  
~~express themselves in English, why make fools of themselves in trying Latin.~~