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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.

Schoolcraft, Mich.,
Nov. 30, 1937.

Dr. Carleton R. Ball,
Washington D.C.

Dear Dr. Ball: -

The specimens of *Salix*, I
wished returned came this morning. Several
days ago your letter with determinations
was received. Five new species or varieties
were added to my list bringing the total
to 23 species + varieties.

Digitized by ~~We now have only a limited number~~ ^{Went Institute for Botanical Documentation}
possible to collect in our county. It is to
be hoped that some of these may be found
another year. There is also the further possi-
bility that there may be some interesting
hybrids to collect. If I find any I shall
be pleased to send you specimens partly
in appreciation of your willingness to
check on ours.

Sincerely yours
C. R. Hanes.

April 28, 1942.

Mr. Clarence R. Hanes,
Schoolcraft, Kalamazoo County, Michigan.

Dear Mr. Hanes:

I have just received your reprint on the Atlantic Coastal Plain Element in the Flora of Kalamazoo County, Michigan, and wish to congratulate you on the paper. As I think I have told you before, it is only through the services of local botanists that we get those details of the various aspects of botany which make accurate geographical, ecological, and taxonomic interpretations possible. If we could have at least one such worker in every one of the three thousand and more counties in the United States, much new information would be developed and many errors and confusions cleared up. While your list of 54 species does not contain any willows, I know that it contains material of interest to specialists in many other groups.

I think we had some discussions a few years ago regarding the occurrence and differentiation of *Salix petiolaris* and *Salix subsericea* in Kalamazoo County. Recently, I have been going through the same difficulties with material from central Saskatchewan.

Thanking you again for your many courtesies to me, I am

Cordially yours,

sent to Erlanson

Carleton R. Ball,
Executive Secretary

Schoolcraft, Mich.,
Feb. 14, 1946.

Dr. Carleton R. Ball,
Washington D.C.

Dear Dr. Ball: -

It has been some time since I wrote you about willows. We expect soon to publish a Flora of Kalamazoo County, Michigan. We wish to have all the changes in names incorporated in it. In the January and February numbers of Rhodora Fernald has been discussing the genus *Salix* and has indicated some changes.

According to him I believe *Salix cordata* Muhl. of Gray's Man. ed. 7. should be *S. rigida* Muhl.; that *S. sylvicola* Fern. is the proper name for the willow that is found along the southern end of Lake Michigan. We have collected it near South Haven.

Fernald makes out that *S. eriocephala* Michx. is of Southern Illinois + southward. Is it proper to give *S. discolor* var. *eriocephala* (Michx.) Anders. as a synonym of *S. discolor* var. *latifolia*?

Are there any other recent changes in names or authorities for the willows? On page 28 of the Feb. issue of Rhodora Fernald has *S. uicana* Michx. with the note "The true candida with flocculent pubescence."

I shall appreciate any help you may give me on my inquiries

Sincerely yours
C.R. Hanes.

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Fernald
Mr. C. R. Hanes,
Schoolcraft, Michigan.

March 29, 1946

Dear Mr. Hanes:

Your letter of Feb. 14 was promptly received and I was glad to hear from you and to know that you expect to publish soon the Flora of Kalamazoo Co. These local floras are of immense value, and, as I have pointed out elsewhere, it is the splendid work of local collectors that makes possible a monographic treatment of any group.

I am sorry that my reply has been delayed but I had gotten far behind during the war and just digging out. Answering your queries:

If, as Fernald says, *S. cordata* Michx. antedates *S. cordata* Muhl. then those who believe that a few months of priority outweighs 140 yrs. of acceptance and wide and continuous publication and use, must use *S. rigida* Muhl. as the next acceptable name for *S. cordata* Muhl.

I just cannot agree that *S. arcticola* Fern. is distinct from *S. cordata* Michx. (*S. adenophylla* Hook., a name of wide and continuous use for nearly 110 years). He calls the differences "amazing" but when he compares the two the emphasis is on minor details like toothings of the leaves and stipules, deepness of the brown color of scales, etc. and the veining of the under surface of leaves. There is more variation on the organs of one plant of either area than between his two descriptions. My photographs of the original Labrador plant show exactly the short broad leaves which he emphasizes for the Lake States plant. I have studied more James Bay material than Fernald apparently has and they are like Lake States "*arcticola*". And the "Labrador" of Dr. Morrison was not far from James Bay and not the Atlantic side.

Fernald probably is quite right in concluding that *S. eriocephala* Michx. is the so-called *S. missouriensis* Bebb (*S. cordata* var. *vestita* And.) and not in any way connected with *S. discolor*. *S. discolor* var. *eriocephala* (Michx.) Anders. therefore would be a synonym of these *cordata* segregates if either is maintained. Studies just completed on hundreds of specimens show no true differences between them & *cordata*.

Andersson describes *S. discolor* var. *latifolia* as having leaves hairy below. This is the plant we have called "*S. discolor* var. *eriocephala* (Mx.) Anders." The plant actually has hairy young twigs also but Andersson mentions only twig color and not hairiness. He may have had old material.

S. incana Michx. is antedated by *S. incana* Schrank, 1789, so that *S. candida* Fluegge, 1806, still holds.

Most of hundreds of thousands of articles granted patents by the U.S. Patent Office required at least as much study and effort as describing a new species of variety, yet the Government allows the inventor a priority (exclusive right) for just 17 years, no more. Why give eternal priority to plant names nearly three fourths synonyms, instead conserving the names of well-known and long-used species? Have the botanical students, workers, and amateurs no rights to stability rather than constant change, in the name of "stability"? Do we cite the names of philosophers with the ideas
Carlton R. Ball

P.S. I note that I have signed off before I had finished. You have followed Fernald in your willow treatment, which is your privilege. You will find my discussion of his two new Sections of *Salix* in the latest paper sent you separately. He accepted it for Rhodora himself.

You follow him in discarding *S. petiolaris* Smith as "British", and accept his "*S. gracilis* var. *textoris* Fern.". My manuscript covering that has just gone forward for publication. *S. petiolaris* was described in 1802 from a plant in an English botanic Garden, the source of which was unknown. Later the species was credited to two localities in Scotland, bot near cities and in an area of willow holts for basket making. The male plant was never found in Britain and the female only as noted. The garden where it was growing was noted by Smith as having endeavored to get every willow growing in the world.

As early as Pursh in 1814, American authors recognized the plant as American and all have done so ever since, including Hooker (British) in his *Fl. Bor.-Am.* By 1840 all British authors had acknowledged that the plant was an American introduction (except Smith, who admitted he had no proof). Then after all botanists in Britain and America had agreed for 125 years that *S. petiolaris* was an American willow, Fernald blandly asserts that it is "British", and, without giving one word of proof, describes it as a new variety of *S. gracilis*. Why? Not only that, but he asserts that it is different in several stated characters from our American plant. I have taken up his seven errors of fact, one by one, and showed that he is wrong, based on a study of hundreds of specimens, in herbarium and field. "More plant study: Fewer plant names."

I could go on about some of the other changes he makes, but what's the use. The whole science of taxonomic botany is prostituted to the game of getting your name on more combinations, without regard to fact and truth.

Hope you survive the reading of this, if you read it.

GAB

Reg. - Libr.

Schoolcraft, Mich.,
Mar. 28, 1947.

Dr. Carleton R. Ball,
Washington, D.C.

Dear Dr. Ball:-

Mrs. Hanes and I are sending you a copy of the Flora of Kalamazoo County, Michigan. You have always been so willing to give us advice and to check our specimens of *Salix* that we desire to show our appreciation.

So far we have received many favorable compliments on the work.

Sincerely yours

Ledwina R. Hanes.

3814 Jocelyn St., N.W.,
Washington 15, D.C.
April 2, 1947

Mr. Clarence R. Hanes,
Schoolcraft, Michigan.

Dear Mr. Hanes:

I have just received your letter of March 28, and the copy of your Flora of Balmain County. It was very kind, and not at all necessary, to send it to me.

I have glanced it thro with much interest and wish to congratulate you (and Mrs. Hanes) on the completion of this stupendous job. As I have said many times, it is the careful and persistent collecting of local botanists which makes possible any adequate knowledge of our flora, especially by mammal publishers and specialists. Let the good work go on. The volume is attractively gotten up and I have no doubt that it is full and complete.

Of course, I am interested primarily in the willows. I note that you have 15 native species and 4 introduced species. It seems desirable, in mammals, to state which species are introduced, as readers ought to know these facts. You have so stated for two of the four, but not for *S. alba* and *S. fragilis*, except that *S. alba* is called the "European" White Willow. Is there also an American White Willow?

I have noted with interest also your discussion of *Crataegus*. There were some 25 or 30 species recognized in the U.S. and Canada. Then suddenly 3 men ran the total of so-called species up to 1289, ~~in~~ in about 10 years. This was not taxonomic botany but a race between three dishonest men to see which could get his name on the most "species" in the least time, and before the other two did. We know that at least three of the "type specimens" came from one tree. Until botanists are willing to repudiate such work as unscientific and dishonest, there will be no progress possible in our knowledge of such plants. These men died and left the real and thankless job of studying *Crataegus* to a later generation. Do you think for a minute that there are 1289 species of *Crataegus* in America. Yet another freak has just described a lot more from Florida. He had previously (with Palmer) described as new a willow well described before he was born and included in every southern manual since. Do you think that I should immediately describe 1000 "species" of *Salix* from North America? I would have a much better case than the creators of *Crataegus*. To include such stuff in a local flora does not help people to know plants, but to avoid them.

Rubus seems to be getting the same fake treatment. I have not counted the number of new "species" created in the past quarter century but I note 14 new species from your county. There are more than 3000 counties in the U.S. Do you believe that there are 40,000 or 60,000 species of *Rubus* in our country? I see that you express doubt about the *Crataegus* treatment in your own Flora.

I know nothing about *Rubus*. But a friend of mine has been collecting *Rubus* in his State for Bailey. He got interested in my discussion of variation on a single plant, and on differently-environmented plants of the same species (See *Illus. Plant Organs* sent previously, and the last two papers sent herewith). After that, he went out and collected all the expressions of a single *Rubus* plant, from early spring till late fall and from the older growth in the center of the clump to the new shoots at the circumference. He sent it all to Bailey, telling him that it was all from a single plant. Bailey couldn't do anything with it because it represented several of his species. If he had not been told that it was from one plant it would have been named as several species, and probably some of them "new". If that is taxonomic botany, the Lord deliver me from it. (More plant study: Fewer plant names).

Hope you can find time to read the three papers. Sometimes I get so mad I feel like describing 1000 new species ~~part~~ of *Salix*, just to get even with these others. But it would be dishonest, and burden science.

Cordially yours,

Carleton R. Ball

Apr. 4, 1947.

Dr. Carleton R. Ball,
Washington, D.C.

Dear Dr. Ball:-

The letter with your appreciation and the various criticisms came today. We are always ready to hear what others have to say about our work. If we had had a sponsor for our Flora and had not been required to meet all the expenses for the books, we might have included more data in our descriptions. We counted on a 200-page book costing us around \$1000.00 but finally ours has cost us better than \$2000.00.

We have followed Fernald in many of his changes but not in all for we have found him corrected many times. We have not had access to much botanical literature as we have had to earn our living at the time we were trying to do something in botany. During 14 years we received only \$12.00 from outside. Should we, under these circumstances, have decided it would be better not to publish the results of our studies?

In regard to the genus *Rubus* I believe you are wrong in considering the number of species found in Kalamazoo County to be too many. I can see how Dr. Bailey, with the number of specimens with which he has to contend, can make some duplications. I think he may have done so in one or two from Kalamazoo County. On the other hand there may be and I believe is more than one entity in what he has ^{some times} placed in a single species.

It is not fair to multiply the species in one county by the number of counties in the United

4-IV-111
From G. R. Hanes

States. Many of our 40 are known from a large number of states. Others such as *Rubus Wheeleri*, *R. medialis*, and *R. Schoolcraftianus* should be found quite generally throughout southern Michigan and possibly elsewhere. The fact that *Rubus* has been neglected may make some of ours appear to be endemic while, if given proper study, they could be collected over wide areas.

Of course there can be the possibility that some of our new species have been described under other names when ^{the specimens were} received from different stations. Personally I have no doubt that we have 40 or more distinct species in Kalamazoo County. We have been very careful in our collections, getting several specimens of both primocane and floricanes and following up these collections several times during the season to discover their later habits, whether they are free-rooting at the tips or upright or arching etc. Sometimes we have sent Dr. Bailey four or six sheets, collected from the same colony, - some in bud, some in flower, some in fruit, and early and late primocanes. Many of these plants have been sent Dr. Bailey in a living condition. He has been growing them in his garden. We have nearly 40 sheets of *Rubus* in the herbarium; as many have been sent to Dr. Bailey.

There have always been students of *Crataegus* and *Salix* but not of *Rubus*. *R. Wheeleri* for a time was considered an endemic found only near Lansing, Michigan. We find it throughout the southern half of Kalamazoo County, sometimes in quantities. Still so far Lansing and Kalamazoo County have all the reports. This situation would not be true of a newly discovered species of *Crataegus* or *Salix*. Thanks ahead for the papers you are sending. Sincerely yours

(See over) Clarence R. Hanes

4 Jocelyn St., N.W.,
Washington 15, D.C.
April 7, 1947

Mr. Clarence R. Hanes,
Schoolcraft, Michigan.

Dear Mr. Hanes:

Your letter of April 4 is just here and read with much interest. In my last letter I paid a deserved tribute to the local botanists like you who contribute so much to the sorely needed knowledge of our plants. I therefore am especially glad to know that you have collected *Rubus* from the same plant throughout the season and throughout the age-expressions. That also is highly valuable and greatly needed in all plants.

There is no profit in pursuing the discussion of *Rubus* farther. If any one believes those are species, no more need be said. In the case of *Crataegus*, you were wrong. There were not always students of that genus. There were some 25 05 30 American species. Suddenly one man began publishing new species by the score. Two other so-called taxonomists saw an overlooked pathway to eternal glory. They began publishing species by the score. The three raced against time and within ten years had run the number to 1239 (if I remember correctly). Do you think that they were "students" of *Crataegus*, or fakirs getting the largest numbers of combinations with their names attached? For 110 years, no one will be able to do anything about the genus unless the botanists have the honesty and the courage to declare the whole racket invalid and start over. This they will never dare to do.

You are quite right in saying that it was not fair to include all the more than 3000 counties in arriving at a possible total, but that merely changes the final total and not the principle involved. What is Bailey going to do with the different "species" represented by that collection of my friend's from a single plant?

Two taxonomic rackets have developed abroad since World War 1 crippled Europe. The German taxonomists have decided that there are eight Latin-named categories lower than the genus. These are species, subspecies, variety, subvariety, form, subform, a sub-subform, and subsub-subform. This keeps them busy making new combinations, of which four or five can be found on one plant. The Swedes decided that all plants in *Salix* are hybrids and began naming the alleged hybrids with Latin binomials. That was not sport enough, so one of them evolved the idea that, besides ordinary dihybrids, there were triple, quadruple, and quintuple hybrids which he could distinguish in the fragments which make up our herbarium material. Any geneticist, who has made hybrids by the thousands, knows that you can be certain of one parent always, make a fair guess at the second in many cases, and none at all about any additional parents. Does that stop these fakirs? Not on your life. Their names are immortalized on the combinations and that is the end for which they work. Now the American school of species splitters. Do they deny variation within a species?

Now a final query. I noted in my previous letter that I can sit down tomorrow and describe 500 species of *Salix* just as good as those in *Crataegus*. Leaving out any question of intellectual honesty, do you think that American botany and American botanists would be benefitted thereby? And will you include the Michigan species in the next edition of your *Flora*?

Cordially yours,

Carleton R. Ball

P.S. I am inclosing a check for \$3.00, as an addition to the \$14.00 previously received. Perhaps it will pay for part of the time spent in reading these letters.

Schoolcraft, Mich.,
Apr. 10, 1947.

Dr. Carleton R. Ball,
Washington, D.C.

Dear Dr. Ball; -

The copy of the Flora was freely given. Since it was meant that way we do not feel that we should accept the checks so we are returning it. Thanks just the same.

When we collected Rubus and found we had some new species our next aim was to discover how widely each new species was represented in our county. We found some quite local as might be expected and others growing throughout. Naturally we wished to have these scattered specimens, which we considered the same species checked by an authority. However, we never tried to collect the freak forms of a species so as to confuse the person who was to judge them. We had too much respect for anyone who would devote their time in trying to throw more light on a difficult and neglected genus. We knew Dr. Bailey had a long record of solid achievement and that he was not one that sought or needed the cheap fame of which you write.

After ten years of careful collecting and studying of Rubus in the field should we give up our honest conclusions because some who have not given study to the genus, think it impossible to have a large number of different species in a single county? That would be a defeatist attitude.

I agree with you that it will be better not to continue any further this discussion. I have read your pamphlets with interest. I agree with you in most things as I am naturally conservative. (See over)

Sincerely yours Clarence R. Hanes

3814 Jocelyn St., N.W.,
3814 Jocelyn St., N.W.,

April 16, 1947.

Mr. Clarence R. Hanes,
Schoolcraft, Michigan.

Dear Mr. Hanes:

Many thanks for your letter of April 10. I am sorry that it leaves me with some questions unanswered.

1. It is generally admitted by American botanists that the making of more than 1000 new species of *Crataegus* by three taxonomists in a 10-year period was quite unjustified, in that the species cannot be distinguished even by experts and that the only effect has been to make any real knowledge of the genus impossible because their specific names have legal standing under the rules.

Two of these men were internationally known botanists, given large honors over many years in their own country. The third was not known internationally but was well known in America. Dr. Bailey is internationally known, just as two of these others were. They had no need to seek "cheap fame", yet their work is generally discredited (in *Crataegus*, I mean). Is it not just as likely that Dr. Bailey's extensive "splitting" will be discredited when appraised? Why should that not be true in the one case, as in the other.

I am fairly well known as a student of *Salix* in America, and have a fairly large correspondence abroad. Will you approve if I publish from 500 to 1000 new species within the next five years? Or would you join the great majority who would say that it was not helpful, but harmful, to American botany?

2. You refer to some of the numerous normal expressions of *Rubus* plants as "freaks" which you would not demean yourself by collecting. In my 157th publication, I showed that there normally three different types of branchlets on a *Salix* shrub, and that there normally three different forms of leaf on each branchlet. Which is the "freak" and which are the normal? All bushes produce all of them normally. I showed also that there is great variation among plants of the same species growing under different conditions. Likewise, there is great variation of color of twigs, buds, and sometimes leaves, between the shaded and the sunny side of the same plant. Which is the normal and which is the "freak". When these different normal expressions of *Rubus* were collected from one plant and sent to Dr. Bailey, he could not name them because they seemed to represent more than one species.

Yet these fragments of total expression are exactly what fill our herbariums and on which thousands of new species are based. Wouldn't it be much better if the total expression of a species could be known and studied before species are made? That seems like good scientific method.

This letter requires no answer. I only venture to hope that it will give you a little broader perspective on taxonomic botany, which science really is intended to lead to more knowledge of more plants.

Cordially yours,

Carleton R. Ball

Schoolcraft, Mich.,
Aug. 11, 1949.

Dr. Carleton R. Ball,
Washington, D.C.

Dear Dr. Ball: -

Thanks for the reprints
on the genus *Salix*. You have made
a good case for maintaining *Salix*
petiolaris. I followed Fernald, it
seems, to my sorrow. I

Digitized by Huni Institute for Botanical Documentation
The Willows: Helpers of Man is a
very interesting paper. It is so complete.
You surely have given much study
to all facets of your favorite plant
group. I shall let some of our friends
read it as I know they will be
astonished how helpful willows have
been to man throughout the years.

Sincerely yours
Clarence R. Hanes.