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5th Floor, Hunt Library
Carnegie Mellon University
4909 Frew Street
Pittsburgh, PA 15213-3890
Telephone: 412-268-2434
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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.

Sept. 18, 1931.

Professor Dr. T. Nakai,
Dept. of Botany,
Tokyo Imperial University,
Tokyo, Japan.

Dear Dr. Nakai:

It is with the greatest of pleasure and appreciation that I have received the copy of your presentation of the Salicaceae in part 18 of the *Flora Sylvatica Koreana*. This is a monumental work and one of which you well may be very proud.

I am especially pleased that you give so many measurements of the different organs of the plants you are describing. So many authors content themselves with giving comparative sizes, saying merely that one organ is longer or shorter than another.

I am spending a year here at the University and, while I have not much time to devote to willows, as it is only a side-line with me, we are trying to collect large sets of typical material of the California species, showing especially the different leaf form on fruiting shoots, vegetative shoots, and water sprouts. We will hope to distribute these next winter and one set doubtless will come to Tokyo Imperial University.

While I have not had occasion to make critical use of your most excellent paper, I have looked it entirely through. I am glad that you are not inclined to attribute every little variation to the effects of hybridization, as our German friends are so constantly doing, and as even Dr. Floederus seems inclined to do in recent years.

Under *Salix rotundifolia* Trautvetter, pp. 146-147, I note that *S. leiocarpa* (Cham.) Coville (Proc. Washn. Acad. Sci. 3:338, 1901) is not listed as a synonym.

With kind personal regards and warm appreciation of your generous remembrance, I am

Most cordially yours,

Carleton R. Ball,
Research Associate in Relationships of Federal,
State, and County governments in Agriculture
and Natural Resources.



THE UNIVERSITY OF WYOMING

ENGINEERING BUILDING
LARAMIE, WYOMING

July 16, 1944

Dear Dr. Ball:-

In naming up some of our old collections I came across this willow which has a few monoecious catkins. We are wondering if this is something very unusual or have we just never noticed it before? Most of this collection is pistillate but one branch, in addition to what is enclosed, has four monoecious catkins. We are putting it in the Rocky Mountain Herbarium under the name S. petrophila Rydb.

We thought you might be interested in this.

Dr. Nelson joins me in best wishes to you and Mrs. Ball.

Very truly yours

Ruth A. Nelson.

Not uncommon in *Saxnigolaloides* and *S. Bebbiana*, & probably others. Cite to her the literature on this form of terminology. C.P.B. + look up my records in subject file of catkins

816 Fremont

Laramie, Wyo.

Sept. 5, 1947

Dear Dr. Ball, -

Recently I was going through accumulations of what was supposed to be second class mail only in my husband's office - stuff that piled up while we were in Oklahoma last winter - and came across your postal of Nov. 2, in regard to a willow of our collecting, no. 5056, from Big Bend Nat'l Park, Texas. I presume this information will come too late to be of service to you, but I am answering that you may know the matter was not knowingly overlooked.

All the Big Bend material is from Brewster Co. This collection was made south of Terlingua as we approached the river at the mouth of Santa Helena Canyon. Sorry we did not receive your card. It should have been forwarded with other first class mail.

We have been spending the winters the past three years at the Univ. of Okla. where I have a part time position as Herbarium assistant in the Plant Science dept. The climate there is more favorable during the winter for Dr. Green, and we enjoy the early Okla. spring. We plan to leave for Norman in about two weeks.

We joins me in greetings and best wishes to you.

Sincerely, Ruth A. Nelson.

(See over)

3814 Jocelyn St., N.W.,
Washington 15, D.C.

September 15, 1947

Dr. Ruth A. Nelson,
Herbarium, Dept. of Botany,
University of Oklahoma,
Norman, Oklahoma.

Dear Dr. Nelson:

I am delighted to have your letter of Sept. 5, in reply to my card of Nov. 2, 1947, and to know that the card came to light with the accumulated second-class mail.

I am sending the information to Dr. C.L. Lundell, and am quite sure that it is in time for I do not believe that the manuscript is yet in press. Thank you.

At the moment, I cannot find the Nelson card in my alphabetic list of botanists to whom I send reprints. Separately I send a few recent ones. If you have already had them, you probably can find some one there who will like to have them.

I shall still count on the "Nelsons" to find *S. rigida* Muhl. (*S. cordata* Muhl. not Michaux) in the mountains of SE. Oklahoma. I wrote you that it has been found near Fayetteville, Ark., not far from Oklahoma., as well as at several points in Alabama.

With all good wishes to you both, I am

Cordially yours,

Carlton R. Hall



From Ruth A. Nelson

THE UNIVERSITY OF OKLAHOMA

NORMAN • OKLAHOMA

April 21, 1950

Dear Dr. Ball;

Your letter came just as I was taking off on an Easter trip to the southeast corner of the state. I spent several days with a friend in the Kiamichi mountains which are particularly lovely at this season. When we left the redbud had passed, the dowswood was in full bloom and the hawthornes and Viburnums coming. The hard wood forest was leafing out in all its delicate spring beauty. I have a project for a small publication on Oklahoma shrubs at present, which is giving me opportunity for considerable field work. *which I thoroughly enjoy!*

In regard to the Salix you mention. I feel pretty sure it was never published. His last papers were in the American Journal of Botany except for a few items in the Wyoming publications. I feel sure that by now everything would have gotten into the indices. But when I return to Wyoming in the summer I will make a further check and write you if I find out anything. I will also look in the collecting book, that might indicate how many sheets were distributed. But the chances are very good that Dr. Johnson has seen a sheet with that name on it, as we collected a lot of duplicates on that trip and they were distributed to most of the large herbaria. I do not think he discussed the name with Dr. Nelson, in fact I am quite certain he did not.

(See over)

Dr. Ruth A. Nelson,
Dept. Botany, Univ. of Oklahoma,
Norman, Oklahoma.

3814 Jocelyn St., N.W.,
Washington 15, D.C.

24 April, 1950.

Dear Dr. Nelson:

Many thanks for your letter of 21 April, just received. I am glad that you could have an Easter vacation in the mountains. Apparently your spring was earlier than ours. We had hot summer during Jan.-Feb., with temperatures up to 97°. The first half of March was warm. Then it froze up and is just thawing out. Nobody knows yet what the damage to fruit crops has been. Ga. peaches are total loss. The Appalachian apple crop is flowering with hundreds of thousands of smudge pots burning. The Delaware-New Jersey peaches are still in doubt. Furnaces still going here

I am indeed sorry to know of Dr. Aven's condition. However, he had an unusually long life of physical and mental competence. I am happy to know that he is in mental hospital where he can receive both care and treatment. The old prejudice against admitting mental failing caused untold tragedy. Now those deserving ones get the best that can be given to them. One of Mrs. Ball's most loyal intellectual friends has a husband (a brilliant man) in our St. Elizabeths Hospital, where he is well cared for and happy. Several of my friends have gone there voluntarily when their minds became clouded, are released when better, and go back when they feel the symptoms returning. In that way they and we are happy and they have no feeling of shame or discredit.

(See over)

I must tell you about Dr. Nelson. Since last August he has been in a hospital at Colorado Springs. He is fairly well physically, considering his 91 years, but has failed so much mentally that it became impossible to care for him any longer at home. This will be sad news to you I know and it has been heart-breaking to me. But it seemed to be the best solution. He has good care and Neva or I see him at frequent intervals. He remembers his old friends but his mind wanders and he does not have normal association of ideas, due, the doctors say, to arterial sclerosis.

We appreciated your Christmas message very much. I just didn't seem able to send out cards at that time so practically no one heard from us the past Christmas season.

I hope that you and Mrs. Ball have recovered from the flu and are enjoying the spring season. With very best wishes to you both,

Sincerely,

Ruth A. Nelson

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(See over)

Now as to Dr. Johnston's *S. taxifolia* var. limitanea. I was intrigued by his having used a name which I never had seen before except on your collection label. While the two plants are of closely related species, they would never be mistaken for the same species and he did not cite your specimen, so evidently recognized it as not belonging to *S. taxifolia*, if he saw it at all.

Dont spend any more time on it, as I will check it in the indexes soon. I think I wrote you that it represents *S. exigua* var. *nevadensis* (Watson) Schneider, which reaches its southern limit of distribution in the Baboquivari Mts., so far as known. I have record of three other collections from them.

Give my warmest regards to Dr. Aven, if you think that he would appreciate them, and, again, many thank for the information.

Cordially yours,

P.S. You may be interested in the fact that my Treatment of the Texas willows will appear in the Flora of Texas this coming autumn.

Carleton R. Ball

Jan. 21, 1925.

Dr. S. Hohara,
Koto-Jakko,
Mito, Japan.

Dear Doctor Hohara:

I am just in receipt of your two papers entitled "Experimental Studies on Pollen of Some Salix", and "Genetic Studies on Spinacia". The former I am reading with much interest; the latter I have presented to one of our geneticists, who will make very good use of it.

Under separate cover, I take pleasure in sending you copies of some of my separates which you may find of interest.

I will greatly appreciate receiving any other papers on Salix which you have published or may publish later.

With kind personal regards, I am

Very sincerely yours,

C. R. Ball,
Senior Agronomist in Charge.

CRB/ecs

11 paper sent 2/31/25
and rest posted cord



松 龍 臥 見 鐘 (詩 名 房 安)



きかは便郵

Mito, May 12, 1925

Dr. C. R. Ball,

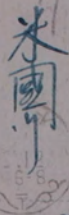
117 W Street

northwest

Washington, D. C.

U. S. A.

UNION POSTALE
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My Dear Sir:-

A packet of willow papers
with your kind letter came
in my hand safely a few
days ago. I am very much
interested in reading papers
of this peculiar plant.

I thank you very much here.

Very sincerely yours

(Sigeroku Nohara,)

Mito Kôto-Gakkô.

Mito Japan.

Sigeroku Nohara

Duke University
DURHAM
NORTH CAROLINA

DEPARTMENT OF BOTANY

October 3, 1938.

Dr. Carleton R. Ball,
Extension Service,
U. S. Department of Agriculture,
Washington, D. C.

Dear Dr. Ball:

I am today expressing to you a package of mounted willows. As Dr. Blomquist wrote, our Salix collection is a meager one. No one here has been particularly interested in them and consequently they have been badly neglected in our collecting.

Since the pile seemed small anyhow I simply instructed the herbarium assistant to pack up all of our North American specimens hoping that you would be kind enough to check through them. Sorry there are so few southern specimens but we will certainly keep your desires in mind and try and pick up a number of specimens for you in the near future.

Yours truly,

HJO:w

Henry J. Oosting
Henry J. Oosting.

(See over

Address:

Extension Service, U.S. Dept. Agric.

October 27, 1938

Professor Henry J. Oosting,
Department of Botany,
Duke University, Durham, No. Car.

Dear Dr. Oosting:

This is in reply to your letter of October 3 and Prof. Blomquist's letter of Sept. 26.

The box of willows came safely by express. I have determined all the specimens, attested those correctly named and attached an annotation label in cases where the name was incorrectly applied. You have a fair collection, serviceable to students.

I have made record of all southern specimens for my distribution lists. If I can accumulate enough data, I hope to give a short paper with maps at the midwinter meetings.

I have arranged the specimens alphabetically. Inadvertently I labelled two covers in ink, forgetting I was not working with my own material. All the specimens of *S. amphibia* Small are under *S. longipes* Shuttleworth. Not a single character of Small's species differs from that of *longipes*, a much earlier species of which Small was well aware. As to *S. Harbisonii*, I am in doubt. The characters mentioned by Schneider do not separate it from *longipes*. In general, the leaves are less whitened beneath, being somewhat intermediate between *nigra* and *longipes* in this regard.

I am returning the package by express tomorrow, tightly packed and tied. I have included a few duplicates of my own, together with two or three copies of my paper on southern willows. I think I sent one to Prof. Blomquist when I wrote him. If not, he should have one of these. One is for you also.

May I suggest that in mounting woody plants not all the stems be placed in the middle of the sheet. Already your rather thin packets are thickest in the middle and cannot be stacked without slipping. Also the leaves and aments tend to break off with the strain and those on the right hand side slide out of the cover and are lost. I mount with stem butts in the four different corners, including the label corner. This keeps the piles level. They stack well up to two or three feet high, and do not break. I also mount a packet on each sheet to contain leaves, aments, etc. Samples are inclosed. I use spoiled letter heads and mss. pages, mostly, costing me nothing.

Many thanks for putting County names on the N.C. specimens. It helps me in mapping. I will be glad to name material at any time. It will be fine if you and your students can collect as they go about the State. I hope that this eternal pressure will slacken some time and let me get out on vacation for collecting.

Cordially Yours,

Carleton R. Ball
Executive Secretary

October 2, 1934

Dr. Henry J. Coetzing,
Dept. Botany, Duke Univ.,
Durham, No. Carolina.

Dear Dr. Coetzing:

In sorting some papers on another subject, I was greatly chagrined to find your letter of N. V. 2, 1933, with no evidence that it ever had a reply. My humblest apologies, if that be true.

Your inquiry can be answered only by the frankest of speaking, let the chips fall where they may.

Many years ago, Dr. Floderus evolved a theory that most all northern willows were hybrids and proceeded to determine American, European, and Asian material on that basis. He published floras with the willows so indicated.

He postulated not only simple hybrids (dihybrids or hybrids of 2 species) but also tri-hybrids, quadri-hybrids, and quanti-hybrids (or those with 3 separate spp. involved). He named the Canadian Natl. Museum material for Perssild in that way and now one cannot find anything in that herbarium. He further complicated the herbarium problem by arranging the names of the four or five supposed parent species in different order on different specimens.

Anyone trained in genetics, and familiar with the laws of dominance or recessiveness of characters, and the resulting trans-segregation, would have known that it is scientifically impossible to recognize, with certainty, more than two parents in any hybrid form and that postulating triple, quadruple, and quintuple hybrids in nature therefore was wholly unscientific and therefore merely botanical faking. As a matter of fact, after examining many hundreds of hybrids I have learned that it is possible to be sure of one parent in practically all cases and of the second one in less than half the cases.

When other botanists pointed out to Floderus that many of his supposed parents of hybrids did not occur in the areas, and often not even on the continents, where the supposed hybrids occurred, he claimed that the new characters possessed by a hybrid would enable it to penetrate where the parent could not. This could be true, over geologic ages, provided his basic theory of hybridization were true.

Perhaps the worst effect of Floderus' theory of *Salix* hybrids has been his unwillingness to recognize valid species in the areas where he predicated 100 per cent hybrid populations. The situation in Greenland is a case in point.

So far as I can discover, *S. arctica* Pall. and *S. glauca* L., or its American varieties do not occur in Greenland at all. Four species occur, at least in some parts of the island, viz. *S. anglorum* Chamaec., *S. arctophila* Gill., *S. herbacea* L., and *S. cordifolia* Pursh in the form of one or more varieties, the most common of which is var. *callicarpaea* (Trautv.). *S. anglorum* is the species which explorers have mistaken for *S. arctica* and *S. cordifolia* var. *callicarpaea* is the plant which they have mistaken for *S. glauca*.

When Gelting says (Medd. on Grönl. 101 (2): 115, 1934) "The plants I have included here under the name of *S. arctica* Fellas are presumably never "pure", since they vary greatly and, especially in favourable localities, show great resemblance to *S. glauca* L.", he is talking about two species, *S. anglorum* and *S. cordifolia*, and perhaps a third, *S. arctophila*, also. Why should there not be variation between individuals of two or three species? There certainly is variation between the individuals of any one species. *S. cordifolia* vars. occupy the "favourable localities" and naturally look more like *S. glauca*. Theory

Cordifolia vars.

Salix arctica

San Diego, Cal. April 14, 1899.

4-26-99

Carlton R. Ball,

Very dear sir.- Your favor recd. It I found awaiting me on my return from Mexico. I do not expect to be in the field much more if any the present season. I will be glad to do what I can for you if I find I can do anything. I am working mainly on cacti still and if you chance to get anything in this order I will be glad to know of it. I think all my specimens of *Salvia* were sent to the Mo. Bot. Garden. I did not collect any species of the genus on the trip just completed - in Baja California and Sonora, if I remember rightly, but my collection has not yet reached home.

Sincerely,
C. R. Pratt.

I shall be glad to receive your papers on *Salvia* or other botanical subjects - I am trying to build up a library, but not an herbarium - having access to the Branderage Herbarium.

COLLEGE OF SCIENCES AND ARTS
DEPARTMENT OF BOTANY
April 18, 1947

Dr. Carleton R. Ball
3814 Jocelyn Street, N. W.
Washington 15, D. C.

Dear Dr. Ball:

Thank you for your continued interest in the Suksdorf willows. I had intended to send them to you long before this time, but simply have not found time to get them ready. You may expect them, however, one of these times. I am very anxious to get your determinations on them so that we may distribute the extensive series of duplicates to other herbaria where they will be of value.

I am glad to receive the reprints, and heartily concur with your position as put forth in "More Plant Study: Fewer Plant Names." I should paraphrase the title as "More Critical Plant Study: A More Natural Classification." In Salix, with its ecologically and physiologically variable species, the result would certainly be fewer plant names. In less exhaustively worked groups, more intensive study may increase the number of names in many instances. That has been the case with the groups with which I have worked. I both Calochortus and Allium, I recognize as valid more species than any previous author. That is because I know these genera better, in the field, in the garden, and in the herbarium. The crux of the whole matter is that species must be delimited as natural biological entities. This is not a new idea, but too many taxonomists do not study their material critically enough to know whether their species is a natural unit or merely a convenient pigeonhole in which to stuff aberrant and sometimes wholly unrelated specimens (for instance, Balsamorhiza terebinthacea which is a catch-all for hybrids between various species of the entire leaved section Arctorhiza and the pinnately leaved section Eubalsamorhiza). The difficulty seems to be a failure to realize the value of the geographical approach, and an undue emphasis on differences rather than similarities.

We are definitely beginning work at the State College of Washington on a flora of the state of Washington (probably including Idaho north of the Salmon River), and would like to ask you to prepare the treatment of the genus Salix. We would, of course, send you all our mounted material at our expense. There is no urgency about the matter, however, as I merely want to suggest it so that you may have an opportunity to think it over.

With best regards, I remain,

Very truly yours,

Marion Ownbey

Marion Ownbey
Curator of the Herbarium

(See over)

3814 Jocelyn St., N.W.,

Washington 15, D.C.

June 16, 1947

Professor Marion Ownbey;
Dept. of Botany,
Wash. State College,
Pullman, Washington.

Dear Dr. Ownbey:

Your letter of April 18 was promptly received and I appreciate your comments on the ways of taxonomists. I am not a real taxonomist so that I have wide ignorance of most plants other than grasses and willows. I have a feeling, however, that most trees and shrubs show much of the variability of the willows, which may be why *Crataegus* got such an awful man-handling some 40 years ago. No one seems to have dared tackle it since. Bailey now is giving *Rubus* the same deal. I noted 14 new species in a recent *Flora* on o county, in addition to several "old" species. One of my collector friends collected specimens from the same plant (clum) throughout the season, and from center, midway, and outer canes. When they got to Bailey he was stuck.

As to the *Suksdorf* willows, it probably will be best not to send them now until fall, as I am buried in a study of species basic to my monograph so that I can get on with it. Have just studied the ~~at~~ isotypes of *S. desertorum* Richardson, basic to the Section *Glaucae*. Worked over Schneider's three new Canadian species and found one (*S. fullertonensis*) to be valid and two (*S. anamesa* and *S. ludsonensis*) to represent *S. cordifolia* var. *caryocarpa* and *S. anglorum*, respectively. Now I am studying Fernald's types of so-called relict endemics from Quebec and Newfoundland. Already, I am fairly certain that three (*S. laurentiana*, *S. paraluca*, and *S. stenocarpa*) are hybrids, the two latter of the same species. And so it goes.

I have written up his flat rejection of *S. petiolaris* Smith as an American species in spite of the complete agreement of all British and American botanists on this point for more than 100 years. My manuscript analyzing his seven errors or fact and conclusion is now in press. So is one discussing Schneider's three species.

I have written the willows of *LEWIS* Nevada, am writing those of Idaho and Alaska, and will be glad to do those of Washington, but it cannot be this year.

sent A, sending separately a few of my older reprints. (all that remain)

Cordially yours,

Carleton R. Ball