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5th Floor, Hunt Library
Carnegie Mellon University
4909 Frew Street
Pittsburgh, PA 15213-3890
Telephone: 412-268-2434
Email: huntinst@andrew.cmu.edu
Web site: www.huntbotanical.org

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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.

University, Ala.,

Sept. 19, 1934.

Dr. C. R. Ball,

University of California,

Dear Dr. Ball:☺

When I saw you in June you said you expected to pass through Alabama in October, and I thought I better write now to suggest that you let me know a week or two ahead, if possible, of the probable time of your arrival here, so that I can make my plans accordingly. I want to make two or three field trips in October, and the time of one of them may be fixed by the movements of other persons, but the others could be moved back or forth to suit your convenience.

I saw your brother in Tucson, as he may have told you, and I had a pleasant conversation with him, and found that he is interested in some of the same things I am, though I couldn't follow him into the leaf-hopper field. After I saw you I spent about ten days in Palo Alto with my mother, and then took 16 days to come back here, visiting and exploring in Arizona, Texas, Louisiana and Mississippi. The last week I collected some prairie plants in Louisiana and Mississippi, that ought to be interesting, and some of them ^{may} ~~ought to~~ be new.

On the way back I saw the country between Sanderson and San Antonio for the first time, having traversed it in the dark going west in 1915. I have now seen the country all the way from coast to coast on the southern route by daylight, and have taken field notes in about 1100 counties, or over a third of all in the United States.

Sincerely yours,

R. M. Harper.

(See ove

Prof. Roland M. Harper,
University, Alab.

Dear Prof. Harper:

Your good letter of Sept. 19 is just here. My time of leaving is still uncertain but not for a few month yet.

Please dont take me into account in making plans. I will know a few weeks in advance of the general date and will let you know.

My first paper published here is just out and I send a copy separately. It was about 18 months in press altogether.

Have just received the Forest Service Tech Bul on the Popalrs and Willows of N.A. The maps ofndistribution are hopelessly inc or- rect. Probably were made before Sudworth's death but in any case I was not consu ted about them, other than to name F.S. collections.

For instance, *Salix nigra* does not reach Nebraska or either of the Dakotas, as I have shown convincingly. *S. interior* reaches the Lower Yukon, but does not occur across Colo. and Wyo., etc. *S. lasiolepis* occurs from New Mexico to the Coast and north to Washn. and Idaho. and so for most of the species discussed.

Glad you had a good trip across. That is one of the joys of life- getting more fact to use.

Cordially Yours,

Carleton R. Ball

Agricultural Division
January 30, 1937

Prof. R. M. Harper,
Department of Botany,
University of Alabama,
University, Alabama.

My dear Professor Harper:

A recent study of manuals of southern botany shows that the distribution of willow species in the South, as in other regions, is not well known. As I am now spending much time in the South, I would like to help carry forward a survey which will determine the occurrence and actual distribution of all *Salix* species and varieties entering the States south of the Potomac and Ohio and west to and including Oklahoma and Texas. To this end, I venture to make the following suggestions:

1. I will be glad to receive, identify, and return promptly, all willows in the herbarium of your institution which have been collected within the area described. For all specimens collected within your own State, please add the name of the county, if possible, as I wish to map distribution. The approximate elevation of mountain localities will be very helpful.

2. If you or your associates are willing and able to make additional collections in your own or other southern States, I will gladly determine them for you at any time. Each collection should be given a number, if convenient, to facilitate exact reference to it in future publications.

3. While complete specimens, showing flowers, fruit, and mature foliage, are very desirable, practically all species and varieties can be identified from less complete material such as young immature shoots, flowers and fruit without leaves, or mature foliage alone. For these reasons, specimens collected at any season of the year, except winter, can be identified.

4. My own *Salix* herbarium contains nearly 12,000 sheets and forms the basis of the service I am able to render to botanists, ecologists, foresters, nurserymen, browse specialists, park naturalists, manufacturers, and erosion controllers, and nature lovers.

5. All specimens sent will be returned to the sender unless otherwise directed. Where duplicates are collected, I will be glad to receive a set for my herbarium. For these I will be glad to pay cash or to exchange other willows. I am equally glad to determine specimens for collectors when all the material is returned to them, as I record the locality records.

6. It is becoming apparent that there are more species, and more widely distributed, in the South than has been known. Last summer I found *S. petiolaris*, a species of northern glaciated areas, in the mountains of Rabun Co., Ga., apparently native. It has been found in Oklahoma also. Collectors are almost certain to make other finds equally surprising.

If you can give me the names of others who might be willing to help in this survey, I will appreciate the assistance.

With best wishes, I am cordially Yours,

Carleton R. Ball
(Address) Extension Service, U.S. Dept. of Agriculture,
Washington, D.C.

University, Ala.,

Feb. 3, 1937.

Dr. Carleton R. Ball,

Extension Service, U.S. Dept. Agriculture,

Washington, D.C.,

Dear Dr. Ball:-

Your letter of Jan. 30 ~~sent~~ from Knoxville, reminds me that I have never answered yours of May 13, 1935, from Washington. You didn't say how long you would be in Knoxville, or what the study of willows has to do with the T.V.A., or how you could be studying them at this season, but you seemed to desire to be addressed at Washington, so I will do so.

I haven't done much with Salix lately, because 99% of the willows in this part of the world seem to be nothing but S. nigra. But last fall I saw a small one on the banks of the Alabama River in Dallas County that appeared to be different, and I know where a few of the still smaller shrubby ones can be found. But the chances of my being in those particular localities when the plants bloom are rather small. I am interested to learn from your last letter that you can identify the species any time when they have leaves.

Why not come down this way a month or two from now and let me direct you to some of the more promising localities in Alabama, Georgia and Florida? I rather expect to be away from here from about the 10th to the 20th of this month, and I would like to be in several different places in April, all the way from Georgia to Texas; but I have no trips planned for March, except perhaps a few one-day trips.

If you come I can also give you some literature along various lines you are interested in, but I guess I won't undertake to send any now, as I don't know exactly where you are.

Sincerely yours,

(see over) R. M. Harter.

Agricultural Division

February 14, 1937

Professor R. M. Harper,
University of Alabama,
University, Alabama.

Dear Professor Harper:

Your letter of February 3 has been forwarded from Washington and I find it awaiting me as I return from Illinois and Ohio.

My permanent address is Extension Service, U.S. Dept. of Agriculture, Washington, D.C. As I represent equally that Department, the State Colleges of Agriculture, and the T.V.A., I am away from home much of the time and largely travel status. Hence I am likely to write from wherever the letters reach me and I have time to answer.

My study of the willows has little to do with my official job. I began the study while an undergraduate at Iowa State College because they were difficult and Dr. Bebb, the American authority, had died and left no successor. I am only a reformed botanist, as I went almost immediately into agriculture after graduation. But I have maintained my study of the genus *Salix* through the years partly because I enjoy it, partly because every man needs a side line he enjoys in order to guard against getting into a rut and also to safeguard him from collapse after he retires and no longer has an official job. Partly also it was because it has been very helpful to several government agencies, such as the Forest Service and the Natl Park Service which are concerned with the browse for domestic animals and wild game. Also to those and the Soil Conservation Service which are concerned with erosion control, to which willows contribute. I determine the material also for many private and institutional collectors, and for engineers.

My reason for starting the letters when I did was the beginning of another collecting season, when men will be in the field on various errands. I mentioned the ability to identify in practically all stages, because most men do not collect after the fruits are gone, and yet more people are in the field in midsummer than at any other time.

I am working up a key to southern species in the vegetative condition and when complete will distribute copies. Your smaller shrubby willow probably is *S. interior* Rowlee, the sandbar willow. The other you mention, besides the ubiquitous black willow, probably is *S. longipes* Shuttleworth, somewhat like the black willow but with the leaves white beneath. Ecologically, I think it always grows in rocky places even on floodplains, while the black willow sticks to the alluvium. I will appreciate your observations on this point. The range of *S. longipes* in the deep south is yet unknown.

Other species of the Piedmont and Appalachians are *S. humilis*, a small shrub of dry ground; *S. humilis*, its little brother, *S. sericea*, with silky serrate leaves, and *S. petiolaris* with glabrous serrate leaves. Leaves of the first two are oblanceolate and tomentose; those of the latter two are lanceolate (narrowly). Others may be found.

Thanks for the invitation to come down and be directed to promising localities. I will if and when I can. I am going here and there, on call, and mor to the north than the deep south just now. However, I'll let you know when I am coming, if any. As to publications, anything not covering *Salix* in some way is pretty much wasted on me.

Cordially,

Carleton R. Ball

3-II-371
from R.A. Harper

From editorial page of the Miami (Florida) Herald,
Saturday, February 26, 1938.

PASSING OF DR. JOHN K. SMALL.

Editor Miami Herald:

Information from friends in your city seems to indicate that the death last month of Dr. John K. Small, noted New York botanist, who "put Miami on the map" in botanical circles, received very little notice there; perhaps because he outlived most of his Miami friends.

As a representative of the New York Botanical Garden, Dr. Small made his first visit to Florida in 1901, when Miami had only about 2,000 inhabitants. At that time he explored some of the country along the projected route of the Florida East Coast Railway in the vicinity of Homestead, which was then practically a virgin wilderness. He found so much of interest that he returned nearly every year after that, and sometimes twice a year. On nearly every trip Miami was his headquarters, and he concentrated his work in that vicinity at first. For several years, beginning about 1917, the late Charles Deering provided boats and automobiles to facilitate his explorations, and he was enabled to cover more ground on each trip, which usually lasted about a month. After Mr. Deering's death Arthur C. James, another Miami winter resident, was his patron for awhile.

It would be impossible at this time to count how many new species of plants Dr. Small discovered around Miami and elsewhere in Florida, but it was a large number, probably more than that of all his predecessors combined. Some of his early finds were described in the first edition of his "Flora of the Southeastern United States," a book of over 1,300 pages, published in 1903. That was revised and enlarged in 1913, and in the same year he published four small books, "Flora of Miami," "Flora of the Florida Keys," "Florida Trees," and "Shrubs of Florida." A completely rewritten edition of his large book, with many additional Florida records, appeared in 1933. He also published short technical and popular accounts of his travels and discoveries in Florida in many magazine articles, perhaps a hundred or more.

Several years ago his heart began to warn him to avoid exertion, but his enthusiasm continued unabated, and he made a few more trips to Florida. Two or three years ago he was knocked down by a taxicab in Miami, and confined to a hospital for awhile, but he took it very good-naturedly, and soon recovered. He had a very genial disposition, and will be greatly missed by all who knew him.

ROLAND M. HARPER.

University, Alabama (Feb. 11, 1938).

Digitized by the University of Alabama Botanical Department

University, Ala.,

Aug. 1, 1938.

Dr. Carleton R. Ball,

Dept. Agricultural Relations,

T.V.A., Knoxville, Tenn.,

Dear Dr. Ball:-

Your letter of July 30 arrived this morning, and I must answer it now, as I expect to leave in a few hours for a field trip of a few days.

I have visited Fairhope, Ala., once or twice, but don't think I ever collected any plants near there, and I don't remember anything about the willows of that neighborhood. But if you are going down that way soon you better come by here, and I can show you the one I collected in Georgia in April, and pictures of the decumbent tree in Florida, and various other things that ought to interest you. If you are traveling by train you can leave Knoxville early in the afternoon and get to Tuscaloosa at 10:30 p.m. And there are two or three ways to get from here to Mobile and Fairhope by train or bus. My office is in Smith Hall, on the east side of the campus, about a mile east of the business center of Tuscaloosa. Or if you should be traveling by auto, the highway from Birmingham goes right through the campus.

Dr. H. K. Svenson of the Brooklyn Botanic Garden is here, and I am planning to go with him today toward the eastern part of the state, but will be back in about three days, if nothing happens. I would be glad to show you some of the vegetation around here if you have time, though I don't know that we have anything in the way of Salix but S. nigra.

(See over)

Sincerely yours,

R. M. Harper.

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Dept. Agricultural Relations
TVA, Knoxville, Tennessee

August 2, 1938

Dr. Roland M. Harper,
Dept. of Botany,
University of Alabama,
University, Alabama.

Dear Dr. Harper:

Many thanks for your good letter of yesterday. It was good of you to give it immediate attention in spite of your obligation to leave with Professor Svenson so soon.

After cudgeling my memory again and examining a map not before me when I wrote you, I am inclined to think that the specimen referred to came from the State Park on the Gulf southeast of Fairhope. I still cannot recall who sent it.

On this particular trip I am not by myself but a member of party not on botany bent, and while I will zizzag across Alabama in several directions, I am sure that there will be no opportunity to stop at University next week.

Some day, however, when investigations are over, I am going to accept your offers to visit there and to see something of the State.

I hope to get back to Washington by August 20 for a brief time, at least, but may not get at willows until later in the fall.

Cordially Yours,

Carleton R. Ball
Executive Secretary

March 26, 1942

Dr. Roland M. Harper,
University of Alabama,
University, Alabama.

Dear Dr. Harper:

Your letter of March 19 is here and noted. *S. humilis* extends quite generally, but sparingly, into the southern States from Florida to Arkansas.

You might not be surprised at what Dr. Small did if you had accumulated a 50-year record of the doings of men who have successive volumes for sale. Such record undermines your faith in human nature and it is not confined to taxonomic botanists either.

The New York Botanical Garden hunted for all available material of *S. Chapmani* Small in the Herbarium of Columbia University and in the Garden Herbarium. From the former came a fragmentary isotype of *S. floridana* Chapman, and from the latter an excellent isotype of *S. floridana* labelled *S. Chapmani* Small. In the Garden Herbarium also were two sheets of your No. 1381, undoubtedly the basis of Small's Georgia reference. These four sheets tell the story. Furthermore, Small said that the type of *S. Chapmani* was collected by Chapman but said that it came from middle Florida instead of west Florida, as Chapman stated. The descriptions of the two species are identical and there is no other Chapman specimen than the type anywhere. Enough said.

Unfortunately there are not two main groups of willows in America but about fifteen. Practically all the species can be determined by leaf specimens if one is sufficiently familiar with them. From the foliage only one would assume that *S. floridana* Chapman belonged to the group including *S. discolor*. From the pistillate aments it might belong to one of several groups, as the *Cordatae*, *Committatae*, *Pentandrae*, or *Bonplandianae*. When foliage and fruits are taken together it does not belong properly in any recognized section. Staminate flowers of *S. astatulana* were collected and they indicate that actually it is related to the *Bonplandianae*.

- 2 -

I would dearly like to come down and see you and do a little scouting with you, but as long as the duration endures there does not seem to be time, money, nor energy for that.

Cordially yours,

Carleton R. Ball,
Executive Secretary

University, Ala., *from R. A. Hoyer*
Jan. 8, 1947.

Dr. Carleton R. Ball,
3814 Jocelyn St., N.W.,
Washington (15), D.C.

Dear Dr. Ball:-

I have your letter of Dec. 22, and your reprint, "More plant study: fewer names," which came last week. But you didn't say anything about my letter of Nov. 11, in which I told you the location of Dozier, Ala., and about Dr. Mohr's station for Salix cordata in Butler County. Soon after that I looked up Clyde T. Reed in "American Men of Science," and it appears from that that he belongs in Texas. I don't know what he could have been doing in Alabama, unless perhaps visiting relatives.

Your latest paper, illustrated by examples from Salix, is all right, I suppose, but you leave the matter so complicated that it may discourage some people from trying to identify the willows, and impel them to send you specimens for identification. (That is about what I would do.) But of course it is no fault of yours that those complexities exist, and you do well to point them ^{out} ~~up~~. I don't believe you are trying to split up the genus until species hardly mean anything, as has been done in Rubus, Crataegus, Tilia, etc.

In one place you mention three species of Crataegus having been based on one tree. I heard rumors of that long ago, but have never known what the facts were. Can you not dig them out some time, instead of leaving the matter indefinite? I believe P. L. Ricker is one who claims to have fooled Prof. Sargent that way, and Dr. Wherry may know something about it too. Perhaps you see Ricker occasionally, and can get his testimony before the opportunity is gone.

I am interested to know that you have exposed some of Fernald's errors in Salix, and that he is willing to publish your findings, which should considerably weaken his assumption of infallibility. (He is pretty accurate

most of the time, more so than Small was; but very critical of the work of (Asme) others. He won't accept a manuscript from me if he can pick the slightest flaw in it; but at the same time he will accept half-baked stuff from younger writers, some of it having little or nothing to do with the northeastern states, that Rhodora started out to represent.

Two rather recent examples of this have aggravated me. I discovered only a few months ago, in a set of Rhodora recently acquired for the Geological Survey library, that one of his girl students published a monograph of Rhynchospora in 1944, running through several numbers. She did about as well as anybody could who depended entirely on herbarium specimens, I suppose, but she didn't examine enough literature. She completely overlooked my flora of the Altamaha Grit region of Georgia (1906), in which 27 species of Rhynchospora were listed, with pretty full notes on distribution, and most of my notes on the genus in the Torrey Bulletin in the few years preceding. All that was before she was born, but Fernald certainly knew of the flora, and must have seen the other notes when they came out, and could have told her about them. In most cases that would not have made much difference in what she said about the species, but in the case of my R. solitaria she said it was known only from the type-locality, which wasn't true. Both in the flora and in the Torrey Bulletin a few years before I reported it from two other counties. But I didn't bother to collect it every time I saw it, for it was unmistakable.

In discussing R. Harperi (which Small described over thirty years after I collected it), she mentioned the upper part of the stem being curved, as if she had discovered that. But in the Torrey Bulletin, 33:231-232 (1906) I had emphasized that as one of the differences between my plant and the Cuban species to which it had been referred, though I didn't venture to make it a new species. The stem is not only curved, but stiffly~~X~~ so, a fact that would hardly be guessed from herbarium specimens.

About 15 years ago I had some correspondence with a Miss Gaiser, in Canada, who was making a study of Laciniaria (or Liatris, as the Harvard people prefer to call it). I had heard nothing from her since, and ~~th~~ought she had given up the idea; but her results appeared in the last few numbers of Rhodora. She could hardly avoid citing some of my specimens, if she cited any at all, but she does not admit getting any information from my publications, or correspondence. (And she tries to conceal ~~her~~ sex, I don't know why.)

I remember particularly writing her about L. Boykinii, which apparently no one else but myself has collected in the last 100 years, and she wanted me to go and get some more specimens, but I could not very well do so at the time. I must certainly have told her about my note on it in the Torrey Bulletin, 28:481-482 (1901), where I said it must be a hybrid. But she disregarded that, perhaps because it was published before she learned to read, and instead quoted a note I made in the herbarium that it was intermediate between L. elegans and L. tenuifolia. She attributes the first suggestion of hybridity to Small in 1933, but neglected to say that he put it in a section by itself in spite of that, and not even next to one of the presumed parents. She is doubtless right in calling it a hybrid, but all the evidence of that came from me, for if ~~any~~^{some} body else had collected it, ^{instead,} without noting its proximity to the two other species, it would probably have passed for a good species to this day.

She too overlooked my Altamaha Grit flora, in which six species of Laciniaria are listed, probably because she knew nothing about botany at the time it was published; but that did not make so much difference in her case as in the other.

There ought to be some "pussy-willows" blooming in Alabama in a month or two. Are you coming down any time soon to look at them?

Sincerely yours,

R. M. Harper

P.S. If you are interested in tobacco I can show you hundreds of clippings on the subject. Did you ever notice what Nuttall, Darlington and Dewey said about it, over 100 years ago?

Professor Roland M. Harper,
University, Alabama.

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

3 February, 1949

Dear Professor Harper:

Your letter of January 31 has just come, and I am glad to be in touch with you again. I am much interested in what you say of Dr. Murrill's article on *S. floridana* in the publication of the Florida Acad. of Sciences. I have not seen it but probably will find it cited in the next issue of the Taxonomic Index. In any case I will look it up at the Dept. next time I go down.

My paper entitled "*Salix floridana*, a valid species" was published in Jour. Arnold Arboretum 24:103-106, pl. 1, 1943. It was sent to you, according to your file card, on July 7, 1946, with five other papers. I sent you two more on May 14, 1947, and am mailing you my last three today.

In that paper, I discussed the history of *Salix floridana*, named by Chapman in 1860; renamed *S. Chapmani* by Small in 1933, without reason or explanation; and redescribed and named by Murrill and Palmer in 1941 as *S. astatalana*.

In my discussion, I expressed doubt as to the Section to which *S. Chapmani* belongs, but thought it probably belonged in the *Triandrae*, with *S. triandra* of Europe.

It is my recollection that Dr. Murrill had some staminate twigs of *S. longipes* in his collections, the not perhaps in those which served as the type. I did not record, in my paper, whether he had mature foliage known to have come from staminate plants, but I think that there was none. In that case there still is doubt about the staminate plant. The pistillate certainly is a good species (*S. floridana*) and *S. chapmani* and *S. astatalana* are synonyms of it.

I agree with what you say of Dr. Murrill while at New York. I think he had a strong sense of frustration and that he blossomed out in to taxonomy of spermatophytes because it offers wonderful opportunity to erect tombstones with your name permanently on them. (whether valid or synonyms).

You will recall that soon after going to Florida he published 15 alleged new species of *Crataegus* from Florida. After Sargent, Britton (or was it Small), and had there 10-year footrace at the turn of the century, our species of *Crataegus* had been increased from some 25 to 1289, and now no one can know anything about that genus. We know that all three had specimens from one individual tree and that each erected a new species thereon.

Dr. Bailey is doing the same thing to *Rubus* now altho I believed that he has not yet gone beyond 500 species or so. I have numerous friends who collect willows for me from the same plant at different times in the year, so that we know its different expressions. One of these friends collects also for Bailey. One spring he started to collect from a single large *Rubus* plant, from the outside to the center of the clump, from spring to fall. He sent the material to Bailey, telling him it all was from one plant and Dr. Bailey couldn't do anything with it. It probably represented 5 or 6 of his species.

I still hope to get south again. Will keep the April meeting in mind and talk with Mrs. Ball about a possible driving trip to the South at that time.

May write you again after I read Murrill's latest.

Cordially yours,

Carleton R. Ball

(over)

University, Ala.,

Jan. 31, 1949.

Dr. C. R. Ball,

3814 Jocelyn St. N.W.,

Washington, D.C.,

Dear Dr. Ball:-

I haven't written to you for several months, but something has come up lately that you ought to be consulted about.

In the latest number of the Quarterly Journal of the Florida Academy of Sciences, vol. 10, no. 4, published last month, Dr. Murrill has a short note to the effect that Salix Floridana Chapm. is not a good species. He refers to a recent article of yours in the Journal of the Arnold Arboretum, but doesn't cite volume or page, so that I don't know whether it is one you have sent me or not. But his logic is not at all convincing.

He gives the impression that he is familiar with Chapman's type specimens, or specimens, ~~which~~ which I suppose are at the New York Botanical Garden, though he doesn't say so. But as far as I know, he hasn't been in New York for many years, and when he was there he knew little and cared less about Salix. What would you say about his article? If you haven't seen it, I suppose it can be found in some library in Washington.

I thought you were coming down here some spring to give the Salices of Alabama a "once-over." I have no special problems in that genus in mind, *now*, except that I would like to get down into peninsular Florida with you some time and show you the Salix that always has a decumbent trunk, and see what you call it. Perhaps we could go to the type-locality of Murrill and Palmer's S. Astatulana, and see what that really is.

The Southeastern Biologists are to meet in Knoxville about the middle of April, and perhaps you can get at least that far south. I hope to be there, but can't be sure so far ahead.

Sincerely yours,

R. H. Harper.
(See over)