



Hunt Institute for Botanical Documentation
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.

NOTES ON LEON H. KELSO

1. He was a Junior Biologist, with A.B. degree, in the Biological Survey of the U.S. Dept. of Agriculture from 1930 to 1937, apparently. He appears as such in the Organization Lists of the Dept. from 1930 to 1933 (other before 1938 not consulted) but does not appear in the 1938 issue.

The biennial directory of the Washington Academy of Sciences lists him as a member of the Biological Society of Washington in 1937, 1939, and 1941. His address is given as Biol. Survey, U.S.D.A., in 1937 and as Cornell University, Ithaca, N.Y., in 1939 and 1941 (later issues not seen).

2. Publications: In 1932 he published two brief papers on *Salix* in *Rhodora*.

In 1933, he began the publication of Biological Leaflets (442.8 B 538 in Dept. Library) No. 1 was issued on _____ and No. 38 on Oct. 31, 1947. He is the sole contributor and apparently it is printed wholly at his expense. No place of publication, address, or subscription price is given.

The contents cover chiefly, grasses, sedges, and willows, the latter increasingly in later issues. He also includes owls, with which he was concerned in the Biol. Survey (Food Habits Div.). He also has a small volume on owls.

He has dropped the initial "H." from his name.

3. Attitudes. In nearly every issue he takes a crack at his superiors and all others in authority, claiming conspiracy to hog all the credit of the work of subordinates and to prevent them from doing taxonomic work. In Biol. Leaflet No. 27 in 1944, he complains that everybody wanted to get stomach analyses of birds made by the Food Habits Sect. and is very sarcastic about these outside demands. He states, finally,:

"It may be noted in passing that regulations for government work provide that none of it shall be performed for outside individuals or institutions". (p. 1).

On Oct. 30, 1947, however, he wrote to the Director of Personnel of the U.S.D.A., criticizing the Dept. botanists for preventing any one else from touching taxonomic work but demanding that the "Dept. determine an accompanying packet of willows/he had received from Ernest C. Smith of Colo. Agric. College. _____ and other plants

From Ball Archives, Kelso folder
Coll. # 56

COLORADO STATE COLLEGE

OF AGRICULTURE AND MECHANIC ARTS

ROY W. GREEN, PRESIDENT

FORT COLLINS

Jan. 17, 1947.

Mr. Leon Kelso,
1370 Taylor St., N.W.,
Washington, D.C.

Dear Mr. Kelso;

I acknowledged the receipt of your Biological Leaflet No. 34 when it came, but as I remember, without comment. Now, after excursions in other fields I am coming back to a critical consideration of Colorado Willows and am rereading all of our correspondence.

First, I wish to express my appreciation of the leaflets and the type or cotype specimens you have sent to us. Our department is pushing exploration and research with regard to all plants in the state flora. Dr. Harrington is nearly halfway through the preparation of a manual of the state flora. During the past three years he has explored the areas just inside the state limits and has added more than a hundred species to the state list.

Secondly, my personal attitude is that the description of new varieties and forms is definitely in order, perhaps needed, but that they should first be published in the botanical journals or pamphlets and subjected to criticism before being included in manuals, much as in the case of introduced plants.

While, in view of your published work it does seem as if you had been discriminated against, you should remember that most of the journals are sponsored by societies and that one must join the society in order to be entitled to publish therein; the society risking its standing in presenting these papers. The Botanical Gazette is published by the University of Chicago and in the same way vouches for its contributors.

A new writer on scientific subjects has to show credentials of training, experience or access to extended amounts of material in the large herbaria and to the world literature on the subject covered. Extensive changes in the naming of groups, changing of status from one genus to another seems to belong to exhaustive studies of large numbers of specimens from different localities, in some cases supplemented by acquaintance with type specimens and descriptions of allied European species.

Plants identical or similar to some of your new varieties are not uncommon in my collections. That is to be expected since we have collected in some of the same areas. Personally I am over cautious because so many earlier writers described as new plants quite familiar ones in Europe. Also I have been in correspondence with Dr. Ball for many years and with Dr. Raup for a shorter time, sharing the problems presented by the maddening variations in some species of wil-

Contrary to the first statement nothing sent there
over a period of 10 years has been acknowledged.

L. Kelso

lows, partly due to hybridization, partly to internal urges not yet completely expressed, partly to difference in ecologic factors. Dr. Ball has been writing on willows for more than forty years, had over ten thousand sheets of mounted plants of that genus when I first met him more than twenty five years ago and has had access to all the large herbaria of our country. Yet in the series reaching all the way from *Salix glauca* to *Salix brachycarpa* the nearest he ventures to come is a tentative grouping of certain plants in a "Complex". Dr. Raup, with extensive field work in both eastern and western Canada and many accepted publications uses even more vigorous language regarding this series. Much the same is true of the series *Salix Barclayi-monticola*. Intermediates are so common that sometimes one could make varieties galore. Since men with these backgrounds are still uncertain I hesitate to add more names to the present confusion. Each and every one concerned says "What we need is more information from the field, more cooperation, more knowledge of distribution and the variations due to ecological factors as distinct from genetic factors."

Yet, when all this has been said it still remains that practically we need different names for plants that look different, despite the saying of Prof. Farlow regarding a brother botanist "If that man can imagine a difference he makes a new species; if he sees a difference he makes a new genus".

I am wondering if you tried to get publication in "American Midland Naturalist". I have found them ready to publish on a greater variety of subjects than the older journals, some of which will not accept papers on local floras even from their members.

Your contributions in the "Leaflets" are worth while and helpful whether or not your suggestions find final acceptance. My only suggestion would be (When you make a change, give a full description, including the shape and size of leaves in different parts of the plant and at different seasons of the year; state whether specimens are rare or common, whether in your opinion the differences are due to ecologic or genetic factors. Have you observed them in more than one locality? Have you seen them in other collections than your own? Are there intermediates?) You see what I am driving at. People know nothing about you and your preparation. You present an autocratic statement, very brief, without any statement or least any sufficient statement of the reason for your conclusions. That may have had something to do with rejection of your earliest contributions.

Please excuse this unasked for advice and the poor typing. I am an old man, retired, not accustomed to typing, but interested and sympathetic in a brother worker who has met with discouragements. My own contribution on Colorado Willows is already outlawed. I know some things I didn't know when that was written and fewer things that are not so.

If you care to give me any information as to your training and experience I shall be glad to receive it. They didn't seem to know much about you at Laramie.

Cordially yours,
Ernest C. Smith.
Ernest C. Smith.

17-I-47

Ball - to L. Kelso from
E. C. Smith

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C O P Y

Washington, D.C.

October 30, 1947

Division of Personnel,
U.S. Department of Agriculture,
Washington, D.C.

Dear Sir:

Since your Botany division has for years maintained that only an arrived caste and class is entitled to touch taxonomy or taxonomic work in this country, the responsibilities implied in this letter and the identification of the accompanying plants is transmitted to your department herewith, I trust it ~~it~~ will be very easy.

Sincerely,

(signed) Leon Kelso

Leon Kelso

(This letter was referred to Dr. Lambert, A.R.A., and by him to Dr. Salter of PISAE, for reply direct. The 4 willows were sent to me by Dr. Blake) CRB.
by E.C. Smith of Colo. Dept. Bot.,

Attached to the Kelso letter was a letter to him, at 1370 Taylor St., N.W., discussing recent collections he had made in Colorado, and which apparently were being sent to Kelso, referring to still earlier correspondence.

Smith states that Kelso credits to Colorado the following willows but that he (Smith) has seen none of them in the State:

- S. monochroma Ball
- S. Farrae Ball
- S. pedicellaris Ph.
- S. candida Fluegge
- S. cordata var. platyphylla
- S. melanopsis Nuttall

The four willows named for Dr. Blake were

- 1998. S. monticola Bebb, 3 m. high, Lone Pine Creek, Bellair-Redfeather, 8/22/47.
Two summer shoots with small to medium leaves, with stipules
- 2002. S. monticola Bebb, 3 m. high, bank of Glaeser Cr., 1 mile west of Administration Bldg., R.M.N.P., 9/6/47. (1 branched shoot with small bluntish leaf without stipules (probably fruited earlier) and 1 late season twig with large oblong-ovate leaf without stipules).
- 2027. S. monticola Bebb. No real data but only "Cp Adn. 7/7/47". Normal but small leaf probably a staminate twig (earlier)
- S. planifolia Pursh, var. Nelsoni Ball. No data on paper. A fragment with deperurate leaf, the apical sparingly crenulate. One large detached leaf of S. monticola Bebb).

The twigs of all the S. monticola were blackened. as were buds, but traces of yellow were visible.

Professor E.C. Smith,
Herbarium, Dept. Botany,
Colorado Agric. College,
Ft. Collins, Colorado.

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

November 12, 1947

Dear Professor Blake:

Recently, Dr. S.F. Blake asked me to identify some Colorado Willows which had been submitted, with other plants, by a ~~Mr. Kelso~~ Leon Kelso. When I determined them I found that they were from you. There were four specimens. One had a label, one had fill data on the paper, one had merely the date, and one had no data whatever. (All had the numbers).

Attached to the packet was a letter from Mr. Kelso reading as follows:

"Division of Personnel,
U.S. Department of Agriculture,
Washington, D.C.

"Washington, D.C.
October 30, 1947

Dear Sir:

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Sincerely

(Signed) Leon Kelso"

With the Kelso letter was a letter from you to him, at 1370 Taylor St., N.W., with reference to sending him your collections of 1947 and noting that Kelso had credited some 6 willows to Colorado which you had not found there.

I think that we will agree that the Kelso letter was extremely foolish and I can assure you that if the botanists in Plant Industry are determining his material for him it is not because he wrote a letter to the "Division of Personnel". I have seen all the annotations on Kelso's letter from its arrival until it reached the botanists.

I do not know Kelso personally but I seem to recall that he was formerly in the Biological Survey of our Dept. and later discharged for cause. I will check on this presently. I am not familiar with his botanical work but a form of *S. Dodgeana* he published in 1932 has no standing. Blake tells me that he has published some Biological Leaflets at his own expense, including a *Flora* of Denver. Will check up on these too. When a man cannot publish in regular journals and has to have his work printed at his own expense there is something wrong with him because an awful lot of poor works gets published in journals. Perhaps he quarrels with everybody and curses them out as he did our botanists. That quarrel must have been years ago but the present staff do not know about it.

Do you know what he was proposing to do with the material you sent him? The only ones I saw were three *S. monticola* and 1 *S. planifolia* var. *Nelsoni*. (depauperate).

Cordially yours,

Carleton R. Ball

Prof. Ernest C. Smith,
Dept. Botany, Colo. Agric. College,
Ft. Collins, Colorado.

3314 Jonelyn St., N.W.,
Washington 15, D.C.

November 16, 1947

Dear Professor Smith:

Since writing to you on Nov. 13, I have looked up Biological Leaflets, published by Leon Kelsey, so far as they are in our Dept. Library. I have noted the willows in the Flora of Denver and Vicinity and the second supplement. Also I have found the new combination, *S. glauca* var. *pseudolappomum* v. Seeman in No. 38.

When I got into No. No. 29, "The Rocky Mountain Flora II. New Varieties" I was completely flabbergasted by his discussion, conclusions and new varieties. I am at a loss to know what species he really had in hand. Certainly there is nothing in Colorado closely related to *S. cordata* Michx., which he evidently assumes to be a member of the Cordatae, like *S. rigida* Muhl. (*S. cordata* Muhl.). His *S. Barclayi* var. *uncompagere* var. nov., if really representing *S. Barclayi*, must be *S. Barclayi* var. *hebecarpa* Andersson. I cannot guess what the others may be.

In this issue he refers to other previous combinations so there must be one paper containing willows which is not here. His No. 25, on the creeping willows, contains a *S. pseudolappomum* var. *kenosha* var. nov. Now that he reduces *S. pseudolappomum* to a variety, what becomes of var. *kenosha*?

I shudder to think what he is doing to your specimens, with all their variable aspects. By the way, do you know from whom he got the 3 *S. monticola* he refers to in No. 37? And do you know by whom he is now employed? He was in U.S. Biol. Survey, 1930-37; Cornell University 1939(?) - 4-?

Cordially yours,

Carleton R. Ball

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

December 4, 1947.

Dear Dr. Cotton:

I checked with the Personnel Office of the Dept. of Agriculture, and later with Dr. Reed, Chief of the Bureau of Dairy Industry, concerning recent service of one Leon Melsio in that Department.

The records show that from Sept. ⁹, 1941, to June, 1942, he was employed in the Bu. of Dairy Industry as a subprofessional grade 3, Jr. Agr. Aid., and worked out at the Research Center in compiling pasture data. The records immediately at hand did not show whether he resigned.

For 77 days thereafter, in June to Sept., 1942, he was employed in the Bt. of Entomology and Pl. Quar., on a w,a,e, (when actually employed) basis. Since then he has not been in that Dept.

In his Biological Leaflet, No. 37, May 29, 1947, he reviews the episode of the Glyceria paper and says, among other things:

"After eight months of opposition and harangue the article was passed, but only after appeal to the Director of Information of the Department of Agriculture.

It has been stolidly and steadfastly maintained, particularly by W. L. M. and C. C. that the older men were flouted and insulted in the affair, and is so maintained to this day, to all botanical and ornithological circles." (p. 4).

Thanking you again for the information you gave me, I am

Cordially yours,

Carleton R. Ball

8/27/41

See

Reg: Okla. Agr. + Mechan. Coll.

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Depth of the ice at the station 32° N. 60° W. December 4, 1904.
 Name of ship, name & position of observer,
 Dr. Clarence Coffin, Washington 12° N. 60° W.

Исходный 12° D.C.
Экваториальный 24° N.M.

Rez. —

Mr. Leon Kelso
1370 Taylor St., NW.,
Washington 11, D.C.

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

December 8, 1947

Dear Mr. Kelso:

In looking thru your published papers on willows it seems that, in some cases, you have have described, as novelties, the juvenile or otherwise not completely developed stages of already described species or varieties.

In plants, of course, as in human beings, there are many and striking differences between the juvenile and undeveloped stages and the fully developed individual. I tried to explain some of these differences in two of the papers I sent you (Illustr. Plant Organs and More Plant Study).

A baby, for example, usually is chunky, often oboval, narrowly obovate, or elliptical in outline. The legs may be broadly oblanceolate and the arms oblong. When this same child reaches the "stretch-age" at 5, or 6, or 7 years, the body outline may be linear or linear-oblanceolate and the legs and arms truly linear. Later, in adult years, the outline may continue linear, or become oblong, or even broadly elliptical, but it is still the same individual as the baby, the stretch-age child, and the early adult.

Take colors, for another example. The human skin may be all white, or tan to brown on hands and neck and feet, or tan throughout, on the same person at different times. This all varies with the time of year and exposure to sun. Plant organs (buds, twigs, & leaves) react in much the same way, all on the same plant at different seasons & exposures.

You have put stress on hairiness in some novelties. But consider hairiness in the human individual. For the first 10-15 years there is pubescence or pubescence on most of the body and long hairs only on the top of the head. In later years, long hairs develop on many parts of the body (groin, genital organs, armpits, breast, and face). Still later, the hairs elongate on legs, arms, & elsewhere. But it is the same individual throughout all these changes.

These hairs may all be nearly white at first, or the long head hairs may be red or brown or black. Much later, the long dark one, on all parts of the body, may slowly or rapidly become partly gray, then wholly gray, and then white. Colors change progressively but it is the same individual, not a new form or variety.

Finally, the hairs may drop off, partially or completely from smaller or larger portions of the top of the head. But it is still the same individual.

Most herbarium specimens from shrubs and trees are but fragments at best. They are fragments in quantity, in total seasonal expression, and total life expression. It takes 3, or 4, or 5 from the same plant on any given date to show full expression at that time. I often collect 5-7 from the same plant on the same day, to show all of its expressions.

When a leaf-bud opens in spring, the leaf tips emerge first and begin to broaden, followed successively by the middle and the basal portions. If collected before this process is complete, the blades will not be of normal shape but narrower ("attenuate") at base. If competition for food by the leaves on a twig becomes acute, the lower suffer first and often permanently. So the lower leaves on any twig usually are shorter, broader at tips and narrower at bases than those farther up the twig. Fall sprouts, with little competition, often develop enormous leaves.

Its loads of fun to study and record the variations appearing on single individual in a single season. Why don't you go out and tag a few willow plants, visit them about once a month for a year, and record the variations in the different organs. You'll probably be surprised and it will make a good paper to publish and a helpful talk before a botanic society.

Cordially yours

Carleton R. Ball
Carleton R. Ball

→ It so happens I have done precisely this.
Why didn't they send you Prof. Smith's letter along with the specimens? Or all of the specimens? (See over)
(over)

The sole interest of your caste and classish group is to protect the old status quo, and you act only when it is threatened. Let's see that democracy the war was about.

Sincerely,

L. Kelso

Why wasn't the botanical master race down there able to identify those specimens? I knew what they were. I also know how the rural Lynch law mentality trages at any questioning of established ideas.

On the Argentine Weevil project, station at Florala, Alabama, Bureau of Entomology, the summer of 1942, there was a young man collecting Mutillid ants in his spare time for taxonomic study. He said he had to keep it secret because the Bureau didn't want anyone to build up private collections in entomology. If the Personnel office does not know and has no control over what goes on in your Department somebody should ~~have~~ know about it and is going to know about it. Writing in "Science" to recruit able young scientists for the Department on one hand; botching and destroying their careers on the other. Everything in taxonomy is not going to be slanted totally to favor an arined master class. We find much of Agricultural research directed by individuals who have no college degree and never did any research nor made a discovery, e.g. the Argentine Weevil project in 1942, yet from there are to come rules to control all taxonomy, maybe.

Dr. Ball:

This, one of several is what
would have been sent to my super.
if I had been working in U. S. D. A.

What are the haughty and
self assertive leaders of the numerous
scientific groups doing to save
Taxonomy as a science or a career?
L. Kelso

Mr. Leon Kelso,
1370 Taylor Street, N.W.,
Washington , D.C.

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

August 22, 1948

Dear Mr. Kelso:

Some 2 weeks ago I received a copy of your Biol. Leaflet No. 41 and your appended note of Aug. 4. More recently, I received the processed copy of the letter of Prof. Ernest C. Smith to you and dated "an. 17, 1947". I thank you for the copy of the Leaflet, and ~~xxx~~ am returning the receipted bill you inclosed with your comments thereon.

I have studied your Leaflet 41, "*Salices Monticola*", very carefully but am quite unable to determine what you are talking about in your references to the "cordate willow" and the "Barclay willow". Apparently you do not know yourself.

The real "cordate willow" is *S. rigida* Muhl. (*S. cordata* Muhl., not Michx.; *S. missouriensis* Bebb). It has never been found in the Rocky Mts. The species of Section *Cordatae* which do occur there are *S. ligulifolia* Ball and *S. lutea* Nutt. and varieties, with *S. pseudocordata* (And.) Rydb. more abundant in the western part of the State, but occurring in the Rockies.

The real Barclay willow, of course, is *S. Barclayi* Anderss. So far as known it does not occur in Colorado, the few specimens listed by Smith being wrongly interpreted. Other species of Section *Commatae* which do occur are *S. monticola* Bebb and *S. pseudomonticola* Ball and var. *padophylla* (Rydb.) Ball, with pedunculate aments.

Several other species also occur in that general area, including two (*S. irrorata* Anderss. and *S. Wolfii* Bebb) which are close to the *Cordatae*. You speak of "26 specimens from as many shrubs of this willow (*S. monticola*) were collected. The question, then, is what did you collect that day and just what do your 26 specimens really represent? It is clear that you had more than one species, but what ones.

Most writers, in giving such an account, would first have listed all the species of *Salix* which they found growing in the area and perhaps would have given some notes on their respective habitats, abundance, and degree of development. You did not.

Your comparisons of specimens, on pp. 2 and 3, give no useful information to anyone, because you did not know, and they cannot guess, what species you were comparing. If you had compared specimens of any 2 or 3 or 4 species of willows you would have gotten about the same results. Moreover, you do not cite any collection numbers for the 26 collections, so if they ever are seen by anyone else they cannot be recognized as the ones discussed by you. Likewise you give no indication of the relative degree of development of leaves or aments on the 26 specimens, so no one may judge whether they could properly be compared if they been all of one species. Do you call that real "field work in systematic botany"? *Who is doing as much or better on this plant?*

So far as one can tell from the fragments reproduced in plate 1, both belong to *S. pseudomonticola* var. *padophylla*, but that cannot be certainly known from the plate. They certainly are not *S. monticola*. *Which proves neither you nor Prof. Smith have seen adequate material of Bebb's *S. monticola* from the type locality.*

I have not today consulted your earlier leaflets on Rocky Mtn. willows, but I recall distinctly that I was unable to determine from your discussions of the larger species, just what one you had in mind. I noted also the the leaves you inserted as illustrations in several cases were actually smaller than the minimum size indicated in your descriptions. What does that illustrate?

Months ago, after learning about your series of private leaflets, I looked up your entire series so far as represented in two libraries, and also went over you record in the Dept. of Agriculture and the papers previously in journals, including those by one who seemed to be your wife.

I can only state that most of the Cordate-Commatae shrubs along Clear Creek + elsewhere in Colorado have peduncled (See over) (over) and silasile Cathartes both. Georgetown and Leadville cannot

Not carefully enough to note that collection numbers accompany the illustrations. What writers are going to work on at all?

be brought into the Gray or National herbarium.

Some of my work had to be published in her name because Coville obstructed many publications at the time, a fine state of things!

The fact remains that we still have no Flora of Alaska. I am determined against any more one man dictatorships in botany.

I belonged, as a matter of fact, was a member of the Biol. Soc. of Washington for about 6 years. It was governed year after year by the same circle, or council, as are all Washington scientific groups. The younger members rarely or never were asked to present papers.

You might tell the forthcoming meeting who is to pay for the higher and more exclusive standards you want.

Why is it the business of you or your department to maintain a Spanish Inquisition over taxonomists, "who is ahead of who; who is fit and who is not?" Should taxonomists carry their birth certificates and draft papers with them?

It is too bad I am not a member of something from which I could be fired for the taxonomic stand I have taken.

As long as western born and raised taxonomists will ~~the~~ humbly submit their specimens for use of Dept. Agric. authorities that and they are all right. When they assert themselves and their opinions to any degree then they are louts and bores, that is the tradition of which I am thine.

There is enclosed herewith a picture of H. A. Hardy No. 18945, identified by yourself as S. Barclayi "approaching var. rotundifolia Anders." Manning Park, British Columbia 7/28/45. The twigs ^{and leaves} are glabrous; also the leaves; peduncles 6-20 mm. long. The La Plata specimens are pubescent, on both twigs and leaves, permanently.

Mr. Leon Kelso,
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I have not today consulted your earlier leaflets on Rocky Mtn. willows, but I recall distinctly that I was unable to determine from your discussions of the larger species, just what one you had in mind. I noted also the the leaves you inserted as illustrations in several cases were actually smaller than the minimum size indicated in your descriptions. What does that illustrate?

Months ago, after learning about your series of private leaflets, I looked up your entire series so far as represented in two libraries, and also went over your record in the Dept. of Agriculture and the papers previously in journals, including those by one who seemed to be your wife.

From your record and your publications, I had a strong feeling that you were isolating yourself too much and that you need much more in the way of contacts. So I invited you to come down and see me, thinking that your interest might be awakened, but you did not reply and you did not come. Instead, you got out Leaflet 40, another diatribe in exactly the same "haughty and self-asserting" manner you so constantly ascribe to others.

You ought to mix up with others a lot. Apparently you are not a member of any of the local or national botanical or biological organizations. In that case you contribute nothing to the support of their journals, yet complain that they will not publish your papers. Life ~~is~~ is a constant give and take, which tones down prejudices and takes the kinks out of exaggerated egos like yours. You feel that you must "give" and apparently spend a large part of your income paying for it privately. Is it because you can't "take" that you keep away from everybody and everything that is alive. Where has it gotten you?

I urge you to get out and mix up with the botanical groups here in Washington. The Botanical Society of Washington is a good place to start. Every kind of a botanist belongs and most of them attend, although the taxonomists are mostly absent. Is there something in taxonomy that prevents a man from mixing with and enjoying his fellows? Or what is the reason? In mid-September, in connection with the celebration of the 100th Anniversary of the Am. Assn. for the Advanc. of Science, the Systematic Section of the Amer. Soc. of Botany and the Amer. Soc. of Plant Taxonomists hold a joint meeting. You ought to be there, even if you get up and go out when I start telling why taxonomy is ill-supported. Come down anyway and hear the others. Visitors are welcome.

By the way, you state in No. 40 that "organized science did not say a word when Coville throttled the Flora of Alaska. Evidently you don't know either side of that story. But while you growled and cursed, some of worked at the job. As a result, six Division in the Bureau of Plant Industry were abolished and their work consolidated with other Divisions. Coville and five other chiefs like him, men of about my own age, were out of their administrative jobs and allowed to retire or work for others. Science not only said words but it got action, and that was years ago. Which was the better way?

Hoping to see you soon, in the scientific crowd, I am Cordially yours,

Carleton R. Ball

P.S. I do not recall seeing more than a very few of your *Salix* collections in any herbarium. When taxonomists describe new species or varieties, or discuss species from the standpoint of characters and relationships, they usually deposit sets of the material in other herbariums, if it is of their collecting. Where are yours? If you don't like the Natl. Herb., there are Gray, N.Y. Bot. Gard., St. Louis Bot. Gard., Field Museum, Phila. Academy, and the many State Universities and Agric. Colleges.

Also, the lady to whom I referred earlier mentioned specimens in her herbarium, in the papers she published. What became of that herbarium? And are there duplicates of such specimens in any other herbarium?



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Mr. Leon Kelso,
1370 Taylor Street, N.W.,
Washington 11, D.C.

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

Sept. 7, 1948

Dear Mr. Kelso:

1. Your comments on my letter of Aug. 22 are received. When a scientist refuses to accept suggestions or supervision from his administrative officers, or from any one else, he thereby obligates himself to practice the most painstaking study of materials and scrupulous accuracy in statements. Only in that way can he attempt to justify his attitudes. This obligation you have constantly refused to meet.

2. You make no reply to my statement that the leaves you inserted with the descriptions of willows are actually smaller than the minimum sizes of leaves you described. What does that teach students trying to learn about willows? Would others be justified in describing such specimens as new small-leaved varieties?

3. I asked you what other species of willows were found with *S. monticola* in Clear Creek canyon. You do not say.

4. I said that you did not describe the plants you collected and wrote about as *S. monticola*, from Clear Creek, but that your comparisons of the 26 specimens shows clearly that you had more than one species. You give no indication of what really is contained in the 26 specimens you call *S. monticola*. No pubescent-capsuled *S. monticola* ever has been mentioned or described. Should one suppose that no other species would grow in Clear Creek canyon at or near the type locality?

5. You say that my comment proves that neither I nor Prof. Smith "have seen adequate material of Bebb's *S. monticola* from the type locality". How can your statement possibly prove anything about *S. monticola* when it is so apparent that you were discussing more than one species?

6. What has a specimen of *S. Barclayi*, glabrous or other wise, from British Columbia have to do with a specimen of *S. pseudomonticola* var. *pedophylla*, pubescent or otherwise, from La Plata, Colorado? I fail to see any connection and you do not explain it.

7. One of my published papers is entitled: "More Plant Study: Fewer Plant Names". One might say now: More plant study, less published plant confusion. Study and accurate knowledge are an excellent preparation for publication.

8. This letter ends our correspondence, so far as I am concerned. I begin it in the hope that you might be helped to a better viewpoint and a more helpful study of plants. You have shown conclusively that you do not wish to spend time and money in studying plants and in helping others to know more about them. Apparently you merely wish to publish whatever unverified idea pops into your mind at a given moment.

9. If you believe that that type of publication will give you a reputation as an accurate and helpful scientist, and a standing that will remain after you are dead, go to it. No rules prevent you and no "superiors" refuse consent.

Wishing you joy in the job, I am

Cordially yours,

Carleton R. Ball

BIOLOGICAL LEAFLET No. 40

EMBRYO TAXONOMISTS

BY LEON KELSO

"If embryo taxonomists were required to go out and measure and record the range of variation in organs on a single large living plant or on numerous smaller plants of several species, they would return both tired and amazed. If they were required to follow the progressive variation of all organs on a plant through all of the seasons of even a single year, they would become both enlightened and humbled. Knowledge, with amazement and humility, is an excellent foundation for future taxonomists." (Journal of the Arnold Arboretum, Vol. XXVII, 1946, pp. 371-385, 378.)

One source of amazement for "embryo taxonomists," and embryos in other lines, is the discovery that one morality is preached and quite another practiced in this "the brave new world" as in the old.

The above article states that there are now no professional standards or requirements for taxonomic workers; that "this lack of standards is responsible in part for the lack of respect in which this profession is held."

The writer of that article has for 50 years been a member of an organization which has had a dominating if not totally controlling interest in some departments of American taxonomy.

Of the 48 States only 15 have a usable, up-to-date flora. For any large genus or family of birds or plants, the number of thorough treatments (i. e. containing keys, and adequate descriptions of all known forms of the group, so that one can identify them without reference to collections or experts) can be counted on the fingers of one hand. For many groups there are none. Why is this? There is no lack of criticism and opposition for any honest taxonomic effort.

Social, economic, and psychic factors have nothing to do with systematics according to the aforequoted article. As the Private said: "There ain't no sich thing as psychology. My Sergeant said so."

There is no mention of the fact that the leaders, the trusted

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Dr. Carleton A. Ball:

are cut out. This means that the loss of months of work, all so some of hierarch's feelings will not be hurt.

11. *Trivial and irrelevant criticism.* Scientific periodicals have many pages devoted to taxonomic criticism; very little or no space devoted to criticism of ecological papers. Much of the criticism may be true but why the severity for taxonomy? It is uplifting to the community for the preacher's kids to come in from the sidewalks at 8 while the others unhamperedly raise hell at all hours according to some people's feelings—but others would disagree.

12. *Editorialisms.* No two periodicals, or institutions, or authorities agree as to how a systematic paper should be written up and every paper can be quarreled to death on the basis of editorialisms.

13. *Caste and class and status quo.* A taxonomic work appears. One of the arrived caste criticizes it and all of his group must side with him. If the writer replies to the criticism scientific dignity is offended; delicate feelings are hurt.

14. *Types of criticism.* These consist of:

- A. *Setting up and knocking down a straw man.* Saying: This should include such and such. Since it does not it is wrong.
- B. *The blast of criticisms in the shotgun scattering pattern.* Giving many criticisms which do not apply, but the refutation of which is tiresome and useless.

15. *Coöperation, and our fellow workers.* The systematist is one person when his help is needed; another when his help is not needed, especially to ecologists.

Point 5 would not be included were not some developing the idea that only head curators, not revisors, have the right to discoveries and records based on materials in large institutions.

The foregoing conditions add up to a situation from which most embryo students "return both tired and amazed," also, discouraged, disgusted, disappointed, disillusioned, displeased, disenchanted, disenfranchised, disheartened, dissociable, dispunged, and distraught. For the Rocky Mountain West this means the dominance of the Rydberg manuals for a half century or more.

By removing some of the above conditions, Dr. Carleton R. Ball, author of the article quoted, and his fellows, could rescue systematics from behind that other Ball, the 8-Ball, in this case consisting of the baneful blue eye of the ecologist and anti-taxonomist on the one hand, and established group interests on the other. Otherwise, the work is left to free-lancers.

You entered this as a stooge for S. F. Blake and for no other reason. Ecologists have to take no grilling. You have picked on the wrong tenderfoot, and I just can't take it.

Let there be no more pressuring or trying to dominate taxonomy in help or any other sphere.

Mr. Leon Kelso,
1370 Taylor St., N.W.,
Washington 11, D.C.

3214 Jocelyn St., N.W.,
Washington 15, D.C.

Sept. 25, 1948

Dear Mr. Kelso,

Having finished and delivered my paper at the biologic meetings recently held here, and having revised the manuscript and sent it to one of our botanical journals, I find time to read your childish comments written on a copy of your B. L. No. 40. I still ask you where that persistent attitude has gotten you, over the years?

I was raised on a cattle ranch, lived in the saddle, and can still shoot from the hip in a running draw. I have traiked the Great Plains and foothills from the Rio Grande to norther Alberta and Saskatchewan. I have climbed peaks in the Rockies, the Wahatch, the Sierra-Cascade, and the Coast Range systems, as well as in the East. I have argued with rattlesnakes, and slept on the grass of the Texas plains while a Mexican jaguar made his nightly kill. At 75 I still take on anybody who wants a good mountain climb. I can take it, physically.

In taxonomy, I study hard and long first, then publish. As a result, I have made comparatively few new species of *Salix* in 50 years, have reduced to synonymy about as many made by others and am still doing it. When I make an error, and later find it out, I acknowledge and correct it myself, instead of leaving it to burden others. I criticize when criticism is needed, and take it when offered by others. All I ask is that it be true.

You know perfectly well that the reasons why ecologists, for example, are not being criticised, by me and others, is that no one has to pay any attention to what any ecologists print, if he does not want to. But when new species and varieties are created by persons who know nothing or little about them, all taxonomists forever after have to pay attention, under the priority rules. And it is these others who have to do the studying and the hard, thankless work, to bring out the truth.

Now, can you take it? I challenge you to make the 26 alleged *S. monticola* specimens from Clear Creek available for study: —

1. By letting me come to your house to study them;
2. By sending or bringing them to my house for study; or
3. By bringing them or sending them to the U.S. Natl. Herbarium for my study there.
4. The findings to be published, either by me or by you.

How about it?

Cordially yours

Carleton R. Ball

Bob -
Mr. Leon Kelso,
1370 Taylor St., N.W.,
Washington 11, D.C.

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

November 27, 1948

Dear Mr. Kelso:

I seem to have had no reply to my letter of 25 September. Does this mean that you intend to leave your readers under the false impressions given by your discussion of the supposed *Salix monticola* specimens from Clear Creek, Colorado?

Or are you making a detailed study of them, with the intent of publishing their correct identifications when you have completed the work? Those who maintain absolute independence, are obligated to extremely careful work, especially when they refuse to make their material available to others.

Cordially yours,

Carleton R. Ball

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Bob - Burroughs' paper
Joe -
3/13/49
Hym' 68

3/3/42

See -

Reg. Beresford, Robert

Reg -