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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.

WHY IS TAXONOMY ILL-SUPPORTED?

Science June 14, 1946

Carleton R. Ball

Foxberg and Diehl, in "Present Status of Foreign Herbaria and Museums" (Science, March 4, 1946), make several observations which are important to biological science and with which most botanists will heartily agree. I wish to comment on only one phase of the problem, mentioned by them in these words: "Systemat^{ic} botany has traditionally been ill-supported." This is true, and it will continue to be true, and deservedly so, as long as taxonomists maintain present principles and practices. Taxonomy is the only branch of science where this is true.

1. Rewards are bestowed, in taxonomy, for creating names and not for knowledge of the organism named. Some persons have renamed scores, and hundreds, and probably even thousands of plants which they never saw and could not possibly recognize if they did see them, but the names so applied must be acknowledged under taxonomic rules, either as valid or as synonyms. And no stigma or penalty whatever attaches to the creation of innumerable synonyms.

2. Permanent rights are accorded to the creator of a name, without regard for the general welfare of scientists. This is true whether the name be valid or one of the many kinds of synonyms; whether it was based on knowledge or ignorance; whether it represented an addition to, or merely confusion of, the existing knowledge; and wholly without regard to the inconvenience caused to tens of thousands of other workers. When a man wishes to patent an idea, under United States law, he must submit it to a jury of experts (U.S. Patent Office). If they determine that it is sufficiently new and different to warrant recognition (patent), he has to pay a sum of money to obtain such recognition. And after all that, he gets exclusive rights for only 17 years, - - not forever.

3. Taxonomy has no required standards of preparation or fitness, yet its products may help or harm many. Any one who wishes may name organisms, and the names must be recognized forever, ^{or} as valid or as synonyms. School teachers must be trained and must pass examinations in order to teach even the three Rs in the rural schools. Auto drivers, electricians, plumbers, and public accountants must be examined by experts and must obtain a license before they may operate. Dentists, and doctors, and lawyers, and ministers must be examined and certified before they can ply their trades. But any one may apply Latin names to organisms and the names automatically have permanent status in one or the other of two categories. The products of an ignorant, careless, or dishonest type ^{may} cause untold and continuing labor to many others much more able.

4. Taxonomy is the only science that openly appeals to, and openly rewards, the innate selfishness of man by guaranteeing the permanent association of his name with the organism-name he coined, whether it be of value or not. Chemists, physicists, geologists, mathematicians, physicians, and philosophers make discoveries and develop theories of immense importance to humanity, but we are under no compulsion to attach their names when referring to such discoveries and theories. Sometimes we do, in the case of a dozen out of tens of thousands (Boyle, Darwin, Einstein, LaPlace, Newton), but no rule compels it. What is the value of a single plant name, out of a million or more, as compared with one of these great products of the human mind?

5. These principles and practices ~~have~~ of taxonomy have created another field of labor, synonymy, fully as difficult, more far-reaching, and even more expensive, as taxonomy, and we are but at the beginning. Some now devote their time to the study of names and never learn about plants. All real taxonomists are compelled to spend an increasing percentage of their time in the study of names, leaving less and less of their energy for the study of plants. Which are the most important to humanity, names or plants? Which are the most important to science?

6. All of this has been done under a ~~fetish~~^{is} known as stabilizing nomenclature. Fosberg and Diehl refer to systematic botany's contribution in studies to "stabilize nomenclature". I will defy any person to compare the successive manuals of botany issued in the last 100 years and ~~find~~ produce any indication whatsoever that nomenclature is being stabilized. If the permanent ^{ce of} patent (priority) rights to a name never had been acknowledged in taxonomy, we would have had a stable nomenclature long since. As it is, all workers in botany have to learn a new set of names for most plants every 25 or 30 years. This not only is maddening, but absolutely unnecessary.

7. Suppose that we applied such a rule in the realms of chemistry, economics, geology, cosmogony, mathematics, philosophy, physiology, theology, etc., and had to cite the name of the original promulgator of an idea every time we mentioned it. Suppose that we always were finding (as we are) that some one just a little earlier had evolved what might be claimed to be the same idea? Suppose that we had to append the name of the architect or builder every time we mentioned a great structure or addressed a letter to a given house of office building? If we did, we would be approaching the futility of the situation to which the stupidity and unrealism of taxonomists have brought that science.

8. If the taxonomic world really believed in, or desired, stability of nomenclature, they would have worked to achieve it long ago through limiting prior rights to a specified term of years or through conserving well-known and widely used specific names when they had been in such use for 100 years, or 50 years, or 25 years. This would prevent the present disastrous absurdity of letting a few months priority displace names well-known and widely used for 140 years.

Carleton R. Ball

U.S. Dept. of Agriculture.

INCIDENTAL COMMENTS ON MY STAND ON TAXONOMY

Prof. Russell G. Brown, Dept. Botany, Univ. of Md. College Park.
Comment in a letter of April 23, 1946:

"I've heard that you are starting a crusade to stop all the senseless "name-changing" that is such a headache at present. Your idea of a time limit beyond which a name must stick has merit. More power to you in your work".
(letter explaining erroneous inclusion of *S. missouriensis* in Md. Flora)

More Pl. Study sent 11/23 -----

Dr. H. A. Davis, Senior author, Willows of West Va., Dept. Math., Univ. W.Va.,
Morgantown, in letter regarding manuscript., June 20, 1946.

Strausbaugh, Core, and I discussed your recent article in Science and fully agree with you. I hate to give up old friends such as *S. cordata*. One of the hardest for me to swallow is *Chenopodium Berlandieri* Moq. ssp. *Zachackei* (Murr.) Zobel in place of the long familiar *C. album* L.

More Plant Study sent 11/23

3814 Jocelyn St., N.W.

Washington 15, D.C.

Mr. Fred A. Barkley,
1620 University Station,
Austin 12, Texas.

November 13, 1946

Dear Mr. Barkley:

Your card of Nov. 8 is here and you have my sympathy, for none of us likes to be criticised. Yours is the first word of protest I have received, probably because no taxonomists ever read Science. The fan mail is still coming in. Like it or not, we all have to face facts.

There were some 25 species of *Crataegus* recognized in America. Some 40 years ago three taxonomists raced each other to run the total to 1089 (or was it 1189) so-called species. We now know that at least 3 type specimens came from a single tree. No one has had the heart to tackle the mess and try to determine the truth. What good end was served by that dirty contest for names?

Very recently a plant pathologist, wholly untrained in angiosperm taxonomy, and wholly unfamiliar with *Crataegus*, published 13 (or 15) more "species" from his State. What right had he? Exactly that of a burglar, who obtains at the expense of others.

A certain botanist, about to lose his college job for incompetence, sent three specimens of *Salix* to a specialist in Germany, with the suggestion that they were new to science. The unfamiliar German immediately named three new species, on, of course, for the sender. All three were well known American species. Three needless synonyms and the sender lost his job besides.

How about the taxonomists who publish manuals or "floras" for private gain and put out a flood of "new" species and varieties before each edition, or into it, so that other taxonomists and institutions will have to buy it? And how about editions only two and three years apart, with not enough change to make a good journal appear? You know the men I mean.

How about Small, who suppressed the valid *Salix floridana* Chapman, appearing in every southern U.S. manual, including Small's, and published a *Salix Chapmani* Small with not a word of explanation, but citing the same type specimen?

How about the German taxonomists, bottled up by World War I and subsequent poverty creating 4 new taxonomic categories subvariety, sub-form, sub-sub-form, and sub-sub-sub-form, and naming every plant in the European herbariums down to the last of these foolish categories? I can get five of them off any healthy willow in plant in America.

How about the paleobotanist Floderus in Sweden, running out of something to do, and evolving the absurd theory that all willows are hybrids. He then got a Swedish friend at the Natl. Museum of Canada to send him their arctic collections and sent them back named as hybrids, simple di-hybrids, tri-hybrids, quadri-hybrids, and even quinti-hybrids claiming that he could distinguish five different species entering into the specimen. More than that, he claimed that he could see that one species had had more effect than another in different specimens, so that the five species are in different orders on different sheets. All geneticists know that there is not a word of truth in those claims. In natural hybrids, one can be sure of one parent but rarely of both, and to discern three or more is absolutely impossible. And now no one can find any thing in the herbariums he got hold of. And he denied that certain species occurred in certain areas so had the supposed parents living afar off from the point of collection.

Until we have standards for training of taxonomists, and until names proposed may be disregarded unless submitted to a competent jury before publication, the selfish chaos will continue and the expense of legitimate workers will pile up. I could fill 20 pages with illustrations like the above but I haven't time and you wouldn't read 'em.

Cordially yours

THE UNIVERSITY OF TEXAS
AUSTIN 12

DEPARTMENT OF BOTANY AND BACTERIOLOGY

17 November 1946

Dear Doctor Ball:

Thanks for your letter of November 13. I still feel that you are overemphasizing certain phases of the situation. Should I be fortunate enough to meet you in Boston I would like to discuss the matter further if you would care to hear my side of the question.

Cordially yours,

Fred A. Barkley
Fred A. Barkley

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

November 24, 1946

Mr. Fred A. Barkley,
Dept. of Botany,
University of Texas,
Austin 12, Texas.

Dear Mr. Barkley:

Your letter of 17 November is received and I will be very glad to hear your side of the taxonomic story at any time convenient to you. I regret, however, that I will not be at the Boston meetings.

In scanning my previous letter, I note that I omitted on recent and significant item which I had intended to include. When I described *Salix Setchelliana*, the Univ. of California distributed the abundant cotypes widely while my manuscript was in press. One set went to the Univ. of Uppsala, where Floderus is botanist. The labels read: "*Salix Setchelliana* Ball, n. sp." and "Staminate type" and "pistillate type", respectively.

Floderus immediately sat down, wrote a description of it as a new species of his own, rushed the mss. to the Acad. of Sci. there, and had it published promptly, out of it rightful turn, as shown by the printed dates of the receipt of all manuscripts printed in that and succeeding issues. In short, he tried to steal my species, with the lame explanation that he had not seen any publication of it by me. Talk about 'honor' among thieves!

If these are relatively rare instances, and not the rule, why do not these pure and honest taxonomists immediately proceed to conserve the many old and widely used specific names which are being displaced continually. Which would be the greater service to science, conservation of old and long-used names or substitution of new and unknown combinations with their names included? When you show me the taxonomists doing that, I will change my mind and not before. *Salix cordata* Michx. has just been thrown out after 142 years of universal use, just because of a possible few months of priority by Michaux. What good does that do Michaux? And what untold harm it does to tens of thousands of students and teachers. Damn!

Cordially yours,

Carleton R. Ball

P.S. I send separately a copy of "More Plant Study: Fewer Plant Names".

also Salix floridana

ATON FOREST

NORFOLK, CONN.

October 24, 1946

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

Dear Dr. Ball:

I apologize for the lapse of time between this writing, and my receipt of the June 14 issue of Science. Even after that interval, however, I feel moved to congratulate you very warmly for your article that appeared that week.

I am so thoroughly in accord with your views that there is no reason for my repeating in this letter the ideas that you have already expressed so well. It is a source of extreme comfort to have such opinions come from one who himself is a taxonomist.

Altho I have taught taxonomy myself, I am primarily an ecologist. During my teaching at the New York State College of Forestry, and later my connections with the chewing gum industry, I had perhaps an unusual opportunity to view the nomenclatorial neuroses of the science. Treated as an outsider by the taxonomists, and as a taxonomist by the outsiders, I was the recipient of many freely expressed opinions.

Personally, I feel pessimistic about the possibility of early improvement in the troubles of which you speak. If there were more like you within the field, certainly something might be done in the way of international rules. On the other hand, I feel you struck at the Achilles' heel of many taxonomists, at a weakness which is highly psychologic, and hardly amenable to improvement by clear thinking. In self defense, the very men who are most guilty will become more rigid in their attitude. One recently published reply to you seemed to miss the point entirely, taking what I thought a really naive attitude that the entire problem could be (and has been) settled by clear thinking. Personally, I think the trouble is not with our scientific rules and procedures, but with the men themselves - courses in psychology should be given, and psychiatrists called in. Lest you think I am hard on certain taxonomists, I can add that the more active of the younger ecologists look much more harshly on the psychologic aberrations of the older ecologists, at least some of them.

This letter requires no answer from you. I should be interested in calling, if and when I get to Washington again, to hear how various colleagues reacted to your article.

Sincerely yours,

Frank E. Ealer
Frank E. Ealer

November 6, 1946

Prof. Frank E. Egler,
Aton Forest,
Norfolk, Connecticut.

Dear Professor Egler:

Your letter of October 24 just came to hand and I appreciate it, as it is both interesting and informing.

I have no optimism regarding any early reform in either rules or practices of the taxonomic type of which I was writing. Men make the rules, not the rules the men. Therefore, the rules are what the makers want, so far as they can agree among themselves and get them.

Education of the coming generation is the only answer to this, and to many other of the ills which afflict us. That is where I am making my effort.

My invitation paper, "More Plant Study; Fewer Plant Names", in the Oct. (Merrill Memorial) issue of Jour. Arnold Arb. is a start. Another has been accepted (by Fernald) and will appear in Rhodora before next spring. It is not wholly complimentary to Fernald himself, any more than is the last page of the Arn. Arb. Jour. paper. A third probably will go to the Amer. Midland Nat. It will picture a worse but much less important series of foolish blunders and may be refused because not so important. My write-up of the Texas willows for Lundell's Flora (just being completed) had to call attention to more of the same. Other papers later will be offered to Castanea, Madrona, etc.

(By the way, it was a paper which was invited for the Merrill Memorial, not that title, but I did get a hearty appreciation from Dr. A.C. Smith, the editor, on the scope and content of the paper.)

The acts of certain taxonomists are of exactly the same nature as those which characterize the present increasing flood of juvenile and adult delinquents and criminals. For a long time, we have been building bigger police forces, jails, courts, and penitentiaries to handle them, under the wholly mistaken idea that we could "cure" the evil. Now, belatedly, we are getting started on prevention, the only remedy, and much less expensive. "Synonymy" is the taxonomic counterpart. The remedy is prevention, and that's what I am trying to get the coming taxonomists to realize.

I am not quite sure that I know what critic of mine you refer to, without naming. Perhaps Metcalf. I am citing to him a few cases in point and asking him if he believes that those men had the motives he ascribes. He is old and it won't penetrate. (I'm comparatively young, being only 73 past).

You will get a copy of the Merrill Memorial paper as soon as the reprints reach me.

Cordially yours,

P.S. Is Aton Forest a Federal, State,
County, city, or communal forest.
I am writing on my 3rd Calif. volume
on natural resources and cooperation

Carleton R. Ball
Collaborator

of official agencies in them.

ATON FOREST

NORFOLK, CONN.

November 9, 1946

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

Dear Dr. Ball:

It was a pleasure to receive your very interesting letter. I am indeed glad to know that we will be having several articles from you on this subject in various botanical journals. I shall trust they will be read by the proper people. Yes, I thoroughly agree with you that the cure lies in a prevention program, among the coming generation. What a pity we do not have any agency to provide us with a list of active graduate students, whom we might bombard with reprints of such articles. I fear I have little hope or respect for a change of mind for others. Human nature is such, that few of us can continue to learn after middle age. Most rarely is it true that one "has 73 years" (as say the Latins). Our English is more honest when we say that one is "60 years old", 50 years old, yes, even 40 years old.

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I checked the critic to which I referred in my previous letter. Yes, it was Metcalf. On re-reading the article (and not knowing the man or his work), I still feel that he completely missed the point. With no reflection on himself, I feel that his interpretation of others is extremely naive. Perhaps he is so wrapped up in his own apparently rational life, that he cannot impute ulterior motives to the actions of others, or see the irrationality of those actions.

I do believe that early education in psychology might help a lot. We cannot change basic human nature thereby, but we can educate men to watch themselves for those deeds and actions (and taxonomic research) that stem from egotism, vanity, pride, insecurity, without which psychology training, they would naively rationalize as "good science".

Aton Forest is nothing more than my home, ear-marked for a "permanent" research area. All but my most recent vagaries are in American Men of Science. I attained a certain kind of success and fame, using many of the techniques so bluntly exposed in AAUP Bull 31 (4):668. I cut clean, right after VJ Day; came to these 400 acres of New England forest and field; and find myself busier than ever. I refuse to call it Retirement (I am only 35); it seems more like Commencement. (over)

P.S. I am sending a few reprints. You may consign them elsewhere, if not within your interest.

Cordially yours,

Frank E. Egler

Frank E. Egler

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

Mr. Frank E. Egler,
Aton Forest,
Norfolk, Connecticut.

November 13, 1946

Dear Mr. Egler:

I appreciate your letter of Nov. 9, and the accompanying reprints. I am glad you gave me permission to pass the latter along, because, at 73, I have stopped accumulating what I shall not use in my work. I have kept several of the special papers on vegetal studies, orthography, etc. Several I am passing along to the Div. of Plant Exploration, and a few to Dr. Dayton of the Forest Service.

I did not reply to Metcald by a letter to Science but wrote him personally giving several outstanding examples, including the three who messed Crataegus, etc. He is a fine man but has been cloistered and does not know life or human nature. So many of the taxonomists I know personally are of that type, without all his goodness. Many of them seem to have adopted taxonomy as the result of an inferiority complex which made them afraid to mingle with their fellows or to try to make good in the rough and tumble of contactual competition. In taxonomy, the plants cannot object to the treatment given them, and there is a conspiracy of silence on the part of others taxonomists who do not want some one to criticize them. Most of the men in the U.S. Natl. Herbarium are of that type. Only one attends the meetings of the Washn. Bot. Soc.

Prevention is the remedy, as you say, but there seems no way by which any standards of preparation can be erected and a self-selected, inferiority-complex brotherhood is not going to reform itself because they live on the lack of reform.

By the way, one of my interests is reform of our atrocious English language. Educators tell us that the average child takes two years longer to reach the end of high school (from kindergarten) than would be needed if we simplified our language (8 pronunciations of "ough"). Years ago I wrote a paper for American speech on the use of Latin or Greek plurals for nouns adopted into English in the singular. There are about forty different plurals in the Latin. It would simplify our language greatly if we anglicized the plurals also (datum, datums; equilibrium, equilibriums). That would save a lot of wholly useless learning for the millions who go forward in education. I note your use of "equilibria". Also, "an histroical". Why not "a" when the h is not silent? I should have noted that only about 50% of college grads in science know that "data" is a plural form, and none knows that "agenda" is.

I cannot find your first letter in my file, nor any card in distribution file. Did I send you any reprints?

Cordially yours,

Carleton R. Ball

(See over)

University, Ala.,
June 28, 1946.

Dr. Carleton R. Ball,

U.S. Dept. Agriculture,

Dear Dr. Ball:-

I read with interest your note on taxonomists, in Science of June 14. I do not have it before me now, so cannot comment on it in detail, but you are quite right about frequent changes of names being a nuisance. But you do not seem to suggest any remedy.

As you of course know, changes of plant names may be matters of either nomenclature or classification. The reformers of fifty years ago, when they revolted against the more or less authoritarian system used by Gray and his predecessors, thought they would soon have everything stabilized. But their expectations have not been realized, partly on account of the frequent digging up of old and almost forgotten names, and partly on account of occasional changes in the rules.

One change, that I think was a good one, was outlawing the duplicate binomials that sprung up in the 90's and flourished for a few decades. But one that made a good deal of trouble was establishing lists of "nomina conservanda." That struck me as entirely too arbitrary, and the list has been changed from time to time, and any one not constantly engaged in taxonomic work in a large institution can never be sure that he has got the latest list. When such lists were first established the compilers neglected to designate the types of the conserved genera, and that caused some uncertainty, as some objectors soon pointed out. I don't know whether that has all been straightened out yet or not.

Still more absurd is the requirement of Latin descriptions. Most scientists these days have never studied Latin, and are liable to make a mess of it, unless they have expert assistance. I have seen Latin descriptions that did not agree very well with the English description alongside; and in that case which should one be guided by? Furthermore, even the English vocabulary is

(2)

1 standard
2 no perm.
priority

Dr. Roland M. Harper,
University, Alabama.

3814 Jocelyn St., N.W.
Washington 15, D.C.
July 6, 1946

Dear Dr. Harper:

Your very interesting letter, of June 23 and accompanying reprints are very much appreciated. I answer at once because I have a bit of leisure at this minute and because I may never answer if I delay, being still under many pressures.

I have received a lot of fan mail about the letter on taxonomy. The opposition probably will lumber up soon, though I suspect that the professional taxonomists do not read Science.

You state that I did not suggest any remedy for the taxonomic mess. True, if you mean definite plan. I did state plainly that they have no standards, that present practices encourage selfishness, and that there was no reason whatever for permanent priority. These are true and harmful. Remedy is to adopt the reverse, and I felt that suggestion from a person not a professional would be better than proposing specific rules.

You may be interested in two papers now in press wherein I suggest taxonomic methods to better results. The first, entitled "More plant study: Fewer plant names," will appear in Oct. in the Merrill Memorial volume from Harvard. In it I discuss the normal variations in the successive seasonal sheets on will each individual willow, the changes on individual organs as the year advances to the following spring, and the variations due to sharp differences in local habitat, as alluvial bench, stony bluff, sandhill, etc.

The second, entitled Studying Salix or Making New Sections, will appear in Rhodora new winter. It takes Arnold to task for his two recent new Sections, each based on one species. In one case no comparison was made with any other species and the other was compared only with the type species, both foolish procedures. I then described the characters of about 6 additional species belonging to each of the sections from which his were removed and showed that there was no need for removal whatever.

Nature provides variation everywhere and always. Man desires and imagines uniformity and therefore names as new every variation he happens to see, which is not science at all, but carelessness. Pubescence is one of the most universal and variable of Salix expressions and all tend to lose hairs in age. So we have a flood of vars. and forms named tensa, glabra, glabrata, etc. Is a partly bald human a new variety?

One of my fan letters postulates that taxonomy attracts the weak-minded and the frustrated, who find a unexcelled opportunity for insuring perpetuation of their names for all generations. Europe, after World War I, gave us some examples of what I believe was frustration. Funds were scarce and foreign excursions reduced. So, in self-defense the Germans began to extend the number of categories in taxonomy. At last accounts they were naming spp., subspp., vars., subvars., forms, subforms, sub-subforms, and sub-sub-subforms, or eight categories with Latin names (and theirs attached). The Swedes (Floderus) imagined that all willows are hybrids and renamed them accordingly. Not only that, but the insisted that di-, tri-, quadri-, and quintihybrids could be identified from dried herbarium fragments, something that no geneticist would admit as possible from entire living plants. But no rules stopped them and new species cannot be found in some herbariums.

write me Re Tobacco

inadequate to describe such complicated things as Sarracenia leaves and the flowers of Labiatae, etc., and the Latin vocabulary is still more limited.

I don't know when the Latin rule was first promulgated, but I think about 1920. However, the dead-line has been moved forward once or twice since, and we have no assurance that it will not be moved again. It is impractical ~~to~~ to use Latin for new species described in a book (which is often done, though I don't recommend that practice). The Latin rule was in force when Small published ~~his~~ his Manual in 1933, but he disregarded it, and I believe the dead-line has been moved forward since, to validate his names. If that had not been done, anybody, theoretically, would have had the right to re-name his new species. But anybody who would do that would be a very contemptible person, and I have never heard of anything like that being done.

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There are still other complications in nomenclature that I can't recall now. Dr. H. K. Svenson, when he was here to see me in 1938, had with him a pamphlet on the subject, by some man in India or Ceylon, that brought up some questions I had never thought of before; and I don't know whether they have been answered satisfactorily or not.

We might conceivably some time get a perfect set of nomenclature rules, that would never have to be changed. But we would still have our troubles with classification. There is much difference of opinion about generic limits especially, and I don't know how we can ever reach a final decision on that. The Harvard people are very conservative with genera, while Small and Rydberg tended toward the opposite extreme. Fernald, who has described hundreds of species and varieties, has never described a genus, that I know of. Just in the last few years some ultra-conservatives have been merging genera that had been kept separate for 100 years. And a man in St. Louis wants to put many of most of the genera of Asclepiadaceae back in Asclepias, thus discarding the work of many of his eminent predecessors. (over)

The fundamental wrong in taxonomy is the rule of permanent priority. It has no justification. Taxonomy was made for man, not man for taxonomy. Use (service) is what counts.

The name *Salix cordata* Muhl. has been in international use for 142 year. Now, because of a claimed but not certainly proved priority of some 4 or 5 months for *S. cordata* Michx., the use of 142 years is instantly discarded. And that process will go on forever. Some are yelling for the priority of *S. chilensis* over *S. Humboldtiana* Kunth, but there is grave doubt that *S. chilensis* was a willow at all, no flowers being described. That does not worry the prioritists at all.

Private publication of manuals of botany by taxonomists tends to straight fraud. *Salix floridana* was published in 1860 and appeared in every manual thereafter. But Small, getting out a new book, inserted *S. Chapmani* Small without a word of explanation, and even cited the type (and only) specimen of *S. floridana* as the type of his "new" species (or name). There never has been any question whatever of the validity of Chapman's name *floridana*, but manuals seemingly sell in proportion to the novelties therein, hence *S. Chapmani* Small. Damn. Rydberg was a whiz at that.

If one does not like any restraints on indiscriminate publication of new names, why not a time limit after which they need not be recognized if invalid? I cited the 17 years of prior rights permitted by the U.S. Patent Office, and suggested that we set some limit on name rights.

The whole present set-up encourages shoddy work, extreme selfishness, and rank fraud, with no compensating advantages whatever. There is no stability whatever, nor will be, because rules constantly are being changed. Witness the flood of new combinations when the rule on varieties was quietly changed. The rules congresses naturally are packed by the selfishly interested. Damn and damn again.

If the remedy lies in editors, you get complete totalitarianism, as some of your own examples show. It is much less democratic than a committee of several, with a separate committee for each major field.

of being

I had the interesting experience ~~XXXX~~ asked asked by Raup if certain Mexican specimens represented a new species. I mailed him material of certain long-published Mexican species from my herb. and the U.S. Natl. Herb. He replied that, while his material was not exactly the same he would not publish a new species from it. Glory be

The facts of normal variation are almost universally overlooked, as noted. In addition, herbarium specimens of shrubs and trees are mere fragments, and seasonal fragments at that, in all stages of development. One has about the significance of a baby's ear, another a youth's foot, a third is a middle-aged man or woman, and the fourth a decrepit grandparent's head. Yet they all may represent the same individual plant, as well as the same species. Try to recognize an adult from its baby picture!

But enough of this. Why don't you write up your views on taxonomy and state the remedies as you see them. It will take many and long-continued publication to make a dent in the professionals.

With all good wishes, I am

Cordially yours

Carleton R. Bahl
Collaborator

P.S. If you address me at Dept. please add "Extension Service".

Another recent fad is trying to distinguish between subspecies, varieties, forms, etc.; and when a man ~~changes~~^{shifts} a plant from one of these categories to the other he claims a new combination, and attaches his name to it. Still more foolish, when a variety is split off from a well-known species, the supposedly typical form is re-named var. typica, or something like that, and the meddler attaches his name to that. More than once I have seen a species re-named var. typica without any indication of its having any varieties. And a colossal egotist could go through a manual and re-name every species (not already thus treated) in that way, on the assumption that varieties that differed a little might some time be found, and attach his own name to every one.

The zoologists, especially the ornithologists, when they described a variety, used to repeat the specific name for the supposed typical form, thus making most birds have trinomial names. I remember many years ago Clute commented on this facetiously, and said that if botanists adopted the practice, in the case of a plant whose name was already a duplicate binomial, and the common name was the same as the generic name, we would get such a monstrosity as this:- "Sassafras Sassafras Sassafras. Sassafras."

Some people who have nothing better to do, especially Fernald, are carrying the splitting of varieties and forms to ridiculous extremes. If he treats all genera as he has been doing those discussed in his Virginia narratives, his long-delayed new Manual ~~will~~ may be as bulky as Small's Southeastern Flora, and very difficult for beginners to use. (You may remember that at Richmond in 1938 he discussed the prospects of completing that book, and I got the impression that it might be out in a year or two. But now I am beginning to doubt if he will live to finish it.)

I don't know just what can be done to keep taxonomy from getting too complicated. I believe you intimated that it might be a good idea to have a committee to pass on proposed innovations. That is done by American ornithologists, but their task is fairly simple, for there are only a few hundred birds in the United States. With our thousands of species of plants, it would be too big a task for any committee; and a new author whose proposed new species were turned down might reasonably accuse such a committee of prejudice. Generally, a man who is making a special study of a particular genus ought to know more about it than any committee, whose interests were in other groups.

I believe Gray, about 60 years ago, did exert pressure to have some of Marcus E. Jones's proposed new species vetoed, and Gray was nearer right than Jones in most cases (as pointed out in a comparatively recent number of the Contr. U. S. Nat. Herb.). But such pressure smacks too much of totalitarianism, and is repugnant to American traditions.

I remember that about 45 years ago I found a Rhynchospora in Southwest Georgia that I couldn't fit into any ~~known~~ known species, and I wanted to describe it as new. But Dr. Britton didn't think much of it, and as he was considerably older than I was, and supposed to be an authority on that genus, I let it drop. But about forty years later Miss Gale came along and found my specimen, and some others that evidently belonged with it, and gave it a name, in Rhodora in 1944.

The best remedy I can think of for the situation you complain of is for editors to be more vigilant. But I don't know how you can make them so, and that would not cure the blunders already made. One of the worst editorial blunders I have seen lately was when ~~August 1934~~ the editor of the American Midland Naturalist let C. H. Müller describe a supposed new oak from Texas. It was nothing but the common and well-known Q. Michauxii, as almost anybody

could see from the description, and as E. J. Palmer pointed out not long afterward.

A common fault of editors is letting their contributors overlook important previous literature. Back in 1915 S. F. Blake discussed in *Rhodora* the identity of some of Walter's species, about which I had reached the same conclusions in the *Torrey Bulletin* in 1906. And when I complained I was told that his studies were based on examination of the specimens, in London, while mine were mere guesses, that deserved no consideration; or something to that effect.

Miss Gale's monograph of Rhynchospora, published in *Rhodora* in 1944, completely overlooked my flora of the Altamaha Grit region, 1906, in which 27 species were listed, with full notes on distribution, and several notes on the same genus in the *Torrey Bulletin*, in the years immediately preceding. Fernald should have known about those, but if so he didn't tell Miss Gale anything about them, and she was working right there alongside of him, I don't know how long. That brings up two questions. What better means could I have devised for making my notes known to future workers than publishing them in the oldest and best known botanical magazine in America? And if old botanical magazines are not supposed to be consulted, why keep them at all?

Some editors who are very critical of my offerings accept ~~all~~ without hesitation all sorts of half-baked stuff by inexperienced persons. One (who also rates as a bibliographer, and should have known better) turned down a paper of mine, partly because I didn't cite all the literature he knew about, and then a few months later published a paper on Georgia mosses by a girl who didn't know that I had listed 28 species, including some not on her list, in my Altamaha Grit flora of 1906. That was published probably before the girl learned to talk, and before the editor knew anything about botany, but a bound copy of it had been standing on a shelf a few years from him for many years, and if he had taken the trouble to look at the Georgia books he would have soon found it.

But enough of this. Now that you have passed the retirement^e age, and the war is over, and plenty of gasoline is available, when are you coming down this way to study Salix some more? This may not be the most comfortable season, but you could at least spot some of the localities, and plan to visit them again in spring, or whatever you think would be the best time.

I haven't done much traveling lately, mostly on account of the war, and the consequent crowding of trains and buses. And since the war ended the hotels are said to be pretty crowded, with people who can't find any other place to live yet. I haven't been up against that much myself, but I hear a good many rumors to that effect. Another reason is that I got married about three years ago, and I don't like to be away from my wife very long at a time, and it is not convenient to take her with me much.

We might, however, manage to take about a month off some time this summer, to go to California, where most of my relatives now live.

I will enclose a few of my recent publications, botanical and otherwise, that may interest you.

Sincerely yours,

R. M. Harper.

(over)

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

December 22, 1946

Professor Roland M. Harper,
University, Alabama.

Dear Professor Harper:

Harking back to your letter of June 28, and my reply of July 6, I wish to thank you for the reprints you sent at that time. I have just had opportunity to read them and have profited.

Separately I send copy of the paper on More Plant Study: Fewer Plant Names, mentioned in my letter as being in press.

Another is in proof, for *Rhodora*, and takes Fernald to task for making two new Sections in *Salix*, comparing one only with the type species of the Section from which segregated and the other with nothing at all, but with a statement that it was so different from all other willows that it needed no comparison.

That last was *S. argyrocarpa* Anderss., so closely related to *S. polluta* Anders. that there is great difficulty in determining specimens with immature foliage.

I compare both his type species with all of the species commonly assigned to the sections from which Fernald segregates them and it makes a very different picture.

With all good Christmas and New Year wishes, I am

Cordially yours,

I am accumulating a large amount of material on tobacco using Carleton R. Ball



CANADA

DEPARTMENT OF AGRICULTURE

DOMINION ENTOMOLOGICAL LABORATORY

P.O. Box 308, COURT HOUSE,

VERNON, B. C.

Dr. Carlton R. Ball,
U.S. Department of Agriculture,
Washington, D.C.

June 25, 1946.

Dear Dr. Ball:

I am writing on the chance that you will get reprints of your letter "Why is taxonomy ill-supported", in the June 14 issue of Science.

If you obtain reprints, will you be so kind as to send me one? Though not a botanist, I am greatly interested in taxonomy and especially nomenclature. Your article was greatly enjoyed.

Sincerely,

Hugh B. Beech
(over)

Prof. Hugh B. Leesh,
Dominion Entomological Laboratory,
P.O. Box 308, Court House, Vernon, B.C.

25

July 8, 1940

Dear Professor Leesh:

Your letter of June 25 is here and I am glad that some of the discussion appealed to you. I much regret that I have no reprints.

sent 11/23
You may be interested in two papers of mine now in press, which enlarge on one phase of the problem. The first will appear in the Merrill Memorial volume from Harvard Univ. in October. Titled "More plant study: Fewer plant names, it describes the normal variations on successively-produced sheets of *Salix* during a season, the gradual changes in individual organs as the season progresses through to the next spring, and the variation in plants from different local habitats.

The second, entitled "Studying *Salix* or Making new Sections", will appear in *Rhodora* next winter. It takes Dr. Fernald to task for naming two new Sections in *Salix*, one not being compared with anything and the other compared only with the type specimen of the section from which removed. I compared all the 6 or 7 species of each section and showed that no new section was necessary at all.

In the first paper, I illustrated by comparing some 1000 specimens of two supposed species through out their range and showing that they were one and the same. More study: less taxonomy (names). There will be reprints of these papers.
Cordially yours

Carlton E. R. Ball
Collaborator

July 18, 1946

Professor Harlan Lewis,
Department of Botany,
Univ. of Calif. at Los Angeles,
Los Angeles, California.

Dear Professor Lewis:

I have noted with interest the abstract of your paper given before the Botanical Society of America at the meetings in March.

I am especially impressed by the statement that: "comparable leaves selected from the same individuals in different years may show a greater range in variation than is present in an entire colony in any given year."

For a long time I have been burdened by the common practice of naming every minor variation noted by taxonomists and recently I have rebelled.

Sent 11/23 (You may be interested in two forth-coming papers. The first, entitled "More plant study: Fewer plant names", will appear in the Merrill Memorial Volume from Harvard in October. It describes the normal variation (1), in the leaves of each Salix twig from base to apex; (2), the variation between the leaves of successively produced twigs in the same season; (3), the ~~change~~ variation between plants of the same species or variety in differing local habitats (alluvium, benchland, sand hills, stony ground, etc., etc.), and (4), the changes which occur in an individual organ through the year.

The second paper, entitled "Studying Salix or Making new Sections", takes Dr. Fernald to task for creating two allegedly new 1-species Sections in Salix, of which two one was not compared with anything and the other only with the type species in the section from which removed. I describe the several species in each of the two recognized sections and show by the characters common to the species in each that no new sections were needed. This paper will appear in Rhodora next winter.

Should you be interested, I will be glad to send reprints when they are available. If your paper is published, I will appreciate a reprint.

Yours for more study of what plants do and are.

Cordially yours,

Carleton R. Bell
Collaborator

June 29, 1946

Dr. ^BHarold H. McKimsey,
Plant Industry Station,
Beltsville, Maryland.

My dear Mac:

On my return from a mid-West trip, I find my letter to Science in print and a considerable fan mail, including your welcome letter of June 17. I note that there was a letter in the issue of June 21, on similar problems.

I cannot be of much help to you on virus nomenclature because I knew little about it. Probably the newness and the difficulty of study and establishment of "type specimens" complicate that problem greatly.

Probably I gave a somewhat misleading idea of my stand on citing authority for names. Really I was protesting the priority right and showed that it was not recognized in other equally important fields of thought. Were it not for the absurd priority rule, there would not be the endless changing of names and therefore citation would be easy and finally almost unnecessary because people would know the authors by heart.

If the names would only stay put, citations would be simple. As I noted, a few-months priority displaces *Salix cordata* Muhl. after 142 years of universal use. No one is benefitted, not even Michaux, and tens of thousands are injured. I firmly believe that science, including taxonomy, is for man, and not man for taxonomy. Therefore, it must serve mankind, or it should perish. Human persistent foolishness may make us all perish yet.

Stabilization of nomenclature will never come from priority rules but from conserving the long-accepted and well-known. That is just what is not being done.

sent
11/23
You may be interest in two papers now in press. One, entitled "More plant study: Fewer plant names," will appear in the Merrill Memorial Volume from Harvard in October. The other, criticizing Dr. Fernald's two new Sections in the genus *Salix*, will appear in *Rhodora* next winter and will show what study he should have made and the making of which would have proved his two sections wholly unneeded.

If we all hammer away long enough at this racket, some good may result. One of my correspondents asserts that the taxonomic field recruits the feeble-minded and the frustrated because there they are assured of recognition through the simple process of attaching their names to alleged novelties. Whether his theory is true or not, there can be no question that the rules stimulate the selfish rather than the helpful in man's nature.

I wish I could live another 50 years to have joy in the scrap, but you have many more years to enjoy and publicize it. Go to it.

With all good wishes, I am

Cordially yours,

Carlsten R. Ball
Collaborator

more PL Study
11/23

copy of
more PL Study
November 11, 1946 ~~sent~~
11/23

Dr. L. P. Metcalf,
State College of Agriculture,
Univ. of No. Car., Raleigh, N.C.

Dear Dr. Metcalf:

I read with much interest your letter to Science (Oct. 4), especially especially the third sentence, on motives. Scientists ought to face facts and not deal in glittering generalities. I might mention also that I am a taxonomist, but I study plants and don't publish a new species every time I see something that looks different. I could easily have published 100 "new" Salices.

Do you believe that the three men who ran the published species of Crataegus from about 25 to over 1000 in ten years had any other "sincere desire" than to get their names on species before the other fellow did? Three of the type specimens are known now to have come from one tree. No one yet has dared to try to untangle the foul mess. And do you believe that the plant pathologist (not a taxonomist) who recently erected (nature did not) 13 (or 15) "new species" of Crataegus from Florida, of which he knew almost nothing, had any sincere desire to create knowledge?

A certain botanist was about to lose his college job, through incompetence, and rushed three specimens of a difficult genus to a specialist in Germany with the suggestion that they were new species. The specialist, who never had worked on American material, immediately named them all as "new" species, one, of course, named for the sender. All three were well known American species. What were the motives of the two men, and what knowledge was made available?

What are the motives of the taxonomists who publish a welter of new species just before the appearance of a new ~~ma~~ private-profit manual, or new edition of an old one, or publish these descriptions in the manual instead of in a journal. Is this for the diffusion of knowledge or the infusion of money into their pockets? How about the authors who put out a "new edition" every two or three or five years, using the old printing plates because of no changes for hundreds of pages but making the buying public think they are getting new knowledge? How about the private manual maker who quietly suppressed a valid species name, 70 years old, and gave the plant a new name of his own, but cited the original type specimen as type? He never mentioned the valid species name in the transaction, altho he had used it in previous manuals.

Some years ago, I named a new species of Alaskan Salix for the collector, Dr. Setchell of the Univ. Calif., and distributed the type material widely while my manuscript was in press. A leading salicologist of Europe saw the specimens, bearing my specific name and "n. sp." on their printed labels. He immediately described it as a new species of his own, rushed the mss. to the Journal of his Acad. sci. and got it published ahead of many other waiting manuscripts, as clear shown by the printed dates of receipt of all the mss. in that issue. He tried to beat me out of my species, adding a lame statement that he had not seen any published description by me. Fortunately, my paper, not rushed, appeared first. What if it had not? What was his motive?

Why did Burbank repeatedly publish lying claims ~~that~~ that he had originated various plants he was offering for sale as new, when it was completely proved that he had bought the seed he was selling direct from farmers or other commercial growers. What were his motives?

Cordially yours,

North Carolina State College of Agriculture and Engineering
of the
University of North Carolina
Raleigh

SCHOOL OF
AGRICULTURE AND FORESTRY
RESEARCH EXTENSION
RESIDENT TEACHING

DEPARTMENT OF
ZOOLOGY AND ENTOMOLOGY

November 27, 1946

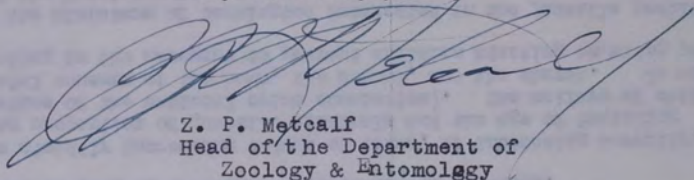
Dr. Carleton R. Ball
U. S. D. A.
Extension Service
Washington 25, D.C.

Dear Dr. Ball:

I have your letter of November 11. I was fully cognizant of the short-comings of some taxonomists when I wrote my reply to your letter. However, you would be the last person who would want to condemn all humanity to the electric chair because of mistakes committed by certain notorious individuals that have crossed the stage of history in the last few years. I could fill many pages with examples such as you cite from any field of biology you would want to name, if I had the time to look them up and you had the patience to read them after I had done so.

It is of course difficult to be sure we understand the motives of the men who made such mistakes, but the important thing now is that they be corrected.

Very sincerely yours,



Z. P. Metcalf
Head of the Department of
Zoology & Entomology

ZPM:ehc

(over)

Dr. Z.F. Metcalf,
Dept. Zoology & Entomology,
N.C. State Agricultural College,
Raleigh, North Carolina.

3614 Jecelyn St., N.W.
Washington 15, D.C.

December 4, 1946

Dear Dr. Metcalf:

Your letter of November 27 is here. I have learned long since that when a man regards all wrong-doing as merely "mistakes", and refuses to help correct it, but defends it instead, there is no gain in further discussion. Condemnation of evil is the first step in prevention, because it then is no longer respectable to do evil.

You are aware of the steadily increasing volume of crime, of increasing severity, with a steadily increasing proportion of juvenile criminals and the age of beginning steadily lowering (statistics of the national crime commission). The welfare of children ought to be the special concern of the home, the school, and the church. Is any one of these agencies leading in the movement to prevent children starting on wrong paths?

When we tried to get the ministers of Washington interested in the juvenile problem they went out and visited the Receiving Home for Juvenile Delinquents and the Reform Schools and came back and preached beautiful sermons. On what? Prevention? No., on the need for bigger Receiving Homes and Reform Schools and more officials to manage the increasing juvenile population.

A nation-wide conference on juvenile delinquency and crime was held recently in this, the capital city, of our nation. Who called it? The agencies named above, who should be concerned with child welfare? Not one of them, but the national law-enforcement agencies, who are compelled to deal with them after they become criminals.

History, ancient to modern, shows us that there have some 8 or 9 successive "civilizations" developed, from the Sumerian to the present "Occidental" or "Technological". All previous have emerged, wrought, increased, grown soft with comfort and wealth, lost their moral fiber, declined, and been swept out of existence. The history is monotonously similar. Read Gibbon's Decline and Fall of the Roman Empire, for a full account of the steps and the reasons. Then plot the curve of Europe and America and make mark where you think they are in the parabola. It is illuminating.

The first symptom of the ~~same~~ decline in moral fiber was the unwillingness to call wrong-doing wrong, or to attempt to curb or punish it. In other words, society (whether botanists or all of us) was unwilling to protect itself against mounting wrongdoing, and so it presently perished. We are traveling that road, with means of transport greatly speeded up. It must be a great satisfaction to know that one has helped speed it.

Cordially yours,

Carlton R. Ball

EDWIN LINCOLN MOSELEY L. H. D.
PROFESSOR EMERITUS OF BIOLOGY
CURATOR OF UNIVERSITY MUSEUM
BOWLING GREEN, OHIO

June 17, 1946

Carleton R. Ball,
U.S. Dept. Agr., Washington D.C.
Dear Sir:

I am in agreement
with your views about
nomenclature expressed
in Science June 14, p. 714.

Yours truly
Edwin L. Moseley.

West Virginia University

THE COLLEGE OF AGRICULTURE, FORESTRY,
AND HOME ECONOMICS

THE AGRICULTURAL EXPERIMENT STATION

MORGANTOWN

June 16, 1946

DEPARTMENT OF ENTOMOLOGY

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

Dear Dr. Ball:

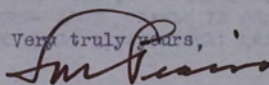
I have just read your article on Taxonomy in the current issue of Science. and am almost peevish about it because it appeals to me as a case of near-plagiarism. You have published my ideas, long held, but have done it in so much better fashion than I should have been able to do that I envy you the ability.

Seriously, I believe that many entomologists will support your ideas whole-heartedly. I am constantly annoyed by the absolute folly of some of the rules as they are applied. I think the system, as it is used in Botany, is even worse than it is in the zoological field; we do not usually cite more than one author. But what it amounts to is this; we have made ourselves slaves to taxonomy when we should, by any logical attitude, make it our servant.

If you can organize a revolt I shall be glad to join in and help to the best of my ability to get things established on a more rational basis. And if you should want to use a little of the editorial space in the Journal of Economic Entomology I assure you that you will be welcome to it.

Thank you for saying what I should like to have said and saying it so well.

Very truly yours,



L. M. Peairs, Editor,
Journal of Economic Entomology

Prof. L. M. Peairs,
Editor, Journal of Economic Entomology,
Dept. of Entomology, Coll. of Agriculture,
Univ. of West Va., Morgantown, W. Va.

July 8, 1946

Dear Professor Peairs:

Your very readable letter of June 16 came while I was away at my 50th anniversary of graduation from Iowa State College and also at the 8-day biennial Council of our Congl. Christian churches, hence delay in reply. I have been both surprised and gratified at the fan mail from both U.S. and Canada. The opposition should open up soon, altho I have an idea the professional taxonomic botanists do not read Science.

I had no real intention organizing a revolt, as you put it, but would be happy if one got organized. Having decided to stir up the animals, I wished to suggest and illustrate a better way, and you may be interested in two papers now in press.

*Sent
H/73*
The first, entitled "More Plant Study: Fewer Plant Names", will appear in the Merrill Memorial volume from Harvard in October. In it I describe the normal variation shown by successively-produced willow twigs in one season, the progressive changes in an individual organ through one year, and the great variation in plants of a single form greatly differing local habits in one place (as rich alluvium, second bench, stony hillside, or sand hill). Then I record a study of some 1000 specimens of two supposed species from throughout their Mr ranges and show that they are one and the same species, the two being based on the extremes of local habitat.

The second paper, entitled "Studying Salix or Making New Sections" will appear in Rhodora in next winter. It takes Dr. Fernald to task for naming two new sections in the willow genus, Salix. One was not compared with anything, the other only with the type species of the section from which removed. I describe the characters of all the species (6 or 7) in each of the two old sections involved, and show clearly that there was no need for two new 1-species sections.

My hope is to focus the minds of our younger taxonomists on the resemblances, to make them realize normal variations which are not hereditary, and to make them realize that creating new species and varieties on seasonal or habitat variations is not scientific. Is a bald-headed man a new variety?

Now as to your kind offer to give me space in the Journal of Economic Entomology. I feel that you ought to use that space yourself. Your readers should pay much more attention to their editor and brother entomologist than to an irruption from the plant field. And you have confessed to a yen to write such a paper. Go to it, and power to your elbow.

Cordially yours,

P.S. Reprints will be available
from the two papers mentioned.

Carleton R. Ball
Collaborator.

West Virginia University
COLLEGE OF ARTS AND SCIENCES
MORGANTOWN

DEPARTMENT OF BOTANY AND ZOOLOGY

July 16, 1946

Dr. Carleton R. Ball, Collaborator
U. S. Department of Agriculture
Extension Service
Washington 25, D. C.

Dear Dr. Ball:

I was "up to my ears" in a study of a collection of Panic grasses when your recent article in Science came to stimulate an "iconoclastic urge." As I read this splendid note, I found myself saying almost audibly, "Amen and Amen," with increasing emphasis on each repetition.

Earlier in the year papers by Dr. Harlow and Dr. Core called attention to the need for a more genuinely scientific attitude in the practice of taxonomists, and I was delighted to see this expression of what is apparently a growing interest in the problem. Recently, Weatherby published a paper (Rhodora) that attempted to rally the cohorts around the old taxonomic standards, but I think the necessity for a more reasonable attitude has made itself felt to a degree, that it can not long be ignored. Your clear, convincing challenge has received general, enthusiastic response here on our campus, and I hope the same will be true in every other quarter.

To-day, I happened to be talking with Dr. Peairs, who showed me your letter written in reply to his word of commendation. I noted that you have in press, two additional articles and I should like to enter a request for reprints,--we shall see the one in Rhodora when the proper number arrives, but the other we may not have. We are profoundly interested in this subject and I hope the reform (or revolt, as Peairs calls it) will become such a strong movement that something will be done to improve an increasingly "messy" situation. My neurons are not yet so completely ossified as to make the learning of new names an impossibility, but God knows the Scientist to-day has enough new terms to assimilate, without the inclusion of those that are unnecessary and wholly useless, to say nothing of those that merely add to our confusion. May the good Lord grant irresistible power to your pen.

Sincerely,

P. D. Strausbaugh
P. D. Strausbaugh
Head of the Department

July 25, 1946

Prof. P. D. Stranbaugh,
Dept. Botany & Zoology,
West Virginia University,
Morgantown West Va.

Dear Professor Stranbaugh:

My letter of July 16 came promptly, and I am greatly interested in your discussion. To tell the truth, I was not trying to lead a "revolt" when I wrote that letter. I was merely seizing a present opportunity but the responses lead me to believe that a real "revolt" might have some chance of getting results.

It would have to be much more than a paper revolt, however. There must be action by interested and affected groups, such as members of a college Dept. of botany, local botanical and biological societies, regional societies, and national societies. I hope the botanists (or zoologists or both) will become

I believe that I now will sit down and try to draft the equivalent of some "Whereas" and "resolved" and see if interested organizations will try to get their own revised versions adopted by their own groups.

Why not start the tidal wave on the W.Va. campus among the botanists and zoologists?

Cordially yours

P.S. Will send reprints if and when.

Carleton R. Ball

copies more Plant Study
Jo. Stranbaugh } 11/23
E. T. Cole }

Digitized by the Institute for Botanical Documentation

AMERICAN CYANAMID COMPANY
1937 West Main Street
Stamford, Conn.

Dear Sir:

We shall appreciate it very much if you will send
us a reprint of your interesting article on _____

Why is taxonomy ill-supported? _____

which appeared in Science 103, 713-14 (14 June 1946)

Thanking you,

M-816

Very truly yours,

Walter C. Tobie

Mr. Walter C. Tobie
American Cyanamid Co.,
1937 W. Main St., Stamford Conn.

3414 Jodelyn St., N.W. Washington 15, D.C.
June 29, 1946

Dear Mr. Tobie:

Regret that I have no reprints of the taxonomy letter
to Science.

A paper, entitled More plant study; Fewer plant names,
will appear in the Merrill Memorial volume from Harvard
in October. It discusses the types of plant variation and
illustrates what happens when variations are studied in-
stead of being named as novelties.

A second paper, entitled "Studying plants or making new
Sections in Salix" will appear in Rhodora next winter. It
severely criticizes Dr. Fernald's two "new" Sections and
illustrates the way in which the material should have been
studied, with no new sections needed.

Reprints of these will be available.

Cordially yours, Carleton R. Ball

sent 11/73

American Association for the Advancement of Science
Massachusetts and Nebraska Aves., N. W., Washington 16, D. C.

Office of the Editor
Science
A Weekly Journal of the Association

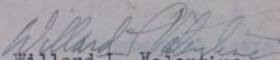
12 April 1946

Dear Dr. Ball:

Your paper, "Why is Taxonomy Ill-Supported?" in reference to observations by Fosberg and Diehl (Science, 1946, 103, 282) will be published as a Letter to the Editor.

I cannot give you a definite date of publication at this time.

Sincerely yours,


Willard L. Valentine
Editor

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

WLV:a

Published
June 14, 1946

Dr. Willard L. Valentine,
Editor of Science,
Mass. & N.E. Avenues, N.W.
Washington 16, D.C.

3814 Jesselyn St., N.W.,
Washington 15, D.C.
July 12, 1946

Dear Dr. Valentine:

In further reference to your letter of 12 April, accepting my letter on "Why is Taxonomy Ill-Supported?", you may be interested to know that I have had surprising fan mail from several parts of the U.S. and Canada.

These writers deplore the unnecessary, expensive, and vexatious burden now borne by the innocent users of the system created by the professionals, and believe that something can and should be done about it.

Perhaps surprising also, I have not heard from the professionals yet. Maybe they'd not read Science. Maybe, like the farmer's horse, they just don't care a daisy.

One editor has offered me space in a professional journal for a follow-up.

Cordially yours,

Sent More Pl. Study

Carleton R. Ball (retired)
Collaborator

Digitized by *H2* Institute for Botanical Documentation

Strawtown Rd.
West Nyack, N.Y.

June 25-

Dear Dr. Ball:

I want to congratulate you on your letter to Science on "Why is Taxonomy ill-supported?" Those of us who are required to use taxonomy for purposes other than making more taxonomy, have had the same experience as you, and we all owe you a debt of gratitude for your fearless and outspoken attack on the system.

I suppose that to go farther than you did might have dulled your thrust; as it is it has plenty of edge. But there is much more that might be said. The taxonomy system has drawn into itself principally mentally weak and pure misfits

from other branches of botany
and even other sciences, so that
Taxonomy is manned by starry-
eyed incompetents who believe
that they live in a rarified atmos-
phere and need take no notice of
those like myself who must use
taxonomy and want to use it
properly, but do not want to be-
come taxonomists.

The claim is that taxonomy
is the expression of phylogeny. Ap-
plying the principles of pollen
morphology and allergenic inter-
actions, which both tell very
much the same story, one soon
finds that the very framework
of the whole structure of taxonomy

is put together with a large pro-
portion of its supporting units in
their wrong places. I have in
mind such arrangements as the
separation into families of the Cheno-
podaceae and Amaranthaceae and
their reunion under the Chenopodiales
coordinately with Portulacaceae, Caryo-
phyllaceae, Nyctaginaceae and others.
And then are the Pinaceae and
Taxaceae with their boundaries cut-
ting across rather well organized and
integrated tribes just as ruthlessly
as the surveyor's line cuts across
mountains and valleys and streams.
Such lines as these merely mark off
the taxonomists' playgrounds ^{which} and
in turn express the personalities
of those who have played in them.
They do not express phylogeny.

Sincerely yours

Roger P. Wodehouse.

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

Dr. Roger P. Wedehouse,
Strawtown Road,
West Nyack, N. Y.

June 29, 1946

Dear Dr. Wedehouse :

I am just returned from a mid-west trip and find my letter to Science in print and a nice collection of fan mail, including your letter of June 25. All response so far has been approving, although one usually gets the opposition first, and now! I suspect that not all taxonomists read Science.

It was not possible for me to go further in a letter to the Editor, but I could have done a better job if I had revised it again. My most recent quarrel with the professional taxonomists is over the present tendency to name every slight variation. Probably this results from lack of ability to travel and collect in far places and the paucity of real novelties at home. The Germans got that way before the war, and began naming smaller and smaller variations as forms, subforms, sub-subforms, and even sub-sub-subforms. Certain Swedes went over to the theory that all plants are hybrids and that it was possible to distinguish up to quadruple hybrids from dried specimens. Any geneticist would have howled if he had had to grapple with their beliefs.

Sent 4/23 (You may be interested in two papers in press. One, entitled "More plant study: Fewer plant names", will appear in the Merrill Memorial volume from Harvard in October. It describes the normal variations in successive willow shoots through the season and the variation in individual organs as the season advances; also those caused by variations in local environment. Then it takes up two species and shows that they are one species if one looks at the means instead of the extremes.

The second paper is entitled "Studying willows or Making new Sections in Salix." It will appear in Rhodora in midwinter or later. It takes exception to Dr. Fernald's two recent new Sections in Salix, without description or comparison with all the species involved. It discusses the characters of all the related species and shows that neither section was needed.

I did not wish to loose ablaze like that in Science until I had made studies which would clinch the point of over production of names because of insufficient study, whatever the cause. Your strictures on the taxonomic mind go further than I had yet considered the matter. Maybe some do become taxonomists through fear or frustration in other fields and seek one where they are bound to be noticed and their names in continual prominence.

Why do you not write to Science also, or to some other journal, and bring in the addition viewpoint. You probably would be shot, but the blood of martyrs, they say, is the seed of future improvement.

I remember with pleasure our correspondence regarding willows, some years ago. Congratulations on having amassed the information in a comprehensive volume. I am working on two vols. but not with as much speed as desired.

Cordially yours,

Carleton R. Ball

Beltsville 6/17/46.

Dear Dr. Ball:

Beltsville 6/17/46

I am much interested in the point of view expressed in your Science note on Taxonomic Nomenclature. I agree with your thesis, and I know that many others do. I have discussed this in our Virus Nomenclature Committee, and James Huisson and I are the only ones who favor the general proposition of doing away with the citation of authorities—at least ultimately.

One problem presents itself—What procedure should be set up so that names which are already in a turmoil can be straightened out, and the reader will know which entity the writer is dealing with.

We thought that a committee could handle such matters, but we found such divergence of opinion on most of the issues that have come up, that it appeared almost hopeless to work out most of the problems presented. This is the reason for my going on record with my "Genera of the Plant Viruses" J. Wash. Acad. Sci. 34: 139, 1944. I am using the usual method for

citing authorities for names, avoiding as much as possible the changing of names, even though in many cases the rules will support such changes. I hope as time goes on we may dispense with authority citations, but if this comes, there will be many cases where a literature citation number should follow a binomial in order to keep the record straight.

I shall appreciate any ideas you may have with regard to working out the problem of nomenclature and dispensing with the citation of authorities.

Sincerely

H. H. McKinney