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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.

DR. SETCHELL AND ALASKA WILLOWS

An algologist went willow-wise. In 1931 and 1932, while the writer was with the University of California, Dr. Wm. A. Setchell, then head of the Department of Botany and 67, took summer trips to Alaska, with Mrs. Setchell. He decided to collect willows exclusively. What willows were known from Alaska? He looked at herbarium specimens, - made mental photographs. What other willows might occur in Alaska? He looked at more herbarium specimens. Said of one (Salix pseudomonticola Ball), "It looks like a service-berry" (Amalanchier). It does.

In two summers, Dr. and Mrs. Setchell brought back 619 numbers of willows from Alaska and the Yukon. These contained 21 species out of some 24 then known to occur in Alaska, as well as many of their varieties. That was remarkable in itself, when we remember that Dr. Setchell travelled only three major north-south routes and the Yukon River. Of the three species he did not get, S. ovalifolia does not occur in the area he covered and S. polaris and S. stolonifera are rare and of local occurrence.

More remarkable still was the fact that he brought back abundant collections of five species not previously known from Alaska. These were S. anglorum Chamisso, S. Barrattiana Hooker (by var. angustifolia And., S. albertana Rowlee), S. lasiandra Bentham (by var. lancifolia (And.) Bebb), S. pseudomonticola Ball, and S. Setchelliana Ball. The latter, from the snout of the Muldrow Glacier, in Mt. McKinley National Park, was new to science and has been found at only two other localities.

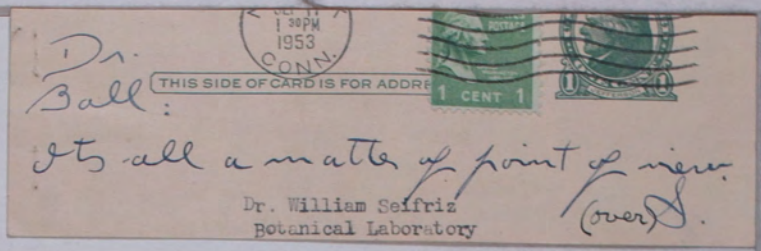
His collections added two other varieties not previously known from Alaska and provided material of what later were described as three new varieties, all previously collected by others. He also greatly extended the known ranges of several species and varieties.

How did he do it? Three answers. 1. By giving attention only to willows. 2. By keen memory and observation. He brought in 6 Alaskan sets of the willow which "looked like a service-berry." Said he, "Whenever I saw a willow which looked like that, I just collected it." 3. By gathering all the different forms observed at each stop, instead of deciding that they were the same as those collected at previous stops. 4. By making friends with every one along the routes.

What method No. 4 meant. The Alaska and Yukon Railroad furnished him a gasoline speeder and "chaffeur" so that he could stop at any willow patch he saw. The steamboat captains told him when they were to stop to load firewood for the boilers, at out-of-the-way places, and whistled him when ready to leave. They let him dry his specimens in the boiler rooms. Auto drivers halted for a quick grab from roadside willow thickets.

Still more surprises! Out in the Missouri Valley, Salix missouriensis is called the "diamond willow" because occasionally it produces diamond-shaped scars or depressions on the stems. These are caused by the atrophy of the lowermost trunk twigs in thickets and the failure of the wound to heal over. If all or most of the lower twigs die, the diamond pattern is that of the phyllotaxy of the species. Such specimens are highly prized for the making of ornamental canes. The closely related S. lutea of the Rockies also produces such diamonds. No other species were know to exhibit this phenomenon. At Gulkana Roadhouse, in Alaska, Dr. Setchell observed beautifully bicolored log banisters displaying diamond markings. On inquiry, he learned that they were willow poles brought by Indians from the swamps. He placed an order. On his return, he found that the Indians had obtained for him a small log, now in the University botanical museum, and the makings of a beautiful walking stick, now a highly cherished posession of the writer. The material was from Salix alaxensis, a tree willow not previously known to produce "diamonds".

Botanists, biologists, agronomists, foresters, and geologists, and explorers have been collecting plants in Alaska and Yukon Territories for more than 40 years. Yet one man, in two summers, added 5 species and 3 varieties of a single genus to the known flora. Exceptional results because of an exceptional man? Yes, and that gives us something to think about. Should a democracy cherish quantity, or quality? When quintuplets or quadruplets are born, everything which money and science can accomplish is done for them, without regard to their heredity or promise of achievement. Who is seeking to discover and subsidize the superior child singly born?



My Greetings.

You deserve more than a note of praise for your words on Walter T Swingle in the current SCIENCE. There are few like him; and few who can write such an evaluation - in days when an enlargement of science, and thus a democratic voting majority in science, is constructed of somatic tissue, not creative germ tissue.

Best wishes,

H—n.

Swingle

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

Professor William Seifriz,
Botanical Laboratory,
University of Pennsylvania,
Philadelphia, Penn.

15 September, 1953

Dear Professor Seifriz:

I have just read your comments on the life and achievements of Dr. Swingle in Science for Sept. 11. They are very informative, as I know from many years of service in the Bureau of Plant Industry, during which the work in my charge was carried on in cooperation with nearly all of the States and with several Provinces in Canada also.

From your own statement, it is evident that Dr. Swingle was to you an idol as well as an ideal from your early childhood, and that fact naturally has colored your feelings and your statements. The fact that he brought you into the Department at the age of 17 would indicate also that you were not a college graduate at the time and therefore not qualified to render real scientific service.

I think also that your statements and inuendoes concerning the character of the Dept. of Agriculture, and your criticisms of those who "checked" on Swingle'S activities and results are wholly unwarranted. No agency is perfect, but to state baldly that the Department is far inferior in quality now requires the statement of facts to support the charge.

Just what is the mathematical basis for your belief that Dr. Swingle embodied more of the spirit of creating international goodwill, than any other 24 persons? How much do you know of the world-wide program of technical assistance to other countries, under the Point-4 program and many other cooperative enterprises?

Finally, I wish to know just what you mean by the phrase: "For he was the Department of Agriculture, and those who kept him in check are today unknown" That is a plain lie, and I ask you to state the fact on which you base the assertion. The funds spent by Federal and most State agencies are appropriated for specific purposes by the Congress and the Legislatures. It is the duty of administrative officers to see that the funds alloted to any given Division or project are spent in furthering the objects of investigations underway. Do you believe that each individual employed by any agency should spend the funds allotted for study and research and experimentation, on his own person pleasure? Is that done in the University of Pennsylvania?

Several years ago, six scientists, including Dr. Swingle and some of his close friends, were found to be spending government funds in quantities far greater than the projects conducted, or the results obtained, would justify or require. A full investigation was made. As a result, the six men were relieved of the administration of those projects and other men placed in charge. Because of long and previously useful service, criminal charges were not prefer red, and they were allowed to retire. But a lot of the money had been spent for personal pleasures, at great loss to the tax-payers. And you sharply criticize the men whose duty it was to "check" on him.

Cordially yours,

Carleton R. Ball

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

Dr. William Seifriz,
Botanical Laboratory,
University of Pennsylvania,
Philadelphia 4, Penna.

22 September, 1953.

Dear Dr. Seifriz:

You are so right: "IT's all a matter of point of view."

You could have written a deserved tribute to Dr. Swingle, and published it without deriding the Department of Agriculture, in which he served a lifetime, or defaming the men who "checked" on him.

Swingle, and the five others, more or less closely associated with him, were removed from their scientific and administrative assignments after careful investigation by competent men. They were charged, and the charges proved, that they had wasted large sums of government funds in buildings, equipment, and land, for alleged projects which were not needed and were unproductive, but gave them a chance to spend joyous winters in southern California at Federal expense.

In addition, Dr. Swingle was charged, and the charged admitted by him, that he sold government property and pocketed the money received therefrom. That is stealing.

You were under no requirement to mention these facts in your tribute, even if you knew them. But in all decency you could have refrained from defaming the Department, and deriding the Bureau officers who proceeded against a criminal as they were required to do, by the laws of the United States, the duties imposed on administrative officers, and the honor of honest citizens. According to your own statement, and your published biographical sketches, you were willing to accept salary from the Department for several years before you had any professional training except what that Department gave you. Is that one of your reasons for deriding the efficiency of the Department? It certainly is all in the point of view.

Criminals of all classes daily take the lives and property of honest citizens. They assume that it is their right and violently resist arrest and imprisonment. Law, honesty, and decency say that it is wrong and that they should be punished. Its all a matter of point of view.

Communists, spies, and other traiters labor effectively to betray their countries to evil powers. They assume that they have a right to do that. Law, public opinion, and honesty say that it is wrong and that they should be punished. It's all a matter of the point of view. How right you are.

Cordially yours,

Carleton R. Ball

#56 Box 10
Folder 18

Ball, C. R.
Profession standards in taxonomic botany.
15 mss. p. Unpublished. 1942?

Written in 1942 as indicated by dates on reverse side some sheets WAA

PROFESSION STANDARDS IN TAXONOMIC BOTANY *

By Carleton R. Ball

Taxonomic botany has a stupendous problem. It is the burden of ever-increasing synonymy. The handling of synonymy (research and reference) takes more time, energy, and money than can thereafter be devoted to taxonomy itself. Synonymy has become a veritable OLd-Man-of-the Sea, destructively encircling the neck of the unfortunate taxonomic Sindbad.

Synonyms are the husks and chaff of the botanical crops of yesteryear. What would we say of a farmer who poured his new harvest of corn and wheat and beans into the cribs and bins filled with molding husks and chaff and pods of all previous harvests? What respect would we have for an office where desk and table were piled high with discarded letters, reports, and manuscripts, through which the worker groped endlessly for a recent and needed document? What would war-minded America say of a plant where the assembly line was smothered in broken and outworn parts and trimmings from used materials? XXXXXXXXX

These conditions would not be tolerated for a day by an executive not ripe for interment. Yet that is the condition of taxonomic botany today. Why tolerate such conditions in this world? What lessons may be learned from the age-long trends in the history of human relations?

The outstanding development of the centuries is the concept that the right of the individual always must be transcended by the greater right of the group. Liberty must not be allowed to become license.

With the increasing complexity of life, the need and the demand for standards of conduct, and commodity, and cost, have become more evident and more insistent. Two obvious reasons account for this fact. First, it became recognized that the selfish actions of one, or of a few, may add great burdens to the lives of many. Second, it became recognized that such burdens reduce the welfare of society as a whole.

* Reading time minutes. Thinking time years.

These two recognitions stem from the lnowledfe that in every group are to be found the lazy, the careless, the mentally feficient, the self-seeking, and the mentally dishonest. Cut of these recognitions has come legislation and regulation providinding standards/of commodity and conduct, for the protection and controls of the society as a whole. The becessity for protection ~~of~~ against the harmful classes named must be stressed/again~~,and again~~. and stressed Histiry has recorded repeatedly that any group or society that can not, or will not, protect itself from injurious wlwmwnts eventually will perish.

Control pf crime always has been attempted. It is the interprestation of what constitutes crime which has changed and broadened. Whatever is harful to ~~xxx~~ the mass of people is wrong, and must be stopped. Whatever profits the one, or r_e few, but harms the group is wrong and must be curbed. In our day we have seen these interpretations steadily wxtended to cover a wide rane of commercial practices and professional activities. This has not been accomplished without resentment and rebellion. Americans are an undisciplined people. War-time restrictions have proved that. T ay want to do what they ~~xxxxx~~ want whent they want to. And they are not too particular about the effect of this on others.

IT is in the commercial field that the average botanist ls most familiar with standards and controls. Perhaps the Food and Drug Act (the so-called pure food law), is the best-known wxample. Makers of patent medicines and other drug and food products were obliged to omit dangerous or harmful materials from their products. Not only that, but no longer could they create demand for even their harmless commodities by false and misleading advertisements claiming values and effects which did not exist. At about the same time, social consciousness awoke to the danger and damage caused by too long hours of labor and especially where insanitary and unsafe condistions prevailed. Again legislation and regulation were employed to create standards of conduct and environment for the protection of society.

More recently, and continuing in our day, is the curbing of gigantic combinations of capotal called corporations and trusts. The first point of attack was their illegal methods of eliminating competitors and thus creating monopolies. With such methods outlawed, competitors thereafter were bought out and the resulting monopolies charged what the "traffic would bear". Next came the corporations dealing in natural resources with an essential distribution monopoly to the citizen, such as water, gas, and electricity, as well as the telephone companies. Their system of exploitation of the masses reached final druition in the wlwctric-power systems, where successive layers of holding xompany sat like bloodsuckers pumping the financial assets from the layer just beneath, at great profit to the "systems" and great financial burden to the consuming public.

All along through the heyday of the corporations and systems corruption of the courts and control of legislatures and regulatory bodies through bribery and intimidation was common. These practices are exceedingly difficult to prove or to restrain, as recent revelations have shown. Another favorite practice of the predatory commercial and industrial giants was the attempt to achieve popular immortality through collosal endowments of some institution for the "people". Some of the moneys for such endowments were obtained through exploiting employees and others through grabbing natural resources rightfully belonging to all the people. Both prevailed because of lack of standards and this because of lack of social concern.

Botanists may observe, on every hand, the varius degrees of standardization and control applied by society to human performers and performance. These standards and controls are designed to protect the public, rather than the performer, against the danger and damage to person and property inherent in faulty workmanship.

Certain classes of skilled or semi-skilled workers, not college trained, must pass examinations and obtain licenses before they can operate in most States and municipalities. These workers include motor-vehicle operators, beauticians, cafe operators, electricians, morticians, rural teachers, nurses, plumbers, stationary engineers, and many others, whose faulty workmanship would endanger life and property. In spite of such standards, faulty performance and even malpractice does occur but one can well understand how much worse these would be without such standards and controls.

Botanists are familiar with many series of professional standards, and with the means employed to determine and enforce them. The granting of all academic degrees is dependent on successive tests of fitness, from matriculation to graduation, continuing After graduation, members of many groups cannot practice their professions in most States until they have passed a State examination and received a State license. This holds for such groups as certified public accountants, chiropractors, dentists, lawyeres, pharmacists, physicians, teachers, veterinarians, etc.

Not only must the members of these professions qualify before being allowed to practice, but they may be disqualified for unethical conduct at any time thereafter. Such disqualification may be ordered by the State Examining Board, or by the professional organization affected, or by both. In either case, the individual is disqualified democratically for further practice by a jury of his peers. Men composing such boards or committees, and organization memberships in general, have not been afreaid to exercise these restraints when necessary to protect society in general or organization reputation in particular.

These safeguarding procedures do not eliminate all wrong-doers. There can be no question but that they do cover the flagrant cases. More important still, the possibility of such action does serve to deter most of the venal and those withonly haze ethical standards.

1. No publication of a new species shall be valid if the description is based on a single specimen or collection.

1 (substitute). No publication of a new species shall be valid if the description is based on a single specimen or collection, unless the plant differs from its ne rest relative by at least 3 characters of reasonably absolute values rather than merely relative values.

Nature specializes in variation. Man imagines uniformity, but it does not exist. The most extreme variations are the ones that attract attention and suggest "new species". Study of a series of collections determines reveals the constancy or diversity of the variation, as well as its average magnitude. If such study is made only after publication, it frequently reduces the supposed species to varietal or formal rank or to complete synonymy. ~~The~~ No harm is done to science if publication is delayed until sufficient material is available.

Every taxonomist is acquainted with "species" described from a single collection exhibiting a shortening or a lengthening, or a broadening or a narrowing of a vegetative or reproductive organ, or on an unusual hairiness or its absence. Study of series of collections would have prevented the publication of "species" by reputable botanists, although it might have permitted the designation of varieties or forms. Unfortunately, in the latter case, the botanical jackals who prowl around the campsites of their betters not infrequently raise such entities to specific rank, especially when about to publish a manual. Taxonomy has a right to protection from both crimes.

The writer has published 12 species and 32 varieties in Salix, over a period of some ~~30~~ 40 years. A check has just been made to see how well he has practiced what he here preaches.

The descriptions of the 12 species (including 2 raised from varietal rank) were based on a total of 333 specimens, or an average of almost 28 specimens per species. One species was based on a single specimen, another on 2 specimens, and a third on four specimens. The other 9 varied between 8 and 70 specimens each, with 7 having more than 30 each. The species described in 1905 from a single specimen has proved to be tenable. Unfortunately, its relationships were misinterpreted at the time of publication and additional existing material was overlooked. The species has been redescribed since and abundant material cited.

The States, Territories, and/or Provinces from which the specimens had come varied from 1 to 8 per species, with an average of 2.8 each. Two of these 12 species unintentionally were first published in a manual of the plants of a National Park, with no specimens cited, but their descriptions were based on 45 and 70 specimens respectively. Fuller descriptions were published later with photographs and citations of even more specimens from a still larger area and a wider geographic distribution.

The writer also has published 24 new varieties and made 8 new varietal combinations in Salix, or a total of 32 varieties. These 32 ~~descriptions and transfers~~ were based on 729 specimens, or an average of 22.8 specimens per variety. Of the new varieties, one was based on 2 specimens from 2 different States, and two on 3 specimens each, from 2 different States in both cases. One variety reduced from specific rank also was based on 2 specimens from one State. The other 28 varieties and combinations were founded on from 6 to 120 specimens each, derived from 1 to 10 States. The total distribution covered 94 States, Territories, and Provinces, or an average of almost 3 such areas per variety, although 9 were known ~~Of the 9 then known~~ from only one State or its equivalent, ~~three were from Alaska~~ the large territory of.

Of the 12 new species, 3 had been published previously as varieties, two by the writer and one by Bebb. One of the new species needs reduction to varietal rank. Of the 8 new varietal combinations, four were reductions of species previously published by others, three were transfers from one species to another, and one was the elevation of a faorm. Of the 12 species, 9 have been illustrated by photographs or nearly full-size drawings. Of the varieties, 3 have had photographic reproduction.

2.

No publication of a spevies or variety shall be valid if based on a type specimen or specimens of only juvenile development of the organ or aorgans on which differentiation is established.

Examples of the publication of species based on extremely young material doubtless will come to the minds of all taxonomists and especially those who work with woody plants, where specimens necessarily are but fragments of the whole. Twice at least this was done ~~xxxR~~ in Salix by Rydberg.

In 1901, he publsihed a Salix stenophylla based on type specimens with flowers and very young leaves. He separated it from S. exigual by 3 alleged differences. Of the two floral characters, one is common to all willows in the juvenile condition and the other is common to S. exigua. The vegetative character was narrow leaves, a characteristic of S. exigua, and the types have leaves altogether too young to permit even a guess as to their dimensions at maturity. Not only S. exigua but some of its varieties and form occupy the type area and probably the identity of the plant never can de determined. But the name must be dragged along forever.

In 1912, Rydberg desxribed a Salix Sandbergii from Idaho, basing it on a single extremely immature specimen. For more than 20 years it lay yndeterminable. In 1932, Ball, Parks, and Tracy collected a peculiar willow in California and in 1933 Tracy collected extremely young material from the same plants. The latter awakened memories and ~~xxxxxx~~ its comparison with Rydberg's type proved the two to be identical. The mature plant clearly was an unusually hairy form of S. lasiolepis and yet Rydberg had stated positively that his plant was not related to that species. No one could have been sure of its relationships, because of its juvenile condition, unless he had young and older states from the same plant. Is it scientific to publish undeterminable material and vex taxonomists forever after.

3. No publication of a species or variety shall be be valid unless the describer compares his plant with its nearest acknowledged relatives rather than only with those so distantly related that the new entity ~~a~~smittedly is abundantly distinct from them.

This greatly needed standard would have invalidated the publication of the first species of Salix published by the writer, in 1905, because of his erroneous interpretation of its relationships. When the error became apparent, he could have republished the species, and , as a matter fact, that was done. Adoption of this standard would protect taxonomy from two injurious classes of species makers. The first is the careless and uninformed who rush into print after guessing rather than studying. The other is the smaller number who make misleading comparisons intentionally and persistently. This latter groups is likely to include some of the developers of botanic manuals for private sale.

The method of comparison just described helps to insure initial acceptance of the species and also to avoid or postpone exposure of the invalid species until after profits have resulted.

One American taxonomist who followed this practice quite regularly, at least in his publications in Salix, used still another step in the process when inserting such "species" in his manuals. In both keys and text he removes them far from the species most closely related , sometimes to the extent of removing them completely from the section to which he has stated that they belong. Such treatment was too common and too persistent to be accidental and this particular worker was as well acquainted with the near relatives not mentioned as with the more distant relatives with which he compared his so-called species. The standard proposed would have acted as a strong deterrent of such practices.

4. Publication in Manuals. No publication of a species or variety in a manual of botany shall be valid unless the description is fuller than is usual in manuals, specimens are cited, relationships are discussed, and synonyms, if any, are given.

The effect of this rule would be three-fold. It would give taxonomists usable description and discussion at the place of publication, enabling them better to understand and evaluate the new entity. It would permit comparison of the description with specimens. It would tend to discourage the "species-for-profit" game. There is no valid reason for the publication of novelties in manuals. By its very nature, a manual is a long time in press. During that period all novelties could be published through legitimate channels and in more helpful form. If this were done, some of them probably never would appear in manuals at all. It is a matter of common observation that manual publication of novelties is more abundant in manuals for private sale than in those for institutional sale.

This standard would have invalidated two species and one varirty described by the writer many years ago. Doubtless if manual publiwation had then be invalid, these entities would have been promptly and more fully treated in some botanical journal. Actually this was done for the species some years later but not as promptly as should have been the case.

Gross Multiplication of Species. No publication of large numbers of species in any genus shall be valid unless and until the designated referees for the family involved have certified the work to be reasonably worthy of acceptance. Such certification shall be based on an examination of the material and a conclusion that at least 75 per cent of the novelties are justified.

The intent and effect of such a standard and requirement would be to prevent such botanical scandal and taxonomic tribulation as occured in connection with the genus Crataegus (hawthorns) between 1899 and 1911. Within that period of approximately 13 years, more than 1700 so-called "species" of that genus were published in North America. It is said that the type specimens of at least three "species" came from the same plant. Nearly all of these were from the eastern half of the United States, an area covered by numerous botanists for more than 100 years. During the 19th century, from 15 to 30 species of Crataegus had been recognized. It is wholly ridiculous to assume that this army of botanists had completely overlooked more than 1799 species of one genus which were growing under their very noses.

Gray's Manual of 1889 contained 10 species; Britton and Brown's Illustrated Flora of 1897 had 15, and Britton's Manual of 1901 had 31, with a statement that many more had been published recently by Ashe, Beadle, and Sargent. By 1908, Robinson and Fernald in Gray's Manual recognized 65 species, while Edition 2 of Britton and Brown in 1913 had 73 species. Sargent's Silva in 1892 had 14 species; Britton's No. Am. Trees in 1908 had 51; but Sargent's Manual in 1905 had 132 and the 2nd edition in 1922 had 153 species.

In the South, Chapman's Flora of 1897 contained 15 species; Small's Flora of 1903 and Edition 2 in 1913 had the same number; while Small's Manual of 1933 had only 33 species.

Most of the mass of "species" were published by four men, Ashe, Beadle, Eggleston, and Sargen, amd most of flood occured in the 10 years from 1900 to 1909. Apparently it was a disgraceful race to see which one could get his name attached to the most species before the others could get there first. Taxonomic botany does not seem to have been concerned.

It is an axiom that no person or droup can be safely trusted with unlimited and ~~xxxxxxx~~ unrestrained power and privilege (private law). Sooner or later it will be abused. There must be sentiment and procedures for providing standards and enforcing regulations. in the public interest.

Taxonomy has both a public and a private interest. Much taxonomic effeort comes from tax-supported institutions. The endless and increasing burden of synonymy eats up tax money in time and effort, just as it wastes private funds. Standards and controls may beprovided from within an industry or they may be forced upon the industry by public agencies in order to defrnd the public purse.

That culture which cannot defend itself against retarding and destructive actions by any of its elements will not long survive. That scientific group which cannot or will not protect itself against a flood of phoney products from its incompetent, self-seeking, and ethically dishonest elements will lose both its productive power and the respect of its fellows. and will be buried in the accumulating debris. At present, there not only is no standard and no regulation but there is no stigma or loss of standing attached to those who follow the practices and ~~xpx~~ provide publish the products herein condemned. Rewards are the same to the competent and the deficient, the responsible and the careless, the dishonest and the honorable. Standards are erected, regulations made, and ing inspection provided, for a thousand other practices and products of human skill. Why not for the practices and products of taxonomic botany? If it be claimed that new species and varieties represent ideas, rather than products, let us remember that patents and copyrights likewise represent ideas and they must be obtained through application to a jury of his peers before the originator of the idea can establish claim and title to it. Are the taxonomists afraid to venture a jury verdict even in these special cases just discussed?

No man would be allowed to offer, and no person would wish to accept or run the continual risk of accept ing chaff and husks, and pods in place of the desired grain, but the taxonomist is in perpetual danger of just that.

It recently has been claimed that —

mentally
If taxonomists in general were unstable to that extent it might help to explain much of what is occurring to bring about the present flood of synonyms. So far as the exist in the professional group, they are a detriment and a menace. Perhaps they should be classed as mental defectives and provided with institutional care. Some may need it. Even so, border-line cases would remain free to reproduce their kind and perpetrate their taxonomic blunders. Rafinesques still would describe and publish their new species of lightning and thunder. It is high time that systematists admitted the obvious fact thatb perils from the mounting tide of synonymy are caused by (1) low standards, (2) incompetence, and (3) downright dishonesty.

What of the American collector who suddenly sent three specimens of Salix to a foreign botanist, wholly unfamiliar with American species, and received in return the manuscript of three "new species", one named for himself and all representing the wilful renaming of long-known American species and varieties?

How about the American botanist who, in preparing a regional manual, described a new Salix therein and cited as the type specimen a collection made there more than 60 years before anf immediately cited as the type of a species published immediately by the collector? The species established 6 decades before had been carried in the manuals succesive ever since and the woeding of the two descriptions was very similar. regional

If this seems asto nding, how about the two botanists who made a new collection of the same plant, not far from the type locality, and immediately publishing it as a new species in spite of its portrayal in the manuals of theregion for 70 years under two other names? Is there no obligation on botanists to look in the literature, at least in current manuals, and in herbariums before publishing "new" species? One of the authors informed the writer later that he would rather

Why then did he not do so?

Shakespeare said: "The evil men do lives after them, the good is oft interred with their bones".
A Synonym is an evil that lives on forever a veritable ball-and-chain on the foot ofprogress.

Standards

1. Sufficient material
2. Well developed "
3. Comparison with closest rel.
5. "Species splitting" not valid
4. Publ of nsp in manuals not valid
6. Shoddy work (palpably)
7 name change without knowledge or treatment
8. ~~Taxa~~ Naming by uninformed foreigners
9.

Patents + copy rights

Brown, Roland W. 470 Sci 2

Supposed extinct maples

Science 96(2479):15. July 3, 1942.

The two species of Acer described by D. P. Penhallow from Don Valley, Toronto, are variant leaves of Platanus occidentalis.