

Acta Botanica Weberi

A JOURNAL OF BOTANICAL RESEARCH, THOUGHTS, THEORIES, HISTORY, AND PERSONALITIES



Rhizoplaca weberi

Photo: Linna Weber Müller-Wille

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EDITORS' NOTE

This issue was originally planned for publication in 2022. However, due to repercussions stemming from the Covid Pandemic, an unfortunate four year long hiatus in publication of *Acta Botanica Weberi* issues ensued. As 2025 marked the 5th anniversary of William A. Weber's passing, we felt it an appropriate time and opportunity to resume publication, with this issue finally appearing in time for the 6th anniversary.

LEON HUGH KELSO (1907–1982) — A NEGLECTED COLORADO BOTANIST A BIOGRAPHICAL SKETCH AND BIBLIOGRAPHY

Editors: Linna Weber Müller-Wille and Ragnar Müller-Wille

based on an unfinished manuscript by William A. Weber (1918–2020)

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ABSTRACT

This fragmentary paper about ornithologist and botanist Leon Hugh Kelso (1907–1982) consists of three parts: a biographical sketch, extracts from Kelso's letters and self-published *Biological Leaflets*, and a bibliography. The biographical sketch details Kelso's career as a naturalist, especially his trials and tribulations to be accepted by the scientific establishment in Colorado. These tribulations are illustrated by excerpts from Kelso's *Biological Leaflets*. Selections gleaned from the *Leaflets* present his thoughts and observations on the vicissitudes of the scientific community, and the ways in which political factors and "closed-shop" attitudes of the "establishment" disregard, neglect, hinder, and even suppress scientific endeavours, including his own, of new-comers and neophytes. The bibliography encompasses all of Kelso's works Dr. Weber was able to find. Some issues of Kelso's *Biological Leaflets* were, and still are, untraceable.

Keywords: Ornithology, Botany, Taxonomy, Colorado, Botanist Biography

ABOUT ACTA BOTANICA WEBERI

Acta Botanica Weberi is published by members of the Weber family and distributed free of charge through the williamaweber.com website. It was specifically founded in 2018 to publish hitherto unpublished papers by William A. Weber. It was decided that at the age of 99, Dr. Weber could no longer wait on the peer review process to see his latest writings published if they were not to become posthumous works. Instead he and we felt that publication in this form to be the most appropriate at his age. The resulting dissemination of these works among his botanical colleagues, known and unknown, and the uses, references and discussions thus arising will be enough of a peer review and contribute to the continuing endeavors to research scientific questions.

William A. Weber (1918–2020) over the years wrote a number of papers on various topics that he never published. We as a family would like to continue to bring some of these writings to light. Further issues of *Acta Botanica Weberi* will continue to be published in his memory. We will endeavour to keep the standards equal to his scientific rigour, and will do our utmost in bringing his remaining unfinished writings to publication.

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FORTHCOMING PUBLICATIONS IN PREPARATION

A Moss Primer. William A. Weber

William A. Weber – An Autobiography
projected publication of chapters and excerpts in instalments

EDITORIAL FOREWORD

William A. Weber wrote this biographical sketch and bibliography of naturalist Leon H. Kelso many years ago. The original intent was for it to appear in the Colorado Native Plant Society's *Biographies of Forgotten Botanists* series in their journal *Aquilegia*. Although this did not come to fruition, it did for a short while appear on a now long defunct website.

Later in his mid 90s he approached the Missouri Botanical Garden for possible publication in their *Annals*, but in the end felt it too bothersome at his age to complete and revise it for publication, and let it lie. It has thus remained unpublished until now.

While we have undertaken some necessary editing, we decided to publish this paper substantially in the somewhat unfinished state Dr. Weber left it. Unfortunately, we have not had the occasion, opportunity and time to fill in all the missing information and references, nor to locate the missing issues of Kelso's *Biological Leaflets*. Nevertheless, having received some enquiries about it over the years, we felt it important and relevant enough to present Dr. Weber's presentation of Kelso's work as it is, hoping it will be useful to others.

Missing information in Kelso's original text (such as incomplete names), as well as comments and explanations of deficient passages appear in square brackets. All Extracts from Kelso's *Biological Leaflets* and letters are double indented. Passages where Kelso quotes other people are italicized and further double indented.

As always, we would be happy to receive any additions, corrections, or comments.

The Editors

LEON HUGH KELSO (SEPTEMBER 4, 1907 - MAY 9, 1982)

A NEGLECTED COLORADO BOTANIST

Leon Hugh Kelso was a government employee in Washington, D.C. at the Bureau of Biological Survey. His specialty was ornithology, but he had studied botany and taxonomy and was also very much interested in the Rocky Mountain Flora. He published in refereed journals as well as in his own self-published and distributed journal called *Biological Leaflets*. It appears that he published at least 107 of these, on topics ranging from the taxonomy of owls, the anatomy of feathers, the Denver flora, phytogeography, willows, and the genus *Carex* in Colorado. I think it is important for all Colorado botanists to know who this man was, because many of his observations show that he was an astute observer and had great potential for Colorado botany, which unfortunately was never realized.

Born in 1907 in Gretna, Kansas, he received his B.A. in 1929 from Denver University, where he had assisted S.M. Coulter in field botany courses as an "Assistant in Biology". He did graduate work at the University of Colorado in 1929-30, but did not finish his degree, instead developing an intense resentment of Professor Francis Ramaley, his professor, and ecologists in general. He felt hampered and restricted by the attitudes of Ramaley and others who were of the opinion that the Rocky Mountain flora had been sufficiently studied by Rydberg and Greene [see his note in Leaflet 36 on page 10]. Taxonomy was undergoing hard times then, considered a trivial subject because of its attention to picayune details of nomenclature and "splitting" or "lumping", at a time when cytology, polyploidy, and the discovery of the virus were revolutionizing taxonomic concepts. Kelso realized how important taxonomy was as an undergirding of all of biology, and did not hesitate to defend it. Kelso also did graduate work at Cornell University [M.S. 1938] and at the University of Wyoming Field laboratory. He joined the U. S. Bureau of Biological Survey in 1930, working there for the rest of his career (Ewan 1981), specializing in ornithology and mammalogy and the analysis of stomach contents of birds and animals. His scientific life was often complicated by the politics of government service.

Besides botany, he was very much interested in the possibility of radiation as an influence on the behavior of birds and animals. He devised interesting experiments to test his theories. Did any of his "bioelectronic observations" influence research on homing, migration, the functions of feathers aside from flight, the functions of spines on plants and animals in desert habitats?

BIOLOGICAL LEAFLETS

Very few sets of Kelso's self-published *Biological Leaflets* remain. The University of Colorado Herbarium in Boulder has had some of them for many years, but I discovered many missing numbers among the papers of Áskell Löve, who must have carried on correspondence with Kelso over many years. In the following pages, I glean from his writings some of the personal remarks (often these are invective) as quoted from them.

Let us begin with Leaflet 71, issued May 1964, which contains numbers VIII-X of a series called *A Niche of Naturalists*. From this selection it is obvious that Kelso had a highly developed civic conscience.

Leaflet 71, May 1964 (extracts)

“The determination of stomach contents is necessarily elaborately exact, in some of its phases almost as much so as watch repairing. The greatest care is taken to identify and segregate the various food elements, and much of it has to be done with the aid of the microscope. It calls constantly for a wide knowledge, not only of birds, but of plants, seeds, grasses, grains, fruits, and insects as well,—to say nothing of many other things. When the examination has been concluded the material examined is filed away in a jar of preserving fluid and carefully labeled. The data thus secured is likewise filed away on an index card,—the food and other elements discovered, the species and type of bird (i.e., whether nestling or grown), when and where the bird was taken, and any collateral information of importance. Cards are also made of the several food elements, insects, grains, grasses, etc., so that information may be readily procured when desired regarding what foods are largely eaten by what birds. Summations are frequently made and averages struck. The value of this great and constantly growing mass of information in working out methods for controlling injurious birds is a thing that speaks for itself.” (Jenks Cameron, The Bureau of Biological Survey, in Brookings Institution, Service Monographs, No. 54, 1929, p. 159.)

It is only fair to state that the information that follows has been abundantly “grapevined” around and among those in favored bird circles for years, much to the damage of those less fortunately situated. Tales have already been told out of school in plenty. Furthermore, in the present-day, personnel managers and professional editors, making use of such information secretly are becoming kings and king-makers of biology. It is their understanding, and to some extent the public’s that while science is important, scientists themselves are a bunch of pip-squeak sissies, and that only big he-men of the back country, of the sage brush, the bull pen and of politics, they alone are fit to boss things.

A Niche of Naturalists VIII

As the months went by we saw the futile struggle of the Hoover administration to overcome the depression; its defeat and replacement by F.D.R. and the New Deal. Following the first Christmas after the New Deal victory we were called into [Waldo Lee] McAtee’s office. “I had to wait until after the holidays to bring myself to say what I have to tell you, and I would feel much better remaining seated while I tell it” were approximately the words with which McAtee opened the discussion. What he went on to reveal was that the Food Habits Section had been cut out entirely from the budget of the next year’s appropriations, beginning in effect July, 1933, and as of then the division and its work was at an end, unless its appropriation should be restored before the coming July 1. The Division was at an end; we, out of jobs. This was in the time of depression, great mass unemployment; no work, academic or otherwise, available, not even for some with doctor’s degrees.

In 1930 and before, while salaries were not high, a preferred student could get on a college faculty with work on his favorite subject, on the basis of an "A" record, although a Master's degree was considered desirable as good insurance. It was understood he would work out the Doctor's degree later as opportunity allowed. Before the 1930s were over a Doctorate had come to be a minimum requirement for even an instructorship in an institution of higher learning.

Now, in fact, many with the Doctorate were unemployed or were selling apples on streets, or working in filling stations. There were innumerable ordinary college bachelor's degree holders with nothing, either in regard to employment, status, or career. And those unfortunates with jobs had better hang on however bitter office discriminations and insults might become. I have often wondered during the twenty-five years that have since elapsed [1955] what difference the loss of or the saving of the threatened jobs would have made in the final destinies of those gathered in that council of impending disaster.

Certainly the present writer, of the whole group, is the only one still in Washington, and none of them remain active in bird work to an exclusive degree. Those two who rose to highest grade in bureaucratic rank were since "ousted" due to political developments. There had been two previous attempts to eliminate the division, and plans to combat this one were made on the basis of lessons learned from those near-disasters.

We would hire a typewriter to print stencils for mimeographed pleas to be distributed to the influential back home, our neighbors, relatives, colleges, universities, state legislatures, congressmen, conservation groups, to anything and anyone who might contribute to the survival of the doomed office. It was against bureau law for a division or an employee to plead for himself, so one of our own typewriters must not be used; detection methods could trace the source of the circulars. In addition, we were to write to our personal acquaintances back in the states personally, to persuade them to exert their influence politically in our favor. These plans and others were carried out. But it was a grim matter to carry home, or to carry around in ones mind during the coming days.

McAtee showed increasing irritation as time progressed. Anyone going into science forthrightly, via the long, lean, scant, slavish, sacrificial years of college and degrees, not via editing, personnel management, popular science writing or through other burrows, should have learned he is no match for politicians and wants nothing to do with them. He, for one thing, is bound to the truth; they are not. He, McAtee, hated having to go through all this again. Perhaps we all regretted having such a slight hold politically on our lives. Unfortunately, science must eat to survive, as do other things. And its practitioners are in the situation precarious. As has been pointed out, any jazz trumpeter can prove his skill, or lack of it, before the multitude, but your scientist, teacher, interne, or whatever, is only as good as the entrenched will say he is.

A Niche of Naturalists IX

Adding enormously to the painfulness of the situation was one of those periodic stews concerning the decline of our "wild life." "The wild life is disappearing; something must

be done about it,” resounded over and over in conservation publications, conferences, conversations, and conventions. Conspicuously absent was attention to the basic cause of the disappearance—the bang-bang of the guns. Not our division, but another section of the Survey was caught in the midst of a poisoning program, and that due to executive and political action outside our influence or initiative. There were resolutions introduced against the Survey, at annual meetings of the Mammalogists’ Society and at gatherings of conservation groups, for the purpose of condemning the poisoning campaign.

I will not take on a review of the history of the connection of the Survey to the cattle industry, nor of its role in wildfowl conservation, propagation, protection, and law enforcement. Those interests increased the size of the bureau, but with that came a multiplication of the hands clutching at it for control, and the number of enemies it could make. These interests had grown to override all the scientific and investigative concerns of the Bureau. Taking on these tasks to ensure the appropriations and growth of the organization may have looked to be obligatory or enlightened self-interest at first. Later, like the growing cuckoo in the warbler nest, they pushed out or smothered the original interests. With the coming of control work, politics, economics, and the interests of the shooters entered the tent like so many camels’ heads.

Technically it has never been proved that it is the burden of scientific bird study to maintain game supply, and to generally restore wild life, or that it can do so unaided in the teeth of complex social changes. The penetration of all non-cultivated areas by roads, drainage, the expansion of cities, military ranges, pollution of streams, the spreading of super-deadly poisons by planes, all these and more, are factors ornithological science does not initiate or control. Nor does it control bureaucratic empire-building, or the attempts to take over Forest Service and Soil Conservation Service wildlife programs.

“Memorandum for the Secretary of Agriculture: *I get from a good many sources suggestions that the Biological Survey spends too much time on scientific experimentalism and that we ought to have a more practical spirit—their aim, for example, at the practical building up of game refuges and working out plans for making birds a valuable crop for the farmer to raise, just as they do in England.*” Roosevelt (F.D.) To Henry A. Wallace, October 18, 1933, Confidential. [(Quoted from *Franklin D. Roosevelt and Conservation, 1911-1945, Vol. 1*, FDR Library 1957, p. 210.)]

It has never been demonstrated scientifically that bearing down on birds as farm crops, if practical in England, saves the bird life in its original state or that it enables the much-advised farmer to survive. If the combined efforts of all human society will save a doomed species and bring it back to a self-sustaining level of abundance, the fact has never been demonstrated on this continent. Making birds a farm crop ignores cultural and scientific values. In brief, [alongside] the pressure for eliminations and economies in the conduct of the Bureau, there was added the weight of criticism against control measures, criticism on account of the decline in abundance of bird and other animal species, yet with demands for more game to shoot, and for enlarged poisoning programs against pests.

Coming at the very start of our careers in the organization it seemed that all these factors were threats aimed directly at our careers. Faults the Bureau certainly had as do all bureaus, I was to realize later, but those working within and subject to the office

pressures could not feel otherwise than that any criticism threatened themselves personally. In summary, then, the mimeographed literature was printed and distributed, the appropriation was restored, the division was saved (for later disintegration), but it was not the end of trouble for any of us.

To the analysis work, Scaled and Gambel Quail from Arizona, Ptarmigans from Alaska, Cactus Rats from Arizona, Sage Grouse from Wyoming, Prairie Dogs from the Rocky Mountain Region, Spruce Grouse from Arctic Canada and others came my way in lots numbering from dozens into the hundreds, as did an acquaintance with the floras of those areas.

The basic work of laboratory food habits study had to include identification. Identification involves taxonomy to a considerable extent. Taxonomy—a hated word among ecologists. As far back in history as Linnaeus, and perhaps farther, there have been those who felt or wanted to believe that too many forms of life have been or are being recognized. For some of these opponents it is not a matter of thinking or feeling, but it is the need for something to hate.

The section had to have its own library of plant, insect, fish, mollusc, and other identification manuals. Also it had to have its own file of books, pamphlets, separates, and notes from publications on economic zoology. Some space and consideration was deemed necessary for papers and data on common names of animals, and for weights taken in the wild from specimens of mammals and birds. A general criminal investigation laboratory serving in cases of charges of depredations by birds and mammals against gardens and crops, being a biological F.B.I., was a prominent feature of the *niche des naturalistes*.

The Biological Survey's task in general was the survey of a continent. Its collectors and explorers were in the field months, even years at a stretch, in Alaska, Canada, the Southwest, Mexico, Central America, and even Argentina. The material on the storage shelves (stomachs of birds and mammals to be examined) reflected this activity. Even with an augmented staff, the work on the shelves would occupy us for years to come without any increment from outside collectors, But with the outside pressures, the bowing to outside students increased in form of the acceptance of more and more special lots for analysis, small and large.

When the July of 1933 passed and it was known that our places would not be abolished there came only a minor relaxation of anxiety. Antagonisms and tensions built during the preceding months held over. If you forgetfully left a scalpel or forceps on a file case for a moment it would disappear and you would not see it again. The droning silent summer afternoons, in heat unrelieved by air-conditioning in the temporary clapboard building were the miserable times of the year. Sometimes the spirit was invigorated by conversations which, in whatever room, could be heard without strain.

McAttee came one afternoon into [Clarence] Cottam's room and during the discussion he said, "Ding [Darling] says I'm not a good administrator. That means I'm ..."

"Now the administration has given us three and a half million to use in saving the wildlife, and we've got to get busy and use it. This work isn't in my line or in my experience but I stand

ready to get rid of anything that stands in the way of you doing the job.” (This with smack of fist into palm of hand.) [This was Jay N. “Ding” Darling, the ex-cartoonist new chief of the Biological Survey speaking.]

In due time following the inauguration of the new administration, Chief Redington had resigned. There had been complaints that he was a forester, not a biologist, but now we had a cartoonist. Now that the chief was in he got busy, among projects and personnel about which he knew nothing, to set things to rights in big western he-man style, and save the wildlife—the breath of fresh air from the wide-open spaces, the sage brush, and the manure pile, that was to save the wild life. One thing coming first as a change was the bi-weekly, or maybe it was daily, staff meeting, held to stimulate effort, report progress and plan future meetings. After the first one or two McAtee refused to attend any more. Therefore it developed that he must resign or be transferred where designated. He was designated to a situation entitled “special advisor to the Chief.”

While this was developing, special examinations had been piling up: Mr. A. E. Borell wanted a set of analyses from Elko Lake, Nevada done. For Mr. Smith, several hundred prairie dogs from Montana. For Mr. Yeatter, a number of Hungarian Partridge from the Midwest. For Mr. Paul Errington, numerous quail gizzards and pellets from Wisconsin. For Mr. Taylor, several hundred *Neotoma* (wood rats) from Arizona. For Mr. Gorsuch, several hundred Gambel and Scaled Quail from Arizona. For Mr. Dalke, a number of rabbits from new England.

Long before this, over a thousand analyses had been taken on for a New England study of the Ruffed Grouse. These occupied most of my time. Into this, however, was intruded a very special study of the Ruffed Grouse for New York State, under Gardiner Bump, formerly of Cornell. They had in mind, whether or not as a model, the H. L. Stoddard work on the Bob White Quail, which included over 1,659 food analyses. For all these and other demands, there was no expansion of personnel, facilities, time, or money.

A Niche of Naturalists X

A car with a family of five (it was in 1929) a couple and their three daughters, a Mormon family from Utah, with engine chugging and choking, came to a stop in midst of dense traffic on a street next to the Library of Congress on a hot summer afternoon. The vehicle, after some cranking I understand, was again brought to life and moved. This was incidental to the coming of the next head of the Food Habits section to Washington, following ten years as a school teacher in various Utah desert towns. This was a part of that stream of mid- and far-westerners into American biological positions, supplanting even ivy-leaguers in the ivy-league colleges. This one was to supplant workers in the division having 25 years seniority, and others with 20 and 15 years, and that within 5 years after arrival.

If there was not some previous agreement among western states senators perhaps to make Clarence Cottam chief of the Survey eventually, there was much in the attitude of himself and others to indicate the existence of such. This observer was too naïve in

Washington ways to believe hints of such at the time. Before two years had elapsed, the “Baird Club,” a non-public, no-dues, private, semi-secret bird club, admitting only such as had written books and attained national prominence in the subject was to enfold him. No one could explain why. With the social and ethical background of the desert town and ranch he did advance to the control of research and careers in the Biological Survey. Shortly after the above conversation with McAtee he was installed in McAtee’s office, there having been no announcement of any promotion in the meantime.

W. L. McAtee, among others, turned himself inside out, so to speak, to advance Cottam and make him head of the division. No sooner was the latter ensconced in that position than he fell out with his benefactor as indeed he did with everyone else who helped him. The discovery of a letter written by C. C. to William Vogt (Dear Bill), then editor of *Bird Lore*, the Audubon Society magazine, condemning McAtee, saw to it that he never became Chief of the Biological Survey as it was, in spite of the hopes and efforts of some dotting elders.

At about the same time, Ira N. Gabrielson, hitherto known to us only as a field leader of ground squirrel poisoning work in the Northwest, having been returned to Washington, had struck the fancy of the new chief. After several months, not being able to set Congress aflame, and having “had it” from the bureaucratic jungle, Jay N. “Ding” Darling chose to resign, leaving the chieftainship to his choice, Ira N. Gabrielson. And he left the Survey saddled with whatever changes he had made, with careers and projects devastated, with whatever benefit might come from the devastation to be worked out by others. His coming and going has been described elsewhere as a paean of triumph. That it was a travesty against the long-labored out merit system has never been noticed. (To be resumed.)

“I can repeat in all truth what I intimated in my previous letter, namely that I spoke up in your behalf as long as you were in the organization, and I know that when the question of your reallocation came up, Mr. Henderson, with whom I had discussed the case, voted in your favor. I said then, and I still say, that at any time conditions were becoming difficult for you and you decided to resign, you were doing the most work and the best work of anyone on the food habits staff.” Letter: W. L. McAtee to Leon Kelso, September 21, 1944

Botany

Kelso’s early papers (Leaflets 1-18), were devoted to bird, particularly owl, behavior and their activities with relation to solar, lunar, and atmospheric phenomena. His first paper on Colorado flora was No. 19. *Plants of Denver and vicinity*. He lists common names for all 718 taxa. The list was compiled from herbarium records (COLO, RM, US), and from his personal experience in Denver as a student at the University of Denver. Here he first vents his dissatisfaction with the [then] current botanical establishment:

The one other publication intended primarily to include all the vascular plants known to grow in this locality is “A Popular Flora of Denver, Colorado,” published by Alice Eastwood in 1893. After the passage of fifty years another list may be worthwhile.

Taxonomic work in western botany had been obstructed by the closed shop policy that has existed in systematic natural history for the past thirty years. [Leaflet 19]

In Leaflet 36, *Additional notes on Rocky Mountain plants*, Kelso writes:

“Who are you,” Francis Ramaley once said to somebody, “to work on Rocky Mountain plants, when Dr. Rydberg and Dr. Greene worked on them for 50 years? You, putting your ideas beside theirs; it is absurd.” Thanks to this and similar attitudes, the lack of activity in recent decades has resulted in the leaving of many problems unsolved and many plants undescribed in the Rocky Mountain area.

Leaflet 37, on willows: a correspondent had sent Kelso three specimens of willows labeled as *S. monticola*. Kelso found that the set contained a second species which he had treated in an earlier Leaflet.

The difficulty in identification [of certain willows] is due to the fact that the leaves in this group, as may easily be perceived in the field, are not actually as unicolored or green on both sides as the extant keys and texts represent them to be. So, the ancient formula of closed shop, push everybody out, one man control, and worship of beef and brawn does not solve or benefit all problems in western botany. The correspondent also makes a point of the fact that others have been working on willows with thirty or forty years. This is by no means the first time we have had this point brought up, but why always and only in connection with taxonomic questions?

In *Rhodora* for July, Vol. 37: No. 439, pp. 262-263. 1935, there appeared an article entitled “A new species and two new varieties of *Glyceria*.” The following is the history of that article.

Knowing from experience what was ahead for it, Dr. A. S. Hitchcock consented to and actually edited and for a good part wrote the article. Although quite brief and simple, the article was accused by F. V. Coville of being wrongly written and having “many misleading statements.”

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

It has been stolidly and steadfastly maintained, particularly by W. L. M. [W. L. McAtee] and C. C. [Clarence Cottam] that the older men were flouted and insulted in the affair, and is so maintained to this day, to all botanical and ornithological circles. This is an example of good American sportsmanship in bureaucratic scientific divisions which expect nevertheless the utmost in “cooperation” from each individual scientific worker to keep their own files complete and their own collections complete with types and co-types, and shows what their idea of individual freedom is. It is additional evidence for the fact that they hope ultimately to control every word in biological research in this country.

Leaflet No. 40, on “Embryo Taxonomists” [nascent taxonomists], covers four pages:

“If embryo taxonomists were required to go out and measure and record the range of variation in organs on a single large living plant or on numerous smaller plants of several

species, they would return both tired and amazed. If they were required to follow the progressive variation of all organs on a plant through all of the seasons of even a single year, they would become both enlightened and humbled. Knowledge, with amazement and mutuality, is an excellent foundation for future taxonomists." ([C.R. Ball in] *Journal of the Arnold Arboretum*, Vol. XXVII, 1946, pp. 371-385, 378.)

One source of amazement for "embryo taxonomists," and embryos in other lines, is the discovery that one morality is preached and quite another practiced in this "brave new world" as in the old. The above article states that there are now no professional standards or requirements for taxonomic workers; that "this lack of standards is responsible in part for the lack of respect in which this profession is held." The writer of that article has for 50 years been a member of an organization which had had a dominating if not totally controlling interest in some departments of American taxonomy.

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

Social, economic, and psychic factors have nothing to do with systematics according to the fore-quoted article. As the Private said: "There ain't no sich thing as psychology. My Sergeant said so." There is no mention of the fact that the leaders, the trusted guides of the embryonics, utterly refuse to agree on any set of reasonable standards. Worker A wishes to publish a certain revision or thesis. Worker B opposes it. Under the psychology of the hive, A has no better rights over his own work than B, who can protest and appeal the case out *ad infinitum*. This worker [L.K.] had his largest published manuscript on the desk of a leader for four months awaiting suggestions. When the paper finally appeared, the latter said the older men were being ignored.

Over 30 years ago a Flora of Alaska was prepared on government time, money, and materials. Waiting official approval, it was revised again about 15 years later. The combined head of the National Herbarium and Division of Botany, USDA, would not pass it. Now, quite out of date, it still remains unpublished. And organized Science did not say a word. Is this supposed to inspire embryo taxonomists? This would not be brought up had not the writer heard every accusation possible made against taxonomy and its workers. With the "bright new world of the future" we had hoped to have less of this frenzy of accusation, pushing around, and making things tougher for the taxonomists.

It should be kept in mind that somebody's cooperation, perhaps that of the embryo taxonomists, may be needed to maintain any higher standards. If anyone is concerned with what embryo taxonomists may or may not do, and it seems very evident that someone is, they might consider the following conditions:

1. *The idea that taxonomy is "done for"*. For generations we have been told in the schools that there is no worthwhile work to be done in systematic natural

history. This has tended to outlaw the subject in the minds of many influential scientists.

2. *The minimizing of taxonomy in college curricula.* In most botany and zoology departments there are but one or two courses touching upon the systematic aspect, and these often come at the close of the college career. Get rid of that taxonomy is the war cry of some ecologists.
3. *Lack of opportunity to do systematic work for an advanced degree.* In almost any graduate school any of the better students who has an average grade of 90 or more may do his research in ecology, physiology, life history, etc. Those wishing to work on a problem in taxonomy are discouraged or told to go elsewhere. And there is scarcely a school left in some States that would allow a student to even try his hand at systematic research on birds or mammals. Consequently most of the biologists turned out today are non-systematists and most of the comprehensive and progressive writing and thinking as well is done in Europe.
4. *Dangerous antagonism against systematic work and workers.* This is too obvious to need elaboration; it is common among ecologists and life-historians. Anything different offends the rural mentality.
5. *Gravitation of small collections to a few large ones.* This material usually concerns types [type specimens]. This brings that field of research under the control of fewer and fewer men.
6. *Territorialism among systematists.* The tendency to divide the systematic field up among a few well-established leaders who never question each other and cooperate to monopolize material and close ranks against the newcomers.
7. *Control of systematic work by those who never did any.* This explains itself. Many if not most biological divisions, bureaus, and departments are superintended by non-taxonomists, some by non-biologists.
8. *Elimination of taxonomy from high school courses.* Just as the anti-militaristic tendency demilitarized our country to a dangerous extent, so de-emphasis of taxonomy or systematics has been carried too far for the good of biology. Many biologists I know make subspecies hating and taxonomist baiting their pastime. The taxonomist is to the ecologist what the Jew is to the Nazis.
9. *The dominance of age.* Those who have been in the field 40 or 50 years have a status that appears gigantic compared to that of the most thoroughly prepared beginners. Men educated in schools of a past century will dominate

the field for years to come. Certainly, the same individuals have dominated it for the past 40 years.

10. *No allowance for individual initiative.* The young taxonomist is expected to agree, above all things to agree. The ecologist is expected to have individual freedom, but not the taxonomist. The latter is told that he must agree with Dr. X because Dr. X “has arrived and you have not,” and agree with Dr. Y because he has “arrived” and with Dr. Z because he has “arrived.” By the time all Drs. are agreed with, the manuscript is so reduced that it shows scarcely one-tenth of the work that has gone into it. Descriptions are skeletonized; detailed ranges are reduced; comparisons are eliminated; synonymies and references are cut out. This means the loss of months of work, all so some of the hierarch’s feelings will not be hurt.
11. *Trivial and irrelevant criticism.* Scientific periodicals have many pages devoted to taxonomic criticism; very little or no space devoted to criticism of ecological papers. Much of the criticism may be true but why the severity for taxonomy? It is uplifting to the community for the preacher’s kids to come in from the sidewalks at eight while the others unhamperedly raise hell at all hours according to some people’s feelings—but others would disagree.
12. *Editorialisms.* No two periodicals, or institutions, or authorities agree as to how a systematic paper should be written up and every paper can be quarreled to death on the basis of editorialisms.
13. *Caste and class and status quo.* A taxonomic work appears. One of the arrived caste criticizes it and all of his group must side with him. If the writer replies to the criticism, scientific dignity is offended; delicate feelings are hurt.
14. *Types of criticism.* These consist of: A. *Setting up and knocking down a straw man.* Saying: This should include such and such. Since it does not it is wrong. B. *The blast of criticism in the shotgun scattering pattern.* Giving many criticisms which do not apply, but the refutation of which is tiresome and useless.
15. *Cooperation, and our fellow workers.* The systematist is one person when his help is needed; another when his help is not needed, especially to ecologists.

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

The foregoing conclusions add up to a situation from which most embryo students “return both tired and amazed,” also, discouraged, disgusted, disappointed, disillusioned, displeased, disenchanting, disenthralled, disheartened, dissociable,

dispunged, and distraught. For the Rocky Mountain West this means the dominance of the Rydberg manuals for a half century or more.

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

Kelso follows up this broadside with Leaflet 40A:

In Leaflet No. 40, a local attempt at a Flora of Alaska was alluded to briefly. We shall note here a flora of the area by Eric Hultén which has been appearing in installments in the Lund Universitetet Årsskrift, N. F., Avd. 2, Vol. 37. 1941, and forward. This is in English, and apparently for English, but in a series mostly unknown to American students and certainly unavailable to most of them, although they, according to standards, will be expected to know all about it.

This Alaskan Flora is based on less than one year of field experience and very extensive accommodations and use of American herbaria, which, needless to say, would be denied native students, even if they had 30 years of field work.

The close relationship between the Alaskan flora and that of the Southern Rocky Mountains is apparent. The genus *Carex*, for example, is represented there by the following species [here a list of 106 species]. Of these at least 37 [now 45. ed.] occur in Colorado.

For the form of write-up it need only be said it is in the massive Scandinavian style, which no native ecology department would support. There are of course debatable decisions involved, e.g., the joining of *Glyceria grandis* to *G. maxima*, no more justifiable than many other possible combinations. Thus we now have reasonably modern floras for Kamtchatka, the Aleutian Islands, and Alaska, but few or none for areas southward to the Antarctic. The author may as well extend his work south through the United States since the Nordic Führers here tolerate no progressive work, and endless wrangling on standards and international rules is of no help.

In the introduction the author mentions that flora of Alaska projected long ago in Washington, which, if published, would have been a landmark for all succeeding work. The more a work is delayed the more unsolved problems arise with more consequent delay, and unless a limit is set, ultra-perfection defeats itself. Getting over-conscientious for perfectionism because systematics is under attack has been futile. Years in government work has shown that in handling large amounts of varied printing, editorial consistency is impractical if not totally impossible from the viewpoint of perfection.

Kelso's allusion to a proposed flora of Alaska is explained by Hultén (l.c.), as follows:

“The largest body of material from northwestern-most America accumulated in one place is found in the [Smithsonian Institution] where it has been kept separate from the main herbarium. This material was to a large extent brought together by government

officials, all of whom sent their collections to the National Herbarium. Its nucleus, however, is the large collection from the Alaskan Coast made during the Harriman Alaska Expedition in 1899. Dr. F. V. Coville, member of that expedition and later Principal Botanist at the Bureau of Plant Industry, Washington, took a great interest in creating a flora of Alaska. He did not, however, undertake that task himself. Dr. P. C. Standley, now curator of the herbarium at Field Museum, Chicago, was charged with the mission of preparing a flora of Alaska and Yukon. His flora, which, at the time when it was compiled, would have formed the basis for all subsequent floristic investigations was, however, never published. Letterpress copies are kept at the National Herbarium, and one of them was kindly put at my disposal.”

Kelso continues:

The reason for the existence of this series of leaflets, now at 50, is to combat the rural bullying dominance of the type that originally opposed the first flora of Alaska, so pusillanimously submitted to by the local status quo.

Kelso's invective rarely appeared in his papers on birds. But in his discussion (Leaflet 44) on territory and homing in birds, he says:

During earlier days on Washington the writer often wondered why the predominant disposition [existed] to discredit all theorizing and thinking in biology. When biologists first come into government work they are usually open-minded and progressive. In time, after a raise or two they are over to the side of reverence for authority, data taking, and fearfully watching the other fellow. As time passes the real pattern becomes clear. The total effect is to maintain an old status quo.

From the nature of past discussions on the subject it could have been predicted that any proffered explanation of “bird navigation” would have been a hard row to hoe. In recent years, when an explanation seemed imminent, some have shown that state which has been described in other words as Formicidae in the nether garments, and have indicated that to even think of a possible solution is pure conceit. On the presumption that all scientific work is confined to a certain few, preferable Scandinavians, such would of course be the case. That was kept in mind when it was first suggested in these pages that the motion of the bird through the physical fields of the earth is a factor in homing, and (No. 34) that the radiation clines in the atmosphere could be another factor in guidance. Ere someone decides that he or his fellows have studied the matter for 40 years and knows what we had better think, the following points might be considered.

1. Birds are creatures of reptilian affiliation and descent. In neither group is the skin provided throughout with minute glands for automatic adjustment to external factors, as in mammals.
2. It should be remembered that we may be dealing with both stimuli and responses which are outside conscious human experience.

3. It has been shown that reptiles, e.g. snakes, are far more sensitive to radiation than one would suppose, being able to discriminate between electric lights of very slight difference in radiated energy. More striking is the example of the chameleons, which readily change color, not always according to the color of the background but according to changes in general radiation,. The bird can likewise adjust to some extent, increasing or reducing its radiative surface by swelling or contracting its plumage.
4. It is only giving a burden to progress to assume that any possible sensibility be located in one special organ. The sense of smell in the honeybee is not confined to the antennae or one special part. In the human young, sex eroticism is at first widely generalized over the body, becoming restricted later.
5. A bird could be funneled back to its home by radiation clines.

A footnote or appendix to Leaflet 67 reads:

He was foremost at all races and cockfights and with that ascendancy which bodily strength acquires in rustic life, was the umpire in all disputes, setting his hat on one side, and giving his decisions with an air admitting of no gainsay or appeal. . . . The neighbors looked upon him with a mixture of awe, animation, and good will.

(W. Irving: The Legend of Sleepy Hollow)

The traditional method of selecting the next generation of scientists is highly personal: The master chooses his own apprentices, arranges some method to finance their graduate education, tutors them for several years and sponsors them as they seek positions of their own. The masters are usually satisfied with this method, and can justify it by a large number of successes. But perhaps their satisfaction also reflects the common tendency to be blind to ones own errors. The statistically-minded critic is always skeptical of a situation in which the same person makes a decision and then decides whether that decision was good or bad. ("D. W." *Selecting the next generation*. Science, Vol. 128., No. 3329, Oct., 1958, p. 867.)

On seeing the article entitled: *Some New floras of Parts of North America* listed on the cover of a recent issue of *Rhodora* the writer began to search therein with some feeling of anxiety. He need not have worried. There was no review of any or all of three recent floras of certain Rocky Mountain States: Idaho, Montana, and Colorado, and there has been none seen in any nationwide publication in the three large libraries of this city. This is unprecedented in my experience, not only with regard to the size of the works, but also in proportion to the positiveness and righteousness with which the institutions involved, unaccustomed as they were to taxonomic works, passed judgment in systematic matters. One writer admits to having ignored certain revisions in his flora, and the same reaction seems to have occurred in reverse.

Whatever the method of selection, whatever the virtue of the rural criterion of human worth, neither has prevented a decline in progress of systematics. Although it would not be stated by any loyal writer on “Rocky Mountain Naturalists,” almost or practically never in history has a native of the region or an alumnus of any Rocky Mountain institution been engaged in any local position in biology. Certainly there is no announced open competition. Like the hired killers in *Shane* or other western cinematic epics, unknowns from the Far West, Midwest, or preferably East appear, unfamiliar with local flora, fauna, or problems. This has been valiantly denied, but a reading of the faculty roster of any large or small college there will establish the truth. Any prospective and enthusiastic writer on western U.S.A. academic history is particularly challenged to controvert that fact.

In his Leaflets which include Nos. 86 and 88, entitled *The Carex Continuum*, Kelso shows that he was well aware of the relationship between the floras of the Rocky Mountains and East and Middle Asia. These papers are worth noting, for the problems have not yet been investigated. Examples:

Dr. Stafleu, editor of *Taxon*, has noted in past issues that there is a plethora of new regional and local floras but too few adequate penetrative monographic works. In volumes of a recent northwestern U. S. flora, re certain *Carex* and *Castilleja* anyway, Dr. Cronquist has remarked the need of special studies. Incidentally, what approving reviews or notices have appeared for Barneby’s two-volume compendium on *Astragalus*? Eh, bien, what then is the reception and appreciation for studies that have been made?

In the *Carex* treatment by Cronquist (p. 239) we have: “*C. aquatilis* in particular occupies a central position and is sometimes hard to distinguish from *C. scopulorum*, *C. prionophylla*, *C. sitchensis*, *C. eurycarpa*, and even *C. lenticularis*.” Comment: It is that undoubtedly if you do not take into account types, or typification, or diagnostic characters, and couldn’t care less.

Unfortunately, by that time Kelso was not able to develop in any thorough way his thesis regarding *Carex*. Invective replaced calm discussion. Thus:

In the recent urge to placate systematics’ enemies by species lumping, there emerges a trend to pass over use of trenchant but microscopic details, i.e. to “see sermons in stones, books in running brooks”, and adequate key characters, or even *Lemna gibba*, in everything. The public doesn’t know the difference. . . . It so happens that some of Holm’s species were typified in Colorado, and that this reviewer, without grants, and at his own expense, collected topotypical material in those localities, which has been distributed here and abroad.

Unfortunately for the new generation of Colorado botanists, Kelso was so angry with his treatment in Colorado that it was impossible for me to communicate with him or to obtain specimens, although I was actually in sympathy with his complaints. I have learned that he did carry on extensive correspondence with Joseph Ewan and Áskell Löve, both of whom had unpleasant histories involving the University of Colorado.

Conclusion

Leon Kelso's experience was a classic example of the damage that can be done by established scientists who, for whatever reason, decide that a young person desiring help and counsel is unworthy or incapable of doing significant work. From what we know about the man, he was a diligent worker, appreciated by his peers in the line of work he pursued professionally. He also was well acquainted with the principles and methods of taxonomic research, but had to pursue it at odd moments, without access to the inner circle of professional botanists and their herbaria. His tirades, which were socially unacceptable, nevertheless contain much truth, which we ignore at our peril. His case is a warning to us members of the "establishment" that a little tolerance and encouragement can go a long way in furthering the ends of our science.

APPENDIX

Selected correspondence

The following was written by Kelso in response to a request for “curriculum vitae”; L. Kelso [presumed written to Joseph Ewan] from Ewan correspondence, undated, archives, Missouri Botanical Garden.

On observation that if one is to have our cherished freedom of speech, or freedom of the press, one must be ready to compose and pay for it himself, and on the example suggested by the Leaflets [of Botanical Observation and Criticism] of E. L. Greene, mine were begun in 1932-1933, now stretching to over a hundred numbers. They are at least catalogued in several international indices and catalogues of scientific periodicals, although I am not in *American Men of Science*.

For my “curriculum vitae”, I recall that regarding a recent review of J. Fest’s biography, *Hitler*, of 1,000 pages, one reviewer suggests that for essential matters it could as well have been condensed to eleven pages. For myself I may repeat the account in my Cornell Thesis: Born in Gretna, Phillips County, Kansas, September 4, 1907. I attended public schools there and in Granada, Prowers Co., Colorado, and graduated from Denver University, June 7, 1929. After a part of a year at University of Colorado, Boulder, as graduate assistant, and a summer in the herbarium at Laramie, Wyoming, I received an appointment as Junior Biologist in the U. S. Bureau of Biological Survey on identification of food contents of birds and mammals. I resigned this position in July, 1937, to come to Cornell for graduate work.

There can be various modes in covering one’s life account. Let this be tried. It was late in 1947. It was just two years after the close of War (World) 2, and much of mankind was picking itself out of the rubble. Persons and institutions were encouraged and pushing for advancement and affluence. An inkling of this was a brief letter from Fort Collins, Colorado, that someone had been in Boulder and seen there my *Plants of Denver and Vicinity* (Biol. Leaflet No. 19) and that he, Harrington, was getting out a Flora of Colorado, and indicated that anyone would do well to supply him with specimens, types, and whatever rarities they might have. The point might have been missed by this reader that he was unheard of, unknown, not a veteran of any monographs or revisions, but it was not since I myself had been tackled on that angle before.

Future and finances promising better, each summer during the 1940s I had been spending about \$250 per summer month with vacation time in solo trips into type localities of the Colorado Rockies, for example for *Salix pseudolapponum* von Seemen, typified in the southwestern Colorado Mt. Hesperus area, on an immature staminate set of sheets widely distributed. Collections were gathered there and elsewhere. Biol. Leaflet No. 22 was issued in late summer 1947 suggesting some revision of species and status. At first there was the usual silence. Then, a two page letter from Fort Collins, followed at no great interval by a four-page, a three-page, then a two-page letter. This series dwelt not just on the controversiality of *Salix* but on the question of my fitness at all. This was the time of just one volume to *American Men of Science*, and it would tell one concerned that one Ernest C. Smith, ex of Bowdoin, ex of Harvard School of Theology, and Methodist

minister for 35 years retired, was curator at Fort Collins Agric. College, of their herbarium, the only consequential one in all Colorado.

In his letters he excused himself as an amateur, "not a professional", not connecting it with the fact that he, in a time of great scarcity of space or place in systematics had had for 24 years additional what might be defined as "all this and heaven too", comparatively speaking. He would be pleased if I would send him my qualifications. I had to go on his inclination, I thought. I could have told him I grew up through Colorado's grade schools, Colorado's high schools, and Colorado's colleges, if I thought he, or his 40-year status boss, would not resort to "Well, we have a roomful of those out here."

Frankly, he had taken a stratagem, common since, of inquiring about me without asking me, and been told the usual, that I was too much of a wrangler and the like. His own tone was so much of the same that rather than be drawn into more such, he should be cut off, which could have been done only by refusing to respond. He might not have been concerned that he was one of the many retirees from other professions taking space in bird work and seed plant botany, and in this case using his years of experience with words on incumbents, and serving his fellows there as their family lawyer; he knew the stratagem of prolonged wordiness and the lawyer's trick of ignoring any point unrefutable.

But worse, to me he pointedly observed that I then was not professionally employed in systematics, nevertheless wanting my specimens and publications. Making clear his many years, he was not too concerned that Rocky Mountain products, that is, students, must go east or elsewhere for the (even then) Doctor's degree requirement, to have even a sniff at a position in home academe. Oh, well, if not so usual in the Rocky Mountains, not so much to do about it.

For some continuation of narrative, when the proposed flora of Colorado appeared years later (1954), it ignored certain revisor's work and records wholesale, and like other of numerous areal floras coming now, disposed likewise of *Artemisia*, *Penstemon*, *Carex*, and others. He and others could operate on the fact that there are those who cannot know and could not care less what is an obovate leaf, what is a proprioceptor, what is the proper attitude?

The critique of Fest's *Hitler* emphasizes that, for adequacy, biography must take the person in relation to his situation, his origin, the general situation. "What was Hitler really like? What did he do, and what heritage did he leave?" So asks the critic Olczewski (*East European Affairs*, 1974). What was the locally worshiped T.D.A. Cockerell, with his 3,000 plus publication titles really like? But moreover, what his heritage? The publications race? The rat-race to print?

Well, this is more of a vignette than a life history. But more can be supplied when I am in a different frame of mind.

The following very small selection of letters from the Joseph Ewan files (Missouri Botanical Garden) hint at the animosity Kelso held toward "professionals" who tried unsuccessfully to communicate with him and to see his collections.

Ernest C. Smith to Kelso, Jan. 17, 1947

Dear Mr. Kelso: I acknowledged the receipt of your Biological Leaflet No. 34 when it came out, as I remember, without comment. Now, after excursions in other fields I am coming back to a critical consideration of Colorado willows and am re-reading all of our correspondence.

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

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

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

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

Plants identical or similar to some of your new varieties are not uncommon in my collections. That is to be expected since we have collected in some of the same areas. Personally I am over-cautious because so many earlier writers describe as new, plants quite familiar in Europe. Also I have been in correspondence with Dr. Ball for many years and Dr. Raup for a shorter time, sharing these problems presented by the maddening variation in some species of willows, partly due to hybridization, partly due to internal urges not yet completely expressed, partly to ecologic factors.

Dr. Ball has been writing on willows for more than forty years, had over ten thousand sheets of mounted plants of that genus when I first met him more than twenty-five years ago, and has had access to all the large herbaria of our country. Yet in the series reaching all the way from *Salix glauca* to *Salix brachycarpa*, the nearest he ventures to come is a tentative grouping of certain plants in a "complex." Dr. Raup, with extensive field work in both eastern and western Canada and many accepted publications uses even more vigorous language regarding this series. Much the same is true of the series *Salix barclayi-monticola*. Intermediates are so common that sometimes one could make varieties galore. Since men with these backgrounds are still uncertain, I hesitate to add more names to the present confusion. Each and every one concerned says "What we need is more information from the field, more cooperation, more knowledge of distribution and the variations due to ecological factors as distinct from genetic factors."

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

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

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

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

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

Carleton R. Ball to Kelso, Sept. 15, 1948

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

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

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

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

Now, can you take it? I challenge you to make the 26 alleged *S. monticola* specimens from Clear Creek available for study: 1. By letting me come to your house to study them. 2. By bringing them or sending them to the U. S. National Herbarium for my study there. 3. The findings to be published, either by me or by you. How about it?

Kelso did have his supporters, but their views were revealed in letters rather than in publications. In November, 1973, George H. M. Lawrence, who was his colleague at Cornell University, wrote:

Dear Leon: It has been a long time since our paths have crossed, when we both enjoyed the tutelage of Wright and of Wiegand. Each of us has reaped variously with the vicissitudes of time. I write to ask if you have a spare copy of your Biological Leaflets Nos. 88, 89, and 90. If so, I would welcome one of each. Nos. 91 and 92 are before me, and I have appreciated both. Like you, I believe that biographical accounts should 'tell it like it is', to use the vernacular, and seldom do I read one of a botanist I have known without thinking that the account in hand rarely gives the reader any degree of comprehension of the man as he really was.

Obviously, you could write a gem of an account of Coville, and perhaps of Maxon, if only you would. Should you be moved to do so, I would invite you to let me place them on deposit in the archives of the Hunt Botanical Library for benefit of the more perspicacious scholars of tomorrow. Does the idea appeal to you?

The Mann Library at Cornell has a series of letters from Áskell Löve bound with the *Leaflets*. Here is a paragraph from one.

I hope it does not give you too much trouble to try to get together a set of the fine *Biological Leaflet* series for us. I looked it up in the Museum some time ago when one of my students needed some special information from a few of them and found that they are scattered there and far from complete. Perhaps this is what could be expected from people who probably thought they themselves were superior to the writer of these good articles? It is difficult to be a prophet in one's own country, especially if one dares to have good and progressive ideas that the self-elected great men could not get themselves because of their own limitation in ideas and knowledge. Your series is no less important than Greene's series and several similar works, though it is perhaps more difficult to get at because you never did advertise it widely or use it to show people your own quality, only to publish fine ideas and good observations. Still, your name will be mentioned together with those other great botanists and biologists of the Rockies when time goes by. That none of us can prevent.

Botanical Collections

In a letter to Joseph Ewan, 21 August 1970 (Missouri Bot. Garden library), Kelso wrote:

“The enclosed items will give an idea of the destination of my collections. Elements . . . ended up so far as I remember, at Denver University, University of California-Berkeley, Fort Hays State in Kansas, about 400, to F. W. Albertson, some willows left at University of Wyoming after my summer there in 1930, Gray Herbarium (types of species published in *Rhodora*). More recently many study series were sent to George Argus (he seemingly finding some as puzzling as I did), some hundred on exchange with U. T. Waterfall, a few grass types to U. S. National herbarium, and a few smut-infested to D.B.O. Savile. Others have been sent to Leningrad and Moscow, especially to [Alexey] Skvortsov, the willow specialist.

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Biological Leaflets

Early numbers of the *Biological Leaflets* were printed in journal style. Later on, most of them were merely typescripts. Very few nearly complete sets exist, the most comprehensive ones being at Cornell University and The University of Colorado, where Kelso favored Joseph Ewan with reprints when he was an instructor. Some correspondence with Ewan is deposited at the Missouri Botanical Garden library. I recently discovered many missing numbers among the papers of Áskell Löve, who carried on correspondence with Kelso over a few years. The originals of these letters are in the Library at Cornell University, bound with the reprints, suggesting that this collection was donated to Cornell by Kelso himself. The Library of Congress on-line catalog lists 15 American institutions that have holdings of one or more numbers .

1. A note on the genus *Pulsatrix* [an owl]. July 25, 1933
2. A new spectacled owl from Bolivia. 1 p. Dec. 21, 1933. Warsaw.
3. A new variety of *Glyceria*. 1 p. A note on *Otus sanctae-catarinae*. 2 pp. June 9, 1934. Warsaw.
4. A key to species of American owls. (with a list of the owls of the Americas, by Estelle Kelso.) 101 pp., frontis., plates. Intelligencer Printing Agency, Lancaster, Pa.
5. A supplement to a key to species of American owls [including *Otus clarkii*, sp. nov. [with Estelle H. Kelso]]. 2 pp. Warsaw.
6. The value and identification of owls in the Rocky Mountain Region. [with Estelle H. Kelso]. July 24, 1936. 6 pp.
7. Supplement to the *Synopsis of the American wood owls of the genus Ciccaba* [including *Tacitathina*, gen. nov.] [with Estelle H. Kelso]. Jan. 15, 1937. Warsaw.
8. Two new owls from South America [*Otus choliba alticola*]. July 24, 1937. 1 p. Warsaw.
9. New owls from South America [including *Tyto alba subandeanae* and *T. alba zottae*, subsp. nov.]. April 21, 1938.
10. A study of the spectacled owls, Genus *Pulsatrix*. Nov. 10, 1938 [including *Pulsatrix perspicillata austini*, *P. p. ecuadoreana*, *P. boliviana*, subsp. nov.] [not seen]
11. Additional races of American owls. July 10, 1939. 2 pp.
12. Additional races of American owls. Nov. 23, 1940. 1 p. Warsaw.
13. Additional races of American owls. July 31, 1941. 2 pp. Warsaw.

14. The ear of *Otus asio*. May 9, 1942. 1 p.
15. Geophysical phenomena and the activity of *Otus asio*. Aug. 24, 1942. 4 pp.
16. Nighthawk activity and the lunar cycle. Oct. 16, 1942. 2 pp.
17. The moon and *Strix*. November 10, 1942. 2 pp.
18. Weight variation in *Otus asio*. Dec. 24, 1942. 3 pp.
19. Plants of Denver and vicinity. May 31, 1943. 15 pp.
20. The bird's electrostatic field. Oct. 20, 1943. 1 p.
21. Bioelectronic observations. Nov. 23, 1943. 3 pp.
22. Plants of Denver and vicinity (First supplement). Feb. 22, 1944. 3 pp. Warsaw.
23. Behavior of the eastern Screech owl (*Otus asio naevius*). Mar. 24, 1944. 7 pp.
24. Bioelectronic observations II. June 8, 1944. 4 pp.
25. The creeping willows of the Central Rocky Mountains. [illustrated by an actual leaf of each species, fixed in place in the text by gummed tape]. July 7, 1944. 9 pp.
26. The red Castillejas of the Central Rocky Mountains. Dec. 13, 1944. 3 pp.
27. Robotized research. Dec. 30, 1944. 3 pp. Warsaw.
28. Bioelectronic observations III. [notes on birds and electrical conductors, and the earth's electrical field]. March 15, 1945. 4 pp.
29. Gray's Peak and vicinity. July 11, 1945. 3 pp.
30. Carices of a Colorado mountain lake. [Lawn Lake, in Rocky Mountain National Park; several new taxa are described]. Aug. 4, 1945. 3 pp.
31. Plants of Denver and vicinity (Second supplement). Carices on Mount Elbert [Describing *C. elbertiana*, sp. nov.] Nov. 10, 1945. 3 pp.
32. Bioelectronic observations (IV). Dec. 18, 1945. 5 pp.
33. A study of the spectacled owls, genus *Pulsatrix* (continued). Mar. 15, 1946. 13 pp., 1 plate.
34. The Rocky Mountain Flora. I. The Willows. [Key to the species, and several new taxa]. October 4, 1946. 11 pp.
35. Bioelectronic observations, V. Irradiation, Vitamin D, Preening, and Anting. Dec. 27, 1946. 2 pp.
36. Additional notes on Rocky Mountain plants. [on *Salix monticola* and relatives]. Jan. 30, 1947. 2 pp., 1 plate.
37. The Rocky Mountain flora II. New varieties. [in willows] May 29, 1947. 4 pp.
38. The Rocky Mountain Flora III. *Carex uncomphagre*. [With additional notes on *Eleocharis palustre* near Leadville, *Carex haydenii*, claimed to be what has been called *C. emoryi* in Colorado, a peculiar form of *Carex nebrascensis*, and a discussion, *Le chat-Huant de Cayenne*, of the Haitian Barn Owl]. Oct. 31, 1947. 4 pp.
39. Bioelectronic observations. [On birds and radio towers, and the electrical capacities of feathers]. Dec. 17, 1947. 2 pp.
40. Embryo taxonomists. Feb. 27, 1948. 4 pp.
- 40a. A Flora of Alaska. (Review of Hultén, 1941). July 10, 1950. 2 pp.
41. The Rocky Mountain Flora IV. Salices monticolae. June 25, 1948. 4 pp., 1 plate.
42. Bioelectronic observations VII. [Electron flow. I. A possible mechanism of sunspot influence; II. An influence on zonal distribution.] Oct. 1, 1948. 4 pp.

43. Bioelectronic observations VIII. [The Organ of Corti and what birds hear.] Feb. 20, 1949. 3 pp.
44. On the physiology of territoriality and homing. [Radiation, adaptation, territory and homing.] June 5, 1949. 3 pp.
45. The feather as a detector of radioactivity. Aug. 20, 1949. 3 pp.
46. Factors related to plumage care of birds. Nov. 12, 1949. 1 p.
47. Nocturnal responses of birds to light. Jan. 20, 1950. 4 pp.
48. The Rocky Mountain Flora IV. Plantae monticolae. [Notes on *Salix*, *Equisetum palustre*, and observations on radiation and pigmentation in flowers, birds, and steppe shrubs.] Mar. 6, 1950. 3 pp.
49. The sharp point [Musings on function of sharp points in plants and in birds in relation to electrical discharges.] May 31, 1950. 2 pp.
50. The post-juvenal molt of the Northeastern Screech Owl. Aug. 10, 1950. 2 pp., 1 plate.
51. The Rocky Mountain Flora V. *Carex* species. Sept. 15, 1950. 2 pp.
52. A note on *Rhea americana*, including thinking. Oct. 20, 1950. 2 pp.
53. Testing the film on the feather. Nov. 25, 1950. 2 pp.
54. Thermodynamics of the desert spine. Jan. 25, 1951. 3 pp.
55. The six journals of Mr. Peale, Naturalist, from the journals of Titian Ramsay Peale. Mar. 31, 1951. 3 pp.
56. An indicator of mass energy flow in the environment. Sept. 29, 1951. 2 pp.
57. Demonstrating the glandularity of the feather. Oct. 10, 1951. 4 pp.
58. Some fundamentals of the feather. Apr. 10, 1952. 8 pp.
- 58a. Gas conversion by the feather. July 20, 1952. 1 p.
59. The summit plants of Mount Evans. [with a list of altitudes of the 14,000-foot peaks]. June 9, 1952. 5 pp., 1 plate.
60. Some fundamentals of the feather, II. Aug. 22, 1952. 6 pp.
61. The Tweedy Willow and evidence of Chrysanthemum stock in Colorado. Oct. 2, 1952. 4 pp., 1 plate.
62. Some fundamentals of the feather, III. Nov. 1, 1952. 2 pp.
63. Some fundamentals of the feather, IV. Dec. 15, 1952. 5 pp.
64. The Rocky Mountain Flora VI. *Carex* species. [*C. appropinquata*, *C. senta*, *C. pachystachya*, *C. oreocharis*.] Jan. 12, 1953. 2 pp., 1 plate.
65. Some fundamentals of the feather, V. May, 1953. 2 pp.
66. The *Carex* species of Colorado. [An interesting attempt of providing a key with coding of characters, such as one would use in a computer-generated multiple entry key]. August 31, 1953. 38 pp.
67. Monticolae: Historia naturalis. [Discussions on various alpine species, notably *Phippisia* and *Juncus biglumis*.] Nov. 14, 1958. 4 pp.
68. The exterior bird, II. Dec. 1960. 2 pp.
69. Biological water factories. [An intriguing discussion of the synthesis of water by organisms, and the potential in nature for providing water in outer space travel.] Aug. 31, 1961. 2 pp.

70. Some laws of the evolution of the external ear in Vertebrates. [translation of the review by V. D. Ilychev, in *Zoolog. Zhurnal*, of Kelso's publication in *Wilson Bull.* 52:24-29. 1940, on his discovery of the dependency of the external ear structure in birds on the environmental conditions in the geographic zones.] May 19, 1962. 1 p.
71. A niche of naturalists. VIII, IX, X. May, 1964. 8 pp.
72. A lesson in *Poa*. [notice of collecting *Poa abbreviata* on Mosquito Pass, named by Yurtsev]. 1958. 1 p.
73. Non-selective elimination [translation by Kelso of the summary chapter of *Evolutionary Ecology of Animals*, by S. Semenovich Schwartz. Sverdlovsk, 1969]. 1970. 10 pp.
74. Vegetation classification: conclusion [Kelso translation of the final chapter of V. D. Alexandrova 1969. *Classification of Vegetation*, pp. 233–236.] Dec., 1970.
75. On plumage quantity in Birds. 2 pp. [discussion of the paper by Frantisek J. Turček. 1965. *Ekologia Polska*, Ser. A, XIV (32):617–634, in English]. 2 pp.
76. The fruit, berry, and seed crops of trees and shrubs, and their biocoenotic significance. [translation of pp. 127–133, A. N. Formozov. 1964. *The Geography of Forest Trees, Shrubs, and Berry Patches*. Moscow Naturalist's Society and Institute of Geography, Akad. Nauk USSR, Moscow.
77. War, by Norman [L.] Russell. [Evidently a page of comments on Russell's book, *Ethics, Killing, and War*]. 1 p.
78. [Translation of] Evolution in the light of cybernetics. By I. I. Schmalhausen. 9 pp., typescript.
79. The *Carex* continuum. [Notices of three papers: Hermann 1970, Egorova 1966, and Popov 1970, with comments, e.g. our *C. stenophylla* ssp. *eleocharis* is the Asian *C. duriuscula*.] June, 1971. 1 p. + 3 plates.
80. Not located.
81. [Caustic comments concerning taxonomy and faculty at Rocky Mountain institutions, a few paragraphs overlying part of a letter by Ernest C. Smith]. Sept. 11, 1947. Clipped to the leaflet is a complete letter of January from Smith, answering criticisms. Also clipped is an editorial by E. O. Wilson on The Plight of Taxonomy (see above) which evidently meant a great deal to Kelso.
82. [Translation of the summation by Academician I. I. Schmalhausen on cancerous tissue, in *Regulation of morphogenesis in individual development*. 1964, "Nauka", Moscow]. 1 p.
83. *At the Borderline of Life*, by S. M. Uspenskii. 47 typewritten pages (pages 18–69 of *Na prdele zhizni*, 1959) Translation by L. Kelso, 1970-1971 (Boys, birds, and bears on Bennett Island, or Mishka – get lost!). Apr., 1972. 47 pages, typescript.
84. [Translation of A. B. Syrkin, *On the possible roles of free radicals in carcinogenesis*] *Uspenskhi Sovremennoi Biologii* 49:305–319. 1960.
85. Electron spin resonance charts [in birds]. Apr., 1972. 3 pp. Warsaw.
86. The *Carex* continuum. [5 pages, typescript. Arguments about the status of some American vs. Asian species, and complaints about his papers being overlooked.] Apr., 1972. 2 pp., 3 plates.
87. [Translation of concluding chapter of] V. D. Ilichev. *Bioacoustics of Birds*, pp. 247–253. Apr., 1972. 8 pp.

88. The *Carex* continuum. [continuing the discussion of western American/Asiatic vicariads]. June, 1972. 1 p., 2 plates.
89. Growth of a genus [translation by L. K. of] T. V. Egorova, 1966. Carices USSR, species of subgenus *Vignea*. 17 typewritten pages.
90. Endogenous water. [translation of pp. 222–223] B. B. Vartapetyan. Molecular oxygen and water in cell metabolism. *Molekulyarnyi kislorod i voda v metabolisme kletki*. 253 pp. “Nauka”, Moscow.
91. Plattdeutsch, I. [Caustic comments arising from paragraphs about Ivar Tidestrom in Intermountain Flora, Vol I. Actually a diatribe against Frederick V. Coville and Francis Ramaley.] July, 1973. 2 pp. + 2 illustrations of *Carex divisa*.
92. Plattdeutsch, II. The Hasty Pudding Flora. [Caustic comments on illustrations of four species of *Carex* in Zwinger, A., & B. Willard. 1972, *Land Above the Trees*.]. Oct., 1973. 1 p. text, 3 *Carex* illustrations.
- 93-98. Not located.
99. [Translation of] Petr Petrovich Semenov-Tyan-Shanskii. *Memoirs, Vol. 2. Puteshestvie v Tyan-Shan*. 1946 ed. Moscow. 48 pp. 1975. Appended with this is the vita of P. P. Semenov, from Great Soviet Encyclopedia, condensed, and other sources. 1p.
- 99b. [Translation of] P. P. Semenov. To Interior Tyan-Shan, and end of a journey. 108 typewritten pages. Ibid. 1976. [These are very important historical items possibly never translated anywhere else.]
100. [review] On color change in the Japanese Crested Ibis [*Nipponia nippon*]. A new type of cosmetic coloration in birds. Misc. Reports, Yamashina Institute of Ornithology 6:54–72. March, 1974.
101. [Review] *The Owls of the World*, J. A. Burton, ed. Pete Lowe, publ. Eurabook, Ltd. John Rignall, illustrator; 4 authors, 14 chapters. Typescript. July, 1974. 2 pp. Warsaw.
102. Ecological aspects of food transportation and storage in the Corvidae, II. (with F. Turček). June, 1974. 6 pages.
103. Methods and strategies in taxonomic research. [Review of] Ernst Mayr. 1971. *Systematic Zoology* 20:426–433. 2 pages.
- s.n. [Review] Reliability and unreliability in scientific biography. One page. *Dostovernoe i nedostovernoe v biografii uchenogo*. B. Kedrov. *Priroda* 1973(3):88–94.
- s.n. Untitled. List of papers presented at the VI All-Union Ornithological Conference, Moscow, 1–5 Feb., 1974. Translation, 8 pp.
104. [translation of] *Questiones geobiologicae*, 10: 8. Birds as indicators of radioactivity. [in] Tureck, F. J. *Birds as Biological Indicators*. July, 1974. Typescript, 3 pp. Warsaw.
105. A corrected list of Colorado Plants. [Typescript, sources not mentioned, but evidently this was produced from the existing books, records and reports available to Kelso. This evidently is the first checklist of Colorado plants since that of Porter and Coulter (1874)]. Oct., 1974. 71pp.
106. What is classification? [translation of V. D. Aleksandrovna. Classification of vegetation. Principles of classification and classification of various phytocoenological schools. 1969.] 11 pp.

107. The bird least likely [notes on the kiwi, from conversations with F. D. Robson, curator of the acclimatization farm, near Napier, N.Z. in 1947]. 1975. 5 pp.

Numbers of the *Leaflets* not yet accounted for: 80, 87, 90, 93, 94, 95, 96, 97, 98
(See Peters' check list of birds of the world that includes 25 of Kelso's papers).

Articles

- 1929a. A new *Castilleja* from Colorado. *Madroño* 1:241-242. [*C. flavoviridis*]
1929b. The English Sparrow and the Western Horned Owl. *Condor* 31:128.
1929c. Barn owl (*Tyto alba pratincola*) breeding in sColorado. *Auk* 46:386. [also *Bird Lore* 31(3):189].
1929d. Notes on the western horned owl. *Bird-lore* 31:113–115.
1930a. Carolina rail (*Porzana carolina*) wintering in Colorado. *Auk* 47:247.
1930b. Some night observations on the western horned owl. *Condor* 32:126–127.
1931a. Some notes on young desert horned larks. *Condor* 33:60-65.
1931b. Notes on birds of Prowers County, Colorado. *Oölogist* 48:78–84.
1931b. (with Francis Ramaley). Autumn vegetation of the foothills near Boulder, Colorado. *University of Colorado Studies* 18(4):239–256.
1932a. A note on *Anemopsis californica*. *Amer. Midl. Nat.* 13:110–113. [var. nov. *subglabra*]
1932b. A new *Salix* hybrid. *Rhodora* 34:67. [*S. glaucops x petrophila*]
1932c. [with C. Cottam and W. H. Ball] The Louisiana Heron in the Washington, D.C. region. *Proc. Biol. Soc. Wash.* 4:207–208.
1933a. Note on *Glyceria neogaea*. *Rhodora* 35:225–226. [*G. fernaldii* is a synonym]
1933b. Notes on Rocky Mountain plants.[with E. H. Kelso]. *Rhodora* 35:347–348. [*Atragene columbiana* f. *albescens*; *Eriogonum annuum* f. *roseum*]
1934a. Notes on Rocky Mountain plants. [with E. H. Kelso] *Rhodora* 36:195–196. [*Salix pseudolapponum* var. *subincurva*; *S. brachycarpa* var. *alticola*]
1934b. New variety of *Glyceria grandis* and key to its allied species. *Rhodora* 36:264–266. [var. *komarovii*]
1934c. [discussion of *Strix perspicillata*, an owl]. *The Auk* 51:234–236.
1934d. [*Asio stygius robustus*, subsp. nov.] *The Auk* 51: 522.
1935a. Notes on Rocky Mountain plants. [with E. H. Kelso] *Rhodora* 37:226–227. [*Castilleja pulchella* var. *acutina*; *C. brachyanthera* var. *subinflata*, *Lathyrus incanus* f. *albidus*, *Artemisia pattersonii* var. *glabrior*; *A. scopulorum* var. *aggregata*,]
1935b. A new species and two new varieties of *Glyceria*. *Rhodora* 37:262–263. [*G. kashmiriana*, *G. tonglensis* var. *honshuana*, *G. striata* var. *mexicana*]
1935c. Bird notes from Fall River, Larimer Co., Colorado. *Oölogist* 52(2): 14-19.
1936. [*Rhinoptynx clasmator oberi*, subsp. nov. E. H. Kelso]. *The Auk* 53:82.
1937a. [*Glaucidium jardinii costaricanum* and *Strix indranee rileyi* E.H. Kelso [owls]. *The Auk* 54: 304.
1938a. Food of the burrowing owl in Colorado. *Oölogist* 66:116–118.

- 1938b. A study of the screech owl (*Otus asio*). 331 pp., 1 illustr., maps. Thesis, Cornell University.
- 1939a. Summer food of the borrowing owl in Colorado. *Oölogist* 56:41–43.
- 1939b. The violet-green swallow at the nest. *Oölogist* 56:90–92.
- 1939c. The food habits of prairie dogs. *USDA Circular* No. 629. 15 pp.
1955. The exterior bird. 51 pp. Intelligencer Printing Co., Lancaster, Pa. [in Library of Congress QL698.K36]
1978. (with R. J. Clark and D. G. Smith). Working bibliography of owls of the world, with summaries of current taxonomy and distributional status, with a foreword by Kai Curry-Lindahl. xiii, 319 pp. Washington: Raptor Information Center, National Wildlife Federation.

About Leon Hugh Kelso

- Anonymous. 1982. In Memoriam: Leon Hugh Kelso. *J. Field Ornithology* 53:362.
- Barnhart, J. H. *Bibliographic Notes on Botanists*. 3 volumes.
- Ewan, J. A. 1981. *Bibliographic Dictionary of Rocky Mountain Naturalists, 1682-1932*. Regnum Vegetabile 107. 153 pp.
- Peters, J. L. 1940. *Check-List of Birds of the World. Vol. IV*. [citations of Kelso's publications on owls] Harvard University Press.

AUTHOR'S ACKNOWLEDGMENTS

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ABOUT THE AUTHOR

William A. Weber was born in New York City in 1918 and died in Longmont Colorado in 2020. He began to study bryophytes in 1933 when he was shown a small collection by his high school biology teacher, Grace Esternaux (born 1901), who had taken a bryology course at Cornell University. His first botanical paper was published in 1940 (see below). He was a lichenologist, bryologist and vascular botanist with extensive field experience in the United States (including Alaska and Hawaii), Canada, Mexico, Costa Rica, the Galapagos Islands, Canary Islands, Chile, Australia, Papua New Guinea, Europe, Nepal, and Russia. His wide ranging field investigations gave rise to a great interest in phytogeography. His long-standing work on the flora of Colorado culminated in the 2012 two volume edition of his Colorado Flora (the latest of many editions), a work that included keys, phytogeographical and historical background material and stories from the field. His publications included not only botanical works, but also biographical works on Wilhelm Suksdorf, T. D. A. Cockerell, and C. C. Parry. He had a broad base as a character actor, choral singer, recorder player, and loved to sing Gilbert and Sullivan at the drop of a hat. He was a unique specimen.



Selected Publications

Flora

1976. *Rocky Mountain Flora*. Fifth edition. Colorado Associated University Press, Boulder, Colorado. xii, 479 pp., 375 Illustrations. 32 color plates. ISBN 87081-068-5
1998. [with N. Corbridge]. *A Rocky Mountain Lichen Primer*. University Press of Colorado. 47 pp. 72 plates. ISBN 0-87081-490-7
2007. [with Ronald C. Wittmann]. *Bryophytes of Colorado: Mosses, Liverworts, and Hornworts*. Santa Fe: Pilgrims Process, Inc. 231 pp., 8 plates. ISBN 978-09790909-1
2012. [with Ronald C. Wittmann]. *Colorado Flora: Eastern Slope, A Field Guide to the Vascular Plants*. Fourth Edition. Boulder: University Press of Colorado, 555 pp., 104 line drawings, 1 map, 4 tables. ISBN 978-1-60732-140-8
2012. [with Ronald C. Wittmann]. *Colorado Flora: Western Slope, A Field Guide to the Vascular Plants*. Fourth Edition. Boulder: University Press of Colorado. 532 pp., 108 line drawings, 1 map, 4 tables. ISBN 978-1-60732-142-2

Biographical works

- T. D. A. Cockerell: A fifty-year project comprising four volumes documenting his life and works:
- *Theodore Dru Alison Cockerell, 1866-1948* (1966) [a bio-bibliography].
 - *Theodore D. A. Cockerell: Letters from West Cliff, Colorado, 1887–1889* (1976).
 - *The American Cockerell: A Naturalist's Life, 1860-1948* (2000).
 - *The Valley of the Second Sons: Letters from Theodore Dru Alison Cockerell written to his sweetheart and her brother about his life in West Cliff, Wet Mountain Valley, Colorado, 1887-1890.* (2004).
- C. C. Parry
- *King of Colorado Botany. Charles Christopher Parry, 1823–1890* (1997).
- Wilhelm N. Suksdorf:
- *The botanical collections of Wilhelm N. Suksdorf, 1850–1932.* Research Studies, State College of Washington 12, 1944:51–124.

Publications in plant geography

1962. Environmental modification and the taxonomy of the crustose lichens. *Svensk Botanist Tidskrift* 56:293–333. 1 colored plate.
1965. Plant geography in the southern Rocky Mountains. [in] *The Quaternary of the United States.* H. E. Wright, Jr. & David G. Frey (eds.), pp. 453–468.
1966. Environmental modification in crustose lichens, II. Fruticose growth forms in *Aspicilia Aquilo*, *Series Botanica* 6:43–51.
1977. Environmental modification and lichen taxonomy. Chapter 2, pp. 9–29 [in] Seaward, M. R. D. (ed.), *Lichen Ecology*. Academic Press.
2003. The Middle Asiatic element in the Southern Rocky Mountain flora of the western United States: a critical biogeographical review. *J. Biogeography* 30:649–685.
2018. Global Plant Distribution and Continental Drift: Two moss species. [with Linna Weber Müller-Wille]. *Acta Botanica Weberi* ABW 1. 24pp.

THE EDITORS

Linna Weber Müller-Wille

Eldest daughter of Bill Weber, Linna Weber Müller-Wille specializes in scientific translation and editing, especially on arctic topics in the natural and social sciences. She grew up in the herbarium and in the field collecting with her father, and was often involved in various of his projects, including proof-reading and copy editing of all editions of the Colorado Flora, starting in 1949 up through 2012. (See also *Acta Botanica Weberi* 1, p23-24, 2018.)

Ragnar Müller-Wille

As Linna's son and eldest grandson of William A. Weber, Ragnar Müller-Wille grew up in an academic/scientific environment that fostered openness, curiosity in all subjects, truth and accuracy. Among his varied activities, Ragnar has in one way or another been involved with various aspects of publication since childhood.