



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

The Hunt Institute is committed to making its collections accessible for research. We are pleased to offer this digitized item.

Usage guidelines

We have provided this low-resolution, digitized version for research purposes. To inquire about publishing any images from this item, please contact the Institute.

About the Institute

The Hunt Institute for Botanical Documentation, a research division of Carnegie Mellon University, specializes in the history of botany and all aspects of plant science and serves the international scientific community through research and documentation. To this end, the Institute acquires and maintains authoritative collections of books, plant images, manuscripts, portraits and data files, and provides publications and other modes of information service. The Institute meets the reference needs of botanists, biologists, historians, conservationists, librarians, bibliographers and the public at large, especially those concerned with any aspect of the North American flora.

Hunt Institute was dedicated in 1961 as the Rachel McMasters Miller Hunt Botanical Library, an international center for bibliographical research and service in the interests of botany and horticulture, as well as a center for the study of all aspects of the history of the plant sciences. By 1971 the Library's activities had so diversified that the name was changed to Hunt Institute for Botanical Documentation. Growth in collections and research projects led to the establishment of four programmatic departments: Archives, Art, Bibliography and the Library.

San José, January 2, 1982.

Dear Bill:

Many thanks for your good Dec. 28 letter that just arrived. I hope you got my letter with the Icelandic transfers and that you have found out, that you actually have several of even the Icelandic letters on the word processor, perhaps not ð which even I have replaced with simple d when needed, except when I now use my Icelandic ball, and æ can be produced either badly by using ae, as Fægri has done for a lifetime, or reasonably by moving a and e closer: æ which may not be possible with a word processor. But you can easily make þ from p and b, and of course you have an umlaut, if you have a quotation mark: Á or á is almost as good as Á and á, and Ö and Ö are hardly different from Ö and Ö...I made a mistake not to mention the apostrophe before I used it, excuse me. And if you need a Danish-Norwegian Ø there is of course the ø or Ø, exactly as I myself get away from not having a dollar sign on this ball so I write \$! Hope you found it easy to help me with the manuscript, though I really hesitated to ask you to use your time for me, because I realize that you are kept so busy with things that are more important. But I asked because you proposed this and because I realize that you always are and have always been as the busy man Ben Franklin talked about in case you needed to get something done, that is your constant hallmark.

Hope the wise men who select for the lectureship realize what you are and what you have done, despite your own personal lack of an ability to go to the market and shout about your achievements, but I know how small the Boulder crowd actually is and how little they realize about the basic philosophy of a university and how science works...but they know politics well. I only would wish that I myself had a possibility to get for you a foreign, preferably Scandinavian honour of some kind, because nobody in American botany has ever fit as well in with their philosophy as you do, one can always dream, and you are, in my view, much better fit for an honorary degree there or for foreign membership in an Academy than those small men with loud voices they hear about. If I see a chance...one can always ~~xxxx~~ dream, and that is what I believe I have been best at, as was the great chemist Kekulé of benzene ring fame (I am sure you would like to read the small book on The scientific life, by John R. Baker, Allen & Unwin 1944, where he tells among others about Kekulé and his daydreaming that made great science). So perhaps we will sometime find something greater than Boulder can give in scientific honors?

I continue to hope that you and Pat will blow the whistle again and so loudly that even Holloway cannot stop it...when you at long last have gotten two great men at the top...but nothing will come out of anything if there is not a strong support and no thought of any swallowing of the whistle again. I was glad to read about what Phil Becker told you and did, I only met him twice, as you know, but got a very good impression of his honesty...so I was shocked when I, two years after, discovered that my so-called pension was considerably lower than he had told me, though it ought to have increased during that interval...and wrote to him as benefit officer, and got a meaningless answer from a Japanese held a year later who referred me to the pension fund! Phil has probably never gotten the letter, and then never had an opportunity to look into the matter, which also was out of his hands when he had left the place, as the Japanese told me at last. But he is one of those I would trust to be with you on my behalf...and also Bill Mayer, who is like a bulldog that never lets loose if he gets his bite into something he feels is wrong and must be corrected...that was the reason that I had him on my board for the department for four years, and that was the reason for that little Smith did not want to see him there.

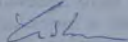
I have not again heard from Bill Carver, who left for a vacation before Xmas and has hardly returned yet. And he is a busy man. But though I can see your points, I think you ought to accept if he should find it wise to print your book, because it will hardly get into the hands of the world outside Colorado or America if it is printed in Denver...and I know that it is going to be in the same luxury class academically as the Greenland flora and the Alaska flora...and of course it must be printed in a pocket size and sold for a reasonable price, which they also can do. If I have any criticism of you it is simply that you tend to be much too modest, perhaps it is caused by that you came so young to Boulder and never got into the habit to sit on others, as did the very small Lincoln Constance, who pushed the mormon to Harvard despite his smallness, and as does the not too great Peter Raven, who sits in the National Academy because, as they said when they elected him, that he had stood for continental drift! An idea that he had fought me for for years, until suddenly that he heard my proposal for the Boulder Congress to have this important idea as one of the symposium subjects...which he jumped upon. You have never done so rotten things and even tried to avoid using ideas that were meant for your use. But the flora ought to be made known the world over.

When you get back to Boecheera, let me help, and also when you get into the Senecio groups that need changes, though I may only be able to tell you about chromosomes. You do not need to bring all the complex into its correct homes, the Australians may like to do their groups after a generation, but if you do both the American and Eurasiatic ones, you have done more than most others. I have not seen Benson recently, but understand what you feel about his Ranunculus, because it is his style not to have clear concepts of any taxonomy...I have recently done some publishing on one of his greatest mistakes from his youth that even Stebbins fell into: the accepting of the foolish Bentham conclusion that the Californian Lophochlaena belonged in the Arctic Pleuropogon, the first actually a genus of Meliceae, the last of Glycerieae, both perhaps weak genera but certainly not congeneric with each other though.

We are having one of our great rains now, and since the temperature here is only about 5 C there will probably be more snow in the mountains around us during the night. But I will have to stop here because I must complete a letter to an old colleague at St. Thomas in the Virgin Islands who has asked me for help in locating cytological informations about a complex in Hibiscus that he and his garden loving friends are planning to breed. It is a complex of high chromosome numbers, probably a hybrid complex with no really known wild relative, *H.rosa-sinensis*, and the numbers already published are too many and too complex to be only caused by hybridization, so probably those counting them were confused with the taxonomy or had no floristic eye, as is so typical of so many of our colleagues who do not hesitate to be cocksure about everything and never admit their own mistakes. I have seen that also you have had a good deal of snow and cold...but that is normal for this time of the winter...though Winnipeg now seems to be worse than we ever experienced it....And something very bad is going on in Canada, even before the French may cut loose from the British, since they now have to pay 30 cents for an ordinary letter!

All the best for the New Year to you all from us all here,

as ever,



P.S.: Thanks for the Manitoba Syllogeus! It showed no sign of that we had collected some 30 000 samples there, also of the most rare plants, and did not mention that the maps they used were started by Doris...but we were not Canadian or Canadian educated of course and had ideas that still are a century ahead of them. A place we never understood that we came there and were glad to get away from...as Boulder later.

Ciliaria austromontana ()

Ciliaria transjurica (Roth.)

Ciliaria funaria (Small)

San José, January 5, 1982.

Dear Bill:

Many thanks for the good letter with the proofs of the Icelandic paper that you want to let the word processor correct before you send it further. Yes, I found one mistake that was not mine, on the first page: Adamson for Adanson, and then there may be a possibility to make a small change of a,d into .d by a pen in another place, if you so feel, but it is not necessary. And, in order to silence those who always follow foolish rules, perhaps it would be wise to add the type species reference for the new genus *Arctogentia*? You are the judge of that. But many thanks for the help, as so often before.

I am glad to see that you like the *Saxifraga* changes...it has been foolish to ignore Haworth's wise splitting of this arch-complex for 160 years, because he had everything correct even if we put modern evolutionary views behind our ideas. And Small was also more right than most of his critics, though he goofed when it came to some of the groups...though that is no reason to throw out even the child with the bathwater that had become a little dirty; even he was no more of a prophet than I and you or even some of those who regard themselves greater and want and have even tried to send us to where the pepper grows...which is the place to where they will be sent at last, of course. I agree with you, and Small's, arrangements for Colorado on most points, even that of *Spathularia* for *foliolosa*, which is one of the genera I accept for Iceland...and it is not *Saxifraga* s.str., which is based on the type species *S. granulata* L. In my view, *Leptasea* is based on *L. aizoides* and thus must be a small genus, perhaps related to *Antiphylla*, which comprises in Iceland only *oppositifolia*. The *bronchialis* group certainly is a distinct taxon that Haworth placed as *Ciliaria bronchialis* (L.) Haw. And *odontoloma* belongs, as far as I can see without having Haworth's review at hand, in *Micranthes* with *nivalis* and *tenuis*...or am I mistaken? There seems to be some nomenclatural doubt there, however, since the American species may fall inside the species *punctata* of Flora SSBR, perhaps as ssp. *arguta* (Engl. & Irmsch.) Hultén; but it may be safer to hold on to the name still used for it at the species level as far as we do not know exactly that it and the other taxa concerned are actually conspecific and not separated by a sterility barrier. As a matter of fact, if the reports available are exact as far as chromosome numbers go, then there may even be some doubt as to their generic relationship: Sokolovskaya has reported 28 and c. 70 or c. 76 chromosomes for *punctata* from Siberia, Packer 1964 found 28 in his Alaskan or Albertan material so named, which probably is what Hultén called ssp. *arguta*, whereas Moore has reported, for *odontoloma* from British Columbia, the number 48, which is not from the same basic number. So perhaps there is something in this that requires more thinking, though for the time being I am inclined to follow the advice of Heller (1912) and call the species *Micranthes odontoloma* (Piper) Heller. Hope this gets you into some still better speculation that leads directly to a satisfactory solution.

I am pleased with your remarks on the Rydbergian splitters and giving them a second thought...or rather not do that as you feel the great men of the east must think in all their proclaimed fairness and total lack of the logic and honesty that taxonomy and all other sciences require. But have they, and many others, here not always followed the good (?) old Colorado "law": let us give him a fair (sic) trial and hang him? I do not understand why they have not hung you long ago, but it may have saved you that you are born here...though you are still not all by far so they may still have their chance! Or perhaps you will survive history, they not?

All the best,

San José, January 12, 1982.

Dear Bill:

Thanks for the help with the Phytologia paper, hope to hear soon from Moldenke. And also thanks for four cards, two from each of January 6th and 8th. I answer them in order:

As far as *Erigeron* is concerned, we never recognized the species as such in Canada, except those from the north, because we always mixed them with *Aster* or *Solidago* or other taxa of these ill-defined complexes. The type species seems to be *E. acre* L. (his spelling!) picked by Britton & Brown 1913, and not *uniflorus* L., picked by Hitchcock & Greene 1929 and included in the list of Linnaean types by Briquet in his 1935 Code, cf. the Index of Species plantarum edited by W. T. Stearn and published by the Ray Society 1959. I believe this is one more of these problems that are caused by unsatisfactory training of American so-called taxonomists for much too long time, and the uncritical attitude of all those trying to let us believe that they are properly trained, which they are not, including Cronquist and others trained at Logan and then believing that they know everything. But I know too little about *Erigeron* to help you, though I can always refer you to details given in the Flora SSSR, where they still are not critical enough of the Asiatic taxa, it seems. I have always accepted *Conyza* and *Stenactis* as distinct, and am in no doubt that you will do the same with *Lonchophyllus* etc. But since most of these taxa even at the generic level have not been studied experimentally or even properly cytologically, the new methods are still of little assistance, I dare to say. Good luck!

You are right, *läkare* or *laeknir* is a word that came into the Slavic languages from Scandinavia, perhaps even as late as Charles XII, but probably already with the vikings, as did *fältskär*. It has been in the Nordic languages longer than the specialists in Iceland know, and came to there through Anglosaxic laecnian from Celtic.

I agree with you on *Symphoricarpos*, with which we had difficulties already in our 1961 chromosome list. Hope you solve that problem finally. I doubt that new fieldwork will solve much there, or even cytology, but expect that the main question is that of definition of the limits of the variation or variations connected with a proper typification of the available names. Though you may be right that more fieldwork could solve some of the problem, which was not simplified by the German original Suksdorff. It is a pity that everybody who thinks he knows plants is permitted to describe new taxa at levels they do never understand how to define, but that is our fate for having mixed the terms taxonomist and florist...and even the latter seem to dominate the field of writing flora manuals, with the results that even the flora by Lid is terribly insufficient. Not to mention some of those from here.

Sorry about the duplication of the *Saxifraga monticola* transfer, I discovered the mistake soon after the book was published, of course. But as you say, this happens. Even in the best of families. So try to forgive!

Pilosella and *Chlorocrepis* have been well known for a long time in the areas where they grow, almost, and I see no reason why we should have explained their differences when making some small correction of a name for one taxon reacing the Arctic. Though I can see your point when thinking about all the alphabets we work with in political botany in America, where you are simply stabbed if you dare to deviate from those who think they are great because they work at a large institution! If we had done the discussion in our transfer paper that time, and other times, we would have had to write several books of the size Hylander did for his checklist. And we neither had time nor could find a publisher for that, Bot. Not. limited length.

Nobody has an easy access to Polyakov here, though we have some of his papers and so could help you some little, for instance by looking up the species that interest you. But you have a proper review of most of his works on *Artemisia*, since I gave you a copy I got as a duplicate of Czerepanov (1973), where *Artemisia* with synonyms -s on pp. 57 - 60. Look through it and if there are possibilities that what we have may give more details that you need, order me to copy it for you!

We do not have the last two supplements of Kew Index, which I hope you have. I am trying to finish the nomenclatural-taxonomical review of the wheatgrasses and would like to know if there are new taxa of any of the genera involved, or, e.g., *Aegilops*, *Triticum*, *Secale*, *Dasypyrum*, *Eremopyrum*, *Heteranthelium*, *Henrardia*, *Hordeum*, *Griithopsis*, *Taeniatherum*, *Pecthyostachys*, *Agropyron*, *Leymus*, *Elytrigia*, *Elymus*...with proper references? I do not expect these to be many, though one never knows, and I have probably caught most of them already. But I need your help especially with four Russian transfers that I cannot locate here, and, in the case of two the first, information on their real belonging to some smaller group: *Elytrigia kotovii* Dubovik, *Elytrigia ninae* Dubovik, *Henrardia persica* ssp. *glabriglumis* (Nevski) Czopanzov, *Elymus boreochoctensis* A. Khokhr., and *E. magadanansis* A. Khokhr. I do not believe that it would be a great mistake if I never could include them in the list, though I would try to limit my omissions to a minimum...if possible. And do not spend much time for this.

We are enjoying nice weather now for some short time between the rains, but the disasters caused by the foolish building of houses where the clay has risen into hills have been terrible...and will not end soon because people continue to be foolish in their try to be different...or to get cheaper accommodation. We are having the longest coldspell we have seen here, with considerable frosts every night, so much of the tender plants are going away...but you seem to be getting a good deal of snow and frost, not to mention those further east and north, and the Europeans. The only ones who are not surprised seem to be the Icelanders, who told me in the summer that everybody there realized that we are heading for a cold period worse than ever in their history, because they believe in good science as that of the students of the Greenland ice and the Icelandic ocean ice pack and glaciers, and not in the desk science of the rich so-called scientists here who continue to babble about the greenhouse effect...as if man really had anything much to say about the climate with his very limited power. Iceland is the world's thermometer now as always before, and they have been having increasingly shorter summer since 1945 when the last warm period ended, and during the past few years the cold has increased very rapidly as had been foreseen in 1970...last year was the coldest since 1870 when observations began, and last spring came in July and the summer ended on September 1, when we had snow in all the mountains around Reykjavik...and two weeks later they had a foot of snow in all of northern Iceland. And all this winter has been colder than ever before there...exactly as all over Europe and America. The iceage is coming, that all those who know better than the greenhouse buffs understand. In northern Europe and Britain those specialists, who are pessimists, agree that it will come very rapidly when it comes, and that it will be here in a century or two...whereas the optimists say that it will warm up again somewhat and the real decline will not come until in 1000 to 2000 years! We will not see it, unfortunately, whatever group is right, but when will the "wise" politicians who think so little and know less, start to plan for the future, or will they just let the new hunger kill away the masses as before...forgetting that democracy now has given the latter facilities to fight back...and the white supremacists and crooks of the Boulder type have even been so foolish as to invent very effective methods to kill those who are poor or powerless...and these methods and weapons will soon be dominated, democratically, by those they were meant to check! It will hardly take more than a new generation before the blacks and mexicans here are in majority, and then they decide about the atomic bombs, not Reagan and his bigots. When this happens it will be bad for us botanists...but funny as long as it goes.

You know that Santesson has retired and that Nils Lundqvist is probably his successor

All the best, *ESK*

San José, January 28, 1982.

Dear Bill:

Try to forgive that I evidently forgot to thank you at once for your good help with the Triticeae from Index Kewensis...but you know that I am grateful nevertheless. Only sorry that you evidently do not yet have Supplement 16 for 1971-75, which was published in 1981, since some of the names I have trouble with may be there, though these are not many and certainly not important. When or if you get the supplement during the next few months, try to remember me again!

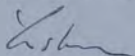
I agree with your Saxifraga division, but have a couple of remarks: The combination *Hirculus prorepens* is being done by us, L. & L., in the May number of Taxon in connection with reports of Manitoba chromosome numbers. And, as we pointed out in our Arctic & Alpine Research paper in 1971, the Colorado populations of *Hirculus platysepalus* are nothing more than a ssp. and should be so transferred: ssp. *crandallii* (Gandoger) W. A. Weber. We have *foliolosa* in *Spathularia* in the Icelandic flora together with *stellaris* and believe that is right. I like this splitting and am sure that so does also our good critic Yurtsev. who complained about our too collective *Saxifraga* in the Arctic Atlas, when we did not dare to do what we should have done already then. Or perhaps there was too much of other changes, I do not remember the reason for the hesitation. For your support I ought to mention that while *Spathularia* has the basic number 7, *Micranthes* has 10, and some similar indications of too little splitting seem to be available for some few others of Small's genera. But not many; and rarely did Haverth go too far or too short. I agree with your remark on *Saxifraga geum* which certainly is at least closer to *Robertsonia* than *Micranthes*...and I am sure that your guess that it may have been introduced to Newfoundland by ballast, hope you have the book by Lindroth where he claims, quite correctly according to Rouleau, that more than half the taxa of plants and insects and lower animals in Newfoundland came just with ballast!

By the way, when you mention that you and Vlad had discussed *crandallii* he had felt it was a species of its own. I disagree, but his concepts certainly are strongly affected by the Komarov school. of course, which ignored sterility barriers and put practically any geographical variation in the species group...and that even the younger Russian generation still does. How is he now, has he found any place that dares to accept him, except you who does not have the funds to help more than a little despite your great heart?

I am presently working on the wheatgrasses slightly, and would have liked to have Candargy's otherwise meaningless book from 1901 with all his transfers in keys so I could be sure that the identifications in the Kew Index are correct. I could probably get it through some of my librarian friends here, though they are all in the humanities, if I only knew what library here might have it. But it certainly is not all important to have a complete synonymy list, because that can never be done.

We are glad not to have been in Boulder when you got the 55 meter per second storm twice recently, hope your house was not damaged. And that you both are as well as can be and you filled with your new and progressive ideas as always.

All the best,



Did you really overlook one description
of the genus in 1935, p. 513, or he did?
Why should we spend space to compare that
description to that of typical *Arctis*, especially
for one who claims to know everything in 1947?

© USPS 1981



↑
Get in file

Dr. William A. Weber,
Museum,
Campus Box 218,
University of Colorado,
Boulder,
Colorado 80309

March 3, 1982. *5/12/82 page 29*

Dear Bill:

DISK 7

Thanks for the New combinations paper and for the jesuitic elegance with which you put the dagger into Rollins; I believe he was one of the "gentlemen" who did the same to me through the Smithsonian in 1972-3, still with impunity thanks to others' timidity, he and other mormons are no gods, only despicable bigot. I agree on almost all points, though I prefer the spelling Loeve rather than Love if the processor knows only English still. However, if you have the newest Taxon, we have already validated *Boechera retrofracta*, which is a diploid, fertile species rather than a triploid apomict as the Greenlandic *holboellii* s.str. ~~from Greenland~~. You may also drop *Negundo* a.ss. *interius* and *Plagiobotrys* sc. ssp. *penicillatus* (botrys is male!) because we make these transfers in Taxon in April, in press. There we also accept Haworth's *Leptasea tricuspidata*, but you are probably correct in using *Ciliaria*, which also has x:13...Your word processor became confused on p. 03!

I am sweating with *Triticeae* nomenclature still and waiting for galley's of the new Icelandic flora! As ever, *X*

Fedde Def
84.746.1974

San José, March 6, 1982.

Dear Bill:

Thanks for the card. My first proposal on the differences between *Gastrolychnis* and *Melandrium* was based on Flora Europaea, and I admit that it was not too clear. Perhaps the *Melandrium* key in Lid's flora may be a clarification, though there you also get *Elizantha* mixed in: *M. noctiflorum* & *viscosum*? But what about the following key based on Flora SSSR:

- 1a Styles 3; capsule dehiscing by 6 teeth.....*Elizantha* Rehb.
- b Styles 5; capsule dehiscing by 10 teeth.....2

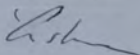
- 2a Flowers perfect; petals exceeding up to one and a half times the calyx..*Gastrolychnis*
- b Dioecious; petals one and a half to twice as long as calyx...*Melandrium* Roehl.s.str.

When put into context with other characters, dioecism certainly is a very important character, as are also size and hairiness of the leaves mentioned by Lid. Actually, the general characters of these groups are such that nobody with a trained eye ever could fail in distinguishing them into three groups...though the great American botanists of our times would probably not see anything and become mixed up!

Yes, I have heard about Grasses and Grasslands, because Dewey has a long and good review paper there, but I have not seen the book itself. Could you help me get a copy of Estes generic concept paper?

Another begging: In your good (yes, I mean it) Colorado list you accept the combination *Puccinellia lettermannii* and refer to Ponert 1974, but that reference is not in your bibliography. If you have it, could you copy the paper for me? I would like to see his reasoning, because I have found it difficult to draw the line between small alpine-arctic specimens of these two groups myself, for numerous reasons. According to my notes (and published in Yurtsev & Zhukova 1972) we have counted the diploid number 14 in a specimen that Defler & Snyder, s.n., collected on Hoosier Ridge and you helped me to identify years ago, but that is of no help for the generic decision, since the abbreviata group of *Poa* the plant may be related to, and the arctic-alpine "*Puccinellias*" (which I am sure must be called *Phippsia* because of the Code and the fact that these are groups that may be separated only by a single gene and hybridize easily) both include diploids. And both do not seem to have so distinct chromosome morphology that this could help, though one never ought to say never in such cases. Do you perhaps still have the specimens that I believe I left with you after we had decided upon their identity around 1970? or perhaps Kapoor "borrowed" them when we planned to mention this number in the 1971 paper? And do you have a specimen of *Poa kolymensis* Tzvelev, 1972, Bot. Zhurn. 57:646, which is very close to *P. lettermannii* according to Yurtsev & Zhukova, 1972, Bot. Zhurn. 57:60?

As ever,



San José, March 20, 1982.

Dear Bill:

Thanks for three cards and the copy of the grass paper by Estes & Tyrl on the generic concept of grasses. They, of course, dare not have any themselves. I did not expect anything new from those educated at Oregon State and continuing in Oklahoma, but I thought that when getting the idea of organizing such a symposium and summarizing the concepts, if any, they might perhaps have searched the immense literature from outside America with somewhat greater success. When having such teachers, can you ever expect that the new generation will cease to stab those who dare to have other ideas or to learn about your ideas and mine that differ so radically from the 18th century philosophy almost all American botanists still are brought up with? I should not ask: even your so-called student Dave Murray continues to act and write as if he never had seen or heard neither you or me or read even a small part of what he refers to of Hultén's works, but that is another story, much depressing though. However, they stumbled over my little IOPB symposium review from 1962 in *Regnum Vegetabile*, but could not be expected to know about my ideas about Triticeae that have not yet been published but come in the next number of *Biol; Zentralbl.*... and we must remember before criticizing them that the book and all its papers (except perhaps their own?) has been "in press" for three long years. But how could we hope that those who hardly have any fortitude, would dare say something different when vitriolic criticism could have come from the floor where sat such great specialists as Gould, Stebbins and you name them? I am glad to have this paper though, and much more so the long one on the Triticeae by Dewey, who again knows that his results are strictly supportive of the evolutionary view, though he dares not go further than Nevski did half a century ago...perhaps because evolution is not the accepted opinion of the Mormon group? Though I know that you and I do not belong to them, I am beginning to wonder who others are not....mormon-educated or brought up botanists are at least predominant in America today, and I believe it is very important to realize the religious attitude, if any, of even the scientists, rather than accept their perhaps foolish and biased ideas to avoid discrimination that seems to be proper mainly for immigrants from non-mormonic countries?

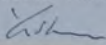
Thanks for the reference to Ponert's paper. I will try to find it and copy it and read it critically, because I wonder if he really has any good reason for moving *Poa lettermannii*, that I doubt he knows well, to *Puccinellia* (which actually must be moved into *Phippsia* if we mean something with our taxonomical treatments and nomenclatural typification). You told me that our material of *P. abbreviata* remains with Kapoor (I have tried repeatedly to get him to send the collection, but in vain as you know)...but is the same true for the fixed diploid specimen of *P. lettermannii*?

By the way, do you have our *Isoetes echinospora braunii* (L. & L. 6992 & 3) and *Nemopanthes mucronata* (L. & L. 6963) and other specimens from Lac Ouraeau in P.Q.?

I do not follow you on Viorna, are you dividing *Clematis* further? And good luck with *Boechera*...our philosophy coincides as you have known so long.

I am enclosing a copy of Rauschert's explanation of the Avenella case, one of many misunderstandings by Tzvelev, who may not know foreign languages well?

Do the wise or unwise men who protect the science at Boulder select you for the faculty honor? I wish I had a possibility to show you my appreciation through some greater foreign honor...one can always dream.

All the best, 

Áskell Löve,
5780 Chandler Court,
San José, Calif. 95123.



Dr. William A. Weber,
Museum,
University of Colorado,
Campus Box 218,
Boulder,
Colorado 80309

© USPS 1981

Digitized by Hunt Institute for Botanical Documentation

9 April 1982.

Dear Bill:

Thanks for the card. Sorry that you do not have the Lac Ouareau plants, but they are among those we left in Winnipeg. I am still trying to get a copy of Ponert's paper in which he transfers *Poa lettermannii* to *Puccinellia* to see his reasoning, though I am critical about this for several reasons.

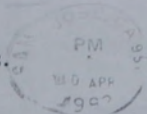
As to the splitting of *Clematis*, we have little experimental about the relationships of these groups, so either way is in order. Though I lean towards distinct genera, in view of *Atroгене* which to me seems strong morphologically and geographically, though I may of course be wrong. I do not find *Coriflora* to be a good name for old and incorrect *Viorna*, and wonder what you feel about *Ameroclema* or some other combination of the latter, which means simply branch? That would at least remember of *Clematis*, too.

Hope you are having spring, which has hardly come to us yet, even fruittrees are slow and most flowers froze so we do not expect much harvest of apricots, plums, etc., but hopefully ^{or} pears and apples. And of course of citrus next winter!

Corioclematis

All the best,

Áskell Löve,
5780 Chandler Court,
San José, Calif. 95123.



Robert Morris



Paid

U.S. Domestic Rate

Kean & Venetian

Walter V. Brown

Boyle

(early 60s)

Dr. William A. Weber,
Museum,
University of Colorado,
Campus Box 218,
Boulder,
Colorado 80309

© USPS 1981

San José, April 10, 1982.

Dear Bill:

I have just gotten a copy of the J. Ponert paper: *Combinaciones novae generis Puccinellia*.- Feddes Repert. 84, 1974, pp. 739-740. Either you or more likely the Gray Index has overlooked that in addition to lettermanii he also transfer to Puccinellia the taxa canbyi, laevis, nevadensis, & scabrella. Since these latter certainly are good apomictic Poa (apomixis is unknown in Puccinellia-Phippisia) I dare to doubt the wisdom also in transferring lettermanii...but have written to ask for his explanation; if and when I get it, you will hear about it. He seems to have worked in Georgia USSR but gives his Praha address to which I write of course.

We are getting one more period of torrential rains as if we had not gotten enough...hope you have not gotten more winds.

All the best,





Dr. W. A. Weber,
Museum,
Campus Box 218,
University of Colorado,
Boulder,
Colorado 80309.

© USPS 1981

Digitized by Hunt Institute for Botanical Documentation

San José, August 24, 1982.

Dear Bill:

Did you get west to your Asterolinon while we were away?

One of our Spanish colleagues is trying hard to locate a specimen of the 88-chromosome Rubia we reported on in 1974 from Sierra de Cazorla, Guadahornillos, Löve & Kjellqvist N 093, but does not get any response from Lund or Liverpool. I hope I left a specimen of this collection with you, since then I know you will send it, or a part of it, on loan to Dr. M.A. Cardona, Departamento de Botánica, Facultad de Biología, Avinguda ~~XXXXXX~~ Diagonal 637-647, Barcelona 28, Spain. It may be identical with a species described from the Canary Islands hitherto unknown from the continent, or a new species. Thanks!

Hope your summer has been nice - ours was and is.

As ever,

130⁰⁰ with lens.

San José, October 1, 1982.

Dear Bill:

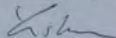
The reference in Index Genericorum to Börner's 1912 "selection" of, as I see it, the wrong lectotype for the Linnaean Polygonum is caused by the fact that whereas the 1935 Code listed the Hitchcock & Green lectotypes for the Linnaean genera, which certainly is critical and not arbitrary as that of Britton & Brown and most others prior to that date, it omitted to decide upon the conservation of this list and to state emphatically that though the type rule was to be retroactive, the priority principle could not be involved and "selections" made prior to the acceptance of the rule could hardly be accepted without a critical evaluation at least. Even those who made the rule certainly must have realized that if the priority principle was to be permitted and used as foolishly as some young turcs have been trying in recent years, the acceptance of the type method would inevitably cause a confusion even greater than the one it was supposed to solve. The Code also omitted to add a rule to state that lectotypes must be so selected that they would not invalidate taxonomically and nomenclaturally perfectly correct divisions of collective genera, even Linnaean ones. That would fit your Polygonum case, because it not only requires a new generic name for Polygonum as Linnaeus certainly saw it, even s.str. with aviculare as the type, but also that Persicaria, as described by Miller 1754 be rejected, when the collective genus is divided. This would be idiotic as would also the opposite logical conclusion that the lectotype rule ought to be dropped...pouring out the child with the bathwater... But the rule ought to have been strengthened by clearly rejecting all arbitrary "typification" prior to the inclusion of the lectotype rule in the 1935 Code. I do not have Börner's small local flora, but doubt that he has intentionally selected any real lectotype because 1912 European botanists ignored this American idea which now seems to be wrecking the very same stability it was meant to strengthen.

For reasons you know best, I am not going to try to organize opposition to this foolishness, but simply ignore the seekers of prior actions even in this field. But perhaps you feel interested in getting several strongly logical and persistent taxonomists here and abroad to stand up and protest before it is too late to save the rule and the idea, for instance through a discussion in Taxon? There are several cases no less important than the Polygonum case you mention, for instance Elymus contra Leymus recently discussed by Rauschert in Feddes Repertorium, and even considerable noise is certainly needed. Why do you not make it first?

Hope the Saint Louis meeting was stimulating and that many listened to your paper on the Central Asiatic - Western North American story, which may have an explanation in the recently discovered drifting of many far-away pieces into the North American plate. But if somebody likens you to Greene, you should be proud, he would also have been ready to encourage your attempts to vindicate us - perhaps even Peter Raven would do it if he only knew what you do?

In Iceland I found that the gardeners had ten or so years ago transferred into the gardens a fine Salix species that Gandoger described in 1900 and learned (?) people ignored or thought was a hybrid cordifolia x phylicifolia, and Floderus even thought was a triple hybrid involving the certainly absent S. arctophila! The plant has 228 chromosomes and is an allopolyploid endemic, the second such from the Pliocene when the American and European elements met. Wish you had been with me to see even the highlands, perhaps a later opportunity will come sometimes? And even in Iceland there still are some hidden species and even genera that sometimes will pop up....

All the best from us all to you both,



San José, November 29, 1982.

Dear Bill:

Thanks for the card. *Boechera Holboellii* in the strict sense is an endemic of Greenland, at least mainly south and west, whereas other material so named from there and adjacent northeastern America is probably exclusively *B. tenuis*, though I have seen too few specimens of both to get a proper notion of their distribution, and earlier information is confusing, because especially Böcher and Rollins lack your sharp floristic eye and logical mind. *B. tenuis* is a diploid and amphimictic species of which *B. Holboellii* is an autotriploid that has survived only thanks to its strict agamospermy. It may be of a rather recent origin, though there is no evidence to back either that or the opposite opinion, because the studies by Böcher and others have been too superficial, for understandable reasons.

Telmo
7-23

I do not know the work by Luer (who is he?) on *Platanthera* that you mention, but he is no more crazy than Webb in *Flora Europaea*, Hylander (1966) in the Nordic Flora, and the authors of all other North European floras, except Nevski and ^{A.L.} Czerepanov in his 1981 checklist of the Soviet flora includes *Limnorchis* in *Platanthera* but excludes *Lysiella* for some peculiar reasons. This is evidently just a case of phenetical logic which is for the predominating creationists or believers of divine guidance of evolution, who still decide in most taxonomy, perhaps not so little because of their skill in using the cloak and dagger on those who dare to think otherwise and show some stature in doing so. Speaking about cloak and dagger, I am hesitant to mention that almost two weeks ago, I received still one more anonymous phone call that perhaps ought not to worry anybody: the caller, a woman who introduced herself only as calling from the office of some vicepresident Vadescu (? I may not have interpreted the name correctly and she did not repeat it) at the University of Colorado at Boulder (is this some Romanian hired recently?), read me a list of several Boulder colleagues, among whom were you, Pat Webber, and Jack Ives, and asked for my opinion and experience of their ethics! When I asked for her name, she said it did not matter and repeated the information from where she said she called. So I answered, without thinking of setting some kind of a trap, that then my opinion did not matter either, I were not an unethical stoolpigeon ready to slander colleagues to anonymous callers, and advised her to study the lack of ethics of the Boulder administration instead. She broke the connection. What, if anything, is going on around you, is the unethical bunch planning to stab also you, knowing by experience that none of your so-called friends will dare to stand up for you? Unfortunately, the call came just the week after the police and telephone company had dropped a block on our telephone in search of threatening and obscene calls that Doris and Lóa had received even in the middle of the night (at 4 o'clock once!), because otherwise we might have found out who the anonymous caller actually was. Better luck next time!

I am sorry that the Press is to disappear, though it never was a wise proposition and contributed little to the mainly absent academic excellence at Boulder, though you made some of the finest contributions through it. I am sorry that this will affect your new Flora, though it is hopefully a blessing in disguise, because I am still convinced that you ought to publish it at Stanford and get prestige for it also. Bill Carver told me at the recent reception for Benson at the launching of his great Cactus book that he had spoken to you about your book but then you had told him that you already had a firm commitment. If you can find some subsidy for the printing, then you ought to let Stanford do the printing of this certainly most modern and excellent flora of any part of North America. If I can help talking to Carver, you know I do it with pleasure, I know him well and admire his abilities.

We are having our rains, hope your snow is light still. All the best from all here to you both,

as ever, *L. S.*

San José, June 29, 1983.

Dear Bill:

Thanks for two good letters, one card, and the first installment towards a New names paper IV. I am sorry that I had to spend considerable time for an evaluation of a couple of fine younger colleagues who have applied for a professorship at an evidently good Nigerian university. Hope you forgive the delay.

It surprised me to see that the third paper in the good series has been published, because when I saw the first blurr of it I had a reason to expect it to include not only some new combinations in *Psilochenia* but also a new generic name of the also biologically distinct North American-Asiatic *Crepis* sect. *Ixeridopsis* for which you had asked my permission to use my name. I gave you the nomenclatural details in early March, in case you do not have Babcock's monograph. You must, however, have abandoned the idea since now you propose to put my name on the endemic Utah-Colorado *Festuca dasyclada*, which in my opinion seems to be a good endemic biological species, although still even cytologically unknown, but certainly not generically distinct. So that honor I hope you permit me to decline.

To other details...not to forget to ask for a copy of III, when you get it.. Your Aletes observations do not astonish me, since I have seen on Constance's arrogant treatment of some European Apiaceae that his concepts are muddled and his taxonomic eye absent...just opposite to what I know about your...thanks to your early genetical training that prevents you to mix with creationism-pheneticism that he adores. The same for *Melica* and *Bromelica*, which are genomically distinct in a way similar to the diploid genera of the wheatgrasses; the latter is close to the Californian *Lophochlaena*, which Bentham and still all Californians insist to identify with the arctic *Pleuropogon* of the Glyceriidae (basic number 10). In connection with a study of *Lophochlaena* (did I send you the reprint?) I came to the, unpublished, conclusion that *Meliceae* (basic number 9) also ought to include as genera some of the other so-called sections of *Melica*, which clearly differ in their haplomic constitution, i.e. *Husnotchloa* (Maire ex Tzvelev 1973), and *Beckeria* (Bernh.) Ascherson, but not *Penicillares* Hempel, which to me seems to belong to *Bromelica*. And perhaps some austral genera as well, though these taxa I do not know enough yet. So you have my sincere encouragement.

In the New names IV, you refer to us as Love & Love. Please, avoid that; if you cannot add the dots over the o which is essential as you know so well, you could transcribe the ö with oe...I never cease to wonder what kind of wisdom it is when ignorant or arrogant American inventors of important printing material and equipment work on the basis of the incomplete Latin alphabet...but why not when they also continue to use the Fahrenheit and inches and pounds! It is not wisdom that gives the Americans world dominance, but arrogance, money and force.

The division of the Eurasiatic *Linum* was correctly made by Reichenbach, who followed the evolutionary definitions by DeCandolle and Lamarck, who were, naturally, ignored for two, and probably many more, generations by the English-American arrogance that later gave up and forgot them for their Darwin, of course. Exactly as they fought the German idea of continental drift and still try to "replace" it with a newer explanation that is an improvement. Rafinesque, who followed the same European principles (as Rydberg did later) and so was persecuted by the rich American botanists of his, and later, time, recognized the distinctions of the eastern American genera. But Small was gravely mistaken in "widening" the perfectly defined small Eurasiatic *Cathartolinon* to include the certainly very remotely related American taxa. As far as I know, your "*Linum schiedeanum* group" shares the basic number 9 with *Adenolinum*. I do not know it well enough to guess if it may be genomically distinct, but then it is nameless. Perhaps the most reasonable procedure for the time being may be to recognize it as a section of *Adenolinon* and transfer the taxa ~~xxxx~~ to that genus, at least for the time being?

I am still waiting for my first copy of the English edition of the Icelandic flora, and have asked the publisher to send you a copy immediately after its publication. Perhaps the severe economic situation in Iceland is delaying the printing, they are now feeling the sting of the stupidity of accepting endless American bribes or swalled loans to keep up a "free enterprise" standard, in addition to the fact that the ocean around Iceland has cooled considerably, because it does not realize the effect of the "greenhouse effect" of some important and wise Washingtonian scientists. A week ago, yes around June 20, there was a severe snowstorm all over the northland, so the numerous tourists who came with the autoferry from Norway to eastern Iceland could not get further for a week. Even the codfish has not hatched for at least two years now, and the newborn lambs in northern and western Iceland succumbed in masses. The new conservative government of the rich for the rich started with a drastic devaluation and cutting of index payments on salaries and of the support to the poor...in a perfect Reaganian style. and with taking new loans to secure the selling of the country as a permanent base for the aggressive conservatives here...no more politics, it is everywhere rotten.

We continue to play with the grasses to get the time to go, but the little work that Lóa and Doris have had to keep the wolf from our door together with our constantly dwindling small savings, has dried up almost completely towards a complete unemployment. As could be expected. Though I admire your perseverance in trying to modernize American plant taxonomy by some silent aid from me as a kind of an encyclopedia, I still hope that sometimes, not too late, you will put that same perseverance towards helping us since that you asked to be permitted to do, with tears in your eyes, almost five years ago, when you got me to give you the material that was aimed for the ethically very strong Jack Fogge. Now he is no more so you stand alone as our hope. Excuse that I sometimes tend to forget your sensitivity when I mention such matters that are increasingly important to us in this country of miscarriage of justice and dominance of justice by money that we do not have for the greedy and lawless lawyers...excuse the outburst.

I hope you will soon find a publisher for the good floras that must get printed to show that at least some American botanists remain scientific rather than creationists. I have mentioned this again to Carver, who is powerless when his superiors take the advice of conservatists as John Thomas etc., but he had no new proposal even for another publisher. But somewhere some such must be found, perhaps even the University of Toronto may be interested, since they are printing the new Alberta flora and have published an arctic flora earlier... or the Cambridge University Press, which has an office in New York and a good circulation service in the United States? Or some smaller publisher in Denver, as the one that published Harrington's large flora years ago? I wish I were rich so I could offer such support that any publisher would come forward....

We read and see about the Colorado floods, though these do not seem to be at the eastern slope this time. Here we enjoy a cool summer, though our fruit grows well and also our vegetables, so Doris and Lóa have been canning extensively. And I caring for the plants, just now I am working on taking away the old canes of our Rubus cultures and replacing them with next year's canes...this year we got very much blackberries of the most delicious kind and want them next year too...

With the very best regards and all good wishes from us, who miss your visits.

As ever,

L. Shell

San José, December 4, 1985.

Dear Bill:

I must apologize for not having written to thank you for three good letters received during November, though I hope you can accept my twopronged excuses: For the first, my backtroubles were at last analyzed as a bad case of arthritis in the fifth vertebra and in the right hip-joint that only time is supposed to help, though I am planned for socalled physical therapy after next week...in addition to certain hormone disturbances connected with another slow virus that never dies... and, second, when my energy has been up I have not had the typewriter because Doris must use it for her translations on which we live...just now she is typing the manuscript of a new ecology book on the Russian high-arctic by Aleksandrova that Cambridge U. P. asked her to do. But perhaps this did not matter too much, because the arthritis of course affects my temper and abilities to think...and you have had enough of acidity from me and others in the past, when you needed only encouragements for your fine work. But since today the machine is available before noon, I took the opportunity to use it.

The New names VI article is fine, of course, and I am sure your solution of the *Agrostis borealis* problem is correct...the only cytological information from the Rockies seems to support it. And the other solutions were needed and are fine, from my points of view. The *Chondrophylla* proposal is, from what I believe, better than that of Holub, since geographical considerations support it, though cytologically *Chondrophylla* and *Ciminalis* both have the same basic number 9 and hybrids between them are unknown or have never been attempted. But this "section" remains critical, since from my points of view it falls at least into the genera *Chondrophylla* and *Ciminalis* with 9 as basic number, *Holubogentia* with 13, and *Kuepferella* with 5... and there seem to be more genera in southern Asia with other basic numbers, according to the Favarger school. I have made a list of the chromosome situation of the European genetians that we have only partially published and may perhaps get it into some later number of *Taxon* the coming year?

By the way, I find that you evidently forgot to send me a reprint of the acronym list from *Taxon*; if you still have a copy I would appreciate to get one, since it might come in handy if I can master energy to make a list of the new names and combinations we have proposed...though I do not know where to print it then.

I am sorry that as advisor I evidently failed you when I did not observe some points in the manuscript in the fall...write it on the account of the arthritis. Therefore, your *Rumex* reports in the *Flora* might win on adding some few changes that will be visible in the *Taxon* list later next year when I at long last report the numbers for *Rumex s.lat.* that I and my students started to study in Winnipeg. Then I will at least legalize *Acetosella paucifolia* as I did its relatives in the arctic Bot. Not. paper in 1975. You are, unfortunately, more than little mistaken in accepting Danser's classification of *Rumex triangulivalvis* as a race only of the strictly Califnrian-Mexican *salicifolius*, and I believe Nina Sarkar showed distinctly that *R. utahensis* is a fine species reaching into Colorado from Utah; and *Rumex fueginus* Philippi is certainly best placed as the ssp. *fueginus* (Philippi) Hultén of *R. maritimus*, and not as a variety only, since it is a major race.

The Teloxys paper is an interesting addition that is likely to be followed by a row of similar adjustments because there is no doubt that so-called Chenopodium is a complex that falls naturally into various more restricted genera, though that problem will hardly be solved without help from botanists from the "evil empire" of the foolish name-callers, and then only by aid of extensive cytological and genetical studies. But this is a fine beginning and comes from the right side.

No, we cannot afford Systematic Botany and so do not see any of the "wise" cracks of its contributors. Is the Crawford you mention in connection not the same who was at Laramie for some few years? and who are the Davis and Gilmartin who dare to doubt what the "great" American "thinkers" say...why should such small and upgoing personalities dare to mention our contributions that the "great" ones ostracize...immigrants are welcome to America but not their European ideas or other points of view...look at the usefulness of the negroes as labor to make the others rich...and the acceptance of agronomists as Dobzhansky dabbling "correctly" in cytogenetics, as compared to the great Goldschmidt. No, no more in that vein today.

I am not sure who originally coined the biological species concept, but Mayr (1942) may have been the first to use it in America. The concept itself, however, is as old as Linnaeus, who required reproductive barriers between his species, and we were brought up on it in Sweden ages before anybody here dared to mention it. Though with one exception...which later on kept quiet: In 1927, Hagerup concluded the first time that the tetraploid *Empetrum* of the northlands must be accepted as a separate species because of its assumed sterility barrier towards the diploid, and 1929 Steere described a South American *Petunia* on basis of its chromosome number difference. That was bold by both, but since the latter never did it again, he may have been forgiven by the "great" we were, I believe, the first in Scandinavia to conclude that since chromosome number differences indicate a strong sterility barrier, they support a separation of taxa so differing as distinct species... a logical conclusion continuing the 1930 review of the morphological and geographical evolution of the taxa by Du Rietz in an article in *Svensk Botanisk Tidskrift* that all taxonomists ought to read and study...though those who tale most loudly about such matters do not even know of its existence. We made our first such conclusion in 1941 in connection with the discovery of *Rumex tenuifolius*, and then based our selection of the categories in our first chromosome list, *Bot. Not.* 1941, on that conclusion and a definition that actually was published the year ~~before~~ Mayr did his "biological species concept" definition. Who would bother about fairness when ostracism has been ordered by the "geniuses" who hurt science the most?

It will take generations before others than you will dare to fight for what we ~~have~~ ~~used~~ was good science in a land of little and illogical but political biology, and since nobody ever dared to stand up for our point of view after we had been "welcomed" by the slander from Berkeley, Cambridge and New York, and since young and well-educated Europeans are hardly likely to risk their reputation by moving into the hell with similar actions as we dared to do...with known results...it is more likely that the Europeans will have to learn to understand what kind of thinking is "acceptable" in the rich land of "opportunity" so they can avoid our fate.

Again, I am getting onto the thin ice again...with the only one who wanted to understand and follow the European logic, so you will have to excuse my rapid exit.

All the best from us all here, who hope to be in an acceptable state for your much expected visit after 8 years of isolation....

All the best,

C. Hill

San José, April 5, 1986.

Dear Bill:

I am sorry that my energy has not sufficed to write to you since you left us after your much welcome visit long ago, or at least since your March 12 letter arrived. But when it came, Doris' sister had just arrived from Sweden for a stay of six weeks, so we and our daughters have been busy showing her round, and I am sorry that my pep still is too low even for doing so little so that most letters have been left unanswered even longer. Hopefully, that will soon change with some of the medicines that I am given by the Kaiser doctors, and by the diminishing arthritis in the lower back that may become chronic and less painful after all the gymnastics on the flour you helped making regular. And though many of our contacts are of little significance for us, your letters and friendship will always continue to be our most important contact with the other side of the world and so I will at least try to write more frequently when our guests become fewer.

We received yesterday the 1985 volume of Novosti, which has a long article by Alexeev on the *Festuca* in Alaska and Canada, but nothing of importance to the Rocky Mts flora seems to be in it. And the other articles are similarly plain. We have been dabbling with some of the old Icelandic and Canadian material we kept in the hope of finding something that had evaded us earlier, but so far nothing has come out of it. And then there are the chromosome number lists in Taxon, in which we have added recently reviews of the North American *Rumices* and the European *Gentianaceae* based on our kind of generic subdivisions that will need another generation before accepted by the "classical" American botanists, as did the Wegener theory that I was so often scolded for accepting. More interesting are, however, some visits by younger grass specialists that Dewey has sent to us now and then, the last one from North Dakota yesterday...the new system seems to be accepted by at least those who breed grasses...though it will need some revisions and polishing for a generation before conservatives accept it, as always.

I have seen from recent advertisements that the new leader of INSTAAR organizes some kind of a summer school in which the great lichenologist Shushan will teach. When do these people learn to separate the grain from the chaff and realize that those who do not know but do not realize that they do not know are only fools that hurt scientific work...even in botany? Or perhaps they are "better American botanists" than not only we but even you...to use the phrase on basis of which our demise was criminally organized?

By the way, I hope you can help me in finding two photocopies of a review of the Icelandic flora that I am told that Malyshev published in the December number of vol. 70, 1985, of the *Botanicheskyy Zhurnal*. I have tried for months to get such a copy, but the number was first said to be on loan at Berkeley and now is said to be with the bookbinders. Please, try to help if you can find time, it is said to be on pp. 1708 - 1709.

Hope everything is normal with you both, despite the heavy snow we saw on TV.

All the best from us all,

Sincerely,

L. Hill

San José, July 5, 1986.

Dear Bill:

Once more I am late in thanking you for help with the *Lophochlaena* note from Madroño, the good letter from June 24 and, not least, for the excellent picture of you and little Suki.

Yes, Doris had discovered that Chernobyl actually means *Artemisia*, and then the *vulgaris* group, according to one of our Russian dictionaries. That should show what kind of prairie they have in northern Ukraine...similar to the Intermountain area?

I am unable to find my copy of the letter with the informations about the divisions of *Veronica s.str.*, but recall that I felt a need to look up the old paper by Dandy on the divisions though I could not get it then..and neither now. However, you are right in that I accepted *Veronicastrum* as interpreted by Fourreau, since that is a reasonable way out of the confusion that even the small minds behind the much too hurried Index selection of the same type as Britton & Brown had used for *Veronica L.* My reasoning is that when such a division is accepted then it automatically excludes later typification, a case similar to that of *Elymus contra Leymus* that I believe was discussed by McNeill in *Taxon* a year or two ago...I have also filed that number too well so I cannot get to it with my present osteoarthritis. As far as I know, *spp. humifusum* has not yet been transferred to *serpyllifolium* in the genus *Veronicastrum*, so if you use it, you must make that transfer. Yes, there is also a proper name for the Tertiary and present E. Asiatic N. American genus *Leptandra Nutt.*, I believe the type species is *L. virginica (L.) Nutt.* and the Siberian one seems to be *L. sibirica (L.) Nutt.*, according to Czerepanov (1981): *Plantae vasculares URSS*, who also lists three other species from eastern Asia.

I am sorry that the Russians called off the Siberian expedition, though this gives you new opportunities to take it easy at home and perhaps even permits you to visit your friends here and there when the vegetation looks inviting? I am sorry that we cannot do that any more as long as my doctor waits with an action against my slowness and rheumatism, though that may change in three months time, he told me a few days ago? As a matter of fact I am keeping myself so busy these weeks reviewing and making notes on a long and interesting paper sent by a Swedish colleague and editor that I have still to take off some time for typing out the manuscript of the next chromosome list that is due to the editorial office in the Smithsonian after the middle of July...for printing in February!

We can see in the paper that the warm summer has come to Colorado, as it has come to us after the cool spring...though I know that the cool night wind from the mountains will keep you more comfortable than we are some of the days and nights when the heat roars in the middle of the day. But most of the time it is also cool and comfortable here during the night, thanks to the coastal fog, so there is no reason to complain. Except for that you are so far away....

With the very best regards to you both from us here,

As ever,

W. S. Gilman

San José, May 28, 1986.

Dear Bill:

I am sorry that my limited energy caused me to wait "until tomorrow" too often with thanks for the copies of the Russian review, but that is hardly acceptable as an excuse, though I realize that you are the only friend here who forgives me everything foolish. But even late thanks are thanks, so here they come.

I envy you the trip to North Carolina, where we spent many weeks every one of our Montreal years driving around and climbing in the Appalachians, including the Smokies. And you were there at the right time, because few floras we know are comparable to the southeastern alpine flora in the early spring. Only a pity that you did not see the Smokies and their phantastic Park, and that you also still have missed Williamsburg, the gem of American national monuments that everyone ought to visit and enjoy...we did it annually in the spring. But there is still time, hopefully....

I have never asked you, I believe, what kind of organization it is that brings you and Cronquist together in the Soviet Union...even though you enjoy it with some reservation. Is this done by the NYBG and thus Cronquist himself, without much influence of the owners of the mountains you visit? And why do you avoid coming to Leningrad, which we understand is the gem of Russian cities and filled with arts and other culture that you...though perhaps not the mormon?.. would enjoy enormously?

Yes, we read the stupid article by Baum, but as always before, we want to ignore it for the simple reason of its stupidity that all real botanists should understand: He came to Canada as a French-Jewish plant breeder trained in Israel, probably by aid of the schizophrenic Bowden (he has been for decades in an asylum, i.e., Bowden, not Baum) and the foolish Senn, and from the first beginning he followed the misunderstanding of even the simplest biological basic logic and of the meaning of nomenclatural rules, etc., etc. Mary Barkworth could not stand him and his lack of logical reasoning ability, and his reputation as "botanist" comes from little learned agronomists and workers on grain crops that are evidently impressed by his "nomenclatural" terminology. Of course, if there are any really critical and logical botanists interested in his article and feel able to teach him and others some more biological and nomenclatural fundamentals, they should do it, but my experience of the wisdom by American colleagues in these fields, and their tendency to use the dagger in the backs of those of other opinions and lack of courage of others when somebody is being stabbed, permits me simply to ignore what people of the Baum character and kind say or write. Why do you not stand up and criticize him?

We sincerely hope that soon the troubles with the Press will be over so that your book will get printed and published and made available to those, who need to be educated and want to be, and the many others who become irritated by one, who sees better than they and is not satisfied with the first plant names he hears about. Though I do not have great hopes for American taxonomy...wars rather than peace, politics and slander rather than honest discussions, and chemistry and "molecular" genetics rather than sciences working for other purposes than killing others, will likely dominate this nation for many more centuries... except if we get a revolution that changes the economic divisions. But who could start it in this land of complacency and hand-outs dreamings?

All the best from us all to you both, and regards to Russian colleagues whom we may know.

As ever, *Wick*

San José, June 13, 1986.

Dear Bill:

We are pleased to read that even the administration at Boulder at long last realizes what you have done the many years you have worked there so they awarded you the Stearns Award...nobody could be better qualified. At the same time we are shocked to read that your colleagues evidently feel otherwise since they were absent at the dinner...all but John Marr, who always has been something special. Yes, I am in no way in doubt that this is envy of the worst kind, since the fact that you get recognized for what you do well when they get no such recognitions for what they do not do, naturally, and then they turn their acid towards you, as is usual with small minds filled with envy. You can, however, forget their attitude and forgive them for their smallness, since the main thing is that the world all the way down to the Boulder administrators clearly sees the greatness of your work. We are only sorry that we did not know earlier and that we were unable to join John in showing our admiration...but hope you forgive us, as always before. Congratulations! And we are sure that this is not the last honor you receive, of course.

I am glad to see that you are growing more optimistic as to the publication of the flora, which will stand alone in America as a representative of the new evolutionary taxonomy that at long last is emerging elsewhere...though not in England, of course, which, together with America, will long remain at the far right. And I even dare to hope that you may not be enthusiastic enough for the possibilities, so keep up with the optimism and propaganda for a fine memorial of the greatness..

I am somewhat astonished that But, whoever he is, feels forced to transfer oregana to Lophochlaena, since as far as I know it has only once been collected and that more than a century ago. But I may be wrong. Could you help me with a xerox? I am sure you have our paper on the matter from some years ago so you can evaluate the truth in the use of the term "redefining" Pleuropogon and the mentioning of good old conservative Stebbins in connection of the change we advocated, because as far as we are concerned it seems evident that he shared Benson's view of the California taxa as representing Pleuropogon of the Gylcerieae rather than Lophoclaena of the Meliceae...despite the fact that both the basic number and karyotypy are those of the latter. By time even the blind may see the negativism we have experienced here...except from you and very few others...but your training in cytogenetics is probably the main cause of your positivism.

Yes, we know well about Jim Phipps good Crataegus work, since I helped him get his fine Indian cytogenetical helper years ago. The problem in the east at least seems to be agamospermy, probably connected with rare hybridization as in European Taraxacum, whereas in the western regions either very few apomicts or only a few strictly sexual species may be represented? Jim may solve that problem by aid of you and some others, though apomicts, especially if not completely so, are never easy to work with.

When you go to the Siberian colleagues, you may give Malyshev my greetings if you see him, and then I hope you will see Kirpichnikov in Leningrad, and Yurtsev thogh he may be elsewhere during the summer. And despite the company, we both hope that you will return with new views and new experiences properly rested and free from influences from the accident in the Ukraine.

Best regards to Sammy and yourself from us all here, who miss you often.

Sincerely, *W. S. Judd*

San José, August 25, 1986.

Dear Bill:

Try to forgive that your welcome card and the sample page of the fine book have been waiting for my ability to type...my increased arthritis in the back added to some other Parkinsonian effects make it difficult and usually impossible to sit and type...hopefully this will change next month when my Indian neurologist takes me off the present drug and gives me levc-doba instead? But we both found the sample to be an excellent one, so we are hoping that soon you will tell us that the press has received the camera-ready manuscript and started its printing! A better flora hasnowhere been printed yet.

Otherwise nothing remarkable, except perhaps that for some reason the Icelandic minister of learning has contacted our old friend, the director of the Icelandic National Library and asked him to look at our library and prevent it to disappear into some foreign institution. We will see what they feel they can afford or what we can afford to accept from them...the librarian says he will visit us in late October around my 70th birthday and stay some time enjoying some of the books that he borrowed now and then when we all were at Winnipeg.

Hope your summer has been as busy as ever...despite the lack of a Russian trip. And that also Sammy is as healthy as possible...I am beginning to understand her and your frustration over a matter that cut her and your lives into pieces and made her an invalid with a healthy mind. I understand that not only John Marr but also Britton of physics share my experience....perhaps research on Parkinsonians in Boulder could reveal what stands behind our troubles? and then find solutions?

All the best from all here,

As ever,

Lshen

San José, September 19, 1986.

Dear Bill:

Thanks for a fine letter, and try to forgive that I did not react at once, but I had, at long last, been freed from the poison symmetril, that actually is for flue and at least affected me by making me slow and apathic, and put on the dopamin related sinemet, first 100 mg per day but am now up to 500 mg that will be increased to 600 mg this weekend...and the reaction is phantastic...I am awake and can even type without becoming tired at once, even walk with some dignity, the micrographic writing has become readable though still it is micrographic, and my voice remains strong rather than vary between almost nothing and normal. Etcetera. So perhaps things will become better before I become 70 in a month's time and then permit me to write some of the papers I am being asked for still? We will see.

We are sorry that you are having worries because your girls are moving away so Sammy will need other help...if this were Iceland or Sweden, such problems would not come up, because we stay with our sick and old without asking or thinking about our own pleasures...but other Americans moved away from their conscience...which then remain "clean" since it never became soiled by useage, as one of my old Swedish friend once said...he had lived in Canada and the American west for several decades before he returned to Scania! Though we would never move back to Boulder, which is a place that has too few men of your kind, we feel sorry that we did it without bring you with us, because otherwise we could have shared living quarters and helped. But we sincerely hope the solution of that problem will come with too much effort. And I am sorry to hear that the friendly and effective Rosie Clark is no more and that cancer took her, that is life...

Hope that you soon will be successful in finding the necessary funds for the good flora...the sources you mention are at least promising. But when that permits you to complete the final manuscript, perhaps there will be an opportunity to accept the name *Myriophyllum sibiricum* Komarov for Hultén's (and Fernald's) "North American and boreal-arctic race" of *M. spicatum*, that Česka has done good work with recently? I have been in doubt as to Hultén's wisdom in regarding such taxa as American and Northern Eurasian geographical "races" of what he and others accepted as Southern Eurasian mountain and lowland main races of several species...or what we tended to look upon as races of southern mountains in the more or less circumpolar zone...since hybrids, natural or artificial, seem to be absent, an old Linnaean sign of specific rank. Taxonomy still matures....

You probably forget that Vera collects only pieces because she walks and is alone collecting...though I can understand your frustration with such specimens... but why fill spaces in a small herbarium on the prairies of America with material from Chinese or Indian mountains, where nobody will ever use them in research? I know your answer, which is also mine, so forgive the question! But if you should find an opportunity to visit Katmandu again, I know no botanist there presently who would be interested in helping you...but you would enjoy contacting Professor S. S. Bir of the Department of Botany of the Punjabi University at Patiala, who is one of the leading specialists in Himalayan plants, a pteridologist and cytologist, but actually most a botanist of your wide interests. My best regards when you write to him, you will enjoy his wide knowledge...he is a sikh.

All the best you both from us here...who continue to miss you both.

As ever, *Carl*

San José, October 9, 1986.

Dear Bill:

Thanks for the call and for the letter that I wanted to answer at once though I let it wait because of several other letters that must get to the mail...my energy is not that which it was, so sometimes I just do nothing instead of using it sensibly... probably typical for Parkinsonians or perhaps just for my age, who knows. But you are one of the few who always forgive.

I am sorry that I misunderstood your words about Sammy's difficulties in losing her girls, I should have known or understood that she needed considerable help from outside the family, despite of what she gets from you and your two Boulder girls. I admire her and you all for how you take such a fate, and am ashamed for even telling about my minor troubles...but some of us make elephants out of the mosquito, as we say in Scandinavia. We are glad to hear that the problems were solved.

As to Melandrium (which is the correct genus for the dioecious taxa, since there are others related genera that fit better for some that are often mixed with it): I looked it up in Lid's Norsk og svensk flora from 1963, p. 751. The name comes from Greek, of course, melandryon, from melas, black, and drys, oak, and was used by Homer (800 B.C.) as name of the dark heartwood of oak. In the present sense it seems to have been first used by Carolus Clusius, 1601, in: Rariorum plantarum historia, published in Antwerp. You may have better information elsewhere, though this ought to suffice.

It does not astonish me that the small minds of the WSC or others of the not too learned Colorado colleges feel they must fight the printing of your good flora on the ground that it differs from what they learned from small professors somewhere out in the bush...you know that we are also used to this kind of philosophy, which unfortunately dominates also in most other groups here...even Reagan may suddenly feel that it is not typical of the few Icelanders he meets today and later, though they have not the strength...^{democratic} strength" they may say here...of hundred of millions of fools with little education. But the flora still remains one of the very finest ever written, and you should not be astonished if the reviewers in Europe and especially the Soviet Union, where the Linnaean evolutionary concepts dominate, will be very positive, because you work with the ideas of the future (I hope Peter Raven has grown wise enough to recommend it highly, despite his background). Yes, that fellow who said...or is, I believe correctly, said to have said: "I disapprove of what you say, but I will defend to the death your right to say it", was nobody but Voltaire, the father of all modern ideas and the real democratic thought...it is, at least attributed to him in the book "The friends of Voltaire" by S. G. Tallentyne, 1907, p. 199, according to our notes...and I heard it first in my class of the history of philosophy in the Reykjavik College my first year there, 1934...it is hardly known by the great crook in religious (!) philosophy at Boulder, though, or by other typical suppressors of new or even old and established European, though not American, ideas around you.

I am sorry that the rare book by D'Antoine on the splitting of Juniperus is hardly available, and that a businessman here, who wanted Doris to translate it and get it printed again, evidently gave up that great idea after consulting some of our great botanists...though Sabina and Juniperus are more distinct than Pinus and Picea, and never have been known to cross. Those who even try to fight those with other ideas after they have been dead long time, cf. Goldschmidt, never give up.

I am glad you differ...all the best,

William

San José, November 3, 1986.

Dear Bill:

Forgive me not to have written at once to congratulate you to the success with the matching funds for the fine Flora, which made us as happy as you must be. But our house has been filled with visitors from Iceland and Scandinavia and even Napa during the past two weeks, so there has been no time for letters...also because my back has been acting up, of course. The visitor who took most time and gave us the most of pleasure was the former professor of Icelandic at Winnipeg when we were there, who now is the director of the National Library and the University Library at Reykjavík...he is just getting a new and very modern Building so we got the opportunity to see his drawings and to visit with him the libraries in the Bay Universities, one of which, at Santa Cruz, is of a similar size as his, or almost 1 million books. Though that did not impress much my nephew, the son of my little sister, who presently is a librarian, not even division head, at the 3 million books library at Lund, the largest in Scandinavia. It seems to be only in America that even the "learned" profession cuts down library funds and money for the printing of floras and other important publications for the future? And today we got a phone call from a Danish lawyer, a relative to Doris, who will visit us with his wife tomorrow afternoon and perhaps longer...the last time he lived in our house for some days was during the war at Lund, where he was hiding from the Germans in Copenhagen and took Coy for a circus, where she learned the phrase that she still remembers: zebbran är ett randigt djur, ränderna går aldrig ur!

I hope the printing of the fine book soon starts and that it goes fast and effectively so your optimism comes true early in the spring. And if it will not be loved by many users, that cannot be your fault...through the fact that it is modern and follows what I dare to look at as the strict Linnæan concepts: biological and evolutionary, and not the British conservatism. My Icelandic publisher has begun to discuss a new edition of the English edition of my little flora, so I am looking through it for corrections and adjustments...any suggestions?

I do not know Barneby, but enjoy his salty letter...about Peter Raven and his kind, I suppose, though Peter evidently agrees with your new concepts, though he has "criticized" them when connected with our names...people are funny sometimes. But I like him nevertheless, though less than I like you.

I have umpteen long letters to write in all directions connected with my recent birthday, so I hope you forgive that the letter stops here. But hopefully a longer letter will come your way soon from here.

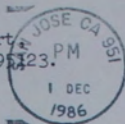
With the very best regards and all good wishes from us both to you both,

As ever,



P.S.: Do you have our Pteridium herediae N 042, cf. Lagascalia 1972:23-25? I have the feeling that you sent it to some lady years ago and never got it back? If you have it, never lend it out, since I have discovered that Heywood has put all our Spanish collection under his own name and his selected Latin names..honesty? And "advised" at least two young botanists to study this species in detail and reject it as nonsense...to him chromosomes mean the same as they did to Lysenko!

Áskell Löve,
5780 Chandler Court,
San José, Calif. 95123.



Dr. William A. Weber, F.L.S.,
Herbarium,
Campus Box 218,
University of Colorado,
Boulder,
Colorado 80309-0218.

©1982 USPS

Digitized by Hunt Institute for Botanical Documentation

San José, Dec. 1, 1986

Dear Bill:

Thanks for a good card. I hope you get help with the final pagination, though even that might require your attention, because one can never be sure of even the best of help when it comes to printing...look at the "champion" in my Icelandic flora that only you observed!

We are sorry that you did not fly the circle and stay here for a night or two...perhaps later? But if some English botanist should ask you for loans of our Spanish material, ~~we~~ refuse, since we have just discovered that Heywood threw away our labels and put all our (Löve & Kjellqvist) material o from Spain into the Liverpool herbarium under his collection numbers, and under names he preferred! English honesty?

Hope the winter still is mild, as here, and no flues yet.

All the best from us all, *L. Sell*

San José, January 24, 1987.

Dear Bill:

Thanks for the good letter, I suspected that you used all your time for the new flora, when we heard nothing from you, and am happy to see that all is going well. It will be nice to see the volume completed with the pictures and some few more new combinations that I have not seen, and you will feel relieved when the printing is over.

As to the typification of Veronica there is no discussion, since both the Britton group and Hitchcock & Green evidently selected V. officinalis. (And I have just been reading a fine manuscript by McNeill etc. for the May issue of Taxon with a proposal to fix 1932 for selection of Linnæan generic types, and since this is evidently to become the law at the Berlin Congress, the American confusion will cease at last). I suppose you have the 1959 Ray Society facsimile of Species Plantarum edited by Stearn, where all typification attempts are listed in the index? I am sorry that I do not have the recent typification proposals from Regnum Vegetabile by Miss Farr and her group, where they list the same species as the type for Veronicastrum, as far as I understand, though that will certainly be rejected as a misunderstanding, or even as ignorance, but I am unable to get to the copy of Regnum Vegetabile where Dandy evaluates the typifications of various genera published at the same period as Veronicastrum. That genus was wrongly identified with the American-Asiatic group by Moench, who has been followed by some later authors, whereas I maintain that it was typified by the species serpyllifolium by Fourreau, in Ann. Soc. Linn. Lyon, N.S. XVII, 128 (1869) through his splitting of the Linnæan genus... I had that paper long ago and made some notes of it, though these have been filed away effectively. You may perhaps be able to get that old French publication from Washington, I had it in the Montreal library at the Garden. In your shoes, I would just mention this simply by saying: typified by V. serpyllifolium by Fourreau, 1869, in....

The new combination of humifusa under Veronicastrum needs only the following reference, plus the mentioning of Veronica serpyllifolia ssp. humifusa (Dickson) Syme as an earlier subspecific status. Or:

Veronicastrum serpyllifolium (L.) Fourr., Ann. Soc. Linn. Lyon, N.S. XVII:128, (1869) ssp. humifusum (Dickson) W. A. Weber, ssp. nov., based on Veronica humifusa Dickson, in Trans. Linn. Soc. 2:288 (1794); V. serpyllifolia ssp. humifusa (Dickson) Syme, in Sowerby, Engl. Bot. ed. 3, VI:158 (1866).

If you go to Russia after all the hesitation, and have Dave Murray with you, perhaps you can get him to understand that he has neither the training nor the literature necessary to make judgements of the kind he has been making, though younger fools may imagine that he knows what he is judging. I am sorry that he must stay in Alaska without a proper library and alone... perhaps he would fit into your chair? But that is another matter that others will decide about, perhaps even so-called "biologists" equally free from a proper background, though "educated" at Berkeley? The fate of biology in the west is likely to be decided by those who know nothing, and understand less of the history and philosophy concerned? If they knew, they would probably be stabbed, irrespective of their national origin.

All the best from all here,
as ever,

Colman

1/27/87

San José, May 25, 1987.

Dear Bill:

I am sorry that the good book evidently will not become visible to us until you have returned from all your travels of the world after the summer, but one can never wait too long for something good. But we will enjoy it when that time comes.

Sorry that I cannot help you with the bibliographic citations for your *Carex rupestris* (not *rupestre*!) ssp. *drummondiana* (Dewey) Holub, though I may have furnished you originally with the combination (?), since I cannot search for it in all the many papers of Holub that are boxed, or in the two journals he uses, since they are also not easily available now. But does it matter? In that case the easiest way would be to send him a short letter.

I am sorry that you did not read further when you found the reference to *Schoenoplectus lacustris* ssp. *acutus*, since then you would perhaps have observed that the next taxon is ssp. *creber* (Fernald) L. & L., which is just *validus*! This is in *Taxon* 30, 1981, p. 848. The name *creber* must be used for the taxon in the new combination for the simple reason that the North American plant is identical to the ssp. *creber* (Fernald) L. & L. 1954 [Bull. Torrey Club 81:33] of *Schoenoplectus validus*, the typical race of which, or *validus* sstr. is South American. If you prefer *Scirpus*, the combination would be *S. lacustris* ssp. *creber* (Fernald) T. Koyama, 1963, *Phytologia* 8:457. I am sorry that you find it difficult that we...as so many others...even dare to make combinations for American taxa in works on European relatives...but what can one do? We actually have made, or scribbled, a kind of card index, on soft papers, of all the combinations and names we have proposed, but since we never have been supported properly in America to have even a technical assistant, still less a personal secretary, we have not typed this out to make it easier for others to look it up. From the point of view of the McCarthyists who even started trying to stop our work when they had succeeded in getting us into the Boulder trap, our works never were worth any assistance, since they were not only written by people of questionable political color but also by ones of doubtful biological leanings, even European geneticists! You and Dewey are probably the only ones who dare to accept such opinions that seem to please our Russian colleagues, cf. the last and recently published volume of the Soviet Arctic Flora.

I should have gone to Iceland now to celebrate my 50th anniversary of the graduation from the almost 1000 year old Icelandic college next week, but I hesitate to travel and cannot afford such luxuries thanks to the Boulder situation. But we are getting visitors from there twice next week...and are perhaps not so eager to get into the cold spring weather and its common cold infections, though even here we have "enjoyed" three cold weeks now, when the prairie temperatures rise.

My health goes up and down, as is normal, but is mainly under control by sinemet pills. Hope John Marr's trouble also is under control, looking backward I believe his parkinson was already troubling him when we arrived at Boulder in 1964, and that it was already then sucking his energy to force him out of the work he did so splendidly before. Give him our best wishes if you can.

With the very best regards and all good wishes...and hope the printing goes faster than you felt when you wrote.

Sincerely,
C. S. Ross

Scirpus validus Vahl var. *validus* Fern., *Phytologia* 283, 1943
S. validus Vahl

CYPSCILS 2

Digitized by the University of Michigan Herbarium

San José, June 11, 1987.

L.H. b 432
Cytotaxonomic notes of the
North John
1975, 598pp
J. Cronin
Vancouver

Low 657. The plants of the Queen Charlotte Islands [revised].
18: 225-229. 1169,

Dear Bill:

Many thanks for the good letter, and I hope your optimism regarding the correction to Schoenoplectus and the completion of the fine Flora comes true, though I can see that your critical mind may want to wait for many more years until it regards the work completed! The characteristics you mention in favor of Lepia indicate to me that this taxon has another haplome than what Linnaeus put into Thlaspi and Robert Brown moved to Lepidium, and that there may also be differences in karyotype between these taxa, though that remains my guess since nobody seems to have looked at their chromosomes in detail sofar, and it would not astonish me if these taxa could not be hybridized. That somebody may find out later. Although I have not seen the monograph by Thellung, I share your feeling that Lepia ought to be revived...since Lepia Hill evidently was a synonym to Zinnia L., as you mention, I believe the name as used by Desvaux still is available and valid, so...Perhaps Meyer, who split Thlaspi recently, cf. Rothmaler's critical flora, has also touched these groups, though I do not have information about that...I believe he wrote in Feddes Repertorium which I have not seen in the libraries in my neighborhood. But it is too late for this edition of the good flora to spend time for the solving of that problem...and the taxon is, also, only a weed in Colorado. But perhaps next time?

As to Veronica nutans, I believe we first mentioned its problem in a review of The plants of the Queen Charlotte Islands, in Taxon 18 (2), 1969: 225-229... actually on p. 229, 20th to 16th lines from below. Its chromosome number, which is only half that of V. wormskjoldii of the eastern American arctic, had been counted by some others, but we published our observation from Niwot Ridge in Taxon 24, 1975, p. 674. A proper taxonomical reference is that of our Arctic Atlas from 1975, p. 430 & 432, which I believe was used by some of the Russians later, though the place has slipped my memory just now...and even that may not be correct, I cannot always trust my brain in later years. But hope this helps you and even gets you on some tracks that I have missed. But these references you have.

We are grateful about your contact with Ruby and sorry that the sinomet and levi-dopa are not helping John now, though they have done it for eleven years, that is what happens to others also, though I hope I will get that many years out of that medicine also...but nobody knows. David Valentine of Manchester, who suffered from the same for years, died of a brain hemorrhage in March when the pills had failed him for two weeks only, but a Swedish-American lady we knew in Denver, seems to get help from other variations of the drug whenever one fails her. I realize that the disturbance of the l-dopa was the cause of the dwindling energy of John already when we were in Boulder, so he may have suffered from this twice as long time as anybody knew...as is often the case. We wish him all the best and hope that he will feel better much longer and that Ruby will not be worned out helping him. Doris still has her energy...what could I do without her? And we continue to admire you and Sammy for your fight against a much worse difficulty.

With the very best regards and all good wishes...also to Ruby and John.

As ever,
Erikson

P.S.: We have just had the house full of Icelandic friends and family, and now Doris has started to conserve our fruit from the gardens. And I sent with my nephew, who directs the largest publishing company in Iceland, my corrections and additions for the second edition of the Icelandic flora in English...but it hardly comes this year...its publisher is another firm though, not his.

5780 Chandler ct., SAN JOSE, CA 95123

Aug. 20, 1987

Dear Bill, -

Here is the translation you requested. Hope it will be of some help to you. I added the figure legends and a bit about the subspecies of two species, of which nothing was mentioned in the key, although I think there is a remote possibility that they will occur in Colorado. But just for the sake of completeness of the information.

Most of all, however, ^{there} is the obligation to thank you for the specimen of your flora that arrived a couple of days ago. I have not seen Askell as really interested in anything for a long time. He has sat for hours looking through it, and that is something he rarely does these days. I am really happy to see that there is something that can arouse him from his increasing lethargy.

Needless to say, we are very proud of the dedication to Askell et company. It is, indeed, a great honor. Thank you also for including me in the acknowledgement, although I hardly feel I deserve it. It is really mostly Askell who has helped you. He has such a rich fond of knowledge and we feel that it is beautifully mirrored in your flora as far as concepts and so on are concerned.

We are both very much impressed by this flora and find it one of the best we have ever seen, modern, accurate and very progressive. It should start a new trend for floras all over the world. Hope it will sell well and spread its message widely.

I found reading the preambles a real treat. It is beautifully written and contains so much a value. I hope people really read it. It contains such a lot of new ideas (to others than you and us) that really should be widely applied by flora authors. And there is some valuable criticism as well, that should be heeded.

The black-and-white drawings are beautiful. But, I happened to open up the book when I first got it into hand, on p. 404. I startled at a very unfortunate mix-up of figure numbers and text. I am sorry, but you should see it by yourself. It will embarrass you.

The color photos, at least in our copy, are somewhat dark and do not make full justice to your beautiful photos, which we know well before. But several are very good. This may of course also be a matter of taste, so no complaints.

Askell is going through the book with a fine comb, but has so far found only a few misprints and remarks to do. He will, when he gets to it (it takes a long time for him now to initiate a move to do something), of course write you himself to thank ^{you} for this gift. I just wanted to express my own sentiments about it to you so you know we have got the book and are pleased with it.

Otherwise things are going its usually slow pace here. I work like mad to get my own as well as all the work Askell used to do, done. The garden is a lot of hard work, but as long as I can it goes. We have a bumper crop of apples just now to be canned. We still eat of grapefruits and oranges fresh from the trees. But if you come to visit again, we cannot offer you any almonds, alas. There were very few, just a handful or two, on the trees, because the blossoms rained away and it was too cold for bees to operate at the time. I had been watching those left on the tree carefully to pick them just at the right moment. But then a large green parrot escaped from somebodys home somewhere in the neighborhood, and in one single night or morning, it consumed all our nuts! We still see the culprit around and hear its hoarse shrieks, and it chomps down on apples once in a while as well. It has been "wild" for several weeks now, but I am sure it cannot survive a winter even here. Hope somebody succeeds in recapturing it soon, although it seems really clever at avoiding human contacts.

We hope that you had a good time in Russia (do you not speak some Russian by now?) and that you returned in good health after all the adventures. We got your postcard and thought of you and almost envied you being there among all those wonderful plants and landscapes.

Please, give Sammy our very best regards. Hope she is doing fair and is able to get around somehow.

And to yourself, with the very best thanks for your generosity, now and always, the very best regards, yours always

Bob

San José, August 26, 1987.

PS: Our area code is 95123, not 133!

Dear Bill:

I must apologize for the too long delay in thanking you for the fine flora, which I have enjoyed daily for more than a week. And for a letter from Siberia and one from Boulder just before you left and another that came a couple of days ago from your good old desk at the herbarium. But since I have some difficulties in even typing or composing letters of the simplest kind, I hope you forgive me that I let you wait, and that this will only be short.

As Doris has already written, you realize that we are both very happy with the only modern Linnaean-type flora of an American area that we have enjoyed reading and studying every day since it arrived. You are aware of that this is a fine work, I am inclined to write...at last, at the very long last we have gotten an American flora that can teach even progressive Europeans how a modern work of that kind should be written, rather than copy the worst conservatism the Europeans, or rather British, can present....but I am sorry that you have prevented me to write a thunderous review of the book by adding me to the list of those to whom you dedicate it...though I am proud of being allowed to share their company, especially that of Rafinesque, Greene and Rydberg, whom the eastern establishment has been hunting and stabbing ever since they did their great works...in the same style as we later experienced, though Rafinesque at last got the understanding of the great Merrill, and Rydberg and Greene were protected by great men as Britton and Abrams. That we still feel stabbed despite your vindication is another matter, since we should have been aware that you do not talk into the lions' mouth, which then and still is carefully watched by the many followers of McCarthy, despite all the talk about "democracy" and fairness...why should those who are brought up on the English classical belief in nurture and phenetics ever accept the classical European Linnaean and genetical opinion...though stabbing for one's opinion looks somewhat peculiar when it is done by those who talk most loudly about freedom of opinion? But you know that we are grateful for having you as a friend, and that we are proud of having been thanked for the little help we gave you, and for being able to see that this great work could not have been done without the availability of our good personal library, which I realize the bureaucrats above you do not understand to value. And without such a library even you may have difficulties in modernizing the flora of the eastern slope, though now it is in a dire need of a proper revision, as nobody knows better than you do yourself. Hopefully you will be able to do that.

You know that I am in doubt as to the wisdom of continuing to throw out the child with the bathwater, when you stubbornly continue to use the highly unscientific system of the alphabet not only for the family arrangement, where I can see your point, but also for the genera of each family, where you have hardly a point of reason, especially since such an arrangement greatly counteracts the educational value of the work...though that may perhaps be disputable. The essential point is, however, that you revive the Linnaean natural (biological, evolutionary) classification of the genus and species...though I believe you misunderstand the basis for the later collective genera that had nothing to do with his definitions, only the "need" of rich and powerful but less wise Englishmen of the Kew group and Germans around Engler. But I believe your book may become the turning point of that unfortunate current.

Though I have not been especially looking for misprints or mistakes, when I have made my daily walks through your pages, I have been unable to miss the fact that you have not avoided such blemishes entirely...no good book does. You will have seen most of these small things, of course, so I do not mention most of them. But I believe you may not observe some few direct mistakes, that in my eyes are sometimes blatant: On p. 17, the two lowest lines are missing; on p. 45 I doubt the wisdom of leaving out viride to replace it with the certainly doubtful trichomanes-ramosum...and then you forgot to replace the translation green! On p. 49 you have ignored our 1967 & 1977 observation that the Pacific species of Gymnocarpium is the diploid disjunctum, whereas the Atlantic species is the tetraploid dryopteris. On p. 61 you have the ssp. alpina (Neilreich) Čelakovský, instead of (Smith) Čelakovský...but that is probably my fault? the correction was made by Christensen in Nov. 1985 Taxon. And you are dead wrong in your reasoning and conclusion on p. 223...Tolmachevia and Rhodiola have nothing to do with each other, since the former is hermaphrodite and the latter dioecious ...in addition to the fact that the latter has the basic chromosome number 11, the former only 9, and distinctly different karyotypes. But sometimes even Zeus nodded.

On p. 229 (and elsewhere) you spell the name Wormskiold with ö, which is a mistake, and on p. 385 you rebaptize the late, fine German Rauschert Räuschert or Räuschert wrongly. On p. 404 there are mistakes in the legend, and on p. 409 you picked the wrong Koenig, instead of Johan Gerhard König, who was a Danish botanist of Polish (Livland) descent who collected in Iceland for a year and then in Zansibar, no Trankebar, which was a Danish colony and missionary station in India...and there he collected more plants than any botanist ever has done anywhere, say the Danes. But this is mainly nitpicking, and I dare to say that you have succeeded nicely in avoiding the ravages of the printing devils that love especially the finest books. So you can be proud not only as the author but also as the essential printer of the book.

Back to your oldest letter, in which you wondered about the Caucasian group of "Minuartia" imbricata etc. Since its basic number is 12 (diploid numbers 24, 48, and 72, as contrasted to 15 for the typical hispanica or dichotoma, there is no doubt that this group of seven (?) species is a good genus in its own right; it could be validated easily based on the name Minuartia sect. Spectabilis ser. Laricinae that Mattfeld used for it, but that is the only collective designation I can find.

And back to the last cards: It does not astonish me that Bistorta bistortoides replaces B. major in Altai...the Russians know as little about Colorado as we do about Altai, except you. But I would hesitate to split Angelica except after thorough experimentation, since I am in no doubt that archangelica and sylvestris (the type) are closely related...and the entire system of small genera of the Apiaceae is so fragile, that it would be unwise to touch only a small part of it. And those who regard themselves as "specialists" on that family, not excluding the stuffy Constance, do not seem to have gotten anywhere towards modernizing what mistakes our more or less good colleagues of the last century did. But an experimental approach to this giant family of the basic number 11 and only small karyotypic variations would be worth while...for a young and critical and well educated botanist of the kind that could not grow up among American conservatives who would stab him at once. I would let Angelica stay as Linnaeus left it, unsplit, until we know much more.

This is the longest letter I have been able to produce for many weeks, so I hope you forgive me if it is filled with mistakes and worse. I do not dare to reread it for the risk that I then might never send it...and you must have something as thanks for the finest flora ever published. Hope you get some positive colleagues to review it properly...why not send it to Holub and ask for review in Folia?

All the best to you both from all of us here, who miss you often.

As ever, *Cronquist*

p. 18: line missing
[initials]

insert
[initials]

Digitized by the Institute for Botanical Documentation

San José, September 28, 1987.

Dear Bill:

Try to forgive my tardiness, which seems to increase. It may perhaps be easier when I tell you that still we have the good book on the table in our living room so we can look in it frequently during the day...and there is always something one observes with pleasure even on pages we have read carefully before. It is a book to be proud of. Thanks for that you sent it to the Icelandic library, where it will be appreciated.

I do not remember if I mentioned earlier that I took out a box of notes on *Bistorta* to sharpen my memory of studies that ought to have resulted in at least some publication...as so many other things that never were published. Although this is a genus of 50 species of temperate North America and Eurasia, according to Airy-Shaw in Willis, I have the feeling that most of the variations so classified are better placed at a lower level, or even ignored, although it is to go too far to ignore them as completely as do most American botanists. I have drawn the conclusion, unpublished of course, that it forms a polyploid series based on the basic number 12, with the diploid, tetraploid, hexaploid and octoploid numbers (and several probably wrong estimates just below or above the latter, some of these counted...or reported...by us), or $2n = 24, 48, 72$ and 96 , the first typical of *B. bistortoides* and the last of *B. vivipara*, although experiments may perhaps show that more than a single species occur at some levels. The group is clearly of Himalayan origin, as the entire family seems to be, and its main variation still is in the mountains of SE Asia, as far as my knowledge reaches. You may well be right that *B. bistortoides* occurs in Altai and other Asiatic mountains, though cytological informations available from that region indicates that only *B. major* is involved there...but ~~xxx~~ that material may have come from lower altitudes than you visited. However, it supports your observation that only the diploid number has so far been recorded from the Japanese high mountains on Honshu (we have confirmed that number, again unpublished). But even there the situation may be as complex as in America, where we have the diploid species in the Rockies and higher Sierras, but a tetraploid taxon near the coast in California, at least. So Greene was evidently wiser than those who ignored him also in this genus! By the way, I seem to have mislaid our copy of his 1903 paper, *New species of Polygonum*, *Pittonia* 5:197-203, so you could perhaps help with a xerox copy so that we can decide what of his names, if any, belongs to this evidently always proliferous and usually sterile and perhaps apomictic taxon? I would be grateful if you could help, so that these two counts could be made available to those interested.

I am sorry that you evidently were in too great a hurry with your explanations of the origin of some of the Latin names, so you missed even what Fernald did right, as, e.g., for *Aralia* and *Koenigia*. And some other names that could have been dug out of TL-2 by Stafleu and Cowan, which I suppose you have on your shelves? But such nitpicking can be made for the next edition...with our help if needed. And I would be astonished if many readers will see what we automatically observe in such matters, thanks to our training by Hultén long ago.

Because of the political situation in Praha, where the rules say that all printed material sent privately to the members of the laboratories of the Academy or to even visitors, must be given to the library...perhaps a wise decision so that later generations can enjoy the material also? Holub prefers to get his letters and reprints and books to his private postal address: J. Holub, P. O. Box 25, 11121 Praha 1, Czechoslovakia. We have mentioned you frequently to him and others in Praha, so you are well known to them all, even the younger generation, so he will not be astonished to hear from you and see your good book. You may even mention to him reviews not only in Folia Geobotanica, which is in foreign languages, or Preslia, which reviews in Czech, but also in Feddes Repertorium, which he coedits. And give him my best regards.

Otherwise nothing remarkable...though if you were here more frequently, there would be much that could be discussed, when my energy is up. But you have so many other matters to attend to...not least if you correct the major mistakes in the good book of which you are not only the author but also the printer or setter. Hope it will sell fast and effectively all over the northern hemisphere and especially on this continent that has been waiting for it since Rafinesque was "murdered".

All the best to you both from us all.

as ever,

Cushman

Digitized by Hunt Institute for Botanical Documentation

P.S.: An error has crept into our address on your last two letters:
CA 95133 instead of 95123!

ARALIA - French - Canadian Aralie *theaynii* species *Savigny*
the Quebec *physion* *Savoyis*, *Fortaneforo*, *usque* that name.

KOENIGIA ?

San José, December 12. 1987.

Dear Bill.

Thanks for the fine copy of the Green paper on western *Bistorta*..which then he still accepted as a *Polygonum*. I have been going through the observations we have made on this group in the past decades and am in no doubt that it does not contain the 50 species that Airy Shaw accepts in Willis, mainly on the authority of Komarov and Greene, who evidently did not share the viewpoint of Linnaeus and the Linnaeans, who looked for various signs of reproductive isolation. We may be wrong...and I know that most American-educated or English-educated present-day colleagues hardly agree...but I am convinced that most of these taxa are nothing but minor variations that are of little significance, and that the genus in its strict sense includes only one taxon at each of the diploid, tetraploid, hexaploid and octoploid levels...or *B. bistortoides*, ~~*B. vulgaris*~~, ~~*B. pa.*~~, *B. major*, *B. plumosa*, and *B. vivipara*: *bistortoides* is not only western American but occurs also in the high-alpine zones of southern mountains in Asia and even central Italy, though still not formally mentioned from there. I am not impressed by the keenness of the floristic eyes of Komarov and Greene, though they both were very skilled in recognizing the more or less complete incompatibility that characterizes the Linnaean genera. Perhaps somebody in the coming generations will substantiate this by good research, but we publish the numbers of the three species we know, and have earlier published the hexaploid number for the northern Pacific species *plumosa*.

We hope you find some time for your revision of the Eastern Slope flora, which will likely be easier to complete than was the completely original Western Slope volume. I have not seen reviews of the latter yet, but since the printing in America goes so slowly by aid of all the formalities and machineries, it may take the winter before a review comes even in *Taxon*. I am in no doubt that most reviews will be highly positive, despite some idiosyncrasies that may not be as positively accepted as they may be worth...as always...it took six decades before those who stood against continental drift had moved to the other side of the sun...

By the way, when you make your new revision, I hope you avoid mistakes as that of breaking the Code rule of original spelling which you recommend so adamantly for *Eritrichum*, but seem to reject for *Botelua*! Palisot made that latter mistake that Bentham and Hooker then followed with all their English followers on both sides of the ocean...though it certainly is incorrect. Reeder was very adamant about that when I followed the rule in my listing of the taxon from Manitoba in *Taxon*, and Soderstrom wrote me a nasty letter also. I wonder if Reeder or some other will mention his death cause in an obituary in *Taxon*?

I have been revising the English edition of the Icelandic flora that is supposed to get out in the spring, though my energy sometimes is too limited for even that kind of work. Though things go forward, still. Hope John Marr is better...give him my best regards if you can.

With the very best regards and all good Xmas wishes to you both from us all.

As ever,

Carlson

San José, January 12, 1988.

Dear Bill:

I trust that you still bear with my tardiness in thanking for fine letters from December and early January, but I seem to be unable to concentrate properly and to sit long enough at the typewriter to complete a letter as I would have liked to write it, so too many of my efforts are discarded and never sent. But I hope you forgive me when the efforts never result in anything...it would sometimes have been easier if we were in the same room, though you never know. And my strength is, unfortunately, far less than that of Sammy, though she has probably not seen a healthy day for half a century of a disease that is much more difficult than mine.

I am sure that as nomenclature goes, Soják and his mentor Dostál, who both try to follow the hopeless efforts by Wagenitz 1955 and 1962 to sort out the phenetic genera of the *Centaurea* complex...is fine, though not when they include more than one basic number in a genus, because that is contrary to even Linnaeus...and the type method as defined by the Code. But I cannot see why you need to worry so much about their more or less Asiatic genera that even Takhtajan has difficulties with in the Russian flora? This is a taxonomic rather than nomenclatural matter, though sometimes it may not be so easy to separate that kind of problems. *Calcitrapa* of Dostál and Soják is a mess even nomenclatorally, and needs concentration and time for explaining or solving even when accepting the genomic definition that we tried to follow in our 1961 chromosome list that I am sure you have. The reference to Heister ought to be dismissed and that to Fabricius is disputable. Perhaps you could get Holub to discuss this by letter, since he seemed to agree with our solution? And perhaps you could get something more out of Soják and Dostál by looking up how the latter does in his more recent Seznam (or enumeration of the Czech flora) from 1982, of which I got two copies one of which I sent to you according to my notes? Though even our advanced 1961, 1962 approach is far from final for this much too confused group. If you can interest Holub in looking up the enormous literature which is available to him in good old Praha, perhaps you can get some rest, but I can hardly be of much help here, unfortunately.

Calcitrapa and *Leucantha* are good natural and biological genera and both valid. What the little learned compilers of the ING probably mean, when they regard the *L. cyanifolia* of S.F.Gray as illegitimate is simply that it is a synonym of *C. solstitialis* that therefore ought to have been used...as we did in 1961. But *Calcitrapa*, typified by *C. stellata* Lam. with 10 as a basic number is distinct from *Triplocentrum* to which *C. melitensis* belongs with its $x = 12$. Confusing?

Keep up your fight with the prairie bureaucrats who do not understand the problems of your mountain biology any more than they understood that they were mixing chemistry into biology when they decided to support the molecular approach that solves no serious problems. But although you may wait out the bureaucrat that now irritates you most, there will be others after him that do what he now feels is right...the problems of Boulder never end as long as this so-called university with a very small *u* is governed by politicians of little moral and less knowledge.

Hope you continue to read the horror stories of the No Ivory Tower, which describe in part our own horror story and our experience of American "democracy" and "honesty" that is even unknown to you and other colleagues. No more irritation...

It may interest you to know that Doris is in full swing with the translation for Art Cronquist that you suggested to him. She was as shocked as I by hearing about how many of your friends have moved recently to a better(?) world, ~~hick~~ though you hardly cry more for Rosella than we do after our far from good experience of that hag. One of our friends told us about that case also and mentioned that Hobart now kept more to somebody we did not know...is that the homosexual connection that she mentioned in connection with their move from Illinois?

Botelua is the correct spelling, whatever anybody says, though I agree with Hylander that all spellings ought to be grammatically correct and thus corrected. But as long as the foolishness is accepted by the Code and not deleted from it, we must accept even such changes...since otherwise the rules and laws are meaningless. At least for Scandinavians who follow the laws, and especially for Icelanders that were governed only by the law according to Adam from Bremen thousand years ago, or 900? Though Americans always seem to arrange their "laws" to support their crimes! But Botelua is hardly a matter to die for? Even Linnaeus made corrections of his names that are unacceptable according to the present Code,...whatever Clayton says.

I am not astonished that your local colleagues do not even mention the good book, and that none of them reviews it...but am sorry that I could not do that for the simple reason that it would have looked like a bribe or a payment for a favor. I wrote to Holub, who hopefully will give it a good review, though he is not healthy. And I look forward to the new volume and hope you give me opportunities to assist you with some of the points even there...whatever the "great" minds in America say, this combined Colorado flora certainly will be the very best such book for any part of the continent...though you may not hear it said yourself until after the soil has filled your ears and the worms eaten your good brain. But you should feel as sure as did Linnaeus, in the forward to the Species Plantarum, that your grandchildren's generation (nepotes) will judge it favorably, very favorably, because it is good, the very best flora ever written in America, though the prairie greets harvest the honors for their very old-fashioned work instead of you. Such is American life....

Now I must stop since it is evident that I cannot write more today. But thanks for your good letters, and perhaps I can give you better answers next time, who knows? The very best regards from us both, especially to Sammy.

As ever,

Wickham

San José, May 8, 1988.

Dear Bill:

Thanks for your letter that came last week; since I have some difficulty typing except in the late afternoon, I let the letter wait until now, because Doris has been using the typewriter for her important translations practically all the time since your letter came; I hope you can forgive me that I did not react at once, as I should have done when it became evident that you have not received a letter that I wrote in early March as an answer to yours that you mention in this one...but I trust you will forgive, as always. You had asked, once more, about the validation of your Minuopsis that I had not been able to help you with when you proposed it last year...I still am unable to do it, though it is evident from a reference to the series Pungentes Mattfeld (not dt!) on p. 511 in Am. Midl. Natural. 46, 1951 in a paper by Maguire that this name has been validated...but where? There is no detailed reference to the basionym in McNeill either, so perhaps he has been unable to find it too in the great library in Edinburgh? Perhaps some of your friends at New York or elsewhere could try to pull the threads that lie to that spot?

I must admit that I find your friends...if they are more than our friends were found to be when we really needed them...are a bunch of peculiar people when they do not find time to review the good book, but perhaps they do not dare to irritate the great governors of bad American taxonomy by showing some sympathy for really Linnaean classification? But I wonder if you forgot to send a review copy to Taxon, where the genius from Swiss, Schmid, would have reviewed it at once, or so I believe he would...if he does not try to do what the clique wants us all to do with revolutionaries like me and you? Look up the February number of Taxon, p. 200, footnote, for his address at Berkeley.

I admire your persistence in even trying to force the bureaucrats to buy compactors for the herbarium which they evidently do not want to make permanent. Hope you succeed so that your memorial will become more permanent than that of Lloyd Shinners, who had built up a similarly sized herbarium in addition to a reasonable library for its support. Though I doubt that taxonomy will ever grow into anything worth while in this land of yours where the military-industrial complex and the lobby of the police state and its astrological "culture" does everything to suppress European scientific ideas and stimulate the other side, and would not be astonished if soon really scientific musea at provincial universities were abandoned in favor of large collections and centra, as has been done with great success in Europe and the Soviet Union. But keep on fighting, as long as you do not need to be afraid of the knife in the back that we felt.... Perhaps the wisest thing to do would be to permit them to donate the Boulder collection to the Rocky Mountain Herbarium, before they decide to move it far east? Perhaps there you could find also the ivory tower that we found nowhere in America, and retire to there and support that herbarium as the center for the mountain states? But this is only an idea...I say that to nobody other who could misuse it.

Keep on fighting as many fights as you can and dare to take on!

As ever, *Robert*

San José, September 15, 1988.

Dear Bill:

I planned to thank you at once for a good letter of early August, but as usually with the good intentions, I waited just for a day or two...and now more than a month has gone! But I trust that you forgive me, as always.

This has been the hottest summer we have lived anywhere, and although we slept on the top of the bed without cover all nights since early June, we had to suffer sweat and light sleep week after week. Now this seems to be over, at least for some time, though you never know before October is over also, because we are told that September and October are the warmest months in California! At the same time as they are usually filled with smoke from the forest fires...I wonder what is different between the two continents that causes these fires here, and not there...?

We have spent the summer sorting up some parts of our library, though we find nobody interested in buying it in this land of no learning...even Colorado would be interested in so-called "art" that perhaps could be sold, but not in science, as you do not need to be told. However, a few places in America and Europe have asked if we were interested in selling our books, though we have the feeling that the next step in their case will be to ask for donation...the richest nation on earth cannot do science except if getting donations or voluntary contributions! That astonished us from the first week in Winnipeg, where even the hospitals, which are extremely expensive and rich institutions talking about "charity", seem to thrive only if men and women work without pay. A "republican" idea?

I never understood the idea behind the Meeker courses, but perhaps that is caused simply by my density? But although you may have felt you were spending your time for little, I hope you got some rest from the daily troubles and also could enjoy other things in the steppe and mountains...even plants that you have learnt to know better than any others.

Soon you become 70...time goes by...and I am sure you will solve the problem of official retirement satisfactorily, as you solve other problems. But since there are so many fools around you who think illogically and only about themselves, would it not be wisest to let them decide what they feel is best and not spend time to convince them that your ideas are better...which they always must be? If they want to replace you with somebody who does not understand that you planned from the beginning to build a memorial, as did Lloyd Shinnars in his place, why not just leave them in peace...and perhaps look into the possibility to move to the richest place in the west...I am in no doubt that Peter would love to get you to spend your retirement in his large library and herbarium at St. Louis, and you would also enjoy the elderly colleagues who have moved to there from elsewhere. And nowhere else would you get more understanding and assistance, I believe.

And nowhere would you be able to complete the Colorado flora better than there... except perhaps where you worked with it earlier. By the way, when you complete some parts that you think we ought to be able to assist you with taxonomy or nomenclature, I trust you do not hesitate to send us material for critical review.

Ann Odasz was here some few weeks ago on way to Davis and Tromsø, where she evidently thrives. Only a pity that Eilif and the two Olavs are not the leaders they planned to become when we met them first, but she has a younger generation around there also, so perhaps she can get them to stimulate each other up to the stage of DuRietz, Hultén, Skottsberg, etc...who need to be replaced. But perhaps the degeneration of botany is going to become international? Even in the Soviet Union no new botanical ideas seem to be forthcoming...though my pessimism may not be well founded?

Hope your trip to Nepal will be stimulating, though I am not fond of India with all its poverty in the middle of their richness. But that trip I would have liked to share...though now I share no such travels any longer.

Best regards to you both and to John...my situation is unchanged still.

Ann O. S.,
L. S.

San José, November 11, 1988.

Dear Bill:

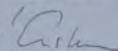
Since I have not written many letters even to my family during the past few months, for various reasons, none of these valid of course, I suddenly came to think of that you will soon reach the maturity of becoming 70 years old, which was more remarkable when we were younger than it is now? So I looked you up in an appropriate place and, alas, I could hardly have thought of this later, since the date seems to be the 16th of November! Our warmest congratulations and thanks for all that you have done for us in the past, and still more for all that you tried to do or wanted to do but could not do in this land of hypocrisy and worse. I am sorry that we could not visit you to shake your hand and show other formal signs of our gratefulness, but know that you forgive that. And we sincerely hope that you will continue to be healthy and strong for many years to come and enjoy the nature around you as ever before and discover new taxa and make new and remarkable observations that others then envy you for. And perhaps you also will find some opportunities to visit California and sit here and discuss common interest.....

I hope your work on your big southern Rocky Mountain flora and its world distributions goes forward and gives you pleasure, and stimulates others to think something great about problems that few others seem to have thought about. I am sorry that you are working with such a miserable library and under other conditions that are less stimulative than they ought to be, but what can one expect from an "academic" institution that is governed by fools who mainly think about how to swindle it economically, and it is hardly your fault that you became stuck thanks to your honesty towards your family and few friends. We have had a similar experience since we left Sweden in 1945 and went to places without understanding of academia where only envy seemed to dominate the small minds, but we could nevertheless get out ideas as the chromosome lists, polyploidy distributions, and even mapping and revisions of interesting groups, by going to places with better facilities to complete what we wanted to complete. The old story about Mohammed and the mountain...as did he when it did not come to him, we visited facilities and libraries...so why do you not consult St. Louis or Gray or even New York or Washington for a week or two at a time, or simply borrow through interlibrary loans what you actually need? Hultén's atlases, and the new work by him and Magnus Fries, are certainly available in American libraries, and so are even the Russian Atlases by Tolmatchev and his group, etc. By the way, I hope you have seen the last, tenth, part of the Arctic Flora SSSR that came during the summer and covers a good deal of the name changes that you proposed in the last family that Siberia and North America share. And then I hope you have seen the ongoing discussion of the genomic classification of the wheatgrasses, that I started some years ago and irritates those Americans who understand least of biological classification...if you were here we would doubtlessly discuss that in detail, since you are one of the very few who understand the biological bases of species and genera....

Our summer was the warmest we have lived and also the driest...but now we have had the first little rain of the winter. Hope your winter will be mild.

With the warmest wishes on your birthday and all good regards to you both from us here in the far west.

As ever,



San José, November 29, 1988.

Dear Bill:

I hope you will try to forgive your lazy correspondent, but this time my excuse is: simply that Doris uses the typewriter from morning to late night to compose a list of our books for a couple of, hopefully, serious bidders. One can always hope, even with experience of the kind we have of our "colleagues" and friends and their helpfulness...you are almost the only one, so we have reason to beg you pardon, when you get no answer...but then again there seems to be a valid (?) excuse in good old Parkinson, though the fact that the chief of the neurology at Kaiser suddenly moved me over to his own patients, when he discovered that the Indian I had been given at the beginning, knows next to nothing about the disease. But is that not true of all the doctors, though they do not hesitate to require full salaries....as do also all the so-called botanists, who have felt they are enough "peers" to judge us...without thinking about the background of the "accusations" that were never presented to us, of course, because "good" American taxonomists must know everything better than foreign criminals, sworn as such by "honest" Americans...you know what I am thinking of. No more introduction.

I am sorry that Hultén (or rather Magnus Fries) has evidently confused you when you slumbered as good old Zeus also did sometimes, so that you did not observe that *Rumex alpestris* Scop. is an arctic-alpine plant that has gone under many other names until I made the identification of it in the 1948 chromosome list: In the Arctic Atlas it goes as *Acetosa pratensis* ssp. *arifolia* (Blytt & Dahl) Å. Löve, which, of course, has nothing to do with *Acetosella paucifolia* or its complex of arctic-alpine hemi-pacific plants. So forget about it in connection with your new interest.

I wonder if you judged us in the same uncritical way as you are evidently judging Meredith Lane, when you decided not to believe in our sake but in the judgement by the great American botanists of the Runner-institute and their great political leader Lawson Crowe? I do not know her, though I have heard others differ from your judgement...but that is just typical of how "peer" committees here select new members of their staff...or perhaps they rather follow the giant bird-brain Cronqvist, who told us, when he saw that his "efforts" had gotten us into the trap that all but you understood, that "he hoped that we now would "behave"! But I hope some will even try to protect her from negative judgement...if she is good. Though we do not bother to warn this peculiar "academic" institution of yours against bringing in more fools and incompetents than it has already done for so long time. But be careful so they do not trap you also and prevent you from making the travels you dream of...

No, for some reason your two (only two?) reviews have been lost somewhere in the mail. But I wonder if you have seen the 10th volume of the Soviet Arctic flora that came last spring? it is filled with fine observations that may even interest you for the Eastern Slope revision.

Now I do not have enough energy to continue so I stop...hope our congratulations on your 70th birthday got into your hands. And hope John Marr and Sammy feel as well as possible, and that you get to the Himalayas away from your troubles.

Merry Xmas, etc. from us all,

as ever, 

San José, July 31, 1989.

Dear Bill:

I am sorry that you had to tell Holub about your "problem" with Bromus that you found to be "curious", because he may recognize it as ignorance of some he expected to have a background similar to his...he certainly knows that your flora is a fine work, and realizes the reason for this I hope. But since he has been sick for years and avoids most writing by necessity, you may never hear from him.

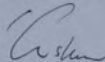
Bromus purgans has long been overlooked by all Linnaeans for the simple reason that even Linnaeus himself could not identify it and it has been kept on the nomen confusum shelf for centuries. Though the "greater American botanists" have ignored it as they prefer also to do with most of the critical code rules that the Kew bunch also ignored. So when you find it used, even as a synonym, you overlook it and replace it with some later name, sometimes with no result, as in the recent lists from here, but sometimes with apparently correct solutions, as I believe Packer came to in his Alberta flora....though his plant may not be identical with that of your trouble?

Hope you are doing well with transferring your collections once more to new cases, and that your time is also taken up with the revision of the old eastern slope flora. And thanks for breaking the long ostracism.

Our situation is like it was before, no great changes except that the many "friends" that kept contacts in the hope of getting some ideas, have slowly been replaced by others...and we do not miss the Boulder and Smithsonian bunch, except you. And I still spend a good deal of time for the wheatgrasses and letter discussions with overseas colleagues working in that field....I am more convinced than ever that the only scientific classification of genera and species that is available to us is the genomic one, and that above that level nothing matters as long as we have not found methods to compare properly the genetical relationships above the genus. So your "unscientific" alphabetical classification ~~is~~ is as good as was that of Linnaeus' higher categories and thus less misleading than other higher groupings.

Hope your summer has been good, our has been extremely dry.

All the best from us all,



Askeff born Oct. 20, 1916.

Oct. 20, 1989

Dear Bill, -

Just to let you know that we escaped without injuries or harm of importance to our belongings in the violent earthquake we just experienced. The shaking was enormous and scary, but all that happened was that all the books in the case behind Askeff's desk were spilled out over the entire room and the desk, chair and lamp. If Askeff had been sitting there as usually, he would have been killed or severely maimed.

Since we took him home from the hospital Sunday, he was now sitting in a rocking chair in the dining room while I was preparing for dinner in the kitchen. He was thrown to the floor, but fortunately without being hurt. I was able to lift him back up into the chair because he is practically unable to move himself. However, with aid he can be put into a walker and slowly with small steps walk from the guestroom or bathroom you used to the entrance door or the kitchen. We both sleep down there because he needs attention day and night. We will have a physical therapist help to train him back to mobility, but he is very weak and without strength after this ordeal so it will most likely take a very long time. His mind is again clear but he is, of course, very tired and does even less than before mentally.

Ingela and I have picked up the books and put them in heaps on the living room floor and to our delight they were practically unharmed after flying around. Only one or two paperbound books lost their outermost cover, but that can be glued on again.

Excuse me for not writing more, but I have so much to do now in addition to all I already did, and I have to inform other people too that we are O.K. We have had phone calls from Turkey, Baffinland, Sweden and Denmark as well as several friends in the US who had heard that San Francisco, San José and Santa Cruz are "leveled to the ground". Somewhat exaggerated except for Santa Cruz which was very badly damaged.

Hope all is well with you both, and best regards in haste from

W. J. Askeff
(73 yrs old
today!)

5780 Chandler ct, SAN JOSÉ, CA 95123

Nov. 7, 1989

Dear Bill, -

Thank you for your letter with the clipping about Bonde and Shushan. I must say the U. of C. does not treat their people very well. We are not so astonished after what we ourselves have gone through, but it still hurts to see what is done to people who were your friends there even if not of any outstanding academic value. Teaching is more demanding than most administrators believe and research demands a different attitude and capacity, something that not often is of equal prominence in one and the same person. Sam and Eric really deserve better and I hope they fight hard for their rights. It is no fun to have to rely mostly on a meager Social Security pension as the only fixed income. Give them our best regards and cheers for a good cause.

We feel for you in your loss of Suki. We know how much a pet can mean and how hard it is to decide that they have to go. But sometimes that is the most humane, far more so than the treatment we sometimes give people who have suffered enough. And I see that you already have found a replacement for her and hope it will soon find a place in your family as firm as that of Suki.

We had already learnt from Kathy Salzberg about John Marr and I have written to Ruby to express our condolences. She must have had a hard time, I really understand that better than most. Parkinsonism is a brutal disease, it sneaks up on you, robs you of all strength and finally, in some cases, even of your mind and reason. I hope she can rest now and that she will be able to live out her days in relative comfort.

We worry about Sammie and her condition. Bedsores are so difficult to cope with because you have after all to lie on your body one way or another. And to be sick and unable to move yourself is awful. I am so glad that Askell is finally able to more or less turn himself around in the bed again. At first I had to get up several times at night just to move him around and my shoulders are still sore and achy after all that lifting. He lost a good deal of weight, but in spite of that he is a heavy body to move, especially the bottom part! But now it is gradually going better although slower than I wish. I have to have patience and that is hard on me.

Boulder Manor was as far as I remember from my visits to Gordon and Marion's mother(-in-law) a nice place to be in but nothing is like home and we hope that Sammie's condition will soon improve so she can return to where she belongs. Give her our very best regards, please.

We are still trying to clean up after the earthquake. The books are still in heaps on the floor, of course in wild disorder, and we also had a closet full of things and papers which totally collapsed, and we are gradually trying to sort that out, throwing away things that have amassed over the years. Why is it that we always put things away "for later use" and never throw it out right away? After all, once we are gone for good somebody else will discard it anyhow, so why not do it yourself? So the garbage cans will be full for a couple of weeks with discards that ought to have gone out years ago.

So in addition to trying to meet the deadlines of my translation work which is a necessary income, especially as it involves our group insurance, we have enough to keep us busy day out and day in.

Your quote on the criticism of Nuttall's generic concepts sounded very familiar. Is the reluctance to accept new ideas and progress a character inherent in botanists? With the exception of a very few "extremists, of course, who, consequently get a "bad press".

Best regards to Sammie and yourself, always

Askeel and

Yours