San José, January 23, 1985.

Dear Bill:

I hope you will forgive that I let the card I got more than three weeks ago wait so long, but the weather has forced me to work in the garden to clear things for the summer if we want to get the fruits then available, in addition to several smaller matters that take time...and of course the approaching old age that makes even me slower in writing letters. But I realize that you have ten times more to think of than we have so you may not even have observed the tardiness.

I am a little worried over that though we sent two weeks ago a package with among others the wheatgrass paper of which I then had received thirty copies, or half of the usual because of the size and costs...I had even added corrections of the round fifteen printing errors, all but one small and insignificant, that could not be avoided because the editors, who sat on the manuscript for one year and a half, could not give me time enough to correct even the last proofs!..., though there are two weeks since it was sent, we have heard from none of the few receiving it, so perhaps some worries are excusable when you have seen before how some mail has atendency to go astray. Of course, one of these copies went to your address...so if you have received it, the others are probably in place too. But if you have not gotten your package, please, tell me so that I can get the mail here to look around in their rooms...hope nothing is wrong nevertheless.

At about the same time as I got my reprints I got one from Dewey on his review of the species growing wild or cultivated here on basis of the same system. We disagree on some small points of misunderstanding I believe...mainly in the Elytrigia group, where he sinks one genus into another and adds complex taxa that causes one of my well-defined genera to take over the role of carry-all from the old Elytrigia...and since he wrote that one of his associates, Wang, has shown that the genomes Dewey claims differ ~~xxxxxxx~~ not are actually distinct in four chromosome pairs but similar in three, just as permitted for different but related genomes according to the definition of the term by Kihara and others, cf. my 1982 paper. I expect that when a little more thinking and evaluating has been done, my Lophopyrum and Thinopyrum will be accepted even by Dewey, also Elytrigia s.str., whereas the group I put as Elytrigia section Trichophorae may need to be lifted to generic status, for instance as Trichopyrum based on the sectional description by Dubovik...for old A. intermedium etc. You will understand my thoughts when you see the paper. In that connection I must ask you if Dewey sent you a copy, since if he did not, you will want to ask him for one copy of his very fine final review of a work that nobody has done as well. His paper is named The genomic system of classification as a guide to intergeneric hybridization with the perennial Triticeae, and is printed in a book on Gene manipulation in plant improvement edited by J. P. Gustafson and published 1984 by Plenum Publishing Corporation. Needless to say, the genomic system is the same I have been advocating for years and which we have shared and Cronquist misunderstood like the other popes of the groups fighting about the meaningless terms lumpers and splitters that Europeans never have understood because they do not know the background...although in the wheatgrasses several genomes differ cytologically and genically despite having the same basic number, in many other taxa the only indication of differences in genomic constitution may be different basic numbers. Or different basic numbers and variations in chromosome morphology, as, e.g. in Crepis.

When you sent me the Index Herbariorum copy of my own listing, I believe I thanked you for it and asked why no mention is made there of the fact that a substantial part of our collections are in your good herbarium. You have not told me the reason, which I am sure has nothing to do with the ostracism, which probably has caused some other omissions in several herbaria that we sent duplicates to already from Winnipeg...perhaps I am being unduly sensitive and the explanation simply that you listed your collectors so many years ago for the Index that we had not even arrived at Boulder? And some as simple explanations may also be valid elsewhere....

Did I not mention to you that we were thinking of that perhaps we ought to use the present possibilities to donate (actually sell) our library to the herbarium to strengthen it, through the newly invented methods of the Foundation...or to donate it to the Instaar library? If you agree that such an action might add strength to your arguments for the permanence of the herbarium, we would like to continue that way of thinking...though I realize that when you now are preparing to move to new quarters you have little time even to think about other matters. But our time is also shortening so you excuse the question which is meant well.

I am sorry that we are so far away that we can only enjoy in our thoughts your happiness over the new Pringle and Penland collections...but your expressions about them are typical of a fine museum man of which there may not be too many.

Yes, Oreophylax we used when publishing the chromosome numbers of some New Zealand and South American gentians in Taxon 32, 1983, August, p. 511. I am convinced that Airy Shaw was right when he accepted that name as validated by Kusnezev in his Gentiana monography in Pflanzenfamilies long ago, because that was done correctly as then required. However, all S. Am. gentians do not belong here, and perhaps fewer than we believe of the Australian-New Zealand ones, which have caught the interest of a young man, Adams, in Canberra...it depends on what species are regarded as types of Selatium D. Don, in Gen. syst. gardening 4:181, etc. and what is to be done with G. thyrsoidea that is basic for Dasystephana Griseb. which may have been meesed up earlier or later, I do not know or remember any more. But Oreophylax I believe is typified by O. magellanicus which certainly is cogeneric with the New Zealand material I have cultivated or seen.

We have seen that you are "enjoying" one of these last cold winters that must please the popes of the greenhouse effect negatively while it seems to fall into the pattern from the Greenland research that two decades ago indicated that we were nearing cold periods and even the coming iceage. In Iceland the past decades at least has gone from one record cold to another year after year...and the colds in Europe and here is greater than ever this century. Hope only that our coming relatives will not suffer too much when revolutions against the few rich that keep the food start in the coming centuries...as the pessimists in the Greenland research team mentioned. Even California is cold this winter so we have had rimfrost on the roofs for a month now and still have cold and foggy days that remind us of good old Scania. But only climatically, of course... and there one cannot see ripening citrus fruit when one looks out through the window.

Not more to take up your time this time...but I hope you, and the others, got the copy of the wheatgrass paper because otherwise everybody may have to xerox it by themselves...if interested in such revolutionary ideas.

All the best to you all for a fine New Year with a finer herbarium in a fine and good new place.

As ever,

San José, December 26, 1984.

Dear Bill:

Hope you had a good Xmas free from snowstorms and cold winds, since we did not hear about bad weather on the other side of the mountains the past few days.

Thanks for the list of collectors in the Index Herbariorum. It is better than Boivin shows in his Canadian list, though far from complete, probably because the curators contacted either did not have time to spend for such unnecessary efforts, or simply because they were asked long before some of the material was delivered, the latter probably being the cause of the missing information about the collections we left at Colorado? And Meyer probably was sloppy, as usually, when mentioning only Doris as the collector of the material we left at Ljubljana, not only 1971 but still more so 1972, such happens. But many thanks for your help.

You demonstrate just what I have been warning about all the time in the making of chromosome lists that do not drop old numbers that have been proven wrong by better technicians or more critical botanists. The Russian list is good as help to find even older works, but bad because it ignores even when authors correct their own mistakes, so it is highly misleading for any botanist who uncritically accepts its reports without going back to the originals. Veronica s.lat. is a genus of small chromosomes that may be difficult to count even in mitotic preparations, but when the preparations were meiotic and the student not a trained cytologist, numerous mistakes crept in...especially when the counts were made by Lehmann or his students, who were skilled taxonomists rather than cytologists...at times when the fixing and preparation and even microscopy were not always perfect. If you had looked into our Slovenian list, perhaps you would have avoided your nervosity for that something wrong was being made by me in my advise about the chromosome numbers of V, hederifolia, and also if you had had later works or references, especially those by Manfred Fischer in Vienna, who is the worlds specialist on the genus at present...but a somewhat conservative taxonomist at upper levels, though of our opinion at the lower ones. So there is no doubt that Cochlidiospermum Opiz is the correct name of the restricted genus typified by C. hederifolia, which has, s.str. 54 chromosomes, as has C. cymbalaria...and both have been split, for SW Asiatic material etc., into several smaller species that are either diploid or tetraploid. Species with other numbers that are sometimes included in these taxa...though not lately except in F.E.... are, e.g., V. triloba Opiz (18), V. sublobata M. Fischer (36), V. hederoides M. Fischer (18), V. sibthorpoides Deb. & Deg., etc. Also, Pocilla s.str. as typified by P. agrestis (L.), always has 7 chromosomes as a basic number. Since I was in doubt that our count and that of some others of 7 for V. arvensis was correct..the fixation was not good enough, I hesitated, as you see in the F. of I., to make the formal transfer to Pocilla. Now I have seen, in works by Fischer and confirmed in a letter from him received just before Xmas, that this hesitation was warranted, since the number is actually 16...as it is also in V. dillenii and verna, the other two taxa that belong in the sect. Alsinebe Griseb. Gruppe Microspermae Lehmann, 1910, in Zeitschr. f. Bot. 2:600, which ought to be used as a basionym of another well defined genus...which might perhaps be called Veronicodes? So why do you not make that transfer...or combine our names for it if you so feel...in the next Colorado report? arvensis is then the type of that genus, whereas the type of Pocilla seems to be agrestis(L.).

It was nice to hear about the additions you got from the Penland collections that certainly increase the importance of the Boulder herbarium immensely. I do not remember if I ever met Bill Penland in Colorado Springs, but I had the fortune to dine with him and his wife at the home of my old friend, Henry Wilhelm Jensen, which was at Harvard working with Rumex, when I first came in contact with him during the war but met only at his Warren Wilson College in Swannanoa in North Carolina in January 1972, when I lectured on cytotaxonomy for the graduate class at Winston-Salem for ten days and took the way back over Swannanoa and Knoxville. Henry was a Danish-American, as was his wife, and though he dis well at Harvard at the same time as Stebbins was there, I understood that some discrimination caused by his religious belief (baptist as Jens Clausen was.. that is the same tabu in Denmark as radical or simply foreign political opinions are here and then especially in the Colorado of the Coors family etc.) resulted in that he came to this small religious college, where he became president and did very fine work with the youngsters...and turned into a poet and author of works that I like. When I came to his house in the beautiful slopes above Swannanoa, the Penlands were there...Bill taught there that spring...and we had nice talk about the west and southwest for several hours, so I understood that his collections would be most important. So, my very best congratulations. I understand from what you say that he is no more...neither is Jensen...but perhaps Bill's wife still is with us, though Mrs. Jensen has also gone? And it was like Jack Carter to give the herbarium also their Pringle collections that will strengthen your place as a Mexican collection also.

As to the Minuartia discussion, the Mattfeld paper of interest is from 1921 in Bot. Jahrb., Beibl. No. 126, but perhaps more so from his 1922 paper in Feddes Repert. Beih. 15, where he distinguishes between the various American taxa related to the section Sclerophylla as representing distinct series. On p. 8 I believe series Pungentes Mattf. is described...that should be the basionym for the western genus encompassing M. nuttallii, which, according to Hartman, has 36 chromosomes, as contrasted to the type species M. michauxii of the section Sclerophylla, which has 30 chromosomes according to Favarger. Hope you find the Mattfeld reference, I could xerox the appropriate pages in the McNeill paper on other nomenclatural details if you so feel. #15821

No, we never have had difficulties in distinguishing the northern and arctic Lidia biflora from the more southern L. obtusiflora...perhaps your difficulties may be caused by misidentification of the latter as the former from Colorado, where one would expect only the latter to occur? Try the key in Packer.

Where is the herbarium to be located...we do not have a campus map and so do not even know where the women's gymnasium is located. I can understand your enthusiasm after all these years of too limited space, but hope you also will have plenty of space for increasing the library which is most important to make this stark growing collection fully useful as a basis for a graduate taxonomy department, which in every respect would be at least as necessary as the molecular group which gets all the money and religious propaganda from above. But perhaps the pooled income fund of the U. of C. Foundation has a sister institution at Laramie, in case Hartman would be interested in adding our books and pamphlets to their library? If you ever come again for a visit, that might be one point to discuss.

We are enjoying Xmas rains, of course, and hope your white Xmas is as nice as we remember it from the years in the Rockies...though we never enjoyed that winter.

All the best regards and wishes for the coming year from all he

As ever,

San José, February 18, 1985.

Dear Bill:

Sorry for the too long delay in responding to your fine letter, perhaps you can write my slowness on the account of age...or laziness? you know that I am busy with both!

Of course I am glad to know that you appreciate my Triticeae effort and that you understand its principles that are the same as previously used for even the family Gentianaceae etc., etc. and identical to the reasons for the ostracism of even Richard Goldschmidt by the Harvard club chaired by the great non-geneticist neo-Darwinian Mayr...but as you, I do not bother what they say or do, even when they tell the young about how dangerous such ideas are...they may see later who is wrong. And even I and you are open for suggestions of corrections and additions to our ideas and proposals as long as they do not break the basic evolutionary principles that we are trying to follow...and know they are those of the future generations. To mention a change that I will add in the August number of Taxon...or perhaps not before in the November number; thanks to new studies, still unpublished, by the young Dr. Richard Wang, the successor of Dewey, which explained to me my confusion (and that of Dewey) of the observations and their interpretations for what I defined as Elytrigia with an EJS genome, whereas the correct genome for that genus s.str. is clearly EES, whereas a new genus Trichopyrum is needed for the section Trichophorum which has J and S. And some other changes are likely to be added when more cytological studies are made, of course, though most of what you find in the conspectus I believe will stand for a long time, perhaps ever. Several others, even in America, seem to follow me and be interested in what has been done despite of the "foolishness" to publish this where great "reviewers" could not stop it, as usually...Although I got only 30 reprints, I have already gotten over 300 requests, mostly from Europe but also from America, Asia and Australia, of course. The friendly Dewey proposed that he might be able to help to xerox some and send them out for me, so I gave him two copies and a list of about 30 names of people who had helped me during the long process, but to others I have answered that since I am unable to send them reprints of this long paper, they should feel free to xerox it for personal use. Dewey was wrong in his nice paper when he claimed that Nevski had been 31 when he died, he was not even 30 in 1938, he must have been one of the geniuses with the gene for creativity and schizophrenia that also we regard as our family misfortune and fortune at the same time, you probably remember that my younger brother Jón has studied its inheritance more than any others...he has the bad luck of having one son with the negative affliction, another with the positive one, and a grandson who is or seems to be autistic. That Nevski belonged here is only my guess, I have been unable to get my Leningrad friends to tell me what was the cause of his death, but the young Avdulov used cytology to build a firm basis for the tribal division of the grasses in 1929-31, when he was about 30, then broke down gradually the next few years, spent a lifetime in an asylum until he was "healed" by the modern chemicals that also kill geniality as effectively as do socalled "shock" treatments, and died in his seventies in the classical state of a former genius. Otherwise many of the untreated geniuses and schizophrenics who create our great progresses and are hated by the small minds end their lives by suicide - to which others have not the courage, according to the ideas of my brother Jón. No more about that now.

I believe I already told you what I know about Minuopsis and its story, but I have evidently filed my copies of the letter as effectively as the great crooks at Boulder file their dossiers so I cannot find the copy. I believe, however, that you may be mistaken to put emphasis on what you call similarities between M. nuttallii and Alsinanthe micrantha, since even less evident dissimilarities count more in logic and taxonomy...and similarities are always difficult to prove if they are real. And I am not convinced that you made a mistake in putting hookeri with Eremogone,though that is only my hunch, because I know both groups too little. Since the problem and its recognition is more yours than mine, why do you not make a blurr of what you think and let me look at it and criticize it, rather than the opposite that you propose?

Since my knowledge of the western pygmy gentian remains superficial, my proposal to put it in Ciminalis is purely nomenclatural, so your friend Spence may well be right in believing that it is generally distinct from the Alpique taxa...but how does he know and what is his definition of genera? Also, only experiments and cytological studies can solve the problem of one or two species here...not simple opinions based on superficial or subjective observations that frequently mislead. But I have the tendency to trust the judgement of good taxonomical eyes, as are yours and those of Hitchcock and ignore Cronquistian decisions that frequently are worth less than his strong voice and large body require...in some way or another I have gotten the feeling that the old story of David and Goliath still has its significance and that intelligence of bodily giants has a tendency to be more restricted than that of the bodily dwarfs...though that may be affected by my size! The genus Ciminalis certainly has preference over the genus Chondrophylla as defined by Bunge (as a section) and Nelson, if both are strictly defined by aid of correct typification...though even that still may not be too certain. But we need firm information about the cytology of the Siberian-American taxa to be sure that they are different from the Alpique material in basic number at least, and since that information is at least not known to me, I would regard it wisest to follow Holub's and mine conclusion as to the very discontinuous distribution of the genus Ciminalis in the wider sense including what Spence seems to think could be called Chondrophylla. Does his taxon fall within that of Bunge as defined in Flora SSSR, or does he have strong data that contradict the other view? Encourage him to try to get to the bottom of the problem by aid of critical typification and strict definition of each taxon in terms that fit the first descriptions of each name, but use my name until that has been done. I will then be the first to admit my mistake, if that is his conclusion and yours.

Thanks for the Alma-Hosier Pass suggestion and invitation, though we have some health problems caused and aggravated by the stress of our case, as originally hoped by the crook that nobody dared to touch for some reasons, that may prevent our acceptance. But it should please you to know that if we could afford to send Lóa and Ingela with you to the Galapagos that interests them much, we would do it because we realize that no better guide will ever be found...immigrants that are stabbed and prevented from working never get rich in your country, or elsewhere. But I am only afraid that you may be doing too many things so that all your activities in fields that give you honors and pleasure may prevent you from getting completed the flora that will be based on future principles more than any such book anywhere, and that they may affect your health as well. Hope not.

I believe I forgot to mention earlier that your good flora may perhaps be affected by our taxonomical conclusions in connection with chromosome reports on some taxa that we published last year in Taxon and have in press there now. The first was in the November Taxon, pp. 759-60 and concerns what we learned to call Matricaria. We have long tried to cross the three or rather four main taxa involved, but never succeeded, so we are in no doubt that Polyakov was right in his review book on the Asteraceae where he placed them in different genera and different subtribes. So the two that occur in your area ought to be named Matricaria perforata ~~xxx~~ Mérat (Matricaria inodora L., Tripleurospermum inodorum), and Lepidotheca suaveolens (Pursh) Nutt. And in the two coming numbers of Taxon this year you will find some adjustments and splittings of traditional Sedum (which is correctly typified by the small group S. acre s.str. with the basic number 10) and some related taxa. So, there you will hopefully be pleased to realize that Bulliarda aquatica (L.) DC. is the correct name for what has incorrectly be called Tillaea, because the former is typified by that species which has 7 as its basic number, whereas Tillea s.str. is typified by T. muscosa with the basic number 8. And then you will see that we validate the following new genera and combinations of Colorado plants: Amerosedum debile, A. lanceolatum, A. subalpinum, and Cockerellia cockerellii. But you may prefer to wait to aceept them until you see this in print, in the February and May Taxon.

As to the discussion of the future of our library, it pleases me that you want it for the herbarium, because that would help to secure that it grows into a good taxonomical institution or department at the graduate level...taxonomy does not need undergraduate courses others than general biological ones...it is at a level similar to the socalled molecular field, though it embraces several other approaches that may never be met with in a biology group in Boulder. And I am sure that at Boulder it would be much more useful than to my Icelanders who are very interested for the sake of their own library offerings. And I agree that though Instaar is interested, it will never grow into a botanical institution, since its staff is hardly of that kind now and has never been. I believe the information I have on the Foundation that we could solve the economic problem through what they call the Pooled Income Fund that has been organized by one Thomas B. Hunt, the director of planned giving at the Foundation. Perhaps you could contact him informally and ask him for his detailed reviews of this fund and how it works and if I understand right that it could solve the problems through an~~xx~~ understanding evaluation of the collection. Without informing him who are involved and in what way, for personal reasons, until there is a good reason that that could be the solution...the sooner the better. Hope you can find time and opportunity for such a contact soon...and that will then open up more similar opportunities for you to solve some of the great problems that have taken up much too much of your time in the past.

Our spring is here, though only some early fruittrees are flowering, and still too early to dig the garden. But time is fast coming for setting onions and sowing hardy vegetables, so you will ~~fixxx~~ forgive me if I again am slow in writing when you have sent me another letter. Though I will try my best.

With the very best regards and all good wishes to you both from us all, who always hope to see you here some time again. What about Jack?

As ever,

P.S.: You forgot the question about the lack of COLO for our Index Herbariorum list.

San José, May 17, 1985.

Dear Bill:

Many thanks for a good letter of May 6, which just arrived. In it you do not mention a couple of letters that I sent last month, but since they contained nothing important I am not astonished...though this is unlike you so I wonder if there may be some disorder on the campus mail during your work on the moving? I can see that you are putting as much energy into this move as you always have put into the herbarium work, so I hope the dream comes true and that the herbarium will at long last get its permanent place in the University. But is it worth it is another question, and perhaps a phonecall from one of our former colleagues, out of the blue sky, that seemed to be only a chat about nothing though he added in the middle some remarks about that the Museum had at long last got rid of you and the herbarium that nobody there or at the Biology actually bothered about, may show that some of the old spirit still lives among these great scientist-specialists in slander. But I know you do not bother about such nonsense from that direction and hope that everything will be as you dream about it.

I do not understand that you suddenly talk about retiring in June to return on an honorarium appointment...is that your idea or somebody's elses who wants nothing good for you or botany? I hope the arrangement will be ironclad and not something that the highly rotten politicians in the daministration have thought up in order to punish you for your great scientific activities in a hated field? And that this will give you an opportunity to complete the flora and many other things with less worries than before....but you say nothing about a replacement for you, so where does it leave the herbarium? Perhaps something is going on behind your back, as always in that place, since the phonecaller, the name of whom I promised never to reveal, as he asked for some reason not given, even mentioned that there was some talk about uniting this and other Colorado herbaria and the Rocky Mountain herbarium. I know you will keep me posted, and so will I, if the slanderer comes back, though I hope I am unduly critical for a good reason of long experience of these "gentlemen".

You mention nothing about the Russian trip that you hinted at some time with hesitation, or about the Galapagos trip that was announced during the winter, but perhaps you have left these pleasures to others for the sake of the herbarium? And I am sorry that your lack of help with the move evidently prevents you from doing your research work...though time for that ought to be plenty if your dream about the positive changes really come true, as we sincerely hope also.

Recently I have been going through material of the Elytrigia group that I did not work out for various reasons during the wheatgrass revision, and have found reasons to propose two new genera to solve the problem that Dewey muddled in in his otherwise good paper...you have to excuse an agronomist who is a perfect cytogeneticist and a real Man that can be admired, though his training in taxonomy may have not been that of our own experience. But without his willingness to be my prophet the revolution might have needed several more generations in America, though elsewhere it seems to go smoothly, at last...and the interest is such that I have gotten more than 500 requests for reprints that I cannot meet, and more arrive every week still. So do not feel that botany is dying, at leastnot the modern approach that is so little understood among the "great", no "tall" bodies who dominate here and try to force the Bentham view into the future. I am shocked about what I see from some reports on the prairies flora that evidently is being composed by Cronquistians as far from modern points of view as it is from the Linnaean on

No more today, but you should know that we continue to wish you the very best.

As ever, Áskell

San José, June 4, 1985.

Dear Bill:

I do not know what has happened to the postal effectivity, but your good letter, dated and stamped in Boulder on May 21, was again stamped in Denver May 28 and just arrived. So I doubt that this will reach you before you leave for the Galapagos in five days time...but then it will welcome you back to the great moving when you return ten days later. Is all this travel really worth it, and at your age would it not be wiser to spend time for other things than showing rich amateurs the plants of the mountains that they will forget the next day? But that is your matter, and if it pleases you and rests you from the great work with the moving, why ask?

We have been spending the time similarly as usually, of course, and since our fruittrees and berry bushes etc. continue to give us good food, it takes a good deal of time to water them and treat them properly so they can give us the berries and fruit and vegetables we need. This cool spring has been unusually good for strawberries (which now are sweet thanks to that they developed during nights about 10-15 degrees Celsius, a fact that I learned when I bred that plant in Sweden and Iceland) cherries, of which we have never had so much and so large fruits, blackberries that are out of this world, and even red currants so many that we have enjoyed the good old Danish rödgröd med flöde...red porrage with cream...which is the Xmas dessert where the berries grow best, and we are beginning to get some gooseberries though these do not seem to like us or our soil or climate...or perhaps I do not know yet how to please them...that kind of porridge I got first in Sweden and like it more than most other desserts for warm summer days...though we have yet to get some such days now. Then are the apples and pears, prunes and plums (Japanese sour Santa Rosa from Burbank), apricots and nectarines, all coming next month, not to forget that we continue since Newyear to pick a dozen oranges and some grapefruit for our morning drink, and hope that this summer will give us flowers on the socalled kiwifruit that is just getting that old...but if so that will be for November, we are told. In addition to this farming work there are the wheatgrasses that I continue to revise, this time I think I have gotten over the mess I had to leave in Elytrigia that needs to be split into that genus s.str. with repens and its races, and two other European genera. You know better than others in this conservative land of Bentham-Hookerians that even their kind of taxonomy is never as stable as they claim, and that that of us Linnaeans evolves with greater speed when we proceed further! But it has been fun, as Hultén said.

Although you believe you know your Pappenheimers, as we say in Scandinavia since the 30 years war, you evidently do not: your guess about Pennak was wrong, but since my promises stand whatever is done to me and I promised to keep quiet about who it was, you will never know it from me. But it was another of the many, or all? in Boulder who have no fortitude and no conscience for what is done wrong to others, and one of these small souls that have a pleasure of laughing at others and wishing them the same fate they have in their laziness and inability to think or work properly. So forget it, I should never had said a word..but that would then have been unlike me as you know me since so long. Pennak never said anything negative about you earlier, but much about the no-good Sam and some others. And he and the others forgot to repay me for having stuck out my neck for those who were going to be denied University fellowships by the criminals in the socalled molecular crowd...why I did it for Rogers and Crumpacker I cannot understand now, or even for the false little Gregg, but that is my conscience, if I had known the rottenness of the place I would have stayed at Montreal, wise afterward

I am pleased to read what you say about Barkley, since I had gotten the feeling that he has no knowledge of the basic principles of taxonomy and the honesty that a scientist must believe in...see how he nastily bypasses a reference to our genus Packera and other revision of his messy socalled Senecio in his miserable Taxon paper... he even had the nerve afterwards to write me and ask for a copy of the Triticeae review adding that he would not be able to even take that into consideration...if he had known how the true Linnaeans write floras, he would have realized that their printers always have complained about the constant changes and corrections even of names all the time until the presses have started. And sometimes even after that. The plains flora evidently will be as plain as those by Cronquist and the other Bentham-Hookerians...I am astonished that even the Russians welcome him and others of similar meaning that go straight against the Linnaean views of all their leading men.... though I have seen how they bend backwards to please and get friendly even with American fascists for whom they let 20 million people be killed by the German fascists to save democracy that the former continue to kill...I am not astonished that nothing is said in the American press about the unrest among Icelandic poor people over the two great new spying stations there that evidently are for atomic weapons though the socalled vicepresident clown from Texas continues to say when he visits "my friend Mr. Hallgrímsson" that atomic bombs are not kept in Iceland! That great man had the stomach to say the other month, when laborers in the northwest, where I was brought up, asked about these bombs, that they should be happy because thanks to these stations close to their houses they would never know when the next war starts because they would become atomized. These are the "honest" men who forced their armies onto a peaceful cultural nation by aid of paid for Quislings...to "protect" it from "enemies" it never had had! Are you astonished that the great Greek former professor at Berkeley went back home to convince his nation that it ought to force out the foreign armies and take up the Swedish socialism system that the rich unwise men here call communism...in their simplicity? No more politics, but I have just received Icelandic papers that shock me...though they do not know with what falseness even honest Icelandic scientists are being treated in the "land of the free" capitalists.

I know that your new herbarium will be a place of great joy for you and the future and that from it will come a proper botanical research and teaching that we dreamt of and thought, in our ignorance of the American academic system, always had been. And then hope that your flora in the style of the Linnaeans will affect the young genration so that it sweepes away the charlatans w ho dominate the field with their politics. One can always dream, that is why you spend all your time to move and improve the facilities in the last few years of your tenure, as only great men can do when they think about others than themselves.

Hope that the Galapagos trip will be rejuvenating and stimulating and that you return from it rested rather than tired. Only a pity that this was not done more than two decades aga so that we could have been a part of that future, as we thought we would be when we went for our American dream that was drowned in discrimination and CIA contacts from the first week and then in the eternal air of lack of courage and fortitude that, unfortunately, seems to have been an American virtue since the first Indians were killed trying to save their land.

All the best to you all from us all...Ingela graduates this week and is already working so she can go to Santa Cruz in the fall...

Sincerely,

Áskell

San José, August 9, 1985.

Dr. William A. Weber,

Museum,
Campus Box 218,
University of Colorado,
Boulder, Colorado 80309.

Dear Bill:

Many thanks for your August 2 letter and for its clearly expressed friendship and generosity. It came first on Thursday the 8th when we had a visit from Davis by our former student Pat McGuire who also admired your amiability. Nothing would have pleased us more than be able to share with you the pleasure of seeing your longwaited victory in the fight for your good herbarium, though we would have hesitated to become gate-crashers at one of the "see-what-I-am-great" symposia at Saint Louis to which we have never been invited, not even when the discussion was about continental drift for which we had long been ridiculed by the same crowd that secured through propaganda for the same idea a membership in the academy for the great man. However, this morning, before I had gotten energy to write a letter of acceptance to send you, I got a short phonecall from what an unknown voice identified as "your supervisors in Boulder", that said that they had "heard that you are being invited to the opening of a new museum division here", continuing: "may we perhaps add an invitation to share (the ? or a?) resting place with your friend Thomas Řiha?" and then slammed the phone down. I believe that decides that our answer must be in the negative. I realize that you and Jack believe that all this phone-matter means nothing and should be ignored, or even that it is simply my imagination, but what would you yourself do in a similar situation among distinctly hostile socalled colleagues and friends that slowly but distinctly has eroded our former horse health and our former boundless energy and optimism?

Naturally, I am sorry to disappoint you. But if you still think I could be of help in improving some of the taxonomy and nomenclature of your new and progressive flora, you should know that I am eager to do so, provided that the manuscript reaches me before my health deteriorates more by aid of the effective ostracism through American fairness and honesty towards immigrants with other points of view. You are different.

All the best from us both,

Yours,

Áskell

Áskell Löve

San José, September 4, 1985.

Dear Bill:

Thanks for your letter in which you doubt that I tell you the truth about the telephone threats that have kept me from sleeping properly for years. I admit that they look suspect to any honest man, even in this land of disappearances and murders of those who think differently. But although I understood that your lack of the promised actions after you had come to intercept my letter planned for the great Jack Fogg in 1978 indicated that you did not believe I was telling you the truth, I am shocked to read now that you evidently think I am such a fool as to lie even to you when there is no need for it. Unfortunately, you and Jack Ives are not the American Emile's Zola of Dreyfus fame that I hoped for, and I have later learned that no Americans react as he did without any hope of remuneration, so you are excused and forgiven. But, please, do not call me a liar, since where I was brought up that would be almost as serious as a murder is where you have spent your life, Icelandic ethics and morals differ widely from American ones.

Your manuscript, which I hope I may keep, has given me great pleasure since it arrived, so I have spent all my time to read it and study and read it again. My admiration for it is such that I do not hesitate to say that the book will evidently become the first really modern evolutionary treatment of any sizable flora (the Icelandic one is too small to matter that much), and when you read my few and not serious notes enclosed, you will, hopefully, realize that I cannot avoid to regard your fine work as sinfully excellent so that I expect that it will start a revolution that will sweep away the Hooker-Bentham and phenetic conservatism that has for much too many decades kept not only American taxonomy from rising to the plateau of evolutionary genetics that was its aim long ago, time has long come for that revolution in biology which would be comparable to the revolution in physics caused by the mind of Newton...so perhaps our field will at long last get to become admired as the center of biology it should be? I hope you will find at least some of my notes useful in making the book still better, and that you will get it published soon so the book can begin to influence the botanical world and push it through the second revolution since Linnaeus.

I realize that we will get a copy as soon as it will be available, but I hope you will then also see to that leading journals in America and Europe will get review copies and also that modern colleagues, as Yurtsev and Malyshev and Rauschert and even Jóhann Pálsson of the Botanical Garden at Akureyri in Iceland and Bengt Jonsell of the Bergianska Trädgården, who is revising Krok-Almqvist, and even the conservative Gjaerevoll who tries to keep Lids Flora old-fashioned, will get their copies. Everybody in our field should be told about the revolution.

We are sorry that we cannot or dare not be at the opening of your herbarium, perhaps only for an imaginary reason as you believe, but wish you all good then and ever.

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

As ever,

Áskell

12 Sept. 1985

Dear A. & D.:

I have a few questions and fewer arguments with you about your suggestions on the Flora.

1. For A. distentifolium and alpestre, see Lellinger, D. B. 1981. Notes on North American ferns 71:91-94. "Fuchs (a974) adopted the specific epithet distentifolium Tausch ex Opiz, rather than alpestre (Hoppe) de Claireville... He claimed that de Claireville (1813, p. 301) published a new species, rather than a transfer of Hoppe's name, which was not cited. Fuchs also thought that Aspidium alpestre Hoppe could be considered a superfluous name because Hoppe thought it might be the same as A. rhaeticum Roth. However, this is incorrect, for a superfluous name requires that the author include the type of the name being replaced, and this Hoppe did not do....De Claireville's work is an excursion flora in which the introduction states that it is not a technical work. DeClaireville's intent was not to name a new species but to transfer Hoppe's name. Therefore, it seems clear that the epithet alpestre (Hoppe) de Claireville should be reinstated."

2. All of your remarks about the plates are moot because those are just the old plates with their old captions, most of the new plates have not been given captions yet. No more comments about these.

3. What is the derivation of the genus name Jacea?

4. I agree with you a hundred percent about the Eutrema. On the other hand, the Braya humilis ssp. ventosa looks like a good species to me, compared to all the other B. humilis I have seen!

5. I would like to accept Atriplex aptera, but I have no clear evidence that I have it on the western slope. Collections rarely have fruit (too many are staminate) and the size of the plant is rarely mentioned. I am sure I collected it in the San Luis Valley because when I got it I simply couldn't believe that it could belong in A. canescens.

6. I have been having second thoughts about Atriplex pleiantha; its characters are enough to justify a separate genus (position of embryo, numerous flowers in the bract pair, perianth segments present etc.). But I'll probably let this sleeping dog lie.

7. Clausen gives Sedum subalpinum as a subspecies of lanceolatum, citing the same chromosome number for both, and intergradations. At high altitudes the rosettes tend to be smaller and more numerous but he wouldn't go so far as make it a binomial.

8. Nelson & Couch in 1984 annotated all of our Myriophyllum as exalbescens Fernald. I shall leave it at that.

9. What is the author of Cylindropyrum cylindricu[m?] and has it been published?

10. Podagrostis would be fine except for the fact that the epidermal net on the palea does not seem to be present in P. thurberiana. This is why I have moved it back for the time being. I used to like Podagrostis too.

11. I cannot agree with you in making Elymus scribneri, one of the most distinctive and distinct tundra grasses, a subspecies of Elymus trachycaulus. Can you ignore the morphology and ecology of this plant?

12. I'm so glad to find a place for Phalaris arundinacea; had meant to ask you whether Baldingera was a possibility but I see Phalaroides has priority over any other possibilities.

13. My feeling about Phippsia is that in this case I would rather see someone successfully propose conservation of Puccinellia rather than force all of those nomenclatural changes.

14. When I was at the British Museum I tried to find that particular volume of Houttuyn to find out who Reynoutria honored, but did not succeed. They went to great lengths to try to help me. Perhaps one of my Dutch friends migfht find it.

15. Rumex salicifolius seemed to be the best wastebasket to put the R. triangulivalvis-utahensis complex in, which I still don't understand. I think I'll leave it at that.

16. Re Steironema and Lysimachia. You should also see the wild thing that they put into Lysimachia (glutinosa Rock) in Hawaii! A big evergreen shrub with big solitary waxy white flowers.

17. Viola aduncoides L. & L. Did you overlook three Rydberg names that apply to our western "V. adunca"? V. montanensis, V. odontophora, and V. monticola Rydb., all published in the Flora of Montana. V. montanensis was the name that our plant went under for a long time, at least in Wyoming. I don't know enough about this complex to take a stand on it at the present time.

18. On the Viola epipsiloides, I need to know whether the plant that I have under epipsila repens (very tentatively) in my key is what you are calling epipsiloides.

19. I don't understand your comment on Cerastium arvense. Cerastium strictum is Linnaeus Sp. Pl., not Haenke, unless you have other information, and it was from Austria and the Alps. None of Colorado's C. arvense is introduced here, and I am unable to know what you mean by "lowland". Our lowland at 5.000 ft?. Tell me more about this.

With the rest of your comments I am inclined to accept practically all of them. However, I have grave thoughts publication. This is going to be so controversial that it really will be a problem for the press to publish it for an amateur user, for which whom it is intended. It will go out to conservative readers, I am sure, who will tear it to pieces and ultimately the press will say that it is not in our interest to publish a book that will be given very bad reviews, and I am sure that this is exactly what will happen. I don't have money enough to publish it on my own and don't want to give it to Cramer who will price it out of the market. What to do then? Even now the people who are using the book complain that it's too hard to find their old friends in the text. This will be helped out by a good index, but still, who am I doing this for?

Let's not talk about telephone calls anymore. You twist my words around. I do not doubt that you tell the truth. This is different from doubting that you received a telephone call. Also I am far from believing you a fool or that you lie to me. On your side, don't accuse me of not trying as well as I can to go to your aid, as Pat and I did before the chancellor that last time. As I said before, nothing can be done or could have been done without you making a personal appearance in your defense. I still regret that you don't dare to come for our celebration because you should be here, more than anyone else.

Sinerely,

Bill

San José, September 18, 1985.

Dear Bill:

Thanks for your good letter of September 12. I hasten to tell you that I have little to disagree about, only the following perhaps minor points...though one that you were mislead to accept from an American colleague without your own language abilities, is deplorable since it was unnecessary...in matters European you ought to have accepted what Jermy found to be right in Flora Europaea, since he reads German in which all the discussions on the problem were made. But we can hardly hang you for a single mistake in selecting your references...though some are hung for still less? I am following your numbers:

1. I do not have Lellinger (1981) or Fuchs (1974...1956 is his first and essential reference, but it also missing in my collection so I have to follow second-hand references to it), but as far as I can understand, the correct name for what you call Athyrium alpestre (Hoppe) Claireville should be accepted as Athyrium distentifolium Tausch, with the ssp. americanum (Butters) Hultén. The discussions that lead to that conclusion are many and long and exclusively in German, though Fernald 1929 made some remarks of no significance, and they were published by the great Janchen (1950), the no smaller Hylander (1945), by the meticulous Fuchs, in Janchen (1956), and then accepted by all later authors, except Lellinger (1981) of course, by Flora Europaea and Czerepanov (1981) and all others of the many recent authors using the name. I do not know more about Lellinger than that he is one of Wagner's students and American at the Smithsonian, where I am in no doubt they select only "better, American botanists" and do not believe in immigrants or Europeans, but that you ought to ignore in favor of the latter, as you usually do. Whatever the "wise" reviewers of your good work may want to smear you with. But ignoring the European botanists who solved this European question long ago because they understand German, would cast a negative shadow on your otherwise excellent work in the eyes of those who understand and know who are really "better botanists", not simply good because they are American and work at the Smithsonian...though there are good men also.

2. Jacea, a name for Centaurea plant used by Otto Brunfels, 1536, according to Lid, but also by Tournefort and Bauhin and other pre-Linnaean authors, according to Species plantarum. So perhaps the best would be to say that it is a pre-Linnaean generic name for this group?

9. Cylindropyrum (Jaub.& Spach) Á. Löve; C. cylindricum (Host) Á. Löve, 1982, cf. Löve, 1984, Conspectus of the Triticeae, p. 500. I hope you got it last winter?

11. Let us agree to disagree on Elymus scribneri...time will solve that question bes

13. Conservation should be used with care, and not be based on personal bias or that we are more used to the older combinations. In the case of Puccinellia it would be meaningless, from my points of view, and serve no purpose...but I would like to see that you propose it and send to Taxon your reasoning that I may be able to help you strengthen with appropriate examples, though not based on my opinions... you know that I can see more than one side of a question, that may be one of my faults in the eyes of the "wise" and "fair" American "colleagues who hunted us even for believing in continental drift and cytogenetics when both were tabu!

17. Viola aduncoides: No, we overlooked the Rydberg splits, one or all of which may well make this a synonym, hope you correct this, if we made a mistake!

18. As far as I understand, your Viola epipsila repens is what we called Viola epipsiloides. The European name that applies to a very different species, was used by Hultén and his many western American and even Russian believers for the species that other Russians had named repens, which has an older American homonym, cf. our paper of 1975, and thus had to be replaced. All recent Russian or eastern Asiatic floras have, naturally, accepted this, so why not you, since nothing of what we have done in America is more solid than this change?

19. Cerastium arvense L. is the 72-chromosome weedy species of Europe and northern Siberia, whereas C. strictum L. (as Haenke correctly clarified it) is the 36-chromosome plant of more southern mountains that rarely if ever is weedy. I made the guess that the former may be found as a weed in inhabitated areas even of the Rockies, but have never bothered to look closer so I may guess wrongly. But your alpine plant is C. strictum, not arvense s.str, as is well documented also cytologically.

I understand your hesitation to let the conservative 18th century botanists judge and reject your fine manuscript, but thought you were as courageous as was the young Linnaeus, cf. his forword to Species plantarum when he faced the ogres of his time...and survived them so that they all are forgotten now. But this is your decision...though I am sure that the "great" would appreciate it if you went back to the nomenclature of their generation of ogres and changed all the correct things back to what was good Bentham-Hooker botany for a century in America. But if your courage is leaving you also in this field, why not turn and run when still possible...it would hardly delay the book many months, and those who do not want to be educated away from their wrong indoctrination will elect you to the Academy and give you various other "honors" that are not for modern botanists... you have to select that way yourself, though then the best American flora will have to wait longer and may not come until long after the next century has gone.

The New names and combinations VI is fine with me, except that I would appreciate it if you put your name as the author of Minuopsis, since I have never studied or even seen the plant and only looked up the necessary data for your use. Though I am in no doubt that it is a very strong genus, from evolutionary and genetical as well as taxonomical and geographical points of view...but I do not want to connect my name to its validation for the reason mentioned above...I have never seen it.

We hope everything goes after plan for the new herbarium place. Now a new trouble has come up that would have forced us to decline your invitation had we only known it then: I had mumps in 1945, very seriously for several weeks in Lund, but recovered with one testicle only in disorder, and never any other troubles. Until now that our doctor has found that during the past year or so my energy has dwindled more than normally...and this he puts in connection with that the virus evidently has woken up again and eaten away almost all the hormon producing parts to I am down to less than 1/2 the testosteron amounts. He has yet to decide what to do, but advises that we stay close in case this becomes acute, which he does not expect... neither does my nephew who is a virologist specialising on mumphs at Karolinska. But the dog does not understand why I have been reducing his walks slowly and then resting after walking with him less than a mile, instead of three or four earlier. We all grow older and more weak...hope you keep strong longer!

With the very best regards from us all here,

As ever, Askell

Rocky Mountain Flora: Western Slope

Preliminary draft

by William A. Weber....1985.

Suggestions for some minor adjustments, by Áskell Löve, September 1985.

Notes (R)P: 01

Author citations. - Following the wording in the Sydney Code, Rec. 46E, the sentence might perhaps be better worded as, e.g.:"The citation, Nutt. ex T. & G. means that Torrey and Gray published the name ~~the name~~ but gave credit to Nuttall for recognizing it and informally giving it ~~a name~~ and perhaps furnishing the description."

Family acronyms. - Printing error in second line, ti o, for to?

Family arrangement. - Weak motivation, almost lame. Would it not be wiser to drop the second sentence:...."This makes...from dicots."

(R)P: 02

Hybridization and apomixis. - This needs to be modified...and the reference to Anderson could confuse others with disputable sense of generic definition, and the ~~xkxi~~ statement on apomixis is perhaps not exact...and neither statements are actually needed in the flora. Would it not be best simply to omit this chapter that some readers may feel is disputable biologically? Your decision...I would drop it.

(R)P: 03

A very appropriate remark, much needed for the mountain area.

Textname: westslope (R)P: 03....p. 13

A. trichomanes L...."should occur", perhaps better: "might occur" or "could occur"?

Westslope (R)P: 04....p. 14

Why change A.distentifolium back to alpestre? I thought that had been properly and finally corrected by knowledgeable Europeans? Some later correction that I am unaware of?

Lellinger, D.B., 1981. Notes on N. A. ferns Am. F. J. 71: 91-94.

p. 21:

✓ There is no reason to continue to ignore the very fine classification by Antoine of the junipers into two morphologically and geographically distinct genera that never cross: Juniperus with the species communis that is northern and alpine, and Sabina Antoine that is more southern. Karyotypically these are very distinct, thus the crossability and incompatibility barriers. The latter genus includes the two last species in your flora, S. osteosperma (Torr.) Antoine and S. scopulorum (Sarg.) Rydb.

p. 24:

I have found that the name Negundo was used for pepper by Plinius (cf. Pinax), but according to the very exact E. L. Little, in his 1953 Checklist of native and naturalized trees of the United States, p. 38, Negundo comes from the Malayan common name of Vitex negundo L., later applied to Acer negundo. Take your pick.

p. 26:

Alisma plantago-aquatica is a Eurasiatic diploid with 2n = 14, whereas the corresponding American species is A. triviale Pursh, a tetraploid with 2n = 28. I pointed this out already in 1954, and it has been amply confirmed by the specialist in the genus, Eugenia Pogan in Kraków, in a paper , 1968, on "The taxonomical value of Alisma subcordatum Raf. and A. triviale Pursh, Acta Biol; Cracov., Ser. Bot. 6: 185 - 202. Hultén knew it but felt he should ignore it in favor of Samuelsson's acceptance of Greene's name brevipes, though reduced to subspecific rank. He never understood reproductive barriers and still less cytology though these amply supported his genial geographical hypotheses. After all he was also human and Swedish geneticists were among those who laughed at him...because of envy.

Allium schoenoprasum var. sibiricum. - Certainly more than a variety, if at all of taxonomical importance. I have the feeling that this name is applied to various more or less meaningless minor taxa that may even be modifications rather than geographical races. Would it not be wisest just to ignore it in the Flora?

p. 29,2 (Fig. 61:

B. S. media , should be Alsine media. OK

elaborate

D. Cerastium arvense seems to be C. strictum Haenke, the native alpine plant that often is confused with the introduced weed of the lowlands, diploid with 36 chromosomes, as contrasted with the tetraploid 72 of the introduction...this needs also to be corrected in the text on p. 32.

C. strictum L., Sp. Pl. 439! Austria & Switzerland.

p. 29,3 (Fig. 59):

OK

C. Melandrium...should be Gastrolychnis and move to that family...but I have not the appropriate part of the key so cannot easily find where to move it... Forgive me if I err in this!

D. Minuartia obtusiloba...should be Lidia biflora, of course, the former seems to be larger because of climatical (?) conditions so far south?

p. 36:

Toxicodendron rydbergii (Small) Greene. This is not according to an evolutionary system but follows closer some less clear definition of species and its races, as e.g. that adopted by the recent monographer Gillis (1971). I believe that the biological species concerned is the wider T. radicans (L.) Ktze. with several major geographical races or subspecies, the one in Colorado being ssp. rydbergii (Small).... These races form hybrid swarms where they meet, of course, since hybrid swarms characterize the meeting places of such races and are one of the best definitions of subspecies in the biological sense...or of Anderson's species?

p. 59:

Centaurea should be Jacea, cf. later.

Derivation?

p. 72:

Centaurea...should be: Jacea Mill., Jacea pratensis Lam. cf. Löve & Löve, 1961, Chromosome numbers of NW European plant species.

p. 91:

Packera:...contemporary Canadian botanist, would it not be better to say: contemporary English botanist in Canada...and correct similarly for Askellia?

p. 97:

tetraploid diploid

Seriphidium tridentatum (Nutt.)Weber and S. vaseyanum (Rydb.) Weber (sp. n.) are well-defined distinct species with tetraploid and diploid chromosome numbers, respectively, as shown by Roy Taylor, who is a good taxonomist, properly critical. You should ignore what agronomists and others with less sharp taxonomical eyes say, as, e.g. Beetle, who could not even understand Lophochlaena, despite his training with grasses. And remember, in case of discussion by someones with feelings rather than logic, that it is wiser to divide what might be similar than unite what is likely distinct...and these certainly are distinct.

p. 98:

Socalled Solidago gigantea ssp. serotina is an illogical problem caused by several misunderstandings by botanists thinking non-biologically. It is solved by simply aceepting the taxon as the distinct species S. serotinoides, as did Löve & Löve 1982, in Taxon 31. May, p. 358 after thorough studies in Manitoba.

p. 126:

9b: [for A. E. Porsild, Canadian botanist...would be better to say simply, Danish (or Greenlandic, which is less clear as he was born in Denmark) botanist in Canada...cf. above for Packera].

p. 128:

One sp. & ssp., E. edwardsii R. Br. ssp. penlandii (Rollins) Weber..... The socalled arctic E. edwardsii includes two species, the one described ~~from~~ from Canada by Robert Brown, and another from arctic Russia, the former with 28 chromosomes and restricted area, the latter with 42 and more widespread; the latter is being considered for recognition at the species level by the Russians. The Colorado plant is a weak geographical race of the tetraploid American species only.

p. 140:

Capparaceae, not Capparidaceae, cf. Sydney Code, family list. Correct also in family key.

p. 149:

Under the name Atriplex canescens you hide two distinct taxa, which doubtfully critical western socalled taxonomists tend to regard as subspecies rather than to accept the sharpness of Aven Nelson: they differ in polyploidy, in addition to morphology and geography-ecology. I believe we saw (and counted) more of the smaller A. aptera Nels. with 36 chromosomes than of the larger and probably more widespread A. canescens with 18. These taxa are presently being introduced and bred for agriculture of the Middle East by some Syrians and Egyptians, so you ought to be first to acknowledge both...before some foreigners do...others of the "great" little American botanists will continue to ignore Nelson's sharpness.

yes!!

what an wasdryer?

what about a pleiantha

p. 157:

Rhodiola has the basic number x = 11, or a karyotype and haplome setting it clearly apart from other taxa put into the same genus by mistakes by earlier botanists unaware of their biology. Some Japanese, most recently Ohba in Tokyo, have recognized this based on morphology alone and unaware of the fact that we recognized some other genera in 1975. What you call Rhodiola integrifolia we called Tolmachevia integrifolia (Rafin.) L. & L. in 1975; it has 36 chromosomes or the basic number 9, whereas Clementia that you correctly accept, has the basic number 7, as we showed in Taxon in February 1985, p. 163. That latter number is also typical of what we called Kirpicznikovia unaware of the fact that it had been recognized as Chamaerhodiola by a Japanese decades earlier...though I doubt that that affects the distinction of the American plant. I would accept Tolmachevia for your plant, as did even Packer!...even Homer sometimes nodded. The Colorado taxon is T. integrifolia ssp. procera (R.T.Clausen) Löve & Löve.

Sedum s.str., type S. acre, basic number 10, only introduced in America. The Colorado representatives belong to Americosedum Löve & Löve, Taxon 1985, p.350. In addition to the species A. lanceolatum (Torrey) Löve & Löve that you include, 2n = 16, I believe we have counted the chromosome number 32, or tetraploid, in a specimen of A. subalpinum (Blankinship) Löve & Löve from the upper slopes of Long's Peak but since we only fixed the plants and did not keep a specimen...in the hope of coming later for closer look...you know how one is optimistic sometimes...this is only a preliminary identification. But perhaps R. Clausen has had material from there, or perhaps you can identify the taxon from other alpine collections?

huntell?

p. 179:

Schoenoplectus lacustris (L.) Palla ssp. creber (Fern.) Löve & Löve, f. S. validus.
Schoenoplectus lacustris ssp. acutus (Muehl.) Löve & Löve, f. S. acutus, cf. L. & L. in Taxon 30, 1981:849.

p. 184:

Arctostaphylos adenotricha (Fern. & Macbr.) Löve, Löve & Kapoor (1971: Arctic & Alpine Research) certainly is a species distinct from A. uva-ursi, since the former is diploid with 26 chromosomes, the latter tetraploid, and they have never been found to hybridize. A slip of pen, or simply slip of memory...even Homer sometimes nodded!

Kalmia microphylla (Hook.) Heller is a diploid, K. polifolia a tetraploid, cf. Löve, Löve & Kapoor 1971, p. 155.

p. 186:

Perhaps better to translate Gr. agalma as "a delight, or an honor"? as does R. S. Woods, 1944: The naturalist's lexicon?

The type of Euphorbia L. is E. antiquum L., a tree- or cactuslike plant from India. You are right in accepting Chamaesyce as a genus, thus splitting the complex into more natural groups, but mistaken in keeping the remainder in Euphorbia . As far as I can find, all your Euphorbiae belong to the genus Tithymalus J. Gaertn., and all but one have previously been transferred as such:

T. robustus (Engelm.) Small,
T. incisum(Engelm.) Norton v. mollis Norton,
T. esula (L.) Scop.,
T. uralensis(Fisch.) Prokh.,
T. crenulatus (Engelm.) Heller

T. spathulatus Lam.) Weber, however, needs help in transfer from the basionyme Euphorbia spathulata Lam. 1788, Encycl. 2:428. I understand that it is an annual related, morphologically at least, to T. helioscopia (L.) Hill.

p. 204:

Oxytropis campestris of American authors seems to be a complex of good species that differ not only morphologically and geographically but also cytologically and form a polyploid series. This European and Russian authors of recent years have observed but of course the skilled florist and doubtfully learned biologist Barneby hesitates to accept chromosomes in his otherwise fine system, because the great men around him, though academically learned, have no understanding of genetics, of course. O. campestris (L.) DC. s. str. is a hexaploid with 48 chromosomes, whereas the "race" gracilis is an octoploid with 64, and so correctly classified as a species in its own right: O. gracilis (A. Nelson) K. Schum.

pp. 205 - 207:

Trifolium is, of course, a monster that especially the Czechs have been trying to divide, but sofar nobody in America, at least not recently. Perhaps a project to be given to a fine graduate student who already has had a good training not only in cytogenetics and its philosophy but also in taxonomic thinking? There is hardly time to try such a revision now, though the cytological data seem to be available, in addition to the morphological ones?

p. [illegible]:

p. 212:

Gentianella acuta(Michx.) Hiitonen.
Since this American-Siberian taxon has been found to be diploid with 18 rather than tetraploid as related European species, cf. Löve, 1984, Taxon, p. 539, and Krogulevich & Rostovtseva, 1984 (Khromosomnye chisla tsvetkovykh rasteniy Sibiri i Dal'nego vostoka, Novosibirsk), it must be accepted as a species in its own right, as long ago concluded by our skilled Finnish colleague Hiitonen.

p. 214:

Additional fact supporting the splitting of Grossularia and Ribes: The species of each genus hybridize between themselves, but it is at least very difficult to hybridize the genera, and such hybrids are unknown in nature, as far as I can find in the breeding literature. But perhaps more work is needed before such drastic proposals are made, especially since there are also two other "subgenera" that must be taken into consideration? A work for another generation?

p. 216:

Check [illegible] ?

Since there may remain some doubt as to the species distinction of Myriophyllum exalbescens Fern. that Hultén called ssp. exalbescens of the species M. spicatum, a name recently shown to be a synonym only of the ssp. squamatum Hartm., I wonder if it would not be wiser to leave it as the amphiatlantic taxon M. spicatum L. ssp. squamatum? If experiments prove this wrong, then the Fernaldian species could be reinstated, but....

p. 234:

I believe you want to leave the last sentence as it is, though to me it looks incomplete without "on the west slope"? It is your language.

p. 243:

That you dare to doubt the next world in this land of religious hypocrisy... if you were an immigrant, you would be ostracized and lynched or at least cut away from living facilities and probably at least threatened with deportation or disappearance, and nobody would dare to even try to help you! But I see your point and find it a good style not to be scientifically serious on every page of a good flora manual!

p. 254:

Is it wise or even fair to ignore the works and opinions of Mosquin, Small and even Löve & Löve (1975: Bot. Not.) on the splitting of the two major groups of Chamerion? The Colorado plants of what you name as C. angustifolium (the name of a tetraploid, 36 chromosome plant) is actually the octoploid (72) more southern C. platyphylla (Daniels) Löve & Löve. And the plant of western mountains that you place under C. latifolium (a widespread octoploid, 72, taxon) is actually only tetraploid (36) and thus correctly classified as C. subdentatum (Rydb.) Löve & Löve. Is there ever any wise reason to ignore the facts, even when the "great" specialists (?) feel (!) differently? Foolishness is always just foolishness...and you are not known for being one of those...that ignore facts for "convenience"...that is one of the points that make your flora great. Hope you agree not to hesitate to follow the facts also here.

p. 264:

Papaver kluanense D. Löve! Just one of the few cases when it confuses if an initial is left out.

p. 265:

Since there is no doubt that the family Plumbaginaceae is a monstrous complex that was correctly divided by Linczevski in Nov. Sist. Vyssch. Rast. 1968:174, as accepted also by the otherwise conservative Czerepanov, 1973, in his fine Add. & Corr. Fl. URSS I-XXX that I gave you long ago, I would like to advice you to aceept the family Limoniaceae instead of the monster, and to change it elsewhere in the text and keys. It certainly is a future family name.

p. 276:

authors?

Aegilops. Since this name needs to disappear from the Flora, it is hardly important to correct its explanation, which is that it comes from Theophrastos and is perhaps related to aex, or goat. But the name of the Colorado weed is now: Cylindropyrum, from Greek cylindricos, a cylinder, and pyros, wheat.

which spp.

Agropyron.
Note: add Trichopyrum, which I am splitting from Elytrigia to meet a fair criticism by Dewey, since the section Trichophorae on pp. 486-487 and the pycnantha-pungens group on pp. 487-488 in the 1984 Conspectus have been found to have different genomic constitutions and so are distinguished as separate genera, Trichopyrum and Psammopyrum, in a paper in the press for the Landolt Festschrift in Zürich.

Agropyron s.str.

The Colorado taxa ought to be classified as:
A. cristatum (L.) J. Gaertn.
ssp. cristatum
ssp. desertorum (Fischer) Löve
ssp. fragile (Roth) Löve
They are all interfertile for the simple reason that they are only simple races of a variable species that have arrived at some morphological distinction thanks to environmental and even artificial human isolation, not reproductive. Such phenomena not only confuse breeders as Dewey but even the finest of botanists as you...and me?

p. 276:

Agrostis.

what does thurberiana [illegible]

I would accept Podagrostis for humilis and thurberiana, just as you accept the equally well distinguished genus Leucopoa as distinct. For the sake of simple biological logic.

p. 277:

The correct name for 7b A. borealis Hartm. is A. mertensii Trin. An easy slip.

p. 279:

Whatever conservative grass men in America say, the name of the grass "bouteloua" is correctly Botelua as originally spelled, cf. Airy Shaw in Willis and the Sydney Code. What is correct should be kept, whatever our "feelings"...you agree?

p. 282:

Critesion brachyantherum... should be:
Critesion jubatum ssp. breviaristatum (Bowden) Löve, cf. the wheatgrass conspectus.

Critesion glaucum (Steud.) Löve is correct, since this is a diploid that has a reproductive barrier against the tetraploid C. murinum. Cf. Conspectus.

p. 283:

Elymus is called wild rye, not ryegrass, which is Lolium...a small slip.
The following adjustment seems needed, for good reasons:
Elymus trachycaulus ssp. andinus (Scribn. & Sm.) Löve (2a)
ssp. scribneri (Vasey) Löve (3a)
ssp. bakeri (E. Nelson) Löve (3b)

p. 284:

As far as I interpret the data available, 5b (longifolia) and 6a (elymoides s.str.?) are minor variants of the same species and subspecies...but if you so feel you make no serious mistake by keeping them separate, as I do also for E. multisetus (6b)

Elytrigia s.str. has the species E. repens and its ssp's only, Löve 1986, Festschrift for Landolt. There E. intermedia is distinguished as:
Trichopyrum Löve
intermedium
ssp. intermedium
ssp. barbulatum (Schur) Löve

Elymus lanceolatus (Scribn. & Sm.) Gould is synonymous with dasystachyum (younger). albicans is also a synonym of the first, or variety, not a ssp. judged by its distribution but you are hardly very wrong if you ignore my present view and instead name it as E. albicans (Scribn. & Sm.) Löve, as I did in 1980. These taxa still are disputable and very vaguely known experimentally so nobody is too mistaken by having other views.

p. 287:

Glyceria grandis S. Wats. is a good diploid (20 chromosome) species, a world apart from the European G. maxima which is hexaploid (60). Hultén as a rule erred when judging chromosome differences which he misunderstood, contrary to Fernald (in other cases).

p. 289:

5th line: Elytrigia intermedia...should be Trichopyrum intermedium.

p. 291:

Phalaris should be PHALAROIDES Wolf [from Phalaris, ancient Gr. name for some grass, and oides, dimin. ending].
One sp, P. arundinacea (L) Rauschert....

p. 292:

Discussion of Phippsia contra Puccinellia, cf. Löve & Löve 1975a, Nomenclatural notes on arctic plants, Bot. Not. and 1975b, Nomenclatural adjustments in some European monocotyledons, Folia Geobot. Phytotax. 10:273-274). Also the paper by Hedberg, 1972: Bot. Tidsskr. 58:157 - 168, and the discussion by Tzvelev, 1976, Zlaki SSSR, p. 493, of their hybrids and their significance. My conclusion of 1970 (Emend. Icel. Fl., Taxon) and later, that these are not distinct genera, remains well substantiated and ought to be accepted, as you indicate though you hesitate. So move from p. 296 and add here:

Phippsia distans (L.) Löve & Löve, 1975, Folia 10:274.
Phippsia airoides (Nutt.) Weber, comb. nov. (and publish elsewhere).

num, Clausen?

p. 294:

Key C, 3b: Butters & Abbe. (spelling mistake).

p? Line 6 from bottom: Elytrigia albicans ahould perhaps be: Elymus albicans?

p. 316:

BM

Reynoutria: This name may be derived from that of a gardener or a friend, but since it was published in Houttuyn, M. Handleiding tot de plant- en kruidkunde Vol. 8, p. 639 that is not found in America and very rare elsewhere...it is mentioned in two places only in Europe, one of them Kew (Merrill, J. Arnold Arb. 19:304-305), you might perhaps be able to get a friend there to look it up...if it is that important? It would be nice to solve that problem once for all, since nobody has had patience to do it before!

4a: Better as ssp. fueginus (Phil.) Hultén

withdrawn

5a: Better as R. triangulivalvis ~~XXX~~ Danser, only, since R. salicifolius certainly is a badly confused concept that covers several, or perhaps almost all, the species of American Rumices.

p. 323:

Lysimachia is much too collective. This taxon is better named as Steironema Rafin. [Name from Greek, steiros, sterile, and nema, Thread, referring to the staminodia.
Steironema ciliatum (L.) Rafin.

pp. 325 etc.:

Ranunculaceae is much too inclusive. I would split out for Colorado:

Helleboraceae: Aconitum, Actaea, Delphinium, Psychrophila and Trollius.

Thalictraceae: Aquilegia, Thalictrum.

p. 332:

Pulsatilla patens. The North American race which is clearly separated from the Asiatic ssp. multifida both by land and ocean, cf. Hultén's map, is better accepted as the (still interfertile but morphologically distinguishable even in the herbarium) ssp. hirsutissima Zāmels. cf. our Arctic Atlas p. 354.

p. 333:

12a: Nothing new has come forward about what you call Ranunculus gelidus, which we (1971), and Tolmachev (in Arkt. Fl. SSSR, 1971) and Tolmatchev & Yurtsev (1963) (cf. our 1971 reference) maintained is a species distinct from the arctic and Rocky Mt plant R. grayi, though Yurtsev has told me that they regard the matter moot and solved and accept the Central Asiatic taxon as a species in its own right, though still cytologically unknown. Therefore, I would avoid the problem by accepting what those who ought to know regard as most correct. And recommend that you retain R. grayii, without mentioning R. gelidus, perhaps not least because Ostenfeld was, in my Icelandic experience, about as good a florist and taxonomist as is Gjaerevoll, and about as stubborn...as was also Hultén. Perhaps not such a great matter, though important if it misleads others to believe that something new has come out since our 1971 discussion.

Whatever you do, the remark that it is "found also in Siberia" ought to be modified, at least for R. grayi, to read:"found also in Alaska and NW Siberia." Whereas R. gelidus s. str. is a western and southern Siberian endemic.

p. 340:

Cylactis Rafin.

According to Index Rafinesquianus, p. 137, the species name C. arctica (L.) Rafin. was validated by Jackson ("ex Jackson").

p. 341:

Fragaria vesca L.
According to the German Fragaria specialist Günther Staudt, 1962: Taxonomic studies in the genus Fragaria. Typification of Fragaria species known at the time of Linnaeus.- Canad. Journ. Bot. 40: 869 - 886, the western North American race is best classified as the major geographical race ssp. bracteata (Heller) Staudt.

And the Rocky Mt race of Fragaria virginiana Duchesne is also distinguished as the ssp. glauca (S. Wats.) Staudt.

p. 347:

Your very appropriate remark on Pentaphylloides on p. 342, bottom, also is very fitting for Rosa acicularis, which we discussed on the same page in the 1971 paper. R. acicularis is a strictly Eurasiatic species with the octoploid number 56, whereas the American taxon R. sayii Schwein. is hexaploid and very distinct, as we have maintained since we first encountered it in 1951 in Manitoba. I believe that this our stubbornness in matters of strict logic may be one of the essential matters used by small minds to criticize us in the past...whereas from our point of view...and I am sure also yours...such logical stubbornness is our strong side. If not, your flora would probably never have turned out to be of the highest class ever reached in North America...and perhaps even in the world...but do not tell the author that I said this, and still less those who envy him...and us!

p. 347:

The eastern Asiatic race ssp. sachalinensis (Lev.) Focke of Rubus idaeus L. is replaced in North America by the ssp. melanolasius Focke. They are interfertile, of course, since they are only races without chromosomal distinctions.

p. 349:

I realize that the Galium trifidum complex remains critical, since it still waits some experimental studies. Though a South African associate of Ehrendorfer who collected somewhat during an American trip some years ago, thinks he can, without experiments, claim that G. brandegei is only a modification, he seems to agree that what has gone under this name from the Rockies and east to Greenland and Iceland may be what we have called ssp. brevipes (Fern. & Wieg.) Löve & Löve of the widespread species G. trifidum. Perhaps you would like to make some small revision on basis of that, leaving G. brandegei as a synonym of that race only, and not replacing it entirely with the main species from Scandinavia?

p. 350:

The Ruppia "maritima" which I have seen from the prairies and mountains in the west is not that diploid (20 chromosomes) species, but rather the reasonably distinct ssp. occidentalis (S. Wats.) Löve & Löve of the widespread R. cirrhosa (Petagna) Grah. (or R. spiralis auct.) which is tetraploid with 40 chromosomes. Its eastern and more coastal race, ssp. longipes (Hagstr.) Löve & Löve in our perhaps not completely indisputable definition, reaches Iceland, whereas in the Faeroes we seem to meet the European typical race cirrhosa. Do what you feel is wise with this note, but change the third line in your remarks on the genus to read:"..species or a few still critical taxa". I do not think we ought to return to the earlier single species concept since we at least know that two distinct chromosome numbers are involved, and thus a barrier of reproduction that warrants at least the acceptance of two species, both perhaps more or less circumpolar?

Just another matter: H. B. Rupp is generally known by its Latinized name Ruppius, which perhaps had already been adopted by his father, as in the Linnaean case? Cf. Stafleu & Cowan, Tax. Lit. vol. 4: 991 - 992.

p. 353:

11b: Salix planifolia Pursh has 76 chromosomes (tetraploid) whereas S. phylicifolia has 114 (hexaploid) (cf. Löve, Löve & Kapoor, 1971, p.146) so they are distinct biological species, in addition to being morphologically and geographically distinct. Is this a mistaken case of stubbornness or simply a slip of the pen or memory, as is human? Drop S. phylicifolia, it is unrelated to S. planifolia or at least very distant.

p. 354:

14a: Salix glauca is a European and Asiatic taxon with 152 chromosomes that reaches westernmost Alaska, whereas other polyploid phases of the morphologically very confused complex of polyploids and their hybrid swarms are met with in NE and W North America. The socalled "eastern phase" is the S. cordifolia complex that has 114 chromosomes and reaches Iceland and the Faeroes, whereas the "western phase" or "ssp. acutifolia" has 76 chromosomes and seems to be what has been described as the species S. seemannii Rydb. Despite what I said above about hybrids, I have no records about such hybrids between these taxa in nature, and all the hybrid complexes reported from Iceland, Greenland etc. with glauca as one parent, by Floderus and other Swedish amateurs, are wrongly determined. Why not acknowledge the Rocky Mt "phase" as the species it is...Rydberg knew botany well.

p. 375:

The two species of Veronica listed under Pocilla should be referred to as:
Pocilla persica (Poir.) Fourr.,
Pocilla biloba (L.) Weber, based on Veronica biloba L., 1771. Mant. pl. II: 172.
The latter needs to be validated as such.

p. 376:

4b: The parentheses should be:
(V. wormskjoldii of Colorado lit. is a NE American tetraploid, 2n = 36, whereas V. nutans is a western diploid, 2n = 18).

p. 384:

Urtica dioica L. is a tetraploid Eurasiatic weedy species with 52 chromosomes, whereas the American and amphi-atlantic U. gracilis Ait. has only 26. Since the southwestern race holosericea also is a diploid, it evidently belongs as a race to the latter, at the subspecific level and needs to be transferred as such:

Urtica gracilis Ait. ssp. holosericea (Nutt.) Weber, comb. nov., based on Urtica holosericea Nuttall, 1948, Journ. Acad. Phila. II,1:183. It enters Colorado only in MF.

p. 387:

why are [illegible]

9a: Viola adunca Sm. and V. aduncoides Löve & Löve 1975, description p. 516, both occur in Colorado. They differ not only morphologically but also in chromosome number, the former being diploid with 20 chromosomes, the latter tetraploid with 40.

11b: The correct name of the taxon Viola epipsila ssp.repens is not in doubt: It was described as V. epipsiloides Löve & Löve in 1975 and has been widely used as such in numerous Russian and even American floras and research papers! Just a small slip of memory, of course. The new name was needed because the otherwise correct name V. repens has an older homonym.

Viscaceae: 1a: If Sabina is accepted, then change it also here! S. osteosperma.

AFTERTHOUGHT: The information about the Typha pollen on p. 352 may be difficult to understand. It should state simply that whereas the pollen remains in tetrads in T. latifolia, it usually (not always?) separates into free pollen grains in the other species.

San José, October 26, 1985.

Dear Bill:

Thanks for the good letter, and I am pleased that I succeeded in convincing you that it would have been a serious blunder to follow Lellinger and reject the correct Athyrium distentifolium. Lellinger, however, may have some points in his other proposal as to Asplenium viride, as did Fernald in 1933, though both overlooked (as do nomenclature-buffs much too often in their not always properly logical reasoning) some pertinent facts that have been known to better men in Europe for a long time. I do not have the originals, but Hylander, in his Studien über nordische Gefässpflanzen from 1945 that I hope you have, pp. 56-58, points out that already Heufler (1856) in his Monographie Asplenii species Europaeae, drew the conclusion on basis of thorough studies, that the drawings concerned, in Tabernaemontanus and Bauhin, could not be definitely identified as either A. viride or A. Trichomanes. Hylander, who was a meticulous taxonomist, though we did not always agree, made his own studies of the originals and concluded: "A. Trichomanes-ramosum L. gründet sich wenigstens zum grössten Teil auf eine von Bauhin abgebildete monströse Form einer nicht näher bestimmbaren Asplenium-Art, und der Name muss verworfen werden, weil erstens ihr Typus nich mit Sicherheit zu deuten ist und zweitens die Art auf eine Monstrosität gegründet und also nach Art 65 der I. R. ungültig ist. " With other words, he, and others than Lellinger or his cothinkers in America, agrees that the Linnaean name must be rejected as both nom. dub. and pl. monstr....I believe this is one more case that never ought to have been revived by modern "nomenclaturists" who evidently do not read the literature if it is in German...for the simple reason that they do not realize that most taxonomy was published in that language until the military might recently and probably only preliminarily switched to the other part of the boreal world. Hopefully, these buffs soon will irritate enough important botanists so the rules will be changed to forbid such "interpretations" that only secure that good men dislike being governed by rules that lack in logic and especially in reason...but on that we may not agree...not yet? So forget about Lellingers pipedream. And continue to complete the flora as the best piece of evolutionary taxonomy and correct nomenclature even when compared by the formerly representatives of the finest floristic work in the world.

My back etc. are trying to invalidize me, more so now than ever before, and my Chinese doctor at Kaiser, who does much wise, stands with little help, so I can hardly play around with the plants in our garden...though I do it. Yesterday I dug some holes for some Narcissi and cut down some sick branches of bushes, with the result that I hardly can walk around except as a very old fellow today and must break the hopes of Ingela's little dog of his daily walk. But that ought to be better around January 11, if you should find an opportunity for a visit...I can always crawl to some chair or even talk to you in my bed! Though it is not as bad as it may look, despite the fact that I hesitate to lift things and stay more inside. You were no better years ago...how did you solve the problem so you can travel around the world and climb mountains and even sleep in tents?

Our winter fog has come, but otherwise no winter colds...they are not expected for two more months, fortunately...and Doris still is working on conserving though she already has more than 200 glasses of all kinds of goodies...just now she is working on the last few bottles of grenadine from the pomegranates of which we have one tree that gave several hundreds of fruit this summer! Something to enjoy when you come!

All the best from us all...hope the herbarium is fine.

P.S.: We have not heard from Jack since he left for Tibet China in the spring. Somthing wrong?

San José, November 9, 1985.

Dear Bill:

Thanks for a good letter and for the information about the return of Jack, who must have extended his China visit considerably from what he had planned. Hopefully it will not take too many years before you are a regular vistor there also, since they are in dire need of wiser advice than other American botanists can give, and even need some help in their cytogenetics, as far as I can see from the few papers on chromosome numbers that mainly come from the fern people, who may not realize that when coming for advice to Herb Wagner in that field they are in a goats' house seeking wool. Old China always has fascinated me, and so do its landscapes and ancient and constantly modern buildings, but I am of the feeling that many of its "wonders" would not make it pleasant to even visit there. Think about the poverty, of which we see more than enough even in San José! But you could greatly stimulate their botanists as does Jack their geographers....

My back continues to disturb me, and so does a hormonal disturbance that may be getting controlled now. I would not be astonishe d if the back reason would be remedied in the same way as your was, because it increased after my stay in Iceland three years ago, when my weight grew with 20 lbs in one month... thanks to that I had a free card for the finest conditory in the city that is owned by my nephew, who also directs the largest publishing house. But it takes too long time to reduce when you eat as we do and get the same amount of food on your plate as others do...and are brought up never to leave anything on your plate, as is required in Europe. But I will try to counteract this, in the hope that everything will get to normal during the winter. Jogging I do not believe in, based on statistics from Sweden, but though I had been walking for a couple of miles with Ingela's dog for years, I had to stop this last winter when it tired me too much...caused by the hormonal disturbance mentioned. Even that may go....

While you struggled with Agrostis, I looked once more through the fine manuscript and found to my dismay that I had overlooked one fact that breaks your good rule on using a genetically based phylogenetic classification also of the families: Your splitting of the American Liliaceae is insufficient, as you may see in the recent publication of Dahlgren, Clifford and Yeo, 1985: The families of monocotyledons. Springer Heidelberg...or in Dahlberg's fine little Swedish review in Svensk Botanisk Tidskrift 79: 1 - 16 (1985). Since you split out, quite correctly, Agavaceae, Alliaceae, Asparagaceae and Iridaceae, you could complete the revision by doing the same for Calochortaceae (Calochorton), Convallariaceae (Smilacina), Melanthiaceae (Veratrum), Uvulariaceae (Disporum and Streptopus) and then leave Liliaceae for the rest (Erythronium, Leucocrinum, Lloydia, Anticleum, Toxicocordium and Fritillaria). Hope this still can be done, and that you excuse that I evidently nodded when looking through the manuscript last.

Yes, we will be pleased if you can visit with us in January...you know we have space for you to sleep and are able to feed you well...and now even my voice has returned with my hormonal treatment so we can talk endlessly as before. But we would appreciate it if you forget about the S.F. airport 60 miles away and instead arrange your flight so that you come (and go) to the S. J. airport instead!

Yes, we have Widén's Agrostis thesis...but it gives no clues to your borealis problem, except to confirm that that species, or mertensii, does not grow in the Rockies. I am sure that you are right in that idahoensis is the plant you have there. These species differ in chromosome number, since the former is an octoploid with 56 chromosomes and the latter only tetraploid with 28, according to a report in the IOPB lists XII, in Taxon 1967, determined by the Swedish-American Carlbom, who moved that year from Oregon to Lund as a postdoctoral fellow and liked life there so much, as did his family, that he decided to stay as a teacher at a gymnasium after his period at the Institute of Genetics was over. As far as I can see you would not gain much from looking through Widén's fine book, so you can wait until you are here...soon.

I am sorry that your herbarium revolution did not come much earlier, since that might have stimulated us to break out an institute or whetever you would call it, instead of staying with the fools that do nothing but politics and slander. That, however, is too late now, though we can hope that later generations will enjoy your revolution so well that they continue it further! That might even help the more general revolution in modernizing botany in America by getting rid of the formalists and conservatives that ride it as in the middle ages still?

Our winter is said to be coming closer, and we are promised a much needed rain this weekend. But we are still enjoying summer weather with sunshine and warmth.

All the best to all from all,

As ever,

Áskell