UNIVERSITÉ DE MONTRÉAL

INSTITUT BOTANIQUE
4101 est, rue Sherbrooke
MONTRÉAL 36 CANADA

Montreal, December 10, 1956.

Dear Dr. Weber:

Try to excuse the long delay in thanking you for your two good cards I got long ago. But the time certainly flies away as fast as here also in Colorado during the fall months.

Yes, you really seem to have an eldorado for those interested in arctic plants close to you so far south in Colorado, and it will be interesting to see how many such relicts will eventually be discovered there. Personally, I am interested in getting the chromosome numbers of all this alpine-arctic flora determined some day, since it seems to me as if it might have such a high frequency of polyploids as is typical for the Arctic. There may, of course, be diploids there too, and I am wondering if you have checked in detail the Saxifraga rivularis, since this might well be the typical high-arctic American S. hyperborea R. Br. rather than the other species, though the high-artcic taxon is usually forgotten even by the Danes studying the Greenland material. The taxon rivularis s.str. is a tetraploid, while the hyperborea is a diploid, so it is very easy to check for sure what this is, without even looking for the morphological details which are not very conspicuous. As a matter of fact, I have not yet seen a single American specimen of S. rivularis s.str., but I have not yet seen material from all the regions where the taxon is reported from more southern regions.

It will be a wonderful experience for you to come to Scandinavia and see all their splendid herbaria and meet all their many good scientists in botany. There botany is so well established that when the Scandinavians discussed the need for a certain branch of it at all Universities at the 1950 Congress, they spoke about the need for cryptogamists at the taxonomical institutions, but did not say a word about the need for taxonomy and phytogeography, since botany without taxonomy is not botany except in America where even the taxonomists and herbaria lack all understanding, and the socalled big herbaria are smaller than even the relatively small University herbaria in Scandinavia! I hope you will be able to see all these institutions, but you may benefit most by staying in Oslo, Uppsala, Stockholm, and Lund a little longer than at the other places. And Lund is so close to Copenhagen that you will feel that it does not matter what city you select of these two, and I think you would find it easier to work in Lund, although you have Böcher and several other arctic specialists in Copenhagen. And during the midsummer or the late summer you should stay at Abisko, where they have a scientific station where you can live for little costs. It is owned by the Academy of Sciences, and if I remember right it was necessary to ask for place there well in advance even when we were there in 1942, and now this may be still more necessary. I think you can write to Dr. Gustaf Sandberg, who is the director of the Abisko Naturvetenskapliga Station, Abisko, Sweden, and lives there, and ask him all about the place and its facilities already now. Give him my best greetings. And if you have time, the Helsingfors herbarium certainly is worth visiting, since the Finnish botanists are among the very best ecologists in the world and specialized on the northern vegetation.

I suppose you now have a winter comparable to what we had in Winnipeg during our years on the prairies, but we are now enjoying our first reasonable winter since we left Iceland in 1951. Montreal is a nice city, though perhaps a little too French in its traffic and honesty, but lovely French in its cultural attitudes. And although we have not gone far away this fall we feel as if we again were centrally located, since it only takes a short time to come to Ottawa, Toronto, Boston, New York and Washington and only one day and a night to come to Reykjavík. - By the way, if you are planning to fly over the Atlantic, the cheapest of the good airlines is the Icelandic Airlines which flies you to Copenhagen for a lower price than any other airline, about 100 dollars lower both ways. And if you take it you can stop in Reykjavík for some few days and see the most southern part of the real Arctic and climb very easily up to muntains which are climatically much higher than yours!

With the very best wishes for the season,

Yours sincerely,

Áskell Löve

San José, February 14, 1981.

Dear Bill:

Many thanks for the fine Trek Nepal, which we both enjoy. I was wondering what had happened since we did not hear from you after you left for this peculiar part of the world, but evidently you have been more than busy sorting out all the experiences, composing the good report, and recovering from an experience of this visit into the filth.

Although we would not like to share such an experience in person, for thousand reasons that you can guess, we enjoy to read about it. And are amazed to see how much botany you got out of it all. Perhaps one could say that that could be expected, because you know your boreal flora well (I would not call it Tetriary, though most of the boreal flora, and as a matter of fact all floras, were evolving at that time) but there are numerous genera that even the Indian floras may not be so exact about, and others that even you may have had to name collectively, not to mention the species that may not follow your usual strict concept for obvious reasons. But I am pleased to be able to add one point, but only one: when you accept Bistorta and Persicaria, then Polygonum perfoliatum is out of bond; it belongs to the very distinct genus Truellum Houtt., which Rafinesque had called Tracaulon, which Fernald uses, as a synonym of a section, for arifolium and sagittatum of the eastern parts of America, or should I have said the southeastern parts? That fits your "Tertiary" term, because it certainly is one of the plants of the distribution type that is best characterized by "Arcto-Tertiary"...as do many Himalayan plants of the boreal kind. The correct name seems to be Truellum perfoliatum (L.) Soják, validated in a good review paper by Soják in 1974 in Preslia 46: 139 - 156. It is a pity that you did not have time and help to collect intensely in the region because it certainly is filled with remarkable new and older taxa...as are much too many Indian areas.

We had heard about the leaches that dominate the forests in the Himalayas, and actually heard an Indian description of these pests last fall, when we were visited by S. S. Bir, the head of Botany and dean of Science at the Punjabi University in Patiala, a pteridologist-cytologist we have known since our Winnipeg time; he visited the States last fall and was directed to some places of no interest to him and refused other visits, much like what American botanists complain about when visiting Russia, and he showed us numerous pictures of just the regions you visited, where he and his students have botanized extensively. But even he did not have proper library facilities and was astonished to see here some Japanese and other books and reprints on some of the problems he has been looking at. Library facilities seems to be the main problem all over, because the bureaucrats that are permitted to dominate even the scientific fields always believe books are something ephemeral that can be forgotten when they want to use the money for their own salaries.

Otherwise nothing remarkable. I am just completing corrections of the last phase of a paper on New Zealand wheatgrasses written together with my friend Connor at Christchurch, and am waiting for reprints from Portugal of another correcting the old mistake of putting the Californian Lophochlaena of the Meliceae into the Arctic Pleuropogon of the Glycerieae. Lyman Benson and other stars of the region did it, of course, since Bentham made the mistake and he never failed in any English-speaking eyes so even Stebbins still does not believe what we say on it! Wish only you were closer so we could discuss this and other points with you...and see your pictures

Hope both you and Sammy are well, despite of the severe winter weather.

All the best,

San José, March 21, 1981.

Dear Bill:

Many thanks for several cards and the good list. But you forgot the revised list of families, or perhaps the mail felt it was material of interest for them?

I have several suggestions that I have only scribbled down on paper so far, but want to type out and add to so that they will become understandable. Because of Hultén's obituary and other matters that go slowlier than I expected, this all has to wait one more week, I am afraid, but perhaps you will find it worth while to wait that long with even the manuscript of your new changes, because some of my suggestions might be worth aceepting and validating...you know that I follow the old rule that there is no limit to what good you can do if you only do not think of who gets the credit, and I have never been a believer in priority for my own ideas, only in getting them realized. And because of the attitude in your country among its small botanists, who rather stab the immigrants than following their advise, you certainly are a more effective source of such changes...and still open to the attacks by those besserwissers who always try to force others to accept their beliefs!

I cannot spend a whole sheet without saying some words about some problems! I believe that if you had the Russian Novitates, you would change Trollius laxum ssp. albilforum to Hegemone albiflora...it is a whiteflowered relation to Trollius met with in NE Asia that I am sure is a amphipacific genus...but more about that later, and the needed details. I think you are mistaken in not accepting Bromus "trinii" as a Ceratochloa, since they can easily be hybridized and show some pairing indicating not so distant relationship...though you are right in ignoring it since it is only a rare introduction of no apparent importance yet. Turritis certainly is a very good genus, cf. our Slovenian Atlas and also the paper we wrote on its monocots in Folia Geobotanica. Your Elymus sibiricus-lika plant is likely just a variation of that species, rather than a relation to the strictly American and monotypic section that E. scribneri belongs to...though I have not seen your material of course. I will write more about Arenaria hookeri in connection with remarks on the long list soon, but I believe it ought to go into Eremogone rather than into the otherwise strictly Himalayan Eremogoniastrum, to which McNeill refers it...my reasons are strictly phytogeographical and genetical but his morphological...and I believe mine are more logical because a small group does not evelve in two places, and both groups are close...more about that later too. No more of this loose talk.

Thanks for the good picture of Hultén, he would have loved it. But I am proposing to Kathy that she cuts it to make him the essential part of it, because after all it is his face we want, rather than Old Faithful, this time. Hope you will like the perhaps too long obituary, which has not been easy to compose, and I believe he would also have approved of it, despite my mentioning of his inability to understand cytogenetics. The Swedes, however, may not always like what I say, though they will recognize the truth that none of the others came with their toes where he had the heels, except Britta Lundblad, whom they never have liked either. It is always difficult to be more than average size among dwarfs of the spirit, and for a generation only dwarfs have been in botany in Sweden, except Hultén...and now they have destroyed their publication facilities by "uniting" the journal into one extremely expensive that none but libraries can buy. And they could not even fill the Notiser except with Indian stuff recently!

No more today, more soon!

As ever,

San José, March 29, 1981.

Dear Bill:

I am sorry that I did not answer your cards when they arrived but instead went through the long list and made my notes on points that I hope can be of some help to you...it is enclosed. As you can see, I found it interesting, though I am sure that there still co-ld be found lots of points to ask about before it becomes as closely free from mistakes as humanly possible. And when that stage has been reached, somebody makes a new observation that changes what we thought was final, taxonomic botany never ends, except when we eliminate those few persons who have the training and ability to do it well and consistently and have the memory required. And if we do not eliminate them to get rid of their criticism, then they simply grow old and die...as did Hultén so recently.

When going back through your cards, I observe that although you promised to send the updated family list, it never arrived. I think the fact that Bromus trinii can be crossed to Ceratochloa indicated such a close relationship that there is no need not to accept it...but since this is a South American introduction of little significance here, I agree that it can be disregarded for your purposse, and somebody elsew will doubtlessly take it up somewhere here, if not somebody of the S. Am. botanists gets interested in his own flora. To go back to your family list, I agree that to make an acronym list complete also for synonyms would be a meaningless task, but if you make some few reasonable additions so the list will be meaningfully complete, it will be of assistance to many herbarium workers. So I sincerely hope you complete it soon and send it to Taxon.

I have the feeling that I have answered most of your questions now and earlier, with the exception of the one on Tephroseris kjellmanii contra f. tomentosa. Allow me first to point out that the author of Kjellmanii is Holub, not we, since we discovered in the very last minute that he had just published some of the new combinations we were proposing...thus several are in the Arctic Atlas but not in Bot. Not. As to the problem of kjellmanii versus tomentosus, we followed advice by Yurtsev who is sure that tomentosus is the Asiatic plant that differs even cytologically from the Alaskan-Yukon plant that Porsild named on basis of Kjellman's description! Somebody will later correct this and replace Porsild's name, though perhaps some rule can be used to accept it, I suppose, because his "type" differs from that of Kjellman. I am sure you are right in using kjellmanii rather than ssp. tomentosus, which likely is not met with on the eastern side of the Bering Sea, according to Yurtsev, Porsild was not always a good authority, though he was a good florist. Perhaps Hultén confused him, or he Hultén, in this matter...they can discuss it in their new place but will not be able to tell us the results.

I have sent my rather long obituary for Hultén to Kathleen and hope she finds it acceptable because it has been a painful experience to write so little about such a giant who will grow in size when years go by...the Swedes do not know who he was and little men like Olle Hedberg and the small minds at the Riksmuseum, except Britta Lundblad, have refused to write about him and showed pleasure with his passing, that does not make them greater in the eyes of the world. And Britta is the one who writes about him in Taxon....Your picture is fine, though I cut it to make him the essential part of it, hope you do not disapprove. And that you can furnish a proper text for it, with informations about time and place and photographer.

Hope spring is in full force in Boulder as it is here, our garden must be seeded next week and the fruittrees have already flowered and the grapes are beginning to whow their flower buds....

All the best yo you all from us all here, hope Sammy is well. Áskell

Áskell Löve: Some notes on the Colorado Inventory (1979 edition).

(I believe several transfers are to be recommended, perhaps you would like, for the sake of ethics, to do them jointly?)

Acer Negundo: I believe this is a distinct genus Negundo, with the species N. aceroides Moench. The interior American race is a good subspecies, as Hultén and we have defined that category (major geographical race), and not a species as Rydberg felt: ssp. interius (Britt.). That will only be a simple transfer, since we lifted it in 1954 in a paper on the Vegetation of a prairie marsh. - Bull. Torrey Bot. Cl. 81, p. 33.

Achillea lanulosa Nutt. is a good species, has among other characters 36 chromosomes as contrasted to 54 of the boreal Eurasian A. millefolium L. See Clausen, Keck & Hiesey 1948: Experimental studies on the nature of species. III. - Carnegie Inst. Wash. Publ. 581: 1 - 129. The two Colorado variations were distinguished as subspecies by Keck, i.e. ssp. lanulosa and ssp. alpicola (Rydb.) Keck. If you believe the latter is a major geographical race rather than a minor alpine race, this status is in order,then the varietal name of Rydberg is in order, but not his species status for it.

Acorus calamus L. is a triploid plant that was introduced into Europe in the 16th century and much later into parts of the American eastern seaboard. it has 36 chromosomes that pair so irregularly that during all these generations even a single seed has never been observed. The Colorado plant is a diploid, however, and fully fertile, with 24 chromosomes that pair regularly, and the seed set certainly is good. It belongs to the American species A. americanus Rafi as we showed for the Canadian populations in 1957: L. & L.: Biosystématique du genre Acorus. - Ann. de l'ACFAS 23: 100. Rafinesque was sharper than those who wanted to suppress him because he was a foreigner with other ideas, not only in this fine case.

Adiantum pedatum ssp.aleuticum (Rupr.) Calder & Taylor, 1965: Can. J. Bot. 43:1388.

Agrohordeum macounii: Critelymus W. A. Weber is fine and fits the rules. But is it needed

Agropyron intermedium, should be: Elytrigia intermedia. And the ssp. trichophora is ssp. barbulata (Schur) Á Löve, Taxon 1980. Cf. also Melderis in Flora Europaea.

ok! Agrostis borealis: should be: A. mertensii Trin., cf. Widén, K. G. 1971: The genus Agrostis in eastern Fennoscandia. Taxonomy and distribution. Flora Fennica 5: 1 - 209

ok. Agrostis semiverticellata should be: Polypogon semiverticillatus (Forssk.) Hylander, cf. Hylander, N. 1945: Nomenklatorische und systematische Studien über nordische Gefässpflanzen. - Uppsala Univ. Årsskr. 1945: 1 - 337. (the change is on p. 74).

Alisma geyeri, should be: Alisma graminea K. C. Gmel. -1826., cf. Fernald 1950. gramineus Lej. 1811

Alnus incana ssp. tenuifolia (Nutt.) Breitung, cf. Hultén 1968.

Aquilegia belongs to Thalictraceae.

ok Arabis glabra: should be Turritis glabra L., cf. L. & L. 1974: Nomenclatural adjustments in the Yugoslavian flora. II. Pteridophytes and dicotyledons. - Preslia 46:123-138.

Arabis divaricarpa, drummondii, holboellii, should be: Boechera divaricarpa (A. Nels.) Löve & Löve, B. drummondii (A. Gray) L. & L., B. tenuis (Böcher) L. & L. (not B. holboellii s.str. which is a Greenland triploid apomict!). Cf. L. & L. 1976: Nomenclatural notes on arctic plants. - Bot. Not. 128: 497 - 533. Transfers to the new genus, to reduce heterogeneity of the collective genus, are needed for A. fendleri and some of the "vars." which the not too sharp taxonomist Rollins put under A. holboellii. Arabis s.str. is typified by A. alpina, so A. borealis and hirsuta are OK.

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Arctostaphylos adenotricha (Fern. & MacBryde) L., L. & K. is a very good species and certainly not a subspecies since it is intersterile to A. uva-ursi races, and differs among others in being diploid and not tetraploid, a very important difference as I thought you especially would recognize. Both our splits are. naturally, accepted by Vinogradova and Yurtsev 1980 in the 8th volume of the Arktik. Fl. SSSR.

Arenaria. You asked about A. hookeri. McNeill put it and A. franklinii Dougl. and the minor variation A. pinetorum of the former into the same genus as the Himalayan Eremogoneastrum. I am sure this is wrong, since it is contrary to phytogeographical expectations, and suspect that the American taxa are able to cross with other American Eremogonum, which you suggested as their correct place. Perhaps they are distinct enough to form a section, though I doubt it, but the transfer is required to solve your problem and to help to cut out the heterogeneity of Arenaria still more.

OK

Artemisia. Why not accept Polyakov's division of the genus, as we do in the Arctic Atlas? In Flora SSSR 26 he accepts Artemisia s.str., Oligosporus Cass., and Seriphidium (Bess.) Poljak. You can divide the Colorado material better than I can do here, but be aware that O. borealis (Pall.) Poljak. s.str. does not reach Colorado, where the taxon listed as A. borealis is what Hultén called A. borealis ssp. purshii, which at the species level (differs cytologically and geographically) should be called O. groenlandicus (Hornem.) Löve & Löve 1976.

In addition, there is the problem of Artemisia pattersonii which belongs to no described genus and differs most distinctly in its basic chromosome number x: 7, contra 9 for the other groups, as first shown by Del Wiens & Richter 1966 and confirmed bu usand Kapoor (1971). Wiens made the cytologically untenable and illogical conclusion that it might have derived from A. scopulorum. If you agree, this is a fine case for a new generic name, perhaps even the case that has been waiting for the "name" Askoris that Cronqvist once jested with?!

I do not think Beetle, who is a very old-fashioned taxonomist, realizes that the data support the opinion that his A. tridentata group consiste of at least two good species, perhaps even three or more? They differ cytologically. Perhaps the group even would be better split out of the strict genus?

Astragalus: The northern and southern group of this collective taxon are misplaced in the same genus, for various morphological and cytological reasons which the not so great but independent Barneby of 18th century philosophy fame has no possibility to understand. Would you dare to tackle what he feels he is the single owner of this generation? I believe the Russian solutions may help even here.

prostrata not in Fl. Eur.
no!!

Atriplex hastata L. I have the feeling that you are using this confused name in a wider sense than permitted - though I have not seen the Colorado population. Could it belong to A. prostrata DC. ssp. calotheca (Rafn) M. Gustafsson, or to ssp. prostrata? Cf. Gustafsson, M. 1976: Evolutionary trends in the Atriplex prostrata group of Scandinavia. - Opera Botanica 39: 1 - 63.

to check!

Beckmannia. The last authors are Koyama & Kawano.

Bolboschoenus paludosus: spelling of last authors correct?

Botrychium lunaria in the west is ssp. occidentale L., L. & K. 1971. You missed it.

Bromopsis pumpelliana is definitely not a race of inermis, though both have the same chromosome number. Cf. Arctic Atlas.

Campanula rotundifolia. I have the feeling that the indigenous taxon so identified may be the western C. petiolata DC. which seems to have 102 chromosomes, as contrasted to 68 of the boreal-arctic circumpolar Linnaean taxon and 34 of the arctic-alpine complex C. gieseckiana and its relatives. This could easily be corroborated by a simple chromosome study next summer. If it is confirmed, then the Colorado reference to the Linnaean plant must be dropped...I believe I never saw it in Colorado myself. Your observation, in the Flora, of an alpine race, is interesting. Perhaps it is another species? Or a variety only, in Hultén's and our definition of that category?

Carex bipartita. There is no doubt that Mackenzie was wrong when adopting this name, which actually was based on Kobresia simpliciuscula. I believe the Russians may be right in using C. tripartita All., as we do in the Arctic Atlas, though even you may feel a need to be conservative now and then and instead follow Hylander (1966) and Chater (in Flora Europaea) and use Carex lachenalii Schkuhr? But bipartita is definitely out, thoug even Hermann overlooked it. Perhaps vis var. austromontana is worth transferring to the other specific name? It cannot be entirely ignored without mentioning, though it may be misplaced?

Carex canescens. In the Arctic Atlas we deviate from Hylander etc. and accept the British interpretation, also in F. E., that the correct name should be C.curta Good. Time will show who is right...and pick your choice. I doubt that Hermann knew of Dandy's reasoning, which to me looks sound, as usually.

Carex capillaris ssp. fuscidula (V. Krecz.) Löve & Löve is the Colorado plant.

Carex heliophila. Is your trassfer and new ssp. already published? We have made the same combination on basis of our Manitoba and Montreal experiments in our final monocot chromosome list from Manitoba that is with the Taxon editor, but could drop it in proofs, if you prefer, as you know we do not live for priority as some do, notably Darwin who even became dishonorable for it, cf. Wallace's story.

Carex incurviformis. To me this is a plain synonym of C. maritima Gunn., cf. Arctic Atlas.

Carex magellanica ssp. irrigua (J. E. Smith) Hiitonen. (not Hultén!).

Carex macloviana here seems to be ssp. haydeniana (Olney) Taylor & MacBryde 1978: Canad. Journ. Bot. 56: 185 - 195.

Carex misandra, better: C. fuliginosa Schkuhr ssp. misandra (R. Br.) W. Dietr. 1967: Feddes Repert. 75:15.

Carex scirpoidea: the western race seems to be ssp. stenochlaena (Holm) L. & L., cf. Univ. Colo. Studies Series in Biology No. 24, pp. 25 - 26.

Centaurea needs to be split. I believe this is at least close to be correct: Acroptilon americanum (Nutt.) ...., A. repens (L.) DC., Acosta diffusa (Lam.)Soják, Acosta maculosa (Lam.) Soják, Jacea pratensis Lam., Leucantha solstitialis (L.) Löve & Löve (1961).

Cerastium arvense L. may perhaps be found as introduced, though I never saw it in Colorado, where the indigenous plant certainly belongs to C. strictum (L.) Haenke, cf. Arctic Atlas. It has 36 chromosomes only, as contrasted to 72 of the Eurasiatic weedy plant.

Chamerion platyphyllum (Daniels) Löve & Löve is the Colorado species of the angustifolium complex, cf. L. & L. 1976: Bot. Not. 128:516. I cannot see how the results of Mosquin's and other studies can be used as a basis for keeping such a plant at a subspecific level...it is perhaps crossable to the circumpolar plant of the more northern regions, but the hybrid is sterile. And both are morphologically easily distinguishable, at least to my trained floristic eye, since I have seen both much.

Chenopodium gigantospermum Aellen is a tetraploid that may not even be very close to the diploid C. hybridum. And cannot be its subspecies, if that category is classically defined or circumscribed as Hultén did correctly.

Clematis occidentalis (Hornem.) DC. ssp. grosseserrata (Rydb.) Taylor & MacBryde 1978.

Coptis belongs to Coptaceae.

Crepis in the strict sense is typified by C. biennis, its basic number is 5. In the taxonomically miserable Crepis work by Babcock the Colorado taxa are shown to belong to the very distinct sections Psilochaenia, x: 11, and Ixeridopsis Babcock, x: 7. I believe that a more sensible and taxonomically correct treatment would be to split them out of the extremely collective genus as the distinct genera Psilochaenia Nutt. 1841, typified by his P. occidentalis (Nutt.) Nutt., and Ixeridopsis (Babcock), which is typified by Crepis nana Richards. That species needs to be transferred to the new genus, and the former genus needs to get help with Crepis acuminata Nutt., 1841,p. 437; C. atribarba Heller, 1899, Bull. Torrey Bot. Cl. 26:314, C. intermedia A. Gray, Syn. Fl. 1,2: 432; C. modocensis Greene, 1895, Erythea 3:48, with var. subacaulis Kellogg, Proc. Calif. Acad.5:50, 1873. The basionym of the type species nana of the new genus Ixeridopsis is C. nana Richards. 1823, Bot. App., Franklin 1st Jour. ed. 1:746.

✓ Cryptogramma acrostichoides R. Br. is a good species, cf. L., L. & K. 1971.

Deschampsia cespitosa ssp. alpicola (Rydb.) L., L. & K. 1971 is a younger homonym of a Central European plant so named by Chrtek & Jírasek 1965: Acta Univ. Carol., Biol. 1965, No. 3: 207. Therefore, a new name is needed for the Rocky Mountain plant. Would monticola be acceptable?

✓ Dryopteris assimilis, should be D. expansa (C. Presl) Fraser-Jenkins & Jermy, 1977, Fern. Gaz. 11:338.

Elymus junceus should be Psathyrostachys juncea (Fisch.) Nevski, cf. Flora Europaea 5.

Epilobium adenocaulon should be E. ciliatum Rafin.

E. angustifolium ssp. circumvagum Mosquin, should be: Chamerion platyphyllum (Daniels)L&L.

Festuca brevissima Jurtsev is missing, of course, described after list was completed.

Glecoma L., correct Linnaean spelling, cf. Species plantarum, though h is used in the Index, which may have been made by some other than the author himself.

Gnaphalium splitting, cf. Flora Europaea.

Gymnocarpium dryopteris is replaced in the west and in the Rockies by the Pacific temperate species G. disjunctum (Rupr.) L. & L., cf. L. & L. Bot. Not 128 (1976) pp. 497 - 498, and Arctic Atlas, they differ also cytologically.

Halerpestes should be Cyrtorhyncha cymbalaria (Pursh) Britton, cf. L. & L. 1961, Bot. Not. 114:39, and Benson's Ranunculus review in Amer. J. Bot. earlier.

Hordeum except vulgare etc. is Critesion Rafin., cf. Löve in Taxon 1980.

Isoëtes setacea is a synonym of I. echinospora, which here is represented by the ssp. muricata (Dur.) L. & L., cf. Arctic Atlas and Fern Atlas.

Juncus alpinus: Does the Colorado material belong to ssp. nodulosus (Wg.) Lindb. or ssp. alpestris (Hartm.) Löve & Löve? Cf. Flora Europaea 5.

Juniperus communis L. ssp. alpina (Neilr.)Čelak. is OK! Syn. montana & saxatilis.

Juniperus horizontalis, monosperma, osteosperma, scopulorum, utahensis: should all belong to the very distinct genus Sabina. I am sure transfers have been made.

Kalmia microphylla (Hook.) Heller is a good species, cf. Löve, Löve & Kapoor 1971.

Koeleria cristata or macrantha should be K. latifrons (Domin) Rydb. It differs among other characters in being diploid, cf. L. & L. in Taxon 1980 (Manitoba mat.).

Leucanthemum vulgare, should be L. ircutianum (Turcz.) DC. (36 contra 18 chromosomes), cf. Gutermann, 1975: Notulae nomenclaturales 1 - 18. - Phyton 17:31-50.

Lolium multiflorum should be: L. perenne L. ssp. italicum (A. Br.) Syme, cf. Slovenian Atlas.

Lotus purshianus & wrightii: belong evidently to the distinct genus Lotea Medicus, (basic number 7 contra 6), transfers needed.

Lychnis apetala is evidently Gastrolychnis apetala (L.) Tolm. & Kozh. ssp. attenuata (Farr.) (transfer needed: basionym Lychnis attenuata Farr).

Lychnis drummondii should be: Gastrolychnis drummondii (Hook.)

Lychnis kingii should be: Gastrolychnis kingii (S. Wats.).

Matricaria inodora L. OK. (Tripleurospermum younger synonym as now circumscribed and typified, cf. Flora Europaea 4).

Minuartia should be divided into: Lidia biflora (L.) Löve & Löve, 1976;
Lidia obtusiflora (Rydb.) Löve & Löve, 1976;
Alsinanthe stricta (Sw.) Rchb.;
Tryphane rubella (Wg.) Rchb.
And I would like to propose the new generic name Durophylla, gen. nov., based on Minuartia sectio Sclerophylla Mattf., in Feddes Repert. Beih. 15:22, 1922), type D. michauxii (Fenzl) (chromosome number evidently x : 6 or 9 (2n : 36 known), for the taxa macrantha (Rydb.), nuttallii (Pax), filiorum (Maguire and rosei (Maguire & Barneby), cf. Maguire 1946, Madroño 8: 258 - 263 and 1951, Amer. Midl. Nat. 86: 493 - 511.

Muhlenbergia is spelled Muehlenbergia by Airy-Shaw in Willis.

Myosotis scorpioides L. em. Hill.

Myriophyllum spicatum ssp. exalbescens OK, cf. Arctic Atlas.

Pellaea glabrella v. occidentalis is better as P. occidentalis (E. Nels.) Rydb. cf. Fern Atlas.

Pellaea glabella ssp. simplex (Butters) Löve & Löve, 1977: Taxon 26: 324-326.

Phalaris arundinacea should be: Phalaroides arundinacea (L.) Rauschert var. picta (L.) Tzvelev, cf. Tzvelev 1976, Zlaki SSSR.

Phippsia should include Puccinellia, cf. L. & L. 1976, Bot. Not. 128:498 - 501.
Other combinations: P. airoides (Wats. & Coult.) L. & L. in Taxon 1981,
P. distans (Jacq.) Löve & Löve, Folia Geobot. Phytotax. 10:270 - 276;
P. lemmonii (Vasey).... , P. lettermannii (Vasey).....

Picea glauca ssp. engelmannii (Parry) T. M. C. Taylor, cf. L. & L. 1961, Chr.n. NW Eur. proposed by Taylor 1959 in Madroño 15: 111 - 115, with good reasoning I believe.

Poa alpigena, should be: Poa rigens Hartm. 1820, Handb. ed. 1, tillägg: 448; cf. also Jalas, 1958, Suuri kasvikirja 1: 405 - 406.

Poa pratensis ssp. agassizensis (Boivin & D. Löve) Taylor & MacBryde 1978, Canad. Journ. Bot. 56: 184 - 195.

Prunus virginiana ssp. melanocarpa (A. Nels.) Taylor & MacBryde 1978.

Pteridium aquilinum ssp. latiusculum (Desv.) Hultén in R. T. Clausen, cf. Fern Atlas.

Pulsatilla patens ssp. hirsutissima OK. (ssp. multifida Siberia only).

Ranunculus grayi OK, cf. L., L. & K. ...Tolmachev & Yurtsev knew be-ter than Ostanfeld!

Rhinanthus minor indigenous: ssp. borealis (Stern.) Á Löve, 1950, Bot. Not. (type of basionym from NW America).

Rhodiola integrifolia is Tolmachevia integrifolia (Rafin.) Löve & Löve, Bot. Not. 1976.

Rosa acicularis ssp. sayi not OK: better as R. sayi Schweinf. since it differs not only in morphology and geography (N. Am. contra Eurasia) but also in the degree of polyploidy, cf. Arctic Atlas.

Rosa woodsxii ssp. ultramontana (S. Wats.) Taylor & MacBryde 1978.

Rubus arcticus ssp. açaulis (Michx.) Focke, cf. Hultén 1968.

Rumex utahensis OK, R. triangulvalvis OK, cf. Nina M. Sarkar 1958: Canad. J. Bot. 36:947-

Rumex Acetosella should be: Acetosella vulgaris (Koch) Fourr., emend. Á. Löve.

Rumex paucifolius ought to be: Acetosella paucifolia (Nutt.) Á. Löve,(comb. nov)

Ruppia cirrhosa (Petagna)Grande ssp. occidentalis (S. Wats.) Löve & Löve, Taxon 1981,No.3. for the prairie plant you call R. maritima.

Salix bebbiana should be: S. depressa ssp. rostrata.

Salix cordifolia v. callicarpaea should be: S. cordifolia (no. var.!), not S. glauca, cf. Arctic Atlas, chromosome number differences!

Salix glauca var. glabrescens & v. villosa belong to S. glaucops Anderss.(114 chromosomes)

S. glauca v. glauca is the "western phase" of Argus, i.e. S. seemannii Rydb.(76 chromos)

(S. glauca s.str. is in Eurasia and Alaska, chromosome number 152), cf. Arctis Atlas.

Salix phylicifolia ssp. planifolia is simply S. planifolia Pursh 76 chromosomes, S. phylicifolia has 114!).

Saxifraga needs to be split into smaller and more homogenous genera; I have a proposal for the arctic taxa that could be widened to include those of Colorado, but ~~XX~~ am not quite ready with it...and do not have the Small treatment in the N. Am Fl.

Saxifraga monticola (Small) Löve & Löve, 1976, Bot. Not. 128:515.

Schizachne purpurascens ought to be called S. callosa (Turcz. ex Griseb.) Ohwi, cf. Flora Europaea 5!

Scirpus requires splitting, in e.g., Schoenoplectus, Baeothryum, etc. as we mentioned and did partially in U. of Colo. Studies in 1965. I do not have time to look up all the details just now but could do it later, if needed.

Sedum acre L. This complex in Europe is being divided thanks to the discoveries of the polyploid series of 40, 60, 80, 100 and 120 chromosomes. The most common West European species is S. acre s.str., which has 80 chromosomes, which is the only number so far counted in America, so probably the introductions have all come from that region.

NM – Christensen 189-20ex not in Clausen fide Clausen.

Sedum cockerellii Britton: This seems to be one of these "endemics" described without bothering to look into details in the real area of introduced species and their variations. I have not seen this taxon, which may have disappeared long ago thanks to wise natural selection and then it ought to be dropped quietly...even Harrison did not see it he claims...According to the description, it may be a simple forma of S. acre, differing mainly in an unusual color of the flowers, though that is only a guess. And if it is a forma, in addition of an introduced species, there may not be any reasons to spend time to look at it closer? But it could represent another introduced species and so have another correct specific name from Europe....do as you please, but do not forget to mention its introduced status, because this part of Sedum is not American.

Sedum debile. I have not seen it, and suppose it is safe to refer to Bob Clausen in spite of no specimens. But as far as I understand, it ought to be named Cotyledon debilis (S. Wats.) Fedde, rather than be allowed to confuse Sedum.

not in Clausen

Clementsia rhodantha is OK.

Tolmachevia integrifolia (Rafin.) Löve & Löve is also OK, and so is its var. procera (R. T. Clausen), which needs a transfer.
But I do not have enough information to see where S. lanceolatum belongs when the collective genus is split into more natural units. Do you perhaps have more informations in Clausen's last book, which I do not have? It is not a Sedum s.st

You have already split Senecio wisely, though not sufficiently, so I venture a new listing, with exclusion of the former splits:

Senecio (x : 10)

X acutidens Rydb.see Packera tridenticulata.
atratus Greene
cana Hook. see Packera cana.
crassulus A. Gray.
crocatus Rydb. see Packera crocata.
debilis Nutt. see Packera debilis.
dimorphophyllus Greene, see Packera dimoprhophylla.
douglasii DC. var. longilobus L. Bens.
eremophilus Rydb. ssp. kingii (Rydb.) Dougl. & Ruyle-Dougl.
fendleri A. Gray see Packera fendleri.
fremontii T. & G. var. blitoides (Greene) Cronqu.
hallii A. Gray see Packera cana.
hydrophilus Nutt.

Senecio (cont.)

integerrimus Nutt. var. integerrimus & var. exaltatus (Nutt.) Cronqu.
longilobus Benth. see Senecio douglasii
neomexicanas A. Gray see Packera neomexicana
molinarius Greenm. see Packera werneriaefolia
multicapitatus Greene (not: see Kraschenninikovia!) why??
multilobatus Greenm. see Packera multilobata
pauperculus Michx. see Packera paupercula
plattensis Nutt. see Packera plattensis
porteri Greene see Packera porteri
pseudaurea Rydb. see Packera pseudaurea
rapifolius Nutt.
riddellii T. & G.
serra Hook. var. admirabilis (Greene) A. Nels.
spartoides T. & G.
sphaerocephalus Greene
streptanthifolius Greene see Packera streptanthifolia
triangularis Hook.
tridenticulatus Rydb. see Packera tridenticulata
vulgaris L. (typis generis)
werneriaefolius A. Gray see Packera werneriaefolia
wootonii Greene.

Packera Löve & Löve, Bot. Not. 128 (1976): 520 - 521. (x : 23).

cana (Hook.)..... var. discoidea (W. A. Weber)....
crocata (Rydb.)....
debilis (Nutt.)....
dimorphophylla (Greene)....
fendleri (A. Gray).... var. fendleri, var. lanata (Osterh.)....
multilobata (Greenm.)....
neomexicana (A. Gray)....
paupercula (Michx.) Löve & Löve.
plattensis (Nutt.)....
porteri (Greene)....
pseudaurea (Rydb.)....ssp. flavula (greene)....
streptanthifolia (Greene)....
tridenticulata (Rydb.)....
werneriaefolia (A. Gray)....

Shinnersoseris. Is this correct spelling? Then it is a clumpsy name.

Silene: May I propose a mild splitting, as proposed by Holub 1977: Folia 12:417-432, and Ikonnikov in two papers in Nov. Syst.? That would replace a part of the Colorado genus with Oberna Adans. for O. behen (L.) Ikonn. for S. vulgaris, but otherwise leave the genus intact. we hesitated for the Arctic Atlas but I am accepting this for the new edition of the Icelandic flora.

Sisyrinchium. I wonder if S. angustifolium, which is S. bermudiana L., cf. Ward, Taxon 17 (1968): 270 - 276, actually occurs in Colorado? Strictly it is an eastern or perhaps even only southeastern seaboard species that has been much confused with the other American species, especially S. montanum Greene, which is certainly indigenous in Colorado and on all the prairies and even down to the Canadian coast and that of New England, I believe. And is Bicknell's S. heterocarpum worth anything? I have the feeling from what I have seen of his Polygonum taxa, that he was one of the many florists that fiddled with socalled taxonomy like stamp collectors do without really knowing what he was doing....as several others of us of course. But I may do him wrong.

Sparganium multipedunculatum. I doubt the wisdom of Reveals opinion, based on no experiment and very limited experience with the complex as a whole, though closer and more critical studies may well prove him right. Is it not wiser at least for the time being to wait and see and leave things as they were?

Spergularia. I cannot find the references just now, but the great experimenter Murbeck showed long ago that even the characters three contra five styles and leaves opposite or whorled, that have been used to distinguish the genera Spergula and Spergularia, are not constant. Since these taxa even hybridize, Hylander (again I cannot find the reference) found it unwise to separate them as genera.... I believe he mentions this especially during the discussions at the Flora Europaea symposium 1959 in Vienna that is printed in Feddes Repertorium... I agree, from other points of view though, and depend readily on Hylander's taxonomical conclusions in his checklist and his Nomenklaturische Studien 1946, despite Ratter's traditional ignorance of what Hylander and Murbeck concluded...I doubt t that his authority is as strong as their was? Therefore I propose the following names for the Colorado plants: (the genus belongs to Illecebraceae):

Spergula marina (L.) Bartl. & Wendl.
Spergula rubra (L.) Dietr.
Spergula maritima (All.).... This last taxon is based on Spergularia maritima (All.) Chiov. according to Rauschert 1977: Feddes Reprt. 88: 311, but I do not have the basionym, unfortunately, but could probably find it. S. media must be left out as nomen dubium et ambiguum, especially as used by Rossbach (1940: Rhodora 42: 134-) and those following her. S. marginata is also out because it is illegitimate since DC 1805 cited Arenaria media L. without a ? And now I found what I had overlooked earlier in the sentence: The oldest basionym is evidently Arenaria maritima All., Auct. Syn. Stirp. Horti Taur. (1773):35. Chiovenda made the combination in Spergularia 1912 in Ann Bot.(Roma) 10:22; it needs to be validated in Spergula which is the only genus I propose you accept in the list for this complex.

Stellaria media (L.) Vill. The confusion developing from numerous illogical moves during the last century to define Alsine so that it would include Minuartia etc. lead to the mistake that one forgot what Linnaeus meant when he created this name. Actually, the Linnaean genus is correctly defined by its species A. media L., as McNeill observed years ago, and so it ought to be retained in that meaning despite the confusion in other meanings. That annual species certainly is highly misplaced in Stellaria, a perennial genus typified by S. holostea L. We accepted Alsine media L. in this meaning in the Arctic Atlas, of course.
I believe Stellaria, Alsine, Cerastium etc. all belong to Alsinaceae, which ought to be accepted as a distinct family.

Streptopus fassettii L. & L. for old S. aplexifolius... I have mentioned that earlier.

Thalictrum belongs in Thalictraceae.

Thlaspi montanum belongs to the genus Noccaea, as N. montana (L.) F. K. Meyer, cf. Arctic Atlas and Rothmaler's critical Exkursionsflora, 2nd edition.

Toxicodendron radicans ssp. rydbergii is a geographical race and not a distinct species, even according to what Gillis reports. He seems to be as confused as most American, and even European, botanist as to the difference in definition between subspecies, variety and even these and species, and even genera, though I believe it is correct to separate Toxicodendron at that level. His ssp. are not always major races, though I believe this race may qualify for that ststus.

Trifolium I believe much could be said for the splitting of this collective genus that some colleagues have advocated recently, e.g. Hendrych, 1978, Preslia 50:119 - 137, though what he and his colleagues in Praha have been doing is insufficient even for the European groups and ignores cytology that seems to indicate splitting even of some of the traditional sections as genera. And the western American material add some problems, of course. If you so feel and believe you could come up with a reasonable solution, I would gladly add to it chromosomal inform ation and discussion of points I may find critical...though not promising to find every attempt acceptable. One thing is to propose that a splitting is necessary, another to do it logically and sufficiently well, and I have refrained even from attempting it on paper for myself for the simple reason that I have doubted my competence. But I think you could do it well.

Triglochin debile (M. E. Jones) Löve & Löve is the correct name for what has gone as T. maritimum and T. concinnum in Colorado. We published on this in Montreal and I believe I may have referred to it earlier? That species is widespread on the prairies at least.

Trisetum spicatum. You mention ssp. molle (Michx.) Hultén, overlooking that we showed, in Univ. Colo. Studies Biol. 17:6-8 (1965) and 23:14-15 (1966) that this race is more correctly placed as the ssp. molle (Hultén) Löve & Löve of the hexaploid species T. triflorum (Bigel.) Löve & Löve. Is there actually some evidence that this northern race grows in Colorado, or did Hultén mix or confuse it with some of the alpine races, for instance ssp. congdonii?

Trollius laxus ssp. albiflorus. A. Khokhryakov, 1977: De generis Trollius L. specie albiflora Asiae boreali-orientalis. - Nov. Sist. Vyssch. Rast. 14: 79 - 81, comes to the conclusion that the whiteflowered socalled Trollius of NE Asia, or T. chartosepalus Schipczinskiy (cf. Fl. SSSR 7, 1937 and Bot. Mat. 4 (1923) is more correctly placed in the genus Hegemone Bunge, which is typified by H. lilacina Bunge from Altai and other Central Asiatic mountains. He makes the transfer of the Chukotka-Kolyma plant. He mentions in passing that the American taxon T. laxus Salisbury may belong here. However, N. Schipczinsky, 1923: Generis Trolliii species novae et restituendae. - Bot. Mat. 4: 1 - 15, points out that his T. chartosepalus from Chukchi is affiliated to T. americanus Mühl. & Gaiisenh, from which it differs only slightly, he says. T. americanus is a later sy n onym of the eastern montane T. laxus Salisb. with pale greenish-yellow sepals to which Gray (1862) added the whiteflowered western albiflorus as a variety. We (1971: L. L. & K.) lifted it to subspecific status, whereas Rydberg regarded it as the species T. albiflorus. I have not yet seen the Soviet plant, but know it is a diploid as contrasted to the American tetraploid (both laxus str. and albiflorus). From the description I get the impression that the American plant is actually misplaced in Trollius and fits better into Hegemone, but I am no longer sure that the western plant is a valid race of the eastern one, so perhaps it would be right, at least until experimental evidence shows otherwise, to follow Rydberg and separate it at the species level. That would requires that two American taxa be removed from Trollius and transferred to Hegemone...and in fact both are very distinct from the typical Trollius as I knew it in Europe. Think the matter over and let us discuss it closer, though I do not think it would be a mistake to make the jump and take up Hegemone for the Rocky Mountain species.

Urtica gracilis Ait. is a species clearly distinct from U. dioica, cytologically (26 contra 52 chromosomes), geographically and morphologically, and they are at least very difficult to cross according to extensive Canadian experiments.

Vaccinium cespitosum. Is this Michaux spelling, or do you only accept that of Linnaeus and transfer it to this species? It is then a mistake because Linnaeus' spelling errors are not acceptable as good Latin, though they are, for idiotic majority decisions, to be kept in his names.

Verbena: Moldenke includes V. canadensis and refers to material from Boulder Co., certainly introduced (?), as at least some of the other taxa may also be? He also has V. bracteata f. albiflora (Cockerell) Moldenke from Boulder Co., clearly a forma of the kind that is meaningless and ought to be forgotten in times after Mendel; V. ciliata var. pubera (Greene) Parry, V. macdougalii Heller f. albiflora Moldenke, and V. gooddingii var. nepetifolia Tidestrom, probably all single-gene formae that ought to be ignored?

I also wonder how many of these taxa ought to be further reduced to varieties or formae of more exactly defined species...though I admit that I am not familiar with most of these? And is it wise to give a species name to a taxon as V. gooddingii, which may perhaps be better accomodated as simply as the variety alba Palmer, or perhaps even better as f. alba only, of the species V. ciliata? I am sure that you have also thought about these problems and many other similar ones in other equally critical groups that have been dealt with by less critical taxonomists and even gardeners in the past, but Verbena is a classical example of confused taxonomy everywhere.

Veronica wormskjoldii in the Rockies is not this tetraploid northeastern and Greenland species, but the diploid V. nutans Bong., cf. the Arctic Atlas.

Viola epipisila ssp. repens. This is the same taxon as others may have called V. palustris or V. pallens from the west. I do not know its relation to V. macloskeyi, but the first three are actually identical to the species V. repens Turcz., which unfortunately is a younger homonym and so must get a new name. We proposed V. epipsiloides in 1976, Bot. Not. 128:516. a name accepted by the Russians in the 8th volume of Arkt. Fl. SSSR. It is related to the more eastern V. palustris and to the western V. pallens, but very distinct from V. epipsila, as even the different basic chromosome numbers confirm. I believe it is the only species of this group in Colorado so the other names used may be put into simple synonymy as they have been used in your area.

Viola kitaibeliana var. rafinesquei. This taxon is better accepted as a species in its own right, or V. rafinesquei Greene. Cf. Clausen & alii 1964: Rhodora 66: 32-46.

Vitis. There is something fishy about the Vitis complex in America that has been split into numerous species which are known to be interfertile. They may be better placed as varieties only, of the American subspecies labrusca of Vitis vinifera, which is the species represented in easternmost Asia from which all the cultivated wine has come. vinifera and labrusca are fully interfertile and so are, as far as I am aware, all the others. Though it may be handy to recognize some of the more distinct taxa as varieties, I wonder if it would not be best to reduce the names used for the relatively restricted Colorado populations to one distinct varietal epithet? Though you may prefer some less drastic approach for the time being?

San José, April 1, 1981.

Dear Bill:

Your card of 27 March crossed my thick letter on its way over the mountains, but since I hope to get to other matters that have been waiting too long, I am writing at once to answer the few questions you raised. I have looked closer into a recent paper by Yurtsev in which he seems to have reverted the advice he gave us in 1975 on Tephroseris from NE Asia, though it is too long to relate here. But he seems to support your view that T. jacuticas is a species in its own right, distinct from the Kjellmanii taxon, on which we agree, though the former still may be a subspecies only, as he mentioned earlier. That is a question requiring hybridization. So, use T. Kjellmanii with confidence, but do not refer to us as its final authors, because Holub made the transfer between the time when we completed the manuscript of the Atlas and got our Botaniska Notiser paper printed... you will see that there is a discrepancy between these publications because we dropped our duplicate transfers in the latter when we got Holub's paper.

As to Packera, you have probably seen the list I made of it in my last letter. There is no doubt about its distinction, and many more species than those in Colorado need to be transferred, as you certainly observed. However, it is against my ethics to transfer the Colorado taxa by myself for the simple reason that you initiated the study of these plants. So why not make them, and several other new transfers or names needed for that flora, jointly in a short paper that perhaps could be printed in Phytologia or somewhere else where it can get out fast? Perhaps where you print your other remarks on the flora?

I have looked for informations on the chromosome numbers of Ligularia that you are more familiar with than I am myself. Of course, there are numerous inexact reports by botanists with technical cytological knowledge without being exact or critical, and many seem to have adjusted their numbers to the common x : 10 or 5 of classical Senecio. However, as far as I dare to judge my colleagues, it seems to me that both groups (or are they identical?) have the basic number x : 29 and so are secondary evolved from plants with 60 diploid chromosomes. If that is correct...and I have reason to believe it is...then there is a distinct genetical gap between these genera and Senecio proper. I am sure the same is true for some of the other socalled Senecio groups and would not hesitate to distinguish them even if I only had their morphology to go on...and I would simply ignore all "advice" from "everybody including Nordenstam" and do what I feel is correct. Although Nordenstam studied at Lund and got some cytological technique as everybody of his generation got through Lövkvist, he and the others did never get the genetical philosophy that I was brought up on a generation earlier, and so he, and they, perhaps except Arne Strid, continue to think taxonomy on the basis of the good old 18th century philosophy that even tried to subdue the great Hultén. And that generation did not have the privilege of advice from Hultén as we did, for the simple reason that then he had moved to Stockholm and Lund, as Uppsala, could continue to teach students as if he had never been and as if there had been no addition to taxonomical knowledge and philosophy for a century or more. I wonder if your short stay in Stockholm at Riksmuséet made it possible for Hultén also to contaminate you...with the result that our ideas coincide in general?

With the very best regards from us all to you all,

as ever,

San José, April 21, 1981.

Dear Bill:

Try to find some valid excuse for my delay in thanking you for a fine proposal for transfers to Packera of other American taxa, and for the card I got some time ago. We have had some visitors that took time, and then I have been trying to get back to my Icelandic flora translation and to complete various other things that were slowly getting to me, not to forget the editing and typing of the longest chromosome number reports till now that is due in Washington next week, reviewing of some papers even for Systematic Botany, reading a couple of good Indian theses, and answering letters from the younger generation that still is asking questions that ought to be sent to the great botanists at the Smithsonian, who evidently are not always willing to help, despite their good salary and fine time for travels. Have you seen that they are advertizing for a herbarium chief for the next five years? You ought to let them know that you could become available...that would be good for American botany, and if they had not helped to stab me and others swallowed the whistle, even I would be interested though that would require that I be forced to accept American citizenship. But I realize that no botanist here would approve of that a foreigner with own opinions that are contrary to the still dominant socalled darwinian ideas and based on revolutionary genetics would sit in such a position...though they could accept a Ghanian negro anatomist, perhaps to be able to say that they were not racists? I am sure that that is the single position, other than at Harvard, which we ought to have accepted here, and that the next best...or even best...other man for it is the receiver of this letter.

I am enclosing a list of some changes that I believe need to be made, and certainly many more have been overlooked in this very fine checklist of yours.... do not try to compare it with the socalled list of the American plants because that might cause you to explode of pride. Though I still am somewhat peeved by your reluctance to accept even the good cases when the biological concept requires that our logic be followed, and that you still have some trouble remembering the essential difference between a biological species and its major and minor geographical races that never are intersterile and always are the result of genetic drift during isolation by the various glaciations when they grow in the boreal hemisphere, whereas the species are formed by chromosomal changes and polyploidy during the Tertiary...with very few exceptions. But even that will change when you have time to think matters over, although your original cytogenetical training has been allowed to get into the background when you were forced to spend your time to build up the best organized small herbarium on the continent, at least. I am thinking of your Galium mistake.

I have not seen the Acorus material, but if it is sterile, it may be the Eurasiatic introduction and not the indigenous American species.

Do you want me to add some discussion to the good Packera paper? And could you add the Mexican and South American species from Greenman's treatments? Chromosome numbers have been counted on most of the American and several southern species, so perhaps we ought to mention that and give some references?

Excuse the abruptness, but the paper is filled. All the best from us all,

As ever,

More adjustments to the Colorado list (Á.L. April 21, 1981):

Bolboschoenus maritimus(L.) Palla ssp. paludosus (A. Nels.) Löve & Löve, Taxon 1981.
Baeothryon cespitosum (L.) Dietr., cf. L. & L. 1965 (Colo. Stud.), and
    Czerepanov 1973: Add. Corr. SSSR.
Carex buxbaumii...possibly ssp. alpina (Hartm.) Liro? (i.e. C. adelostoma).
C. capillaris...if not alpine, then ssp. chlorostachys (Steven) L.,L.& Raymond,
    but the alpine certainly ssp. fuscidula (V. Krecz.) L. & L.
rejected — C. curta Good. OK for old C. canescens, cf. Arctic Atlas and Fl. Eur. Löve type is C. buxbaumii But Toivonen (1977) says canescens.
C. duriuscula C. A. Mey. OK for old C. stenophylla, cf. Czerepanov 1973.
C. fuliginosa Schkuhr ssp. misandra (R. Br.) W. Dietr., cf. Arctic Atlas.
rejected C. macloviana D'Urv.
    ssp. haydeniana (Olney) Taylor & MacBryde
    ssp. micropterа (Mack.) Löve & Löve (Taxon, in press)
    ssp. pachystachya (Cham.) Hultén
✓C. pensylvanica ssp. heliophila (Mack.) W.A. Weber, OK.
Eleocharis fernaldii (Svenson) Á. Löve, for E. quinqueflora, cf. Löve 1954,
    Sv. Bot. Tidskr. 48:218-219; the latter Eurasiatic with 132 chromosomes,
    the former American with only 80!
Schoenoplectus lacustris (L.) Palla
    ssp. creber (Fern.) Löve & Löve (for Scirpus validus)
    ssp. acutus (Muehl.) Löve & Löve (for S. acutus, not same as ssp. glaucus (Sm.)Hartm
        which is S. tabernaemontani from Eurasia)
Schoenoplectus pungens (Vahl) Palla ssp.longispicatus (Britt.) Löve & Löve, Taxon 1981.
Scirpus microcarpus K. Presl ssp. rubrotinctus (Fern.) Löve & Löve, Taxon 1981.

Juncus alpinus Vill. ssp. nodulosus (Wg.) Lindm.
Lilium philadelphicum L. ssp. umbellatum (Pursh) L. & L., Taxon 1981.

Luzula spicata ssp. saximontana L. & L. 1965: U. of Colo. Studies.
Smilax herbacea L. ssp. lasioneuron (W.J.Hook.) L. & L., Taxon 1981.
Sisyrinchium angustifolium, should be S. bermudiana L., cf. Ward 1968: Taxon 17:270-276.
Cypripedium parviflorum Salisb. OK.(Cypripediaceae)
Cypripedium pubescens Willd. OK.
Coeloglossum viride ssp. bracteatum (Muehl.) Soó.

Galium septentrionale is certainly a very good American-Asiatic species, as we demonstrated in our first N. Am. and good paper in 1954: Am. Midl. Nat. 52:88-105. I am sure you agree when you read it that our reasoning is considerably deeper and more logical than the very superficial "conclusions" of Hara and some others, not excluding Hultén, who never understood that cytology confirms his fine explanations of the nature and evolution of subspecies or major geographic races. Böcher and some others who also have looked at related plants superficially, all of them also rather weak floristic botanists with an unsharp floristic eye, contrary to you but like most socalled "taxonomists" here who lack the training, have had some difficulties with some European material, which may or may not belong here, so our conclusions as to the western limit of the area of he hexaplois may have to be adjusted, or their material may belong to another species or to an European subspecies of the American-Asiatic plant. I ought to have emphasized above that the American plant is always hexaploid with 66 chromosomes, the European from west of Poland tetraploid with 44, and that these taxa are not two autoploid stages of the same series, but clearly alloploids. Whatever later studies will find in Europe, the American plant is most distinct and a good species.... and I believe you will agree with our reasoning when you read it. The journal is in the library, as far as I remember.

San José, April 24, 1981.

Dear Bill:

Many thanks for the impressive chromosome list from Siplivinsky, but before I say more about it, I must add another correction of the Colorado list that you will appreciate to have: Ceratocephala testiculata seems to have to change its name to Ceratocephala orthoceras DC. according to the usually very exact W. Guterman, 1975: Übersicht einiger ergänzter Sippen und geänderter Namen in den Markierungsformularen zur Kartierung der Flora Mitteleuropas. - Göttinger Florist. Rundbriefe 9: 44 - 52. That paper is a kind of a "Nachtrag" for the 2nd edition of Ehrendorfer, F. (editor)1973: Liste der Gefässpflanzen Mitteleuropas.

ok

Now back to the Belaeva & Siplivinsky list: I am sorry that not only is missing the information on the place of the voucher specimens, which may or may not be LE, but I also need the first names of the authors, also that of the lady, and the addresses of both. I assume that her address may be Leningrad at the Komarov Institute, but should we not also have his New York address so that others could contact him if interested? You see what I mean if you look up the list in a recent number of Taxon. Fortunately or unfortunately, all but the last list for 1981 are already in press, but I still am preparing the manuscript for the November list, which already is by far the longest we have had, so perhaps the other groups may begin to complain about that our list is beginning to get the same privilege as obituaries of Dutch schoolteachers had earlier? But I know that Frans has a soft spot for the list and for the biosystematists since we succeeded in more than doubling the membership by getting them to join, so I only do my best to keep the voucher references short, as does also Siplivinsky. I would like to get his list into the November number so I will retype it and add what I hope is the correct information...with space for his address... and if my guesses are wrong, then I will be able to correct them when I get the proofs later in the spring or summer. So contact him as fast as you can and tell me the results, I believe I will be able to stall the manuscript until at the end of next week.

You never told me, at least not clearly enough, that Siplivinsky had come to the States to stay, and I believe he made a mistake caused by all the propaganda that even fooled us and that he might have been better off in Europe...but where? It is easy to criticize but difficult to come with positive advice, especially to those who are being mistreated or misunderstood at home, and especially if they come from countries with totalitarian governments, either fascistic or socialistic. I know some of his works on Saxifraga and also that some of my friends there did not approve of them because they do not understand the distinctly evolutionary background for cutting down the heterogeneity in such groups in our humanly made systems, and I am in no doubt that he is a good man and that his ideas could benefit from critical cytogenetical additions. Perhaps you could induce him to join with us in proposing such a new revision of Saxifraga, for instance those of the Arctic to begin with, or the arctic-boreal groups, and then later also all the others? At least we know this is needed, and if he wants to do it alone (which he may need for the sake of added competence) I will only be pleased to give him help from the cytological side. I read about your good old big heart between the lines, sorry that the circumstances prevent you from doing all that you want and could do for those who are mistreated.

All the best,

as ever,

Askell

San José, May 2, 1981.

Dear Bill:

Many thanks for two cards, the latter with the N.Y. address of Siplivinsky. I used it in the manuscript for him, but the Komarov address for Belaeva, and listed LE as the place of the vouchers, and no initial for her. All this can be changed in proofs later in the summer, if necessary, but I felt it may be important for him not to have to wait until next year to have this published, although the list does not come until in November Taxon. This one was the longest ever and also the most interesting ever, with more than 960 contributions!

It is like you to try to help Vladimir, though I know that your powers are weaker than you would like...but where there is a will...But since he is not a Jew and thus will not get the help that the Russian dissidents and socalled refugees always have gotten to get into good positions even at Harvard, he may need a good deal more help from many others...and when Reagan and the ultraconservatives now cut down not only necessary matters but all they can see has been used by the poor and the immigrants, his hopes may not be great, I have heard even medical people here talk about diminished hopes for the future, so what can those who came too late or from the wrong places expect to get when the country goes into one of its not too rare lows...which I at least hope will never be? I am glad that Vladimir has your sympathy and whatever help you can give him and your optimism, but am sorry that he is so far away that we will likely never meet him, but if he ever comes to the far west, or they, we would at least try to do for them what you try now, and enjoy his good botanical skill that I had observed earlier in print, we even transferred one of his Rhodiola species to Tolmachevia in the Arctis atlas. But since he is a good and progressive taxonomist, everybody in that field here will be afraid of him and do what they can to prevent him from doing anything in that field, with only a single exception: you. For the simple reason that that field is filled with people who have reasons to be afraid of those few foreigners who know because they see immediately through the fraud. And also there you are the single exception because you do not need to be afraid since you know that you know. Give him our best regards and wishes, when you contact or see him and his wife.

Good that you found the Galium boreale-septentrionale paper, and it does not astonish me that you had even planned long ago to use it for an addition paper, though it became hidden away. We never went to the efforts to classify the variations in hairiness that are clearly geographical in septentrionale, but others have earlier made some studies of them, especially a lady who wrote in Phyton in the 50's. If you look closer, perhaps you could subdivide the American material sensibly according to the classical Hultén scheme?

I have not been able to find my notes on the Dandy paper, or was it Dandy and Nelmes?, regarding the Carex canescens problem, but I believe it was printed in the Bot, Soc. Brit. Isles reports before Watsonia started, and had to do with typification that Hylander doubted (again, I cannot find that note either). But as you observed, the Linnaean Society material of the species does not fit the Linnaean description, and even Janchen (1959), on p. 766 in his Catalogus Fl. Austriae, uses curta and makes the remark that the Linnaean description "spiculis subrotundis" is for brunnescens, not canescens. See also Czerepanov's Additamenta et corrigenda. That a Finn used canescens in 1977 says nothing, since he follows Hylander, who did not even mention Dandy's and Janchen's observations...for a reason that I know about.

I never told you my reasons for the transfer of the American Carex haydeniana, microptera, pachystachya and even limnophila and festivella as one major and several minor geographical races of C. macloviana (not with capital, it is named after the Maclovian Islands, or the Falklands, not Maclovius for whom the islands were nemed). You will see that Hultén accepted the third as a race in the Alaskaflora. They are all characterized by the same rare chromosome number and by the same number of small and large chromosomes, and by a very similar morphology that leaves no doubt in my mind that all belong to the same species, though I still have not had an opportunity to cross them and study their hybrids cytologically. As a matter of fact, I believe there is a good reason to wonder if the American races have not evolved at two different stages, first as a single subspecies after its isolation in the refugium south of the ice during the great glaciation, and then as more varieties from several refugia during the last glaciation? This is my way of thinking, not that of Cronquist, but I am pleased that you protested because I looked closer on the descriptions and my notes and decided that Cronquist's idea that microptera and festivella cannot be distinguished is a mistake...and the race we saw in Manitoba belongs to the latter only. A problem that we can discuss as long as we do not have distinct experimental evidence that nobody in America has had facilities to produce, though many talk about experiments, though I am sure that I am closer to the truth than you are, if you maintain all these taxa as species, because they certainly surround the widespread macloviana nicely. It may be our different way of thinking of evolution and speciation, or that you have some difficulty in remembering that morphology always is secondary in evolution? So let us agree to disagree, nobody has been able to just that better than you when I have come with ideas that you find crazy...and then slowly adopt.

I am enclosing a proposal for an introduction to the Packera paper, in which I also have made some minor changes. Hope you can use it and modify it if you feel that is necessary. I do not think we ought to put our initials in the author references, because the combination Weber & Löve is unique, but you may have another opinion. But you are right in avoiding including what we do not know for sure...others may love us for that later when they get their chance to make a new transfer of what we avoided now. I also added a list of references that may be worth while mentioning, though I did not include a reference to your conclusion regarding Ligularia of which I am at least as sure as you are after having been thoroughly convinced by oral arguments years ago. Although many seem to have counted 60 chromosomes in some of the species at least, some have counted 58...and I would not be astonished if that were the correct number, thus adding cytological arguments to the good morphological ones. And the coming generation is likely to know Senecio mainly as a weed, an annual weed, and name the others as several genera, as you concluded in your short introduction.

This will have to suffice for now, though I still love to chat with you per letter since the distance has become to large for calling and the funds too restricted for staying on the telephone for the hours we might need. But even that we did not do during the good years in Boulder, though they were good mainly because of you and Jack....and we did not use the opportunity enough to discuss, and we never got any opportunity to build up experimental facilities and work extensively in the fields that you dreamt of when you much earlier wrote to me in Montreal and asked for some student for the approach of ours and Böcher... and you got Del Wiens, who is a talker and politician. Perhaps both you and we ought to become the latter...though I never came to America for that purpose. Or for the purpose to worry about the money that we ought to have gotten for the considerable and certainly good work we did...that is the fate of immigrants who have different ideas and refuse to crawl for false mormons as the one at Gray.

Hope the summer is in Boulder, ours has come with heat and garden pests.

All the best to you both from us both,

~~ACO~~
~~AGR~~
~~CVL~~
~~ILI~~
~~MSP~~
~~PHO~~
SMB
SNC

5
46
SPC = sphenoclea

San José, May 5, 1981.

Dear Bill:

Thanks for the new family acronym list. It is so well done that even I with my evil eye have succeeded in finding only eight omissions of families I know or rather of those of the boreal hemisphere, though there must be a good deal more that could be found by somebody more energetic and more nasty. Especially if you used Willis, where all the tropical and austral families are listed, though even Airy Shaw missed some of the newer boreal ones. You have missed the following that are in our 1961 list, for instance: Acoraceae, Agapanthaceae, Convallariaceae, Ilicaceae (i.e. Aquifoliaceae, misspelled Iliaceae by us in 1961, cf. Fl. Belg. IV, it is not the same as Illiciaceae, of course), Mespilaceae, Phormiaceae, Sambucaceae, Saniculaceae...and then the machine has played a trick on you on p. 08 in the order of Plagiogyriaceae to Picrodendraceae, and again on p. 10 in the order or placement of Trigoniaceae and Tropaeolaceae. If I had done such a good work myself on such a fine idea, I would be too proud to sit on it any longer, although you certainly could make it complete and perfect with little additional efforts by going through the newest Willis. But it is good as it stands nevertheless, and since the idea is too useful to others to be kept in the small museum at Boulder, I hope you will write a short explanation and send it to Taxon, which is beginning to collect manuscripts for the last number for 1981 and the first for 1982.

Hope you can get Siplivinsky to help revising the Saxifragaceae and split more sensibly than was done in the N. Am. Flora long ago...though even that was a great improvement from the unwise and non-evolutionary lumping by Bentham and Hooker, that the Englishspeaking world has forced upon others for a century, at the same time as it made propaganda for Darwin's wrong explanation of evolution that nobody still is allowed to deviate from...despite of almost a century of genetics that proved it wrong together with phytogeography. If the few good taxonomists here had had the wisdom to keep together and work with wisdom and learning, as do the molecular buffs, who even in Boulder have succeeded in talking so much about the excellence of each others that more than one of them have been lifted into distinguished professorship and distinguished salaries that the others do not enjoy, then people like him would easily have been added to the staff. But taxonomy here is weak because those who worked well in it were frequently slandered by those many, who say they are taxonomists though they are only simple florists and that not always good even at that, and because they never learned the truth of the Grimms' story about the father who asked his sons to come to his deathbed and showed them how much stronger five sticks tied together were than any single stick that even the dying man could easily break. Because there is so much to be done in both taxonomy and phytogeography that could be better done here than elsewhere...but not if those few who have ideas and knowledge are excluded by those who have nothing such...as e.g. Crumpacker and Dave Rogers, when they prevented me from hiring or even talking position to one of the best then available cytogeneticists because he was German, they said! They never were bigots, of course not, not in their own views, but perhaps afraid of others that might be more learned and have greater ideas that are needed?

You never said anything about your wish to buy the Meusel Atlas after you returned last year from the Himalayas...I have not offered it to others as promised.

All the best and keep up the good work and ideas,

Askel

[illegible] (L) [illegible]

May 8, 1981.

Dear Bill: I have not succeeded in getting an answer from Soják. Festuca brevissima: my memory fooled me, had not filed Signe's paper. - Eritrichum is the original spelling by Schrader (1820), corrected to Eritrichium by Gaudin (1828), and also more grammatically to Eriotrichum by Saint-Lager (1889). The first must stand. - Rafinesque was right in separating the R. M. Caltha in the genus Psychrophila, but probably wrong in adding a species from the Falklands...I doubt that phytogeography. The type: P. auriculata from Oregon, or your leptosepala DC. that has not been transferred, so that is your privilege. - If you hesitate about Negundo, let me share with you the trans-xfer of ssp. interior since we lifted it to ssp. in 1954. Negundo is a good genus that never crosses with any Acer.

I have also proposed you to the Explorers Club, with hesitation because it adds too many now for business reasons and change it an expensive dinner club for businessmen, at least here. But in Boulder-Denver you will enjoy it since you have not been cut down economically by the crook and their impunity thanks to whistle swallowers. You will fit as nicely as Hultén did.

Could you or Pat again correct the Dept. to prevent them to return my mail and journals etc. as "addressee unknown"... I have received three such complaints last week from Europe...one mentioning similar returns since 1979 three times. If the crooks, which I doubt, have ordered this, the former colleagues ought to have refused...as the Americans said the Nazi generals ought to have their "orders".

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

SAN JOSE
PM

Löve,
5780 Chandler Court,
San José, Calif. 9512

SAN JOSE, CA 951
1 PM
7 MAY
1981

US
Domestic Rate

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

May 7, 1981.

Dear Bill: Thanks for two cards of 4/5. I have mailed Meusel and am satisfied with your offer, of course.
As far as I understand, the species you list can be divided so: Greniera J.Gay: douglasii, howellii (rather than Alsinopsis Hell Alsinanthe: austromontana, dawsonensis, macrantha; Sabulina: tenuifolia (L.) Rchb. (or hybrida (Vill.)?), recurva, viscosa; Cherleria: laricifolia, sedoides, capillacea, frutescens; Porsildia: patula, brevifolia, glabra (var. only, cf. L.&L.1965) Minuartia s.str.: setacea, rubra; Neumayera: austriaca; Chetropis Rafin.: bosniaca, anatolica; Rhodalsina J. Gay: geniculata (Poir.) Williams; Lidia: hondoensis, marcescens (the latter var. of obtusiloba?); and a new genus, N. Am.: for series Pungentes: nuttallii, rosei (better as var. only?).

I do not know the restricted Cyrtorhyncha and Halerpestes enough but followed Britton. But I trust your judgement.

Pity that you did not meet Dr. Malla, he would have liked it but hopefully next time, by warning him ahead of the visit.

All the best,

San José, May 20, 1981.

Dear Bill:

ok. Thanks for the card stamped May 15. I wonder if you actually are using Elymus elongatus and ignoring Lophopyrum elongatum (Host) Löve, Taxon 1980, 29:351? I am just trying to complete a paper, invited by Biol. Zentralblatt for a Festschrift for my good old friend Stubbe, in which I give still more reasons for the use of this new name for this well established genus, but that is another story. Runemark made the transfer to Elymus in 1972 in Hereditas 70:153, and it is accepted by Melderis, of course, in Fl. Eur. 5:195 (1980).

OK In Flora Europaea (p. 197) you will also find Elymus trichophorum as the taxon E. hispidus (Opiz) Melderis, 1978, Bot. J. Linn. Soc. 76:380, ssp. barbulatus (Schur) Melderis, l.c., p. 381. You will, however, not find the combination Elymus trichophorus ssp. intermedius except in our Slovenian Atlas, p. 95, because we discovered before we published a validation that Melderis had uncovered the nomenclatural story that he relates in his paper of 1978. However, there is no doubt that the taxon does not belong to the genus Elymus as correctly typified but to the genus Elytrigia Desv. as typified by Nevski, so its correct nomenclature seems to be: Elytrigia intermedia (Host)Nevski, 1933, Tr. Bot. Inst. AN SSSR, ser. 1,1:14; ssp. barbulata (Schur) Löve, 1980, Taxon 29:350. You will find the details in Melderis' Bot. J. Linn. Soc. paper, pp. 380-381.

You seem to becoming as unorganized as I like to be since you lost Barry, otherwise you would not have lost the details I had given you regarding the original spelling of Eritrichum Schrader 1820. As Janchen, in Catalogus Florae Austriae I, Wien 1956-1960, put it most appropriately: "Der Gattungsname wurde von Schrader 1820 als Eritrichum veröffentlicht und von Gaudin 1828 unbegründet in Eritrichium abgeändert. Sprachlich richtiger wäre Eriotrichum Saint-Lager (1889)." I checked this with the originals long ago in the good Montreal library and, of course, could only confirm what the very exact Janchen said: the valid name is Eritrichum and Gaudin's "correction" must be ignored according to the present Code, whatever the Kew people may have decided, because the Code says so. This was, of course, ignored by the somewhat confused little Chater in Flora Europaea, he has probably never bothered to look through what the great Janchen wrote since it was in German and not in English. He repeats what you got from Index Genericorum...who is the sloppy author of that part of the Index? and has he made other such autocratic decisions that will confuse the few users in the future? Whereas Ehrendorfer and his excellent colleagues use Eritrichum Schrad.in the good Liste der Gefässpflanzen Mitteleuropas, 2. Aufl., Stuttgart 1974, p. 105. As we also did in the European chromosome list from 1961, which certainly used the most exact nomenclature then accepted by any taxonomist in America...though those who felt they knew better without looking up the details ignored it, waiting in the background to get these foreign devils to where the pepper grows...that succeeded through the Boulder trap. There is a good reason to be critical of autocrats, be they at Kew, New York, Washington or Boulder...we ought to have listened to Eilif though then it was already too late because we had got into the trap that even you were too innocent to observe in your dream of at long last getting somebody who would understand the great work you were and still are doing with the flora of Colorado.

OK Have you seen that Leena Hämet-Ahti has (1980) showed convincingly that Juncus alpinus is superfluous for J. alpinoarticulatum Chaix ex Vill.? She transfers the ssp. alpestris (Hartm.) Hämet-Ahti and ssp. nodulosus (Wg.) Hämet-Ahti, the latter of which I believe is in Colorado.

I am a little puzzled about that you do not mention in your card if you have received the Meusel package that I sent on May 7 or the letter that was sent separately the same day, after you had offered me 200 dollars for the book. It was addressed to the herbarium of the Museum care of you, since it was sent as library mail to save a few cents as usually, and for the same reason I did not insure it or register it...one must at least try to trust the American mail. It is so unlike you not to mention such matters, so I wonder if it still can be on its way as very slow library mail? If you have not received it, please, make the necessary enquiries so that I also can do so at this end. But perhaps you only forgot to mention it and are still waiting for the slow and ineffective cheque-mill of the bureaucrats, so I ought to show more patience? As a matter of fact, I have long needed to go to the expensive dentist to get some repairs that bother me since months ago, and had hoped that good old Meusel would help.... through you. So bear with my impatience.

I have seen that more than one of the "great" men, little known abroad though, who helped in stabbing me even long before the final stabbing was accepted by those whom I thought were greater men....that remark excludes you and Jack... and perhaps Pat, but no others there.....have succeeded in increasing their salaries more than skyhigh once more by forcing or luring the small authorities to give them the title distinguished....but it is evidently meant for crooks rather than for those who have worked as well and diligently and successfully as some of the Museum people and become known considerably more widely than others in the place. If I sat down and wrote in my own language I could probably compose books like those of Lord Snow about what I observed in the place, even after the warning by Eilif, which nevertheless came too late because we were then already in the American trap that we had avoided earlier and trusted the words of those who called themselves our friends...again this is not meant to you. But no more acid this time, since you know it all since long ago and have other things to do than to try to act as good old Edmund Burke.

I am presently trying to complete a short article about the genomic classification of the wheatgrass genera for a Festschrift for my good old friend Hans Stubbe that will be published by Biologisches Zentralblatt. It is a little difficult to keep it short, because the material could fill a book for what there is no publisher in America at least, because this is heresy as so much of what we have done here, but I believe it will become an interesting little paper. And I am waiting for reprints of a paper printed in Portugal in a Festschrift for Fernandes relating our studies of the Californian plants usually called Pleuropogon, which is, as all good phytogeographers know, an arctic endemic genus of Glycerieae, whereas the Californian plants so named even by Benson and Stebbins belong to the ignored genus Lophochlaena of the Meliceae. A couple of the meaningless taxa that Benson and Agnes Chase described as species and never have actually been anything more than ephemeral variations of no systematic importance, have even been placed on the list of endangered species, which certainly is a more serious matter for botany as a whole than that it should be filled with taxa that are not or that can only be ephemeral because they are not reproductively isolated and thus never endangered...as the little dart fish that was being used in a political manner in Tennessee. That reminded me of the constant juridical discussions in America of scientific problems as for instance evolution!

We are having a cool May and much trouble with insects and other pests. But still our tomatoes have already started to flower and the garden looks great. You ought to visit us again some time and enjoy it in the summer.

All the best to you both from us all here, Áskell

Thanks for reminding the Dept. of my address so they stop returning my mail, to label, postage, being insignificant who knows, with the remark that the addressee is unknown, as they all are abroad and over here, why not say plainly the truth that this insignificant devil has been stabbed and finished and forgotten? But I have since observed that Listen has done the same. And many may never tell of the returns.

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

SAN ... PM
22 MAY
1981
951

US
Domestic Rate

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

May 21, 1981.

Dear Bill: - Merrill misled me to believe that Rafinesque was the author of Psycrophila, which was so spelled by DeCandolle and not as Bercht. & Presl did...should that spelling not be kept when they lifted the DC section to a genus? The DC type is evidently from the Falklands, but I cannot see that his description could not fit well for leptosepala also, so go ahead There are, however, no other chromosome informations for the group than for that taxon, so the basic number is likely 8 as so often in Ranunculaceae...or better Helleboraceae.

What is this ASC Newsletter, I do not know it or should I? Perhaps you sent them by mistake a copy of your acronyms when you meant to send them the COLO management information, even I have done such things when in a hurry...and am still blushing. But even Mason could have made a mistake, though unlikely, but you will find out what went wrong...and get it into Taxon!

Have you gotten Meusel? And corrected the Hultén date f.AAR?

San José, June 1, 1981.

Dear Bill:

Many thanks for a card and a letter and especially for the cheque for Meusel, which I start today to spend on the dentist. And for the information on the ASC etc. There is, I believe, another volume of Meusel already and the third should be on its way, though he works very slowly and will slow down more when he now has retired, and the last one will probably be most valuable if it includes all the bibliography, also what he omitted in the first volume, which was not so little. And the work complements Hultén nicely because it brings in material Hultén selected to ignore. Thanks also for the new Acrolist.

I am slowly listing the chromosome numbers of all the Manitoba flora that we planned to publish as a book but now feel is wiser to get into Taxon before we decide to move to a better world and much warmer I am sure, because if we try to use the spiritual connections to get the manuscript over the border the fire in the place may scorce it too much for the printer. Because of this, we want to use the same modern nomenclature you are using, so if you have something new on Saxifraga, to mention one "genus" that is only a preliminary complex, I hope you permit me to utilize it.

One point I had overlooked: In the taxonomy you have been accepting, it is a mistake to accept Aster alpinus var. vierhapperi as Cronquist without concepts did, since this certainly is a good major geographical race in the European sense, as Onno also realized when describing his ssp. vierhapperi. I believe I must have made a note on that as a species this is A. culminis A. Nelson sometimes after having heard you say so?

Could you help me find the reference and description etc. for Aster sg. Modestus? And where does Aster pauciflorus Nutt. of the NW America actually belong in that system, whoever may have made it? I found it in Amer. J. Bot. in articles by Semple etc., among descriptions or transfers to Lasallea, but there are no author names.

Our Acer Negundo L. ssp. interius (Britt.) Löve & Löve was published in Bull. Torry Bot. Club 81:33 (1954). As far as I know, var. violaceus seems to be a form occurring within the boundaries of both races so it may not be worth much, but ssp. aceroides of Negundo aceroides is an eastern tree that does not get to or over the prairies.

Hordeum glaucum is of course a Critesion, which we are transferring, in press, your observation on Mertensia is similar to my opinions, though we still do not know if Stenhammaria and Mertensia s.str. actually can cross because nobody seems to have tried. But your lack of European floras is the cause for your question about the splits from the collective Prunus, because that you will find not only in our 1961 chromosome list and the Slovenian atlas, but also in Janchen, the Romanian and Bulgarian and Chech floras, and many more, though the British and Germans keep to the far past collective, as always.

I am astonished about Vera, though I have long observed that she is very promiscuous. That is not a reason for not employing her at the Mountain Station, so I would recommend her, but that is only a formality, because Pat will do just that as thanks for all help, and I am sure she will do the work well though hardly enjoy living there during winters. But try to get out here some time for a long chat, and bring Siplivinsky with you!

All the best to you all from us all here in the long cool spring.

As ever,

San José, June 15, 1981.

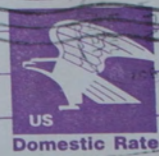
Dear Bill:

Your card of June 9 shocked me, because as far as my memory and notes go, the Siberian Rumex arrived in early September 1979 and I returned them at the end of that very same month. However, I had some discussion with the postal autorities as to the possibility to return the package as library material, since they said they had never heard about this or anything similar, but that could hardly be a reason that the package may have gone astray? Are you sure you did not receive it? I went today down to the post office but found that the one who received it from me is there no more, and I was told that much too long time had gone since I delivered it for them to find anything out but the fact that your letter or card indicates, that it is not yet in Boulder. Perhaps it may come nevertheless....I received last week a letter and a package with my Chinese-Icelandic nephew from Iceland, in which another nephew, who is a university librarian in Reykjavík, told me that he had sent to me per ordinary mail on December 12 a large package of books from the Academy! I thought of writing to him over the weekend, but for some reason I did not do it... and today the package arrived, clearly stamped in Reykjavík on December 12 1980, and then again less clearly as to the U.S. place two weeks later, or between Xmas and New Year! Where it has rested in the meantime....I cannot guess, but it could hardly have been here and affected by my visit to the post office to ask for the Boulder package, because the mailman must have been on his way when I was there in the morning....but one never knows, and perhaps we may hope that something still happens with the Russian Rumex, if somebody here has not some special interest in them because of their origin? But I am sorry that I was not wise enough or suspicious enough to send the package as registered or insured mail and to send the museum receipts separately...one always learns so I will do this next time. Although I had come to the conclusion on basis of the morphology that the species is most likely the tetraploid narrowleaved R. gracilescens from the more southern American mountains, a representative of the Acetosella group I believe, as is ~~KXXX~~ R. paucifolius, and so much alike R. tenuifolius that if the specimens had come from Europe I would not have hesitated to identify it so, this must remain a guess until we or others have succeeded in cultivating it and crossing it with other taxa of the group..... and that may take years or generations in a world with a diminishing number of botanists and non-chemical cytologists.

As to the <u>Critesion</u>, the evidence seems to support the idea that the genus and its genome comprises everything within the large subgenus Hordeastrum, whereas the genome of sg. Hordeum, or H. vulgare and its variations, differs drastically and cannot be crossed with these taxa. Some of the species you mention are already transferred in Taxon, others are in press and still others will get published next year, provided that I am above ground. But if I am not, you are free from completing this transfer, though I doubt that you are in that hurry that you will do it as long as I am waiting with my action! Soják would have done it, however, that is what he did with the material that he does not seem to want to send to me despite of several letters...he knows that Holub told me!

Have you seen Czerepanov's new (1981) Plantae vasculares URSS, a checklist of the conservative side with all synonyms, a good review in a single volume just as we ought to have for the American flora? - I have found Belaeva's initials and seen a reference to her and Siplivinsky's two Siberian papers that I have not seen in reprint...they are in Bot Zhurn. 1976 and 1977 I believe. And we have just received some few reprints of our Lophochlaena paper from Bol. Broteriana 1980.

All the best,



NATIONAL
CHILDREN'S
DENTAL HEALTH WEEK

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

June 30, 1981.

Dear Bill:

Many thanks for a good card that shows you are at last moving into usual summer observations even at Grand Mesa. This I envy you. I am just completing a paper on the genera of the wheatgrasses for Biol. Zentralbl. and the 74th Chr. list with our installment on Manitoba dicots...two more to come soon.

The Packera manuscript is fine as it stands, though perhaps the mentioning of the negative remarks on your former action could be dropped...that is up to you; and do not forget my ´ on the Á. and of course the ¨! I wonder if the paper is not best printed in America, but you decide and I agree.

Wish you were here for a certain discussion..etc.

All the best to Sammy from us both...and to you!

Áskell

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

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US
Domestic Rate

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

July 13, 1981.

Dear Bill:
Phytologia for the Packera paper is fine with me. Reprints? I believe G. drummondii (Hook.) L.& L. (in press) is a good Colorado species, and probably also G. kingii and perhaps "Silene" scouleri, which then need to be transferred, though they may be identical with some Siberian-Canadian taxa. As far as I remember, I identified the Colorado apetala as G. apetala ssp. uralensis. I do not remember the details of the separation of the Hieracium subgenera, but believe you have Peters large paper in Engler where there is a good key. And although there are eeveral other characters, I usually separate Arabis and Boechera simply on that Arabis has sessile basal leaves and is coarse and matted, and Boechera has petioled leaves and is caesp

Could you help me locate the exact reference to the description etc. of Aster subg. Modestus?

Go ahead with the description of the new American genus your discussants do not agree upon..it is better to separate than to unite what may be distinct...

All the best, [signature]

San José, July 20, 1980.

Dear Bill:

Many thanks for the good card, and you apparently had a good time on the Canaries. Hope the long trip to Nepal also will be rewarding and give you the satisfaction you are after, without making you so tired that you cannot do what you do best and want most, we are always running after the rainbow even when we realize that its home end is equally restful and nice.

The selection of _Cyrtorhyncha_ rather than _Halerpestes_ in 1961 was simply based on Benson in the Am. J. Bot. 27:805-806, since he recognized, correctly from the point of view he had, subg. _Cyrtorhyncha_ as the home of the four sections _Eucyrtorhyncha_, _Halodes_ (Halerpestes Greene), _Acteranthes_ (genus acc.to Greene), and _Pseudaphanastrum_ (Kumlienia Greene), all monotypic but _Halerpestes_, Since the chromosome morphology, which is a very important major character for generic division in Ranunculaceae s.str., is very similar in all four groups, there was, and still is, no reason to doubt that these "genera" are biologically congeneric, probably only as sections as Benson suggested, whereas the group he calls subgenus is as distinct as his other subgenera, and so we felt it and they would be better split out at that level. And since _Cyrtorhyncha_ is by far the oldest, we had no other choice irrespective of the fact that before we knew only _Halerpestes_ which had even been used by some Scandinavians. Some of the "many" as you say _Halerpestes_ from Asia seem to be misunderstood _Oxygraphys_, another matter that it would take too much space to review in a letter.

The price of Meusel is MDN 295.- , which according to Cramer should be "at least $150". Two friends have seen it and wanted to bid in it, higher, after I told you about it, but I promised it to you and I never break a promise, least of all for some personal gains, so it waits until you take it or relieve me of the promise. I know that few would find it as useful as you will and am sorry that I cannot afford the generosity any more that I sometimes showed at Boulder...but that may change again in your favor?

I know you will be careful in the Nepal mountains, as you always are in the Rockies, but this time you need not think about height but much more about diseases that may lure in every tree and certainly in every cottage and hotel, because the Indian poor people are not known for their cleanliness. A friend of ours, an agronomist who travels much in India and eastern Asia for his firm, stayed in a hospital here for over three months last spring after a very short Himalayan visit, to regions where he had been often before, and he is still very weak and not quite over the ordeal, though he is a good deal younger than we are and had been properly inoculated, or so he was told. But enjoy the vegetation and avoid the leeches falling from the bushes and look at the views rather than on the flora, which nobody really knows properly and nobody can learn in a couple of months' time. Perhaps Hooker and Bentham were right in naming everything very collectively as a first stage, they could not know that later generations of Indians would continue to sleep and that only a few really good and critical foreigners would continue where they had to end their listing of this enormous flora. If you should happen to come to the Department of Medicinal Plants of the Royal Botanical Garden at Godawari, Lalitpur, Nepal, give my best regards to Dr. S. B. Malla and his colleagues, they are chromosome cojnters, among the good ones.

Best regards also to Sammy, and all the best for the trip from us all,

As ever, Askell

San José, July 21, 1980.

Dear Bill:

The same mailman who brought your new card had taken my letter from yesterday, but since the problems you mention interest me and ought to be attended to before you allow Nepal to take all your interests, I must write at once.

I suppose you typed Streptopus for Smilacina, when you discussed racemosa var. amplexicaulis? Such happens in the best of families, not least mine. The Linnaean species S. racemosa was described from Virginia and Canada, whereas Nuttall described the western and to me very distinct S. amplexicaulis from the west, I do not know the type locality and am too lazy to look it up now, but as far as my memory goes, they are nowhere sympatric. In addition to morphological and geographical differences, they clearly have strong reproductive barriers as indicated by the fact, which we found long ago and I believe others have later confirmed, that S. racemosa is an octoploid with 72 chromosomes, whereas S. amplexicaulis is a tetraploid with 36 only. I do not think any more papers are needed to state this, only the silent acceptance of the facts in a good flora written by somebody who dares to deviate from the illogical opinions of "great" botanists who live with good facilities and high salaries that do not make great minds out of zeroes. So go ahead for your list and next flora edition, as in so many similar cases that the past knew but the period of "greatness" that was actually smallness and unjustice to those who dared ignored or forced in the wrong direction.

After publishing our Mt. Washington paper in 1966, we continued to look at the Colorado Streptopus, alone and with Sandy Shellworth, who unfortunately gave up in the middle, as promising students frequently do despite plenty of encouragement when the facilities are minimal. Fassett (1935) had claimed that his var., our ssp. americanus occurs in "Alaska to Washington, and in the mountains to Arizona and New Mexico", and we knew it to be tetraploid with 32 chromosomes. Wiens & Halleck (1962) had reported 16 chromosomes from Colorado, and that we soon could confirm. But we were unable to find a single Colorado population among hundreds sampled that could be identified with ssp. americanus, and this I see you confirm. I hope the Herbarium got Sandy's collections, at least I remember instructing her and Kapoor to give them to you when she quit. They all certainly belonged to what Fassett called var. chalazatus, with the very correct but perhaps not complete description: "var. americanum simulans; foliis sine dentibus, subtus cum copiosis minutis papillis." Type from Wallawa Lake, Oregon, June 15, 1933, H. P. Hansen, No. 1100. Herb. Wisc. This is one of many species that I was going to describe when I was stabbed, perhaps we two ought to do this together, if you risk nothing? I am in no doubt that it is a good species from the western mountains from Alaska south, very distinct from the practically circumpolar but disjunct S. amplexicaulis with its several subspecies or morphologically distinct interfertile geographical races, perhaps the western diploid is even the ancestor from the Tertiary of the others? It certainly is not only a subspecies or variety in the correct definition of the terms, neither is that state correct for Smilacina amplexicaulis Nutt.

I am typing the manuscript of a paper on our extensive Montreal studies of various hybrids of the Triticeae of boreal and austral regions mainly in order to get the correct place of what the New Zealanders have been calling Agropyron and Cockaynea, two sections of genomically defined Elymus s.str. Then I hope to find energy to compile a review of the genomically defined genera of the Triticeae, a work for the future revolution that never may reach those who do not want any corrections of past mistakes, or who regard taxonomy as a sport with no meaning as the great Stebbins told me recently...why do such people confuse others by writing about what they do not understand, or do botanists here become great by speaking only nonsence loudly enough and with authority that is not? All the best, Löve

San José, July 25, 1980.

Dear Bill:

Only a page to acknowledge your card about Ranunculus "natans", in case you have not found your copy of Kapoor & Löve about it. We only did one of our many corrections of old American mistakes, or rather continued what Russian authors ignored by those who think they are great botanists had already done. If you look up Flora SSSR VII, you will find that Ovczinnikov points out that natans is an endemic Altaian plant, and since later authors had found it to be a diploid whereas the Rocky Mountain plant is tetraploid, we had to follow the logical way of correcting the name of the latter. I doubt that it is anything more than a subspecies, as are so many of the socalled arctic representatives in the Rocky Mountain flora, though that may be discussed if more evidence becomes available, for instance hybridization followed by a critical cytological evaluation based on genome analysis. A case comparable to Ranunculus grayii contra gelidus that even Hultén seemed to misunderstand. But Ranunculus natans has never been met with in the Rockies, whatever the "great" small men without courage have said or say, so their opinion you may safely ignore.

Nice to hear that you got Hooker's Himalayan journal, though I tend to believe that such an old book by a man with no critical concepts, only wealth and arrogance, will help you only little. There are much more modern works that could be of use to you, at least efter your return, for instance Hara's Flora of Eastern Himalaya, of which I have copies of the first two reports I believe, and especially Kitamura's Flora of Nepal in a series edited by Kihara ~~in a series~~ about the Japanese expeditions to look for relatives of wheats years ago; I have copies of that too, but doubt that you will find them in many places on this continent because they may be regarded as contributions by plant breeders only, which they are not.

When you return, you ought to get a copy of the new book on a Delicate Arrangement, which unravels the fraud that transferred the honor of having first gotten the idea about natural selection, which still dominates more than it should, from the young and poor Wallace to the rich and more established Darwin...by aid of the also rich and thus dominating Hooker and Lyall. The same criticism of a perpetual tendency to safe one's own "honor" by taking that of others (I have a certain experience in this field so may perhaps say a word about the method?) was earlier made by Darlington in his excellent little book about Darwin's place in history that costed him the title Sir which no biologist was more qualified for than Darlington...though he also had strengthened genetics and cytology that are contrary to the Darwinian and neo-Darwinian ideas that still must dominate the Englishspeaking world...again I know how those who want to dominate think of those who do not believe what they believe. But no more acid about that, hope you will read the book on your return, it ought to be given to every young biologist as a warning and a teaching of what honorable scientists should not do to get their name into the sky at the cost of greater but less rich men.

All the best for the travels, and be careful with your back and with what you eat and drink, and remember that you are no more 20 so your heart cannot take too strenuous climbing even in the Rockies. And sleep moderately between collections and return home stronger and more vital and richer in ideas than ever!

Áskell

San José, August 29, 1979.

Dear Bill:

Thanks for the card and the papers for the Rumex package; the package itself apparently needs still some time to get over the mountains. I hope the post office does not resist my trying to return it as library rate since I have no formal label to glue to it, as when I send things back to Mason Hale, but I suppose you have no way out of that dilemma, if it should materialize? It will take some time before the sheets get from me after they have come, because I am sure it will not be too easy to identify them with full certainty except by aid of a microscope and by comparing them in detail as to the Rumex characteristics, which Yurtsev did not even mention in his much too short descriptions. We will see...perhaps you have already compared them with narrow-leaved R. paucifolius from the Rockies?

I hope others will also become annoyed with this epidemy of typification of material described before the method was even invented, it is almost as when certain powers created new laws to get to socalled war criminals after the war! Jeffrey seems to be one of those British botanists who think socalled lectotypification is the solution of all ills...but if it ever is, then it must at least be done with the greatest care and knowledge...which he unfortunately does lack. Not only in the cases in Taxon, but also in some cases in the Bot. Journ. Linn. Soc. in connection with Flora Europaea, and such ways of working lead only to confusion. The type method itself is a fine idea, but only if used in its strict sense, when the author himself has designated the specimen he used for his description, and then only a single specimen, because as soon as there are two, one risks that something could become mixed into it, except if one has pressed several specimens from a single cultivated plant. Perhaps somebody ought to propose to the next Congress that a committee be formed by which all such lectotypification ought to be authorized before published if it is to be accepted at all, or perhaps it would be simpler and wiser to rule out all such typification after the fact, to avoid the tremendous confusion that I see creeping in through these holes, especially when old and established names are being affected? Otherwise this may soon lead to a revolution against the method as a whole, a kind of throwing the child out with the bathwater?

In the present case of Jeffrey, I am not familiar with his Cacalia example and not enough interested to spend a day or two in checking it. In the Matricaria case, however, I doubt that his reasoning is logical and his background knowledge sufficient. As far as I see it, he ignores the fact, a very important fact indeed, that the splitting out of Chamomilla by De Candolle 1837 was in fact an effective typification of Matricaria, which then automatically became restricted to the part of the collective genus which later was called Tripleurospermum by Schultz-Bip. As far as I understand, this was overlooked by Pobedimova, when she typified Matricaria with M. recutita L., which is correct for the later Linnaean name M. chamomilla, the type of Chamomilla DC. and thus the basic element, or lectotype if you want, for that genus...and Jeffrey agrees with her. I believe Rauschert, one of the sharpest German nomenclaturists, was right in concluding that the DeCandolle decision must be upheld and Tripleurospermum abandoned in favor of Matricaria so restricted, and then typified with M. inodora L. This may perhaps be said not to be direct typification but rather a strict following of the rule for priority, even for generic division, cf. Code art. 52, since Linnaeus, naturally, never designated a type for his genera, and Hitchcock & Green can be ignored in this case. And so should Jeffrey, but not Rauschert

The case of Filaginella, which you mentioned specially, is similar, since Opiz was in his full right when he separated G. uliginosum under this new generic name, but that automatically restricted Gnaphalium L. to the other species, some of which were later separated from it also. Therefore, when Britton and Brown selected as a type (no more arbitrarily than does Jeffrey himself) of Gnaphalium the species luteo-album, they were not in error, and I believe their selection ought to be followed and not that by Hitchcock & Green, who did not know that they were ignoring the separation of Filaginella uliginosa; after all, they listed only Linnaean genera, though they ought to have taken into consideration their later legitimate splitting into more genera. I am sure that Stafleu or his reviewers were unaware of this or did not check it properly, and I am afraid that those ignorant of this and also unaware of Rauschert's paper or unable to read German and thus take notice of his good reasoning, may tend to accept what Jeffrey says in the protection of his elegant Kew address. But that will only contribute to unnecessary confusion as far as I understand it...though I am anxious to see what the Czechs say about it.

We have a similar case with Elymus-Leymus, which I discussed much with the stubborn but not logical but unfortunately then sick Bowden, who did not trust the reasoning of an immigrant, but agreed to ask Stafleu for a judgement. You will find that in his good paper in Can. Journ. Bot. 35, 1957, p. 991, where he says that Stafleu told him that this case , or Hochstetter's establishment of the genus Leymus, actually was the beginning of its typification and ruled out the choice of E. arenarius as the type of Elymus, which in turn was typified by Hitchcock & Green with E. sibiricus - they evidently were aware of the splitting done by Hochstetter, and later others. With other words, many of us either do not look closely enough or ignore the past, even those who are so fortunate to work with a large group of fine people as at Kew and with their immense library - perhaps after all the good mind is better and more necessary than a good institution, so that the best of facilities are worthless if there are no good scientists to attend to them, as in some large places even outside Kew?

You wanted the details on Minuartia and Lidia. I had them all in the original manuscript, but because of its length they were cut out at the insistance of the editor of Botaniska Notiser who felt a simple key was sufficient in a paper that was essentially on nomenclature. The Lidia species are L. arctica, L. biflòra, L. obtusiloba and L. yukonensis, no others as far as I know. Minuartia s.str., or Minuartia sect. Minuartia subsect. Minuartia series Minuartia, in the taxonomy of Mattfeld, includes only three annual Mediterranean members with 30 chromosomes, or the type M. dichotoma L., M. hamata (Hausskn.) Mattf. and M. sclerantha (Fisch. & Mey.)Thell. The best description of both genera and of the other splits and of several groups that I believe ought also to be given generic status, or accepted at that status when this has been done earlier and later ignored, is found in the excellent key by John McNeill 1962: Taxonomic studies in the Alsinoideae: I.- Generic and infra-generic groups. - Notes Roy. Bot. Garden Edinburgh 24: 79 - 155, which is too long for copying here without a xerox machine. Since it"filled with information, also about Arenaria s.~~str~~ latissimo, that might stimulate you to continue the splitting where we ended, perhaps by aid of chromosome information that I could easily dig up, I believe you might feel tempted to ask for a xerox copy of the entire paper, rather than to ask the author (Biosystematics Research Institute, Central Experimental Farm, Ottawa, Ont. K1A 0C6, Canada) for a possible reprint that he may not have available after so many years? Or you may want to look up the fine revision by Mattfeld, which even may be available at Boulder already?

Though letters are better than nothing, I miss being able to sit and discuss with you and get stimulated as before...though then we also kept too much distance as one risks doing when the essential interests are covered by trivialities of socalled duties in fields that are less important. But that is life. Best regards to Sammy also from us all...and of course to you,

as ever, Aske

San José, October 17, 1979.

Dear Bill:

Thanks for the note. Since it is easiest to copy the short Ikonnikov paper, I just did it and enclose it, hope you find it as interesting as you expected. There is a good deal more to do in socalled Arenaria of the old-fashioned Americans or in Arenaria and Minuartia of the Bentham crowd that still is followed by much too many who never have realized that this was not and is not the socalled evolutionary taxonomy that so many talk so loudly about but ignore, we did a little on resplitting it in connection with the Arctic Atlas, but I would be more than pleased if you felt the same and continued the work for the many American and Asiatic units. I believe time has long since come for American botanists to follow their elders rather than copy the never good British taxonomists...a few exceptions... since good old Michaux and many up to Rydberg were much better and more exact than Bentham and his crowd. But the selfelected popes on this continent will likely ridicule all critical work as they have done in the past and force the small minds through socalled "peer" reviews for grants and publications to leave their castle of medieval philosophy intact...and most will do just that to avoid being assassinated in one way or another. I hope to see that you defy them in this as you have done alone in several other cases, though still too few in my meaning. But even one at a time is much better than none.

There is some misunderstanding in your mentioning of Polygonum ramosissimum of Michaux being a synonym of P. patulum from Europe. As far as I remember, both are distinct morphologically and perhaps even ecologically though most real Polygonum has similar "ecology". The former is, as far as I still believe, an American plant that does not occur at all in Europe....and if some new evidence contradicts this, I would like to see it...and if it comes from an American specialist other than you, I would ~~dixsxxxd~~ discard it without blinking except if I could again compare material from both regions. Have you seen correctly determined P. patulum M. B., which often goes under the synonym P. kitaibelianum, you may even have it in some of your material from Sweden, where it is a weed.

When we saw Polygonum achoreum the first time in Winnipeg, we of course realized that it is a typical Polygonum, but it differs from anything we have seen elsewhere. We tried to look into its relationship with other taxa from other regions, but came to the conclusion that it must be an American endem. I am sorry that Hultén, and even Meusel (do you have his Vergleichende Chorologie?) ignore the group for the simple reason that they could not distinguish its many microspecies which are probably caused by an almost obligate inbreeding, but the former says the complex is worldwide and strongly influenced by culture. I do not hesitate to accept achoreum as a species in its own right, at least as long as nobody has proven it to be only a race of P. aviculare or some other related species, but I must admit that I do not know the Asiatic and South American variations...but it looks distinct from what I have seen in Europe from north to south, in Japan too, although there I may not have come to enough places to be allowed to judge.

Despite our isolation, somebody got the idea to elect me a fellow of the Explorers Club this summer, the second Icelander to become a fellow, the former was Vilhjálmur Stefánsson. There are a few fellows in Boulder, e.g. Cronic in geology, and you ought to be one too. When I get an opportunity to propose, should I?

All the best to Sammy and yourself from us here...we continue to miss you,

As ever,

P.S.: I am through with the Triticeae draft, it looks interesting but certainly will be unacceptable to the wise men here — but the Russians will accept it. I would send you my extra copy when I get it back from others.

San José, November 6, 1979.

Dear Bill:

Sorry to have let you wait for an answer, but your letter arrived when we got a visit by two old friends from New Zealand, who had been with us in Montreal 21 years ago, and since I and Henry Connor have been working on the same N. Z. grasses for some time, what some call Anthosachne and we now will name Elymus, and also the socalled Cockaynea, one of these few groups that are represented by one species in Tierra del Fuego and another, split into two but hardly more than subspecies though we still do not know for sure, in New Zealand, we had so much to talk about and discuss that the days went without other matters being done. And when they left, Lóa was busy setting up an exhibition of her silverwork at Los Gatos that opened last week, so perhaps you find me excused?

Yes, Eremogone is a nice group and very distinct. I was looking at it some few years ago in Boulder and mentioned it to you then, but you were busy with other important matters. I believe Eremogonastrum, which McNeill accepts also as a subgenus of Arenaria, is better placed as a section or subgenus of Eremogone, and the only chromosome number I know from the group, 44 for A. hookeri, confirms that, so if you have an opportunity to write about the need to accept Eremogone, why not include hookeri and also franklinii?

Before I forget to answer the questions, let me do it now: M. ucrainica belongs to section Eremogone. A. michauxii is the lectotype of sect. Sclerophylla and has 30 chromosomes according to Favarger. A. nuttallii with the number 36 also belongs to this section, which is clearly heterogenous, it was counted by Hartman. A. stricta is Alsinanthe stricta (Sw.) Rch., with 30 chromosomes, cf. our paper, A. caroliniana belongs to sect. Sclerophylla, but is cytologically unknown, but I cannot even find A. litorea, squarrosa or nardifolia as synonyms in McNeill's good papers so perhaps we should try to find some other names that he may know?

I believe that I had not started work on my revision of the cytotaxonomical side of Rothmaler's good critical volume of the excursion flora when I wrote last time? When that volume comes, hopefully next year, you will find it to be ages ahead of the taxonomy of Flora Europaea, since Englishmen tend to be more conservative than Germans and hang to even wrong ideas of their selected superiors long after the world has even ceased to laugh at them! And most Americans follow suit, I know hardly more than a single exception!

You ought to write to McNeill and ask him for copies of his two very good works on Arenaria-Minuartia 1962,1963, because you will find them to be a goldmine. If he does not have copies left, he is likely to be ready to xerox them for you, he is a very friendly Englishman, no, excuse me, Scotsman.

We hear that you have already got winter, here we remain in an Icelandic summer with moderate rains and reasonable temperature day and night. And we even still have some apples hanging on one tree, and are still harvesting tomatoes, although now they need some help indoors. And all trees remain green still...when do you come out to enjoy them and sit for long discussions of our many mutual interests?

All the best also to Sammy from us all,

As ever,

San José, February 11, 1980.

Dear Bill:

diffusa (Lam.) Soják

Thanks for the card. I do not know from where you have <u>Acosta</u> Soják, but even Kew Index I lists <u>Acosta</u> Adans., Fam. Pl. 2:117, 1763, though without any species, and it is evident that Jackson did not accept it as distinct from <u>Centaurea</u> L. We had, in 1961, in Bot. Not. and Opera Bot., distinguished this group as the genus Acrolophus Cass. 1827. It was, however, Holub, in Preslia 44: 215-218 (1972) who revived <u>Acosta</u> as the oldest name for this very homogeneous genus, and typified it with <u>Centaurea spinosa</u> L. because that seems to have been the only species included in it by Adanson, though the Kew people did not mention that. I have looked into this years ago and could confirm Holub's conclusion to my satisfaction at least, according to my notes. Holub does not mention any morphological reasons, neither did we, but he has made numerous transfers in 1972 (Folia Geobot. Phytotax. 7:313-316), 1973 (Preslia 45:142-146), and 1974 (Preslia 46:225-229). And I understand from a remark in his 1973 paper that Soják had "hurriedly published" several other transfers in 1972 in a nomenclatural paper in Čas. Národ. Muz., Sect. Natur., Praha, 140:127-134, but that paper I do not have and have not seen. It may help you also to look up the group in Flora Europaea Vol. 4, where Dostál (or rather Heywood who forced him to change his manuscript ideas to the collective genus!) lists this group as the sections <u>Paniculatae</u> (Hayek) Dostál and <u>Maculosae</u> (Hayek) Dostál, the latter of which is at least doubtful as far as my opinions go because it is too vaguely distinct and its species hybridize occasionally with the former. Dostál has recently told me in a letter that he plans to publish numerous chromosome counts for Centaurineae soon and then use the correct and restricted names that did not find acceptance in the eyes of the conservatives and their hopefully last British pope who has disappointed many others of those who worked on the Flora Europaea.

That is all I can say on this problem for the time being at least, but hope it suffices to get you onto the track. And if you need information about the Holub transfers and do not have a xerox copy as I think you may have, then you know how to get this from me.

Are you still interested in the Meusel Atlas? Do you know if the 5th and last volume of Flora Europaea has been published? Do you know of a review of the genera <u>Elymus</u> and <u>Hordeum</u> from South America that you could help us to copy? Are you going to Nepal?...in that case Stebbins may miss you if and when he goes to the Rockies during the summer to look at his first love, <u>Antennaria</u>, as he told me in a letter reviewing his very pleasant invitation to Washington to accept the National Medal of Science that he certainly should have gotten earlier as the doubtlessly leading botanist of the country...despite that his opinions sometimes clash with those of us and other European thinking geneticists. And do you still not see any light in our tunnel...perhaps Stebbins would want to help you, though I have not mentioned it to him, I keep quiet because I promised to allow you to solve the matter and continue to trust in you, with some patience at least. Do you know why one of our former students at Davis said some months ago that he had heard that we had been "vindicated" in Boulder...is that from you, or only his wish, I could not ask him more because others were present? We do not feel it so and have no reason to expect that the new chancellor will clean up the mess that has been going on since times immemorial, if he is not forced to.

We are enjoying our citrus fruit these weeks and hope you can share some of them with us. And the work on the new system for the Triticeae progresses slowly.

All the best to both of you from us all here, Åskell

San José, July 30, 1978.

Dear Bill:

I hope this will reach you before you leave for Siberia - we will follow you in our thoughts there, and envy you the opportunity to be with Malyshev, who has encouraged a good deal modern cytotaxonomical work in that region and who certainly is one of the top Soviet botanists. And he will enjoy being with one who also can discuss geographical problems with which very few are familiar.

Your Anemone question is well put, because the species in question belong to the section Eriocephalus which is geographically and morphologically at least as distinct as those that have already been separated from Anemone proper. Its cytology is not known to deviate more than the others, but I know of no successful hybridization, natural or experimental. Holub may of course be aware of this group but he is not likely to touch upon it because it is not represented in his area but in North and South America and, as A. hortensis, in the Mediterranean...it may perhaps not belong to the group although in gross morphology of the rootstock it seems to be similar. So why do you not look closer at them when you return from the Siberian visit?

No, the Rumex that Yurtsev described very scematically and promised long ago to send me samples of, is a dioecious thing at least related to Acetosella as far as I can see, and if my suspicion is right, it is identical with material from Colorado and other mountains though not inside your particular area. So you may not recognize it at sight, but believe that it would fit to R. graminifolius, as Hultén lured me into thinking when I made my map in 1943...his drawing of Alaskan material fits my suspicion and not his selected name. We will see, but I have hybridized the southern material that I believe is identical to this Alaskan and East Siberian plant and the diploid Acetosella, so they are at leats that related, and the hybrids showed certain cytological relationship too. I admit that R. densiflorus and its relatives are different from other species and groups of the genus in America and that they remind of certain Eurasian species, as Rechinger observed, but as he also found the group to differ from everything he knew from Eurasia and I know nothing alike it from the regions from which I have studied Rumex, I tend to believe that he was right in regarding it as a North American endemic complex. But how has it evolved and what is its closest relationship in Eurasia? You may perhaps find out now.

As to Poa rigens (Jalas has shown that there is no doubt that this name belongs to what Lindman later called P. alpigena) I could send you the good description from Hylander's flora when you return, if you do not already have it, but even the description and picture in Lid would be helpful. But since I have not seen the material that you mention, I am unable to have an opinion.

The F.N. checklist shows only what we knew, that those who dominate American botany at present are incompetent but do not realize it. It is the result of the fact that taxonomic botany has been suppressed by bad ecology and that the few who have a good taxonomical eye, as you, do not teach, and if anybody with such an eye or more knowledge comes from the outside, he is stabbed in the back and those who should have protected him turn chicken. American botany is in shambles... and the socalled endangered species list is scandalous and likely to hurt more, because many of the socalled species are nothing more than local races...and when the public discovers that, those biologists who did not tell the truth will be laughed at.

All the best to you both from us both...
and enjoy Siberia and Russia!

San José, July 22, 1981.

Dear Bill:

Thanks for a good corrected manuscript on Packera and two cards that just arrived. As far as I can see, there is nothing to remark on in the paper, so it ought to get finally typed and sent to Moldenke. For form's sake, of course, I could remark on the forced changes in my name of which you are painfully aware, and then mention that in the 11th line from the bottom on the first page I would write arctic-boreal with a small letter, as I am sure you also do regularly. I could also say that I feel that there is a comma missing in the 4th line from above on that same page, after Senecio vulgaris L., that at the end of the first long paragraph on p. 2 andthe could be separated, and that the traditional abbreviation of forma might perhaps seem to be simply f., not fma. which looks Chinese to me, especially if followed by a period which is against the grammatical rules of abbreviation that pedants love. But the paper is good and well composed, as could be expected from the authors, especially the first one.

As to Phragmites, asked about in your first card: I have packed Björk somewhere, but as far as I recall, his sterile plants were similar to the Boulder plants you mention, or to similar plants we found to be mixed into the population at Delta in Manitoba, where we made considerable studies of them, so perhaps the Boulder population is simply triploid? But we never counted their number.

Although I envy you the new Olivetti, we are reasonably pleased with our IBM and forced to keep it without replacement for reasons that you know and regret, in this country of yours that my son-in-law told me yesterday always gave all people the best opportunities and never discriminated against anybody. He was brought up by a reasonably well-to-be commander in the Navy so he can be excused, but he could not understand why people had a riot like in England in New York itself and asked for job in an area with an unemployment rate far above what the government tells. The schools indoctrinate the children through socalled sociology but forget to teach them about geography and history and the conditions of the world...then they might not believe in all the nonsense by the rich owners of newspapers and TV and radio about the dangerous and foolish socialists! But that I need not mention to a person of your leanings, and of your helpfulness, whom I have seen getting tears in the eyes when mentioning the poverty.

We are trying to publish our Manitoba observations in Taxon in the list, and so I have run into some possibilities to use your system and the new changes. I assume that you are publishing the ssp. interius of Negundo? But I need your help, as soon as you can do it, with the exact reference to the author and description of Aster sg. Modestus, which includes sibiricus, pauciflorus (which I believe we have evidence for reducing to ssp status of the former, and modestus in America and several others in Asia, because when we follow Semple in splitting the genus on basis of its combined cytology and morphology, this group must get its generic distinction...and I hope you do not protest if it will become Weberaster...the time has long since come when you get a name of some plants met with even in Colorado to honor your good and long work. But, please, soon...

You will get a copy of this division of the genus that then ends up as a reasonably well defened group, when that time comes. But in the meantime I must mention that you have overlooked, as I also did when sending you my earlier remarks on the Colorado list, that Boivin has moved Aster ptarmicoides to Solidago, as Solidago ptarmicoides (Nees) Boivin; I have not yet found where, but my reference is: B. Boivin, 1966: Énumeration des plantes du Canada IV. Naturaliste Canadien 93: 989 - 1063 (actually p. 1029)...He has collected the entire series under the same name in Provancheria No. 6 (1967), and I believe that you might still be able to get a copy from him, at Herbier Louis-Marie, Université Laval, Québec City.

And have you observed, in our 1961 chromosome list from Opera, that Iva xanthifolia is Cyclachaena xanthifolia (Nutt.) Fresen....as is also used in the Flora SSSR?

By the way: do you know or have some possibility to look up somewhere the reason for Gentiana puberula Pringle, which belongs to Pneumonanthe? I have seen it only in Boivin's flora and in his Flora of the prairies that you might also ask him about, but this name change may be unimportant pedanterie, as often happens...though it may be necessary.

That must be all for today...but I hope you can help me fast with the Aster reference. We have been sweltering in hot days here for the summer and now see the same is true for Colorado. Ingela is going for a visit to her father tomorrow so hopefully the heat will diminish somewhat also there.

All the best to you all from us all,

as ever,

Askell

I see that Bryological Times list you as a prospective lecturer at Sydney. I also give a joint paper there on taxonomy of polyploids, but will then be in Iceland. Hope you get to Australia — or otherwise enjoy some rest.

puberula (Mx) Greene

puberulenta Pringle
Rhodora 68:213
1966 Gil.

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

SAN JOSE, CA 951
PM
4 AUG
1981

NATIONAL
CHILDREN'S
DENTAL

Domestic Rate

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

August 3, 1981.

Dear Bill:

Your "Arabis" card of July 31: You may be right that A. nuttallii is a true Arabis, but the other odd Ealifornia taxa are not, as confirmed by the chr. n. 2n = 14 for breweri. A. virginica is actually Sibara virginica (L.)Rollins, 1941, Rhodora 43:481, which has 2n = 16 according to his later report mentioned in Munz' Supplement, whereas some other Sibara numbers seem to be only 14...are they safe? This is Rollins' treatment of western Arabis, which was too difficult for his then young and little trained mind, but it is the best we have yet...you should read it. I find no old name for this distinct group, so it seems to have been ignored, as was Boechera, even by the otherwise sharp Greene. Why not risk the wrath of the self-appointed wise men and describe it as a new genus, in order to open the eyes of the young generation for the problem? Good trip to Sydney...I go soon to Iceland.

Sincerely,

San José, August 3, 1981.

Dear Bill:

Thanks for three cards just received...the mail seems to be collected somewhere, perhaps in Denver, but fortunately there is somewhat more than is sent from Iceland, from where we recently got a book package that had been mailed by the University Library in the middle of December! We guess it was put in a new bag just after the Xmas bookmail left for America...and so few packages were sent later that it took half a year or more to get the bag filled! And I must also speed up so that you have this before you leave for Sydney, where I am sure you will enjoy meeting the new generations, though perhaps not to listen to them repeat all the mistakes you had spent a lifetime to correct...as usually!.... Good luck!

Aster subgenus Modesta is mentioned in the Semple ets. article in the Amer. Journ. Bot. you mentioned...but I do not know if they are responsible for it, or who may have defined it as a lower unit after DC. I use Aster for the time being also for this group, though it certainly is clearly distinct as far as I can see from the sibiricus complex, to which it belongs, but if you can help later, my plans for its future are done. Then you also will have time for the American Ligularia. And probably have received the Packera paper and reprints... hope you can reserve a few for my files. And also that you can help me get some copies of my and Jan Dvořák's abstract on the classification of polyploids from the abstract volume of the Congress, since Jan will stay for a year in Canberra.

You are right for Arabis s.str., when it comes to extra-American taxa, but I believe my simle key fits the American groups. All the taxa you listed as A rabis are correctly placed, as far as I know, though I also am in some doubt if virginica belongs here or somewhere else, and have the feeling that the California material is something distinct also, as so often in other groups, though I have only seen such material once or twice. When I first saw the American plants of the Boechera genus I was shocked to observe that Rollins evidently did not even react to their distinction...though after knowing better his floristic and taxonomic skill as compared to his falseness and skill in doing something rotten that I did not was for those brought up as mormons...as he did in the Arnold Arboretum case against even Sax and Merrill, nothing astonishes me...he is only a politician who found it possible to get paid for botany that he never understood....as Fassett once said to me when I wondered why he had been made the head of the Gray Herbarium and not Fassett himself. Good herbarium men are not good politicians, so they stay where the herbaria are smaller though the politics may be no less acid...as where you remain...but the great institutions are the losers.

The fruit flies are more clever than the American politicians who fight them, but though this is a farce seen from scientific points of view, as is so much in this "democratic" country, we hope the fight succeeds so that we at long last again can continue to enjoy our fruit rather than destroy it when the whims comes to these "environmental lawyers" and their schoolboy helpers, and the Jesuit governor. Perhaps the fly is democratic after all and not republican... I do not believe it makes difference between rich and poor and attacks only the latter as the minority government that was elected by a majority of very few voters seems to prefer.

All the best to you both from us all...and enjoy Australia!

Löve

P.S.: I forgot to tell you that I am leaving on August 19 for a month in Iceland, including a trip to the far NW where I first collected plants. It is arranged by my historian-nephew, who is specializing on the NW and the lighthouses, and will be funded by the coastguard. Hope they have dramamine!
L.

5780 Chandler Ct.,
San José, Calif. 95123.

SAN JOSE, CA 951
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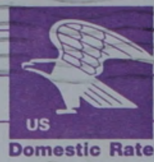
1981



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

Sept. 22, 1981.

Dear Bill:

Could you help me check if sg. or sect. Modesti is described in a paper by Mrs. Jones, 1980, in Brittonia 32, pp. 230-239...and then give me a copy of it?

I have just returned from a fine trip to Iceland, where the snow fell on the mountains Sept. 1, but otherwise the weather was reasonable. The Ferdaflóra was reprinted with corrections this summer, and a new edition, much revised and probably with small maps, is planned for 1982-84...but I must send a final manuscript of an English edition before the end of this year. There are two coastal species new, one European Juncus and one just collected and probably American Phippsia (Puccinellia), that I am trying to count cytologically for a final determination...we will see soon.

All the best, Áskell

San José, August 18, 1981.

Dear Bill:

Thanks for the card received just when you were leaving for Australia. Hope the Congress was satisfactory in every respect and that you returned rested despite all the events that you must have taken part in.

I am leaving for a month in Iceland early tomorrow, hopefully with visits even to the far northwest, where I started botanizing when 15, and where I went to publich and highschools in the next city, where my family had lived for centuries. It will be fun to see it all again...this time together with the next generation, one of whom is a historian specializing in the region wanting to pump me of what I may remember about people and places that were and are no longer. He has even reserved a coastguard ship to bring us to and from the Hornbjarg lighthouse, where we lived for two years, 1930 - 1932, since he is working on the history of the lighthouses. That is, if the weather is reasonable, because there is no harbor, only the open arctic coast, where no boat can get a shore when the northerly storms change the short and cool summer into severe winter overnight, sometimes in August. We will see...but I am sorry that you will not be with us, because I know you would have enjoyed it.

I have been busy before leaving with completing my conspectus of the wheatgrasses and their nomenclature. It is almost finished, though there certainly will be gaps because the Russian literature is not available in America, and my Russian friends are slow in looking up some of the details, of course, only you are always willing to do such things. I have sent a review paper on the principles to be followed in the classification to Biologisches Zentralblatt that asked for a contribution to a Festschrift to one of the leading geneticists of the past generation, now 80, but the main paper or book will wait somewhat and then perhaps go to Feddes, if I find the address to its present editor. But that waits until I return.

I have also been looking at Arabis, of course. You are right that the small California group is distinct ...it seems to have 14 chromosomes, though only one or a couple of species are known yet. The canadensis group certainly also is a genus of its own, again with 14 chromosomes...actually we were looking at it in Montreal, but left it to others and the future then. Perhaps the best thing to do would be to try to straighten out Boechers, which is simple, and then look at the other two groups...and perhaps even the European mess, which is less confusing, though its nomenclature may be difficult...Arabis recta Vill., which ought to be called A. auriculata, has 16 chromosomes and may be a small deviation within the type group of the genus, though I do not know if De Candolle can be trusted more than young Rollins, because when that part of his Conspectus was written, he was at the beginning of his work. Perhaps he had the same difficulty as Rollins to be educated by small schools and small men, though he did not believe later that everything he did as young must be regarded as the absolute truth...it is a pity that sometimes small professors let good students burn their ships by working on problems that require greater facilities...Porter and Constance did not realize their lack of size, though only the latter destroyed Berkeley by adding only his own students and yesmen...and even Gray herbarium by sending Rollins to there. But what Rollins could not see without chromosome understanding, I believe others can see clearly a long generation later...and base the work on what Rollins tried to do. So go ahead putting this all on the machine.

If you can locate the Aster subgenus citation that I got from the paper by the Canadians in a late number of A. J. B., but cannot find elsewhere, this would please me and make it possible to publish the new genus Weberaster. I must change the citation in the following chromosome list in early November if it is to get in the spring number, so perhaps you could contact Semple at the University of Waterloo and ask him for help? If not, I can do this later.

Aster engelmannii has 18 chromosomes, that is all I know about it now.

Otherwise nothing remarkable.

All the best,

Áskell

San José, October 13, 1981.

Dear Bill:

I hope you will try to excuse my tardiness in thanking you for the Almut paper, the reprints of the Packera paper, and the good card from Sept. 25. But when I returned from Iceland, my sister-in-law, my next brother's widow, came with me, so we have been busy helping her to see as much as possible of the land and its people. And the time left I have been using to continue my translation or rewriting in English of the Icelandic Excussionflora that must be ready for the printer at the end of this year for publication next spring. And for various smaller duties, of course.

Among the latter has been the completion of the Manitoba chromosome list for my Taxon list...then Manitoba will become fully known cytologically as only Iceland has been until now, so we can begin to demonstrate to those, who always know better what they actually do not even understand, that the same principles in evolution dominate in America as in Europe...even the frequency of polyploids, which in Manitoba differs significantly between the element in the south that invaded the Lake Agassiz bottom 5000 years ago and less, and the element that survived in the north and dispersed south into the vacuum left after the ice. In connection with this list there will be some name changes, some perhaps even affecting Colorado plants, and among them is the new genus Weberaster for old Aster modestus and its relatives, though we have not yet touched upon more than a single species of the genus. A. sibiricus is probably another that ought to have been transferred, as the other species listed under Radulini by Almut Jones, but perhaps somebody else will do that later? I am otherwise astonished that even people who do not have the slightest understanding of the evolutionary philosophy regarding basic chromosome numbers are starting to use this idea for reasonable revisions, though Almut's paper misses many points, and even the paper by Semple & Brouillet, who are cytologists but not taxonomists. Understanding of both approaches seems to be needed...and I and you may still be among the few here who have it? This paper will be printed in the second Taxon 1982.

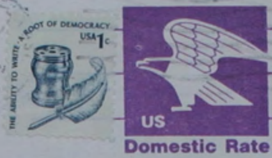
I wonder if you may perhaps have a copy of the Abstracts from the Sydney Congress in which there ought to be a joint paper by Jan Dvořák and me on polyploidy and classification? If so, would you be able to help me get two or three copies of it, with proper information about the title of the Abstracts and the page involved?

Otherwise nothing remarkable...or so much from Iceland and Scandinavia that it would fill too many pages for a letter. But what do you feel about the winter coming with snow in the mountains on September 1, and that October has already been so cold in Iceland this fall that it is by far coldest since 1870! Hardly the new Iceage, but the new coldwave that the Greenland ice indicated 1970.

We envy you the visit by Skvortsov, though it was short.

All the best,

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



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

7125 1956 Emil N Brunelson

November 7, 1981.

Dear Bill:

Your silence indicates that you are more than usually busy, but I hope you can find time to help me. I am trying to locate our vouchers of a supposed Parnassia glauca, that I believe may have been in the material I gave to the Herbarium in 1973: L.& L. 7399,7400,7401,7402, all from a collection made at Bonaventure River flats in 1957, and L.& L. S3937 from Piney, Manitoba, collected in 1954?, or another specimen from S. Manitoba, if any. Hope you have them and can verify their identity, or say that 7399 is something else.

I am in the middle of an English translation or rewriting in English of the Icelandic flora, due with the printer in Reykjavík before the end of the year. But have also been spending some time helping the editors at Stanford U. Press with the fine Benson's Cactus book. Otherwise all as usually.

All the best,

Áskell

San José, December 7, 1981

Dear Bill:

It shocks me to see from your good letter that you evidently have not received a letter and ~~axx~~ card from me thanking you for the fine Sydney abstracts and a couple of letters and cards. But perhaps the postal service will find them at Xmas?

It was interesting to read that you and Vladimir had come to the conclusion that the Alaskan Lycopodium and the Icelandic one are identical and that they represent a species in its own right, as Rothmaler maintained and Hultén refused to even look at. It probably was just spite when he felt he must make his own subspecific transfer based on the American taxon rather than to accept our view of the taxon as a circumpolar one...though we accepted his old and perhaps not always valid theory of arctic-alpine races? He claimed that this was correct, since the Alaskan-American plant could not be identical with the Scandinavian-Icelandic one...I am sure he believed this but never bothered looking closer, as happens so often when we feel sure, or in his case, coque-sure. It interested me still more that you and Vladimir came to the latter conclusion of Rothmaler, since a few months ago, when I was looking closer at my notes and little Icelandic material of Lycopodium, I could not avoid the feeling that there actually is no direct evidence in support of Hultén's view of this taxon as a race only. It may be that hybrids are formed and not recognized because we do not know what to look for, but it is no less likely that they never are formed because the taxa are reproductively isolated. Since it is our old principle that it is wiser to separate as species taxa that may be identical than to identify those that are most likely distinct and reproductively isolated, I came to the conclusion when I wrote the first pages of the English version of the Icelandic flora that it would be wise to accept the Rothmaler opinion and refer to our ssp. combination as a synonym only. We may not be right, but as long as we have no hybrid evidence and only vague cytological support of either side, I think the Rothmaler point of view ought to be accepted...he usually was one of the sharpest taxonomists I ever knew, and very rarely made serious misjudgements. So here we are again in the same boat, independently, and my impression of Vladimir as a sharp observer has increased still more.

By the way, in connection with the Icelandic and Manitoba material we looked into Linum, which in America has been a mess because Greene and others made the mistake to believe that much of the material here belonged to Cathartolinum. Now we know that this mainly European genus has x : 8, Linum s.str., annual, has x : 15, so has also the perennial mainly American Mesynium Rafin (we transfer rigidum and sulcatum to it, other taxa are waiting for you or others), but Linum perenne is Adenolinum with the basic number 9.

Could you tell me about the requirements of Phytologia, I may try to validate some Icelandic names there that are needed for the new flora.

As you imagined, Polunin cannot be worked with, and I knew it before and had experienced his bad morale already when he lured me to contribute to his arctic encyclopedia for which he was paid and never was completed. When I had done my revision of the phytogeography, he wanted me to change it into a book on conservation politics, and was greatly disappointed when I requested that I share the copyright. So it will never come. Most of his book is a direct translation of the Russian book on world vegetation by Wulff that has never come outside Russia, and the publisher is so tired of his ways that they were glad that I pulled out. More personally later.

Good that Dave Hawkins had better friends than I in Boulder who really helped him. John Packer is coming today from San Francisco so I leave for his train..

San José, December 20, 1981.

Dear Bill:

Many thanks for two letters that I had to let wait because I had to complete my Icelandic manuscript so that it could reach the publisher ~~XXXXX~~ before the end of the year, as I had promised. Now it is on its way and I am free, for the moment.

Thanks for the Phytologia informations. Since I still have to compose the short paper, I cannot send you the manuscript for getting it through the word processor just now, but am grateful for your offer which I will use as soon as everything is in order for it...the book will not be published until in the late spring in Reykjavík, so I suppose we still are not in a great hurry to get the validations printed...but that time comes sooner than one realizes though.

Yes, I agree also on Linum lewisii, which is being transferred at the species level in the genus Adenolinum in the 75th Taxon list, which was sent to the printer in early October. And I would continue to ignore the little wisdom and limited understanding of taxonomical principles by the Arabis "specialist", not least when he shows his complete selfishness and lack of understanding for the fact that his opinions in 1941 even then were laughed at by most Europeans, who long since had realized the distinction as genera of Arabis, Cardaminopsis, Turritis and other taxa that he placed in this much too collective "genus". It must be difficult for those who were placed on Mt. Olympus for wrong reasons, suddenly to discover that even those he regards as foreign fools disagree with him. And it will not be easier for him to swallow if you share my opinions even in evolutionary botany that are against his mormon upringning and ideas. I only wish that mormons were as honest as they try to tell you they are, because then he would not have prevented us from getting support in 1957 from the NSF to complete a critical chromosome atlas for the Gray area, and not have counteracted our NSF proposals on arctic-alpine botany or accepted to be one of the "judges" of our Smithsonian proposal that was used to support those who did not dare to face us with their accusations but preferred a much more dishonest way of trying to kill us, directly or indirectly. By the way, I have observed that at long last the crooks at Boulder have been replaced by two honest and well known scientists and administrators from the east, so do you not believe there has come time to burp the whistle that was swallowed and get others to help you do what then was your dream and hope? For instance Peter to whom I recently sent a short letter in support of a certain action for your sake, Albersheim whom I always remember as an honest and energetic man who has done fundamental discoveries in higher plant systematics without quite understanding their significance himself, Jack Ives whom I always trust as much as you and Peter, etc.?

When you feel there is time for the work on Boechera, I will do what I can to help you, of course, and also with other similar problems if you want me to, of course.

I wonder if you have been contacted yet by the senior editor Bill Carver of Stanford University Press? I have told him about your flora that I believe ought to be published only by a publisher of their size and quality...and that you are the only one who could then take up other larger works that interest him and others. And I mean it, as all I say in these fields.

All the best to both of you from us all, and a very Merry Xmas!

P.S.: I am reading the proofs of my review of the Genetic evolution of the wheatgrasses for the Jan-Febr. number of Bot.-Zentralblatt, which will irritate the conservatives. And am trying to make my complete bibliography at the request of the Icelandic Academy of Sciences and Letters. Would you like a copy to keep?
Á.

San José, December 26, 1981.

Dear Bill:

We hope you both and all had a pleasant Xmas despite the gloom over the future that we read about in the papers and especially in letters from our friends even in Scandinavia and especially in Iceland, where the winter is the coldest since 1770 when the Little Iceage dominated; they are afraid that the fish may move south and out of their reach as it did then, though their gloominess is more affected by the Polish situation that has closed for much of the important export...and Iceland cannot survive without export and considerable import and has never been able to keep up its high culture without outside support. And now some of my correspondents, even conservative people who have supported the American occupation, seem to begin to wonder if they may have helped to curtail the possibilities of their children and even seem to have started to think of other places, in the hope that that may not be too late already if the climate is getting down towards the next Iceage. I am sure it is the cold and the terrible news from Poland and even from the American depression that they say they already feel badly, and so the summer after the long winter may give them their optimism again, especially if the Polish question is settled without bloodshed. But even letters from Denmark, which also has bought its luxuries during the postwar years by selling its soul and land to a foreign army through generous loans that soon may have to be repaid, there is this same gloom...though again the severe winter may be the main cause. We can see the gloom coming also here where the poverty is clearly increasing rapidly in step with the increased wealth of those who have the upper hand in the fight for the bread...though hopefully there is more optimism east of the Rockies?

Thanks for your couple of pages from the chromosome paper of the great mormon at Harvard. I thought he might have sent it to me as a kind of courtesy, that is at least my way when I say something nasty about some colleagues, but perhaps he had some doubts or was afraid that I might react as he would have done and written him some nasty truths that he could not avoid recognizing as such, so he took the secure way because he knows that nobody reads what is written now in the Contributions. Not even the compiler at the Gray Herbarium who is responsible for parts of the Atlas , no Index of chromosome numbers, since neither this nor the 1979 paper on chromosome numbers of Cruciferae by the great cytologist and his energetic lady-helper are mentioned there. Therefore, since he sent me the 1979 paper but not the 1977 one, would it be possible to get your help in xeroxing the latter in extenso so that we can use the other chromosome numbers and take part in other wisdoms from the great man who forgot the main purpose of the institution his former western teacher got him to chair for much too many years...in the same humility as when he speaks about his far from better than normal thesis which only was a part of his then not too complete training in a field that he did not have any understanding for? I would appreciate your help, as always.

I also enclose two pages aimed for Phytologia that you hinted at you would help me produce properly. You might even help me to send it directly to Moldenke and ask him to tell me what I should pay him, too, though that may be too much to ask of somebody who has more than enough of good things to work with.

All the best to you both from us all here, who miss you more at this time of the year than else...though we always miss you a good deal, and no others from Boulder.

As ever, Áskell