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

San José, March 4, 1983.

Dear Bill:

I had begun to wonder if you had decided to forget me to avoid certain memories so I am glad to see that the long silence was caused only by work in other fields than we share, and probably in reveries regarding your and Cronquists travels in Russia...why they invite you both, extremes of American taxonomy, one good, the other far from progressive, I understand as little as you do; but am sure that nothing remarkable will come out of spending time there, though you will rest and enjoy travelling at the cost of the communists that Reagan sees as danger to americanism, whatever such nationalism means to the common man. We will see, perhaps you both will agree in bringing the revolution with you west again, though I am afraid that it will differ from that of our far past, because the news from there that I glean from Icelandic newspapers...which differ much from what is published here, naturally...indicate that they now live under fascism even more severe than did the poor people in Hitlerland in our youth. Such is politics and fanaticism, which seems to be on a rampage everywhere, not least against immigrants of every kind in this formerly so democratic country, and even in Sweden, which remains not only democratic, we are told, but also the most socialistic land on the earth. With capitalistic problems though, unemployment and inflation that may seem to ease some little under the influence of the new Palme government...Perhaps?

To go straight to your questions in the letter regarding Linum s.lat.:

a. Stigmas capitate; heterostylous ..... Adenolinum, x = 9, type A. perenne, perennial
b. Stigmas linear; homostylous ..... Linum, x = 15, type L. usitatissimum, annual

Cathartolinum Rchb. 1837 is based on Linum catharticum L., which is the single species of this European-West Siberian genus with x = 8 and its type species. Despite Greene and other Americans, this genus has nothing whatsoever to do with the American taxa that belong to the genus Mesynium Rafin. 1837, with x = 15, which ought to be typified by either M. mexicanum (H.B.K.) Rafin. or, less appropriately, M. texense Rafin., both validated when the genus was described. The Californian Linum digynum A. Gray, which Small also put in Cathartolinum, is something else and still without a separate generic name; it also has x = 8 according to Raven. Yes, types ought to be designated when you pick up such genera, provided that they are properly circumscribed and certainly not heterogenous so that later troubles with the typification can be avoided. That, however, requires additional space if the transfer is made in my Taxon column, so this ought to be your privilege when you continue the transfers, and will thus add to your list of achievements.

I have a couple of remarks on your third list. I do not know how serious you feel the first remark is, but I am inclined to claim that by splitting hopefully properly delimitated and widely distributed species of Astragalus that the rich and over-confident amateur Barneby has described for this genus (which I dare to doubt is correctly identified or circumscribed in America, based on what I read out of the basic numbers, so you may need to look into that also, now or later?) you may be going too far, because such splitting may violate our knowledge of that such variations are only more or less ephemeral pure genetical lines of more or less obligate autogams, which have no geographical or even evolutionary status... they are even less important that apomictic segregates, because they are less permanent. To call these genetically homogenous lines varieties may even be a violation of the biological definition of this category as a stable but minor geographical race restricted to a more or less local area, as, e.g., northern Scandinavia, Scotland, Iceland, southern Greenland, or Colorado or even only the southern Rockies. And they certainly are no subspecies, which is best defined as a major geographical race of considerable stability and with a sizeable regional distribution covering, e.g., eastern or western North America, or Alaska contra Labrador, the prairies, the Rockies from north to south, etc. I prefer, as most European botanists, not to give such pure lines any name, except as cultivars in some of our agricultural plants for practical reasons, because I cannot see any real need for more than to mention the fact that these species include more or less numerous genetically pure lines that may or may not differ slightly in some morphological and physiological characters, and rarely hybridize and give rise to new such homogenous lines after a short period of apparent heterogeneity. If you really feel a need for their geographically meaningless identification, why not leave the still unnecessary varieties of more or less biologically unskilled botanists of the past untouched? They will only confuse the user of the flora manual.

Another remark I feel I can make concerns Humulus lupulus and its variations in America. Although Mansfeld, R. 1959: Vorläufiges Verzeichnis landwirtschaftlich oder gärtnerish kultivierter Pflanzenarten. - Die Kulturpflanze, Beih. 2:1-659; maintains H. lupulus as species without racial subdivision, with H. americanum Nutt. and H. neomexicanum (Nelson & Cockerell) Rydberg as American synonyms, I believe there are good reasons to regard the American populations (they are repesentatives of the Eastern Asiatic-North American Tertiary complex) differ slightly but distinctly in a few characters from the Asiatic group from which the cultivated hops derive, and they are, naturally, interfertile as all disjunct races of this group. But if the western populations really differ from the eastern ones, which I doubt, then they are hardly more than simple varieties, in singularis, excuse me and the typewriter. We made the transfer of ssp. americanus (Nutt.) Löve & Löve in Taxon 31 (1982):121, so if you agree with me in this reasoning, then it may include two varieties, neither one needing a formal transfer, I believe. I must admit that I never bothered to look closer on the western populations, which I nevertheless sometimes felt looked as introduced, but we made considerable studies of the material in Manitoba and Quebec, mainly because we were interested in the sex mechanism and its variations described by some not too skilled cytologists, so they made the mistake in claiming differences in its complex mechanism. If I were writing your flora, I would refer to the western variety only as a synonym to the american subspecies, though I would not dare to challenge you if you believe it is a good variety. But no more.

I like your discussion of the American Crepis which naturally is Psilochenia, except C. elegans and C. nana, as you also point out. But also these two taxa, which have x = 7, are not the real Crepis, which I believe is correctly typified by C. biennis with x = 5. C. elegans has been placed in Youngia by Rydberg, but since that genus in its strict sense has x = 8, that is a mistake. Babcock, E. B. (1947: The genus Crepis. Part two.- U. of calif. Publ. Bot. Vol. 22:212-213) describes this small Asiatic-American 7-chromosome group as the section Ixeridopsis Babcock of the collective genus Crepis. I believe even this is a mistake caused by his peculiar generic concepts so this group would be better distinguished as a genus in its own right. Since the name he selected for it as a section is hardly an easy or beautiful name, and since I believe it is a misunderstanding to name it after the unrelated genus Ixeris, it would seem wiser to give it an entirely new name but refer for its validation to the description by Babcock (p.213) and typify it, as Babcock did with his section, by the species nana, and then transfer to it the following taxa:

Crepis corniculata Regel & Schmalh. ex Regel, 1881, Pl. Nov. Fedtsch., in
    Fedtsch., Reise Turkest. 18:54;
Crepis alaica H. Krasch., 1933, Acta Inst. Bot. Acad. Sci. URSS, Ser. I,1:182;
Crepis flexuosa (DC.) Benth. & Hook. f., 1873, Gen. Pl. 2:515, which was based on
    Barkhausia flexuosa DC., 1838, Prodr. 7:156;
Crepis naniforma Babcock, 1947:536;
Crepis lactea Lipsch., 1937, Feddes Repert. 42:159;
Crepis nana Richards., 1823, Bot. App. of Franklin, 1st Jour., ed. 1:746;
    ssp. nana
    ssp. ramosa Babcock, 1947:542;
Crepis elegans Hooker, 1834, Fl. Bor. Amer. 1:297.

Why not simply add these changes to your paper to make the biologically correct classification of American Crepis complete?

Otherwise nothing remarkable, except perhaps the weather you read about in the papers...we are far from the disaster areas that are either in localities of the very rich or the very poor, of course. I am slowly working on a paper to clarify certain points in the classification of Acetosella, which is being muddled up by silly and actually childishly composed socalled research by a Dutch morphologist, who clearly understands little about taxonomy and nothing about nomenclature and the selection of specific names...I am appalled to see that even in Europe taxonomy and phytogeography~~x~~ have deteriorated remarkably during my lifetime, so soon everybody even there will be like the American mormon educated botanical morons...but does it matters with all the dishonesty and lack of ethics and courage to demonstrate individual thinking and opinions different from those of the few who select themselves as some kind of a pope, without ever bothering to read and think about other ideas? And then stab those who dare. I hope I will succeed in writing this reasonably in a paper of a moderate length before many weeks go by...you will then tell me what you think, but there will hardly be time to send it to you for constructive criticism, because if I can make it for the deadline at the end of April (in Switzerland) then it will be printed in a Festschrift for Favarger.

Though I have been reading proofs of my English edition of the Flora of Iceland since in the summer, it still is with the printer, but this time I hope to be able to prevent the typesetter to keep his errors all the way, as he did for the third printing some few years ago...and that is in Icelandic. I found over 50 errors in the third proofs that I sent back recently, at least half of them left uncorrected...but this time I will not see what they do but let my younger brother, who teaches science and biology at the Teachers University in Reykjavík, go over it at least twice with the printer himself. And hope for the best...though I expect that when you open your copy, there will be some gaping mistake just on the page you look at first!

I hope you soon hear more from Stanford and that they then will want to see you here and discuss the planning of the work, so you will find an opportunity to visit us also, if you feel you want to be reminded of old friends long since forgotten. But that can hardly be until after the rains have ceased so you will not be plagued by the floods and mudslides or the snow in the mountains, in case you should drive west yourself.

All the best to Sammy and yourself from us all here near the big ocean.

As ever,

Askell

San José, March 17, 1983.

Dear Bill:

Many thanks for your fine letter...and again it does not astonish me that you find Rydberg's ideas to be better than those of the great American political botanists, because I have long realized that the education of the latter ~~are~~ far inferior to the Swedish gymnasium education of the former...who had considerably more botany even at that level and a good deal more training in logical thinking and basic philosophy. I only am at odds from where you got a similar training in both, but probably your European family taught you more of the classics than an ordinary New York boy now hears about for a lifetime..and then you may have received an unproportionately better part of common sense than most of those who have tried to dominate American botany by aid of variously seriously criminal conduct and all kinds of miscarriage of justice against those who are better in various ways have never got. That is in your favor. Yes, I have looked up what I can read about your Portulacaceae genera and trust that you soon will revive what the great little men have long tried to suffocate...why I do not understand. But this time I am unable to help you sharpen your cytological sword, since I can only find two unsafe numbers and then the 26 chromosomes characterizing your rediviva.

By the way, earlier in the week I got a letter from a young lady in Eugene, Oregon, asking for some advice on methods for a cytological study of some Arabis, so I took the opportunity and told her about your ideas and mine on that genus...and warned her discreetly, I hope, about not believing in any great prophets that may not be. Hope she contacts you so you could perhaps cooperate with her and do the taxonomy while she looks at the chromosomes. You know better what advice she needs most.

You are right, Askellia has not been used as far as I know, so you are welcome to flatter me with it for these nice American-Aasiatic plants. Dostál asked for a permission to use it for a Central European genus last year, but I could show him that that would have been a mistake, so he backed out.

I am sorry to hear that Bill Carver has been overruled, since he was so optimistic when I saw him last time, and the arrangement would have been so good for both parts. I have not yet asked him, but understood on his next man, whom I know well, a former professor in Latin from England who worked in Calgary before he came to Stanford, that one of our great colleagues at Stanford had objected...the one who once thought he should edit the North American Flora, despite of no imagination. By the way, have you seen in the last number of Taxon that the Soil people have revised their list somewhat and give it away to those who want it, as long as possible. So if you did not see this, react at once..though we know its quality and completeness.

I would like to be able to advice you on the printing, but since you do not want to go to Europe, and naturally not to those who make books too expensive to become rich on them fast...with little result. But Toronto University Press has printed American floras, the one on Alaska and its slope, and I suppose others, and is now working on the Alberta flora, so perhaps that could be a way out? There certainly will be a good solution as for all such problems, but it can take time.

Sorry that your Hawaii trip does not allow you to stop here, but wait patiently. Though we would like to have you alone for a week or more rather than to have somebody less interesting to us divide the visit...whenever you so feel.

All the best from the monsoon place,

Áskell

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

21 JUL 1983

Robert Morris Patriot

US Postage 13¢

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

San José, July 21, 1983.

Dear Bill:

Hope your sweltering weather does not interfere in your usual collection trips or in the work at the Museum. Perhaps you could find some help by contacting Margaret Goodhue, who now lives at her old house at 2490 Topaz Drive in Boulder and needs some work and help...she could tell you about Mary Kirk, whom she knew almost too well (for Mary) during her Boulder years that you seem to have had difficulties in finding evidence of.

I understand that my English Icelandic flora has been sent to you by slow mail so you may get it before the end of summer. But I am spending some time in the last revision of the wheatgrass revision for Feddes, though the manuscript is there.

All the best from comfortable California, where we had the heat before you...now it is in Europe, except Iceland...

We see that Indiana advertising for a geography-director, why?

All the best,

[signature]

San José, August 2, 1983.

Dear Bill:

Thanks for your July 26 letter, which arrived at 1.30 yesterday, but already at 10.15 the now so familiar voice had indirectly informed me that you had spoken to somebody connected with the crooks, since it said:Your Boulder friends seem to have forgotten the fate of Thomas Riha, warn them...and then slammed the phone on, so again I was unable to record them. Be careful if you do not want to hurt us again as last time when they stopped you, and I wonder if it would not be wisest to avoid the Boulder crooks until something has come out of the, hopeful, actions by Fosberg, whom they do not know about, yet? And why not discuss any actions first with Jack and Bill Carter and some others who are solid and honest around you, rather than warn those who still evidently are afraid that their sinister role may become known to their new superiors? If this is not simply the CIA that greeted me in Boulder already the first week I was there...and constantly after that...though I believe the Smithsonian action was connected with the Galilei type treatment of one who dared to do more than the permitted cytological observations and drew conclusions that are contrary to those of the dominant creationists...did they not kill the BSCS? I know you do not rush into anything that may hurt us more...but Margaret ought to be contacted, despite the fact that you correctly criticized her thesis that I then helped her get some form to...she will not bite you if you do not bite first, and she will do whatever needed to help us, if given an opportunity. And according to Favarger, she is a fine worker though her botanical background is limited, so I hope you can help her find a proper place to make some living.

I am sending the publication list as promised, it is up to date as to what is formally in the press, though, naturally, I do not have the pages of some of the last contributions, among them the Triticeae paper the manuscript of which is 185 pages. And am sorry for the late sending of the Lophochlaena paper...that genus of Meliceae confirms what you do with the other group...of course it is similar to Triticeae in genomic divisions, though too little has been done with it yet.

Linum and Antennaria certainly will be problems of the past when you again find time to show them interest...and I am not astonished that Adenolinum grandiflorum has come in with foolish seedings by socalled ecologists who rarely know what they are risking in their well-meaning reseeding of more or less large areas, without the slightest thoughts of taxonomy or genetics. But that is American botany today, and other points of view will be suppressed, as not only my experience but also that of Léon Croizat vouches for...he is now being reinstated after death, since now the Harvard crooks may only be nervous for his ghost.

I am sure that Rydberg...and now you...was right in separating from Rubus Oreobatum and Rubacer...and Juzepczuk did the same with the latter in 1941 in the Flora SSSR. There have been considerable crossing and genetical experiments with these groups, not least in Sweden and Finland, so there is no doubt that also Chamaemorus and Cylactis are good genera that ought to be revived, and perhaps also Ideobatus (Focke), whereas Rubus s.str. or Eubatus Focke, remains untouched and typified with R. caesius, the blue "hallon" that we collected and ate with pleasure around Åhus years ago...and you probably have never even seen? This entire relationship is worth looking closer at...even Fragaria and Comarum are able to cross, though they do not directly mix. I believe it is a case similar to the Triticeae.

I am pleased with your III names list and transfers, though I can see that it is only a step in the correct direction as far as your Astragalus goes, because this complex belongs to the distinctly American, south of the glacial border, group of taxa with x = 12 rather than 8 chromosomes as basic number...they certainly have very little to do with the Eurasiatic typical genus and ought probably to be split out, as I believe Rydberg attempted sometimes? That may be your next step?

I have a proposal for INSTAAR that those concerned will not like since it is their pleasure and bread to be there and do little: simply sell the mountain station to people interested in vacations...and cease to fiddle around with socalled alpine and arctic studies that others seem to do better...and then give the funds to the Museum for more substantial research. But who likes wise proposals in Boulder? But we are pleased to read that the herbarium problems are becoming settled, at long last.

These days we are having some little too much heat, but nice nights, though this is nothing compared with what you on the prairies can complain about and also others in Europe that swelters more than ever after a cold winter. But this is just as we were long since told had been typical during the iceage itself, with long and cold winter and short ans very hot summers....Černohorský, who lost his wife in the spring, tells me that he does not remember suchx a hot and dry summer in Praha that forces him to water his fine garden every day...as we do too.

In our case, take one thing at a time and be aware of that many may feel threatened because of a bad conscience, especially in Boulder. But I still believe that you are not alone as our serious friend so you might have some good advisers closer to you than you realize. At least Jack and Bill Carter, and probably even the district attorney Hunter, whom we experienced as a fine fellow from the very beginning. He might give you advice anonymously...and advise you on the methods of the lawyers in America to do things based on contingency fees? Of course, you are permitted to let him and Bill Carter and others you so feel see the dossier now, but Jack told me last year that he had gotten a copy of it from Pat, though I doubt he has taken time to read it from all his own troubles. Just hints, I know you are careful and a friend not only in need but solidly as nobody else on this peculiar continent where even scientists are hunted for their urge of drawing correct conclusions that disprove the "facts" of the establishment that evidently selects itself in a rather peculiar way, cf. what Constance described in Taxon in connection with the hiring of the little competent mormon to the Gray...

I am taking this to the mail before noon to get it east to you before the week is over. And although I am sorry that you cannot get much out to look at the plants during the heat, we are glad to see that you plan to enjoy once more Chile in the middle of their summer...we have never been on that continent, could not afford it with my low salary and hardly any research and travel support...we ought never have come to Boulder, and when we nevertheless did come, ought to have been relegated to the Museum soon after...one is always wise later...

With the very best regards and all good wishes...what happened to Laila?

As ever,

Aska

San José, August 23, 1983.

Dear Bill:

Try to forgive my tardiness, but this time I believe I have a valid excuse since your letter arrived just when we got a family visit of five people, two of them small children that we had not seen for some time, and they left yesterday. A pleasant visit that we would have liked extended, though it is always somewhat tiresome to have babies around at our age, despite the pleasure.

I am not astonished with the treatment Leila is getting, we have experience of the common bigotry in your land, though you are among the very few who are fair to all. Utah State is evidently going to pots after Holmgren's dominance is over, and I know that the physiologists decided last spring to hire a physiologist with queer ideas as to improving wheat by aid of his dim views of breeding by aid of genetic engineering, whatever that is, he was sent to me for a discussion before he left so I know that he cannot replace Dewey, who is a good cytogeneticist. But America, as many other lands, are leaving interest in taxonomy and evolution for unscientific hypotheses based on similar ideas as Hooker had long ago when he decided about the collective species he and his cohorts described everywhere, and perhaps we ought to blame the almost total lack of genetics in the education of the taxonomists here? You and we are exceptions since we started all with genetics and then got into taxonomy and geography, which I believe is a fundamental approach. But creationism has always appealed to those who do not know and do not care for thinking logically...and those who believe in other approaches must be treated as Galilei and Bruno and their opinions suppressed. I am sure that many of your colleagues have always wanted to suppress what you have done so much more effectively than they could do, it is human to be negative and to kill everything that differs, and few learned men have risen above that animalic thinking.

I wish I could be of some help to Leila, but since I do not know the other parts in the play, I might only hurt her if I mentioned such possibilities to them or others. I also know her very little and never had her as my student, as far as I remember.

Thanks for contacting Fosberg, but was it wise to send him only what you say was a part of the dossier, since that may mislead him or even cause him to react as the Boulder crooks that you gave only a meagre summary years ago and showed them that there was some movement that perhaps could hurt them if it was not suppressed? But I know that you and Fosberg can be trusted, though he told Polunin years ago that I had only been fairly treated by the Smithsonian...he did not know any details and was then not interested, I believe.

Have you gotten Wolf's monograph on Potentilla to help you with the keys? And I hope you have gotten a final positive answer on the space problem. And know that you will soon get my Icelandic flora, which ought to have been sent in the early June but was forgotten because everybody went on vacation abroad...it was needed because Iceland has the coldest and wettest summer since 1887...and now also no grass, I am told. That would have meant starvation if the American interest in shooting the wild Russians had not caused them to buy the Icelandic conservative souls.

All the best from us all here,

Áskell

San José, September 2, 1983.

Dear Bill:

Many thanks for two good cards that arrived on August 29, when our house was full with guests from Boulder...Ingela's halfsister etc. And when they had left, the wife of my Napa brother's oldest son and their two children came on their way to Los Angeles...

Glad that you got the Icelandic flora and like it...it was sent to a few others on the continent but you are the only who has reacted sofar, as I expected. If you want to review it there is little sense in waiting for that you be asked by some of those you so appropriately call the dinosaurs, because they will want to give it the silence of the sea, as everything we have done to keep the cytogenetical and antiphenetical approach alive in a land of lysenkoism and worse. But if John Strother still is the editor of Systematic Botany (he is at Berkeley), he might permit you to write about the copy you already have, if you ask him? Science is not interested in botany except if it comes from those who like alkohol, as Peter Raven & Co., and why should other American journals be better? Do as you like, I was not fishing for a review in a land where real botanists and honest scientists are almost absent, only wanted you to have it. But thanks for the idea...perhaps Stafleu would like your proposal to review it, so he could avoid it himself? He has a copy, though he has not yet thanked for it, he is so busy with his fine Taxonomic Literature, which I hope you receive.

I am astonished that none of the earlier botanists have gotten the excellent idea of naming some of the Artemisiae genera Steppea, because a more appropriate name is not easily invented. The only trouble in this case is, however, that Polyakov already has validated the generic name Seriphidium (Bess.) Polyakov for the group as a genus, in his paper from 1961: Polyakov, P.P.: Materialy k sistematike roda polyija Artemisia L. Trudy Inst. Bot. Akad. Nauk Kazakhskoy SSR 11: 134-177. I thought you had copied it in 1970, when I received some reprints from him, but his transfers are listed in Kew Index Supplement 14 and the genus in Airy-Shaw: Willis.His type for the genus is S. maritimum (L.) Polyakov, not S. glauca. But he does not make any transfers for extra-Eurasiatic species, so the American taxa are waiting for you to do that. Perhaps you could still use the fine Steppea name for the species A. pattersonii A. Gray, which evidently does not belong here, and not even in the tribe Anthemideae, since it has the basic number 7 and chromosomes of another morphology, as Wiens & Richter showed in 1966 in Am. J. Bot., we have verified their observations but not written about this for the sole reason that I could not find enough data to identify the plant with a likely Asiatic genus...I am critical about pan-American genera of certain families at least, since they have more likely come from Asia...though that may be wrong. Polyakov is not likely to be able to help you find a possible genus for the American plant, since he is bor 1902 and may not be alive...but perhaps Kirpicznikov could help, he is a fine specialist of Compositeae and a willing helper whenever asked? Though that may require too much work so the simplest way out might be to describe it as a species of your Steppea and let future Russians identify it with some genus of another tribe? If you do not have enough Russian material in your good collection to guide you to the group it belongs to.

I observed in your mentioning of Crepis earlier that you referred to Stebbins etc. rather to Babcock's fine Crepis monograph...do you not have it? He lists the entire group related to the two little species under discussion...I may even have mentioned them in an earlier letter?

I am not astonished that those concerned drag their feet as to space for a fine herbarium that they probably dream of giving away rather than replace you later...but keep bothering them, perhaps even the biochemists will give up or otherwise stab you as they helped to stab me...not least Runner who is nothing. But I hope your fine flora will be ready before they succeed silencing you too!.. Read Komarov in Flora SSSR 1 for discussions on the nonconservative species...in the introduction, in the translation.

Regards. Áskell

SAN JOSE, CA 951
PM
3 SEP
1983

Robert Morris Patriot
US Postage 13¢

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

San José, Sept. 3, 1983.

Dear Bill:

I suddenly remembered that in my yesterday letter I forgot that we actually published, with Kapoor 1971, our first obs. on A. pattersonii...what I remembered were even later studies.

This is otherwise to ask you for help to get seeds of Oxytropis deflexa to Dr. Morten M. Laane, Botanical Institute, University of Oslo, P.O.Box 1045, Blindern, Oslo 3, Norway, so he can cross it to their extremely rare populations...he has asked several colleagues for help but got no reaction, so I promised him to ask the most helpful one I know. Hope it is not too difficult and that you already have some not too old seeds, they can be kept for some years if dry. I know he will be very grateful.

All the best,

Áskell

San José, September 9, 1983.

Dear Bill:

It was nice to talk with you on the phone yesterday, though it would have been nicer to have you here in person. Of course. Here is the copy of Polyakov's long Artemisia review, I am only sorry that we can hardly send him A. pattersonii for identification with his Turaniphyton, but the description in Flora SSSR seems to fit, though he did not add a picture. Even though future specialists may revise this, it would be an improvement to transfer the American anomaly so that others at least realize that something is fishy...and SW SSSR or rather Siberia and not least Khazakstan seems to remind a good deal of Colorado.

I can offer only a few and insignificant remarks on your Names IV, except perhaps what I mentioned about x, not n, for basic numbers, and the term certainly is basic number, not a base number, which is an American agronomist slang (from Burnham) affected by the baseball terminology? There is no base involved, look at Oxford Dict. If Small thought from herbarium specimens that Cathartolinum s.str. has yellow flowers, I can understand his use of this name for the American group, but as far as I know.... and I have seen the plant not only in Iceland but all the south to Yugoslavia... Cathartolinum has always white flowers, but with a yellow claw, cf. Flora Europaea. I wonder if there is something missing from the latter sentences on this, but then it will be polished before completed.

Your Askellia seems to be in order, though you might perhaps state your reason for not accepting Babcock's Hookerian widening of the genus Crepis. And there are two ssp. in nana and then five more species from Asia, according to Babcock, but you may perhaps want to give the Soviet botanists an opportunity to make these transfers, or Holub, who probably will react when he sees what you are doing, and revive the other genera sunk into the complex and name those that have no distinct names? Though I admit blushing when reading what you write about me, I have some remarks that perhaps are improvements: I am not the student of Turesson, but of his Lundensian colleague Arne Müntzing and then informally but certainly of Eric Hultén, his only student as a matter of fact. I do not think I had such a great part in the Flora Europaea, thanks to the English pheneticists and their money, but the surge of interest in Flora of North America certainly was caused by my (and later also your) stimulation, though it came to nothing but two uncritical checklists that perhaps would have better been forgotten, and will be so in the future. And why not mention IOPB (International Organization of Plant Biosystematists) which was my idea and became most effective during my period as its first president, and perhaps also the fact that without my stimulation there would hardly still be a Canadian Genetics Association, though our Canadian colleagues keep very quiet about it as they do about their mean treatment of us that forced us to accept even Colorado, despite recommendations to the contrary by Stebbins, who perhaps understood better than most what kind of administration always has been there? But do what you feel is wise, I am grateful for your friendship, the only one that was not made in order to utilize my help in a country that greeted us with CIA harassment that started already our second week in Boulder, and with falsehoods that started when the mean Runner and his foolish chemist friends found out that I could not be utilized to hurt the department that he tried to make into his own private ivory tower. The other base colleagues of his came later and continued what he had started, and none of our socalled colleagues were ready to prevent that, least of all Pennak who is a fine man basically but becomes mean in trying to hide his insecurities. Hope he is well and that his very mean colleague Gregg has come to where the pepper grows I, however, react against your mildness in using the term misfortune about our case, since the correct thing would be to tell the truth that it is a case of a Galilei-type miscarriage of justice, both at the Smithsonian and in Boulder, but from various points of view and for various reasons of suppression.

Your review of Hall & Clements and Camp is excellent and will, hopefully, get to the younger generation, not least your former student Murray, who continues to make a fool of himself talking about taxonomy and genetics of which he understands nothing...and you would be shocked if you saw the master's thesis of his hopefully only graduate student J. C. Dawe, which according to Yurtsev is a series of foolish slander about our taxonomical judgements and cytotaxonomical observations...Dave promised to send me a copy but it never came, so I know this only from Yurtsev. You could even have mentioned what Komarov says in Fl. SSSR I...it is available in the Israeli translation, of course.

As I mentioned on the phone, the new sect. Steppea cannot be validated by a reference to Hooker's concept of Besser's Seriphidium, because it nevertheless rests on Besser's 1829 sectional description. But use the distinct, though short, description by DeCandolle 1837 of sect. Seriphidium subsect. Trifida by refering to Prodromus6:105; then you can add a clarification by mentioning also A. sect. Seriphidium Hooker, non Besser, which includes your type species and one from South America. Also, do not accept the taxonomy used by Beetle for A. tridentata, since at least his ssp. vaseyana is biologically distinct from A. tridentata s.str. by being diploid as contrasted to the latter's tetraploidy...and your rothrockii is a 72 chromosome species. I suppose the other two Beetle subspecies are tetraploid and could be left as they stand, though I can find no chromosome information on them.

You certainly know that the list of references is incomplete, since what you sent was only a draft. But when you refer to us for the revival of Oligosporus, this is hardly fair to the good Polyakov, though we may perhaps be mentioned as coming second?

You had hardly put the receiver on the hook, when I got another and much longer telephone contact with Doug Dewey, who has just returned from a summer in China and wanted to discuss further some of our wheatgrass problems. Though he was responsible for getting me to dig up the immense material we had worked on in Iceland, Canada and some little in Boulder, I would not have been able to draw all the conclusions on the taxonomy without good knowledge of his immense material of various crosses, not least unpublished. And though his conservative upbringing slows him in accepting the genetical concept, he slowly comes closer to my point of view, and phoned this time mainly to talk about his discussions with the Chinese, who still live in the world of Nevski's Roegneria, and then to tell me and give me more reasons for that he has now decided to follow my advise as to the acceptance of Pseudoroegneria for the second oldest genus of the wheatgrasses. But he is still hesitant about Critesion, the oldest as far as I understand, that has been misleadingly put into Hordeum since before Linnaeus, though the latter is among the youngest of genera. This will all come to the same end, however, I can wait, and if there will need to be corrections of my system, these will likely come from Dewey. If he lives that long, it is a secret still that he has a serious liver disease and that he contemplates to retire soon, after 30 years of service though he much younger than we are. Perhaps he would be one of those who could help you help me, since he is a man as fair as you and with a strong conscience...and also a sincere friend, though we never have met.

We are shocked about what you said about Paul Maslin, he and Mary were among those few who always were nice and friendly to us, as they are to everybody. If you see them, give them our sympathy, though we are also writing to her at least, Doris and she have always been rather close. But such is live...the ninth of my 21 classmates from Reykjavík died suddenly last spring, he was our family doctor, and two others have been more or less sick for years and waiting for the call that we all get.

Hope Sammy is similar and as well as she can be and that you continue to be healthy. So you can at least play with the grandchildren, some of whom are grown up!

All the best from us all here,

Áskell

P.S.: Give Phil Becker my best regards and thanks. I met him only with you, but he evidently is one of the few Americans who are Men of conscience.

San José, September 12, 1983.

Dear Bill:

Thanks for your letter of 7 september.
I agree that the steppea-Seriphidium question is perplexing, though not in the way you seem to think. Polyakov (you should by now have the copy of his paper) was forced to select as a type of his genus some of the species known to Besser (1829) and included in his section, which Hooker (1833) misinterpreted so there was no way of following him...Hooker simply ignored what Besser did, except the name, as the English botanical "kings" still tend to do, cf. F. E., but DeCandolle did right when recognizing the American group as a different subsection (?) Trifida.
I tend to follow you when you suggest that not only the American sagebrushes but also some others of the Eurasiatic groups may be better recognized as genera, though their distinction may not always be as strong as that of the genera recognized by Polyakov...this has also been done by some others, cf. the synonymy in Flora SSSR. You know my reluctance to accept wholly American genera (except in the Arctic and if they reach South America) without at least some representation in Asia, though I have not yet convinced myself that this is a rule without exceptions, so that alone works against the distinction of the sagebrushes at this level. We would be safer if there were known experimental hybrids, but even Clausen, Keck & Hiesey, who cultivated both groups for years, do not seem to have thought of hybridizing them...or perhaps they were too little interested in pure genetics?...neither did they mention having observed spontaneous hybrids in their fields. Since apomixis may be involved, this is perhaps of no significance, though it would have been nice to know with how much ease for instance the diploid A. maritima and A. vaseyana may mix. And nobody seems yet to have studied the karyotype of different groups within the genus, so we do not know if they are haplomically distinct (genomically if you prefer). [A. pattersonii is an exception because of its distinct karyotype and basic number and because of its morphological characteristics, so I hope you find it possible to transfer it to Turaniphytum or some other good Asiatic genus]. Because of this, perhaps it is premature to distinguish the tridentate American plants as a genus of their own, though cytogenetical experiments may later confirm your suggestion? Therefore, I would like to propose that you include them for the time being in Seriphidium, but not as a subsection, as did DeCandolle, but as a subgenus in its own right, because that is certainly well supported by geographical and morphological characteristics. That treatment might induce criticism or even damnation of your foolishness by some young turks, who then might go out and make some experiments to prove you wrong in your disbelieve of the Hooker concept...and then instead demonstrate how right you actually were in your suggestion. So may I propose that you use the DeCandolle description as a basis, but ignore his name, for the taxon at the subgeneric level and call it sg. Steppea? Mentioning Hooker, non Besser. That would make your fine name available also at the generic level, when your suggestion has been confirmed, and otherwise make it available for another taxon also, if you so feel before some other lifts it.

I am happy that you will review the flora for AAR, and will of course help if approached, perhaps even with general remarks if I see a draft. Yes, there are typographical errors, despite of four or five proofs, as in all good books, because the printer ignored some of my last corrections, and then I of course overlooked some. But your Moss Champion I cannot find, not even in the index, so perhaps you got another printing, in addition to an evil eye?,! Those I have found...and you may criticize,.. are: on p. 107, the number 133a is missing for the lower middle drawing (C. caryophyllea) on p. 384 is Melianthaceae for <u>Melanthiaceae</u>, which is correct in the 2nd edition but wrong in the 1st; on p. 188: stitchwort, not -worth; p. 194: Bog sandwort, not bob; p. 240 & 391: Livelong saxifrage, not lifelong; pp. 344 & 345: herbslopes, not -slobes (Icelandic pronunciation of p!); p. 389: clubheaded, clubmoss family, not clubb; and p. 304 is missing the sentence, below Primula stricta: Grows in moist clay flats. Rather frequent in the XXXXX inner parts of Eyjafj"ordur, N, rare in E...so in the Icelandic version...it will impress those who know only English if you mention this!

No more this time, Doris has to go to the post office with a translation so she wants to leav at once. It is close to gour in the afternoon of a day that was over 100 degrees earlier and now is only about 95...but we stay indoors where it is comfortable thanks to reasonable insolation and the fact that in warm periods we keep our windows open during the night and closed during the day. But perhaps I should not mention such a heat that we only have for some few days at the time, and it is soon ended for the season...not at least when I write to somebody who has been sweltering on the prairies all the summer long...do not mention a snare in a hanged man's house says one of our old proverbs...but I know you forgive me.

All the best,

UNIVERSITY OF COLORADO, BOULDER

Museum

13 Sept. 1983

Dear Askell & Doris:

So many long letters! and the wonderful Polyakov paper too. I have revised again the manuscript and want you to look at it again. Right now I think I am not ready to handle Artemisia pattersonii but take that on later in the next paper.

Do not blush; the words are not flattery. We all need you and respect you for everything. It is time that you got a little back.

I must be very out of touch. This is the first time that I heard about a broblem involving the little Runner. Can you tell me something about that part of the ancient history.

Yes, I talked with Jack last week, and he is probably going to ask you to come along with us and try to present the case in full with the new administrative people. I am certainly willing to go with him if you and he decide it should be done.

Please tell me if I have treated the Komarov statements in Fl. USSR correctly. I find that they are very ambiguous, first flopping to one side and then the other.

I am sending you too the pages from Hooker and ING. ING peovides no cards for lower divisions than genus. I have 1978 code; the new one from Sydney has not been published yet. Typification in the rules is scattered throughout. Perhaps I can glean the necessary parts, but it would be best if I do this when you raise a particular question. I can't find a reference to recignizing as invalid a plural substantive such as "Seriphida". I think that the spelling "Scriphida" is a mistake in the ING. You will see that Hooker says "ubi errore typographia Seriphida." I don't think that Hooker was setting up a "type" by merely listing A. cana. He gave Besser credit for the section. So if Besser did not designate a type, Poljakov must be correct in selecting a type from the species listed by Besser. There seems to be no impediment to the selection of Artemisia maritima.

What do we do with the Oligosporus borealis group in Colorado?

No news yet on space. We should have another meeting with the committee next week. There is a possibility of our getting 7000 sq. feet in a new building, but this means rent (the university paying itself rent for one of its units, so—funny money; maybe we have a chance). The building will be started next month, on Broadway opposite Hale Building.

Bill

Henderson Building • Campus Box 218 • Boulder, Colorado 80309 U.S.A. • (303) 492-6165

San José, September 14, 1983.

Dear Bill:

Sorry that Turaniphyton did not fit...but there must be another name if the taxon is from Siberia, or you could propose an appropriate one. According to Rydberg and again Wiens & Richter (1966) the taxon and A. scopulorum are morphologically similar, though similarity means nothing in view of the basic differences in the chromosome morphology and basic number, which strongly indicate an absolute crossing barrier. Could you give me a copy of the Wiens-Richter paper, I had it only in the journal, which I gave away to save space when we moved.

I share your confusion as to the ING declarations on Seriphidium, but would appreciate some more information and copies so that I can at least try to explain the differences in the Russian and German interpretations I have in Polyakov (English spelling; his Latin spelling in author's name is Poljakov) and in some other and older works...and I am sure that you do not have first-hand references of the Moscow journal either. What I need to see copies of is:

1) The appropriate pages in Hooker.
2) The card[s] from ING with sectional and other divisions of Artemisia (preferably Besser's system). And an information about who compiled them.
3) The card for Seraphidium Poljakov, including decision about typification, if any.
4) Copy of the newest rules and recommendations for typification (I have 1972 Code).
5) The preamble page in the newest Code.
6) Copy of the article on spelling and typographic errors (Art. 73 in 1972 version).
7) Explanation and referate to article for rejection of Seriphida as invalid because of the typographical error [Scriphida] or grammatical deviation (pluralis)
8) Information about when and by whom A. cana was selected as type, reasons if any?
9) Has any other type been selected, except by Polyakov, or rejected?

Although the lawyers may not agree, I think I have some idea what has caused this confusion, and you may perhaps guess what I am aiming at from the above. But I have little confidence to the compilers of ING since I am aware of that several of their selections have already been rejected as arbitrary or worse, for instance the typification of Elymus by Britton & Brown who always selected the first species mentioned by Linnaeus, irrespective of later work of greater exactness...McNeill remarked on that point in Taxon last year. And the Artemisia case may be just one of these so we may have to build up a discussion and conclusion that then might be sent to McNeill in Ottawa and Dan Nicholson in Washington for preliminary approval. There will be a solution that stands firmly, and I am sure that Hooker's utilization of the name correctly spelled does not change the fact that Besser described a European plant well known to him and not an American unknown.

I see in Czerepanov (1973), p. 94, that Hendrych (1966) has written something about the problem in Novit. Hort. Bot. Inst. ~~XXXXX~~.Univ. Carol. Prag. 1966:32. I believe I have it somewhere, but have filed it away so I cannot find it. I may look closer in the boxes, though I doubt it is significant...perhaps Smithsonian has it?

That is all for today. We are having a respite from the terrible heat for a day but promised a continuation of the boiling tomorrow! Hope it is a mistake.

All the best,

P.S.: Jack phoned and said that he and Pauline may come around October 13. Any warning?

San José, September 29, 1983.

Dear Bill:

At long last I am at the typewriter, since Doris is through with her long translation. I have already typed the Böcher obituary, which I hope Kathy will have early next week... a copy for you is enclosed. But although I have made notes on your letter from two weeks ago, and on the revised manuscript, I am not sure that I will get everything on paper this time, because I have some tendency to forget just the essentials, as you know better than do others! Thanks also for the package of copies that help, though they say less than I expected. Perhaps the Sydney Code will say more on typification, I got a letter today from Stafleu announcing that I am getting a free copy, but he does not say when...we will see.

I am thoroughly convinced as to the distinctness of your Argillochloa, though I wonder if it belongs to Poeae, as does Festuca, or to Stipeae? Sorry that we are so far away, because otherwise we could have looked at the chromosomes before winter sets in...is there anybody in the neighborhood who could...perhaps the good Japanese barley man at Fort Collins could help...though that could be the next step. And have you discussed this with Mary Barkworth, who is said to know Stipa well? I also like your good selection of the generic name, but perhaps you ought to add a little explanation, e.g. from Greek argillos, white clay, shale clay (?), and chloë, grass.

You know probably that in the fourth paragraph is a misprint numbe for number, and I wonder if in the following paragraph "but" might not fit between strong and superficial?

p. 02: In line 11 you missed correcting chromosome base number to basic chromosome number...elsewhere it is in order now. I also wonder if in that paragraph it might not be worth changing the sentence beginning "The yellow-flowered group..." into somewhat like "The pale-flowered group consists of two well-defined lines, both with x = 8, though otherwise cytologically distinct; one, Cathartolinum Rchb. (1837) based on Linum catharticum L. with white flowers with yellow claws, and the other, Rogers' L. schiedeanum complex,which Small (1907) included in his broadly construed Cathartolinum, differing significantly in fruit dehiscense....". At the end of the sentence could come: "...with Adenolinum and Cathartolinum s.str. it forms a distinct group."

p. 03-05: Aletes: excellent.

p. 05: Askellia: Babcock's book is from 1947, he was not born 1047. You might also refer to your own recent transfers to Psilochenia, not only to Babcock 1938. As to other points, they must stand at your responsibility, but thanks.

p. 06: (in the middle of the paragraph)...."...Related species in Eurasia were segregated from Artemisia by Polyakov (1961) (Polyakov in English, ja in Latin) as the genus Serephidium (Besser) Polyakov, with the type species S. maritimum , which was the only species of the group known to Linnaeus (1753). The genus is based on Artemisia sectio Serephidium originally proposed by Besser (1829), unfortunately as Scriphida , which is an evident printing error and an orthographic error (or grammatical error) which was corrected formally by Besser (1834), but earlier by Lessing (1832), who accepted Seriphida (pluralis) as a subgenus rather than as a section, according toDT & Harms: Genera Siphonogamarum...I do not have Lessing's original, of course,... according to Flora SSSR, Rouy (1903) also accepted the taxon as a subgenus, but corrected...so I wonder if even there the author ought not to have been (Besser) Lessing...though this may not matter here.

Hooker made the correction for sectio Seriphidium when recognizing that the species A. cana Pursh from Canada belongs here, but he was evidently unaware of even sectional differences between that species and the European group, whereas DeCandolle (1837) regarded the former as a representative of subsectio Trifida DC., which also includes the South American species A. mendozba DC.

There is some confusion in Index Nominum Genericorum Vol 3, p. 1606, as to the validity and typification of the section Seriphidium Besser, which is declared invalid. probably due to the printing error, though no explanation is given. That is not in accordance with the Code; neither is the typification by A. cana Pursh, since Hooker (1833) only accepted (and corrected) the Besser sectional name for this North American species, but certainly had no intention of changing the definition of the section, which must be typified by A. maritimum, its only representative known to Linnaeus.

The North American members xxxx of Seriphidium have been treatedxxx exhaustively by Ward (1953). Earlier accounts include those by Rydberg (1916) and Hall & Clements (1923). They form a very natural unit and I propose recognizing them as a subgenus: (correct "sectio" Steppaa to XXXXXX subgenus Steppea, of course, a small lapsus)."

Hope you can understand what I and the typewriter are trying to tell... the remainder is fine with me - except the printing error "methosd" on p. 09, line 26, and the lack of the following references in literature cited:

Babcock, E. B. 1938: Crepis foetida and four closely related species. - Journ. Bot. 76: 201 - 211. (or am I guessing wrongly as to title here?).
- 1947: The genus Crepis. Parts I & II. - U. of Calif. Publ. Bot. 21-22:1 - 1030.
Besser, W.S.J.G. 1829: De Seriphidiis seu de Sectione II-a Artemisiarum. - Bull. Soc. Nat. Moscou 1 (p. 222).
- 1834: Tentamen de Abrotaris seu de sectione II-a Artemisiarum. -XXXXXX Mém. Soc, Nat. Moscou 3 (p. 5).
Camp, W. H. 1940:
DeCandolle, A. P. 1837: CDXIX. Artemisia Linn. - Prodromus 6: 93 - 127.
Lessing, C.F. 1832: Synposis generum Compositarum, etc. - Berolini, XI + 473 pp.

Then back to the letter: I understand the need to wait a little with A. Pattersonii...but know you will solve that problem also.

The Colorado Oligosporus borealis complex is the diploid, although Hultén was confused on this. As I understand his conclusion in the scientific Alaskaflora, O. borealis s.str. is the tetraploid complex to which some Eurasiatic species belong, as far as I understand the problem, which is likely to be discussed in the last volume of the Flora Arktika SSSR sometimes in the near future, hopefully, they are announcing the latter part of Volume 8 for October, and claim that the last volume is practically ready for the printer...but nobody is as slow as their printer, so we will not want to wait. Even the Danes confused matters in Greenland, somewhat thanks to Hultén's confusion, but the only taxon they seem to be sure of having of this complex, is the O. groenlandicus, which is diploid. And that is the plant met with in Colorado, cf. L.& L. 1975 (Bot. Not.), and L.,L.& Kapoor 1971. I doubt that you need a further clarification though this may not be as clear ad I would have hoped it to be on the paper...but then force me to be more specific.

I think you are correct on the Komarov statements, even he does not try to be concise and may have practical reasons to be "confusing" and Hookerian in part!
It is late and I am tired so let us wait with the political part and the [illegible] saga. But, remind me when you feel you want to listen to that Littany. All the best, [illegible]

San José, October 8, 1983.

Dear Bill:

Many thanks for the good letter and the manuscript, and for the Böcher correction; you never observe such mistakes yourself when they have been made, although when they are pointed out to you, they are very obvious. Please, pass them on to Kathleen.

I will try to fill you in on Runner and some others, when I so feel, hopefully soon, though my experience of him and his ethics was, fortunately, limited and short. I wonder how Boulder get such a high frequency of crooked minds, especially at levels at which I was brought up to believe such things never happen?

The remarks by Signe Frederiksen convince me that Argillochloa indeed belongs to Poeae, not Stipeae. Although I do not share your aversion of Mary Barkworth, perhaps because I know her very little and only by letters and her papers, I understand that you have your reasons...but the fact that your reactions against those you dislike are always so strong, makes one appreciate still more to be permitted into your little ring of friends. Did Fosberg ever react...perhaps it affects him that the Smithsonian did to us what they would have done to Koyama, when they wanted to replace him as the main editor of the Ceylon flora, which was his idea...though they did not need to stab him as they did me? If he had been permitted to continue they would now have a modern flora of the classical type and not a series of papers that nobody reads and nobody can use for identification of plants?

The paper continues to be excellent, but not yet flawless, as all good papers tend to be during preparation and even the bible itself continues to be after all its reprintings. Here are the minor mistakes or flaws that I have seen:

~~p. 01, line 40: Oryzopsis should be underlined.~~
p. 02, line 33: remember the umlaut in Löve.
line 42: same for Löve & Löve.
line 43: and Löve.
p. ~~04, line 15: should not "is" be "as"?~~
line ~~42: is not "as" missing between Cymopterus and discordant?~~
~~lines 49-53: Switch the two species for alphabetical order.~~
p. 05, l~~ines 15 & 16: Would it not look~~ nicer if comb. nov. were in the same line?
~~line 21: al. (period missing).~~
li~~ne 43: I am of the feeling that all generic and lower names ought to be italicized, therefore: Ixeridopsis.~~
~~lines 48 & 49: remember Áskell & Löve, Müntzing & Hultén~~.
p. 06, line 24: Feddes Repert. (perhaps a matter of taste?).
~~line 26: parenthesis missing: (p. 18 in...~~
~~line 42: were, not wwre.~~
~~lines 45, 48 & 49: underline Seriphidium and Trifida (cf. above)~~.
line ~~46: type concept unknown 1833, therefore perhaps better to~~ say:
~~"accepted by Hooker (1833) for Artemisia cana Pursh~~.
p. 07, ~~line 02: underline Seriphidium and Trifida.~~
~~lines~~ 06 to 08: ~~ought to be moved before S. canum (alphabetical order~~).
~~line 26:.....Club 27:274. 1900.~~
line 54: underline Seriphidium.

~~p. 08, line 18: underline Seriphidium.~~
p. 09, line 11: confrères (remeber the accent grave).
line 19: citation sign is missing.
line 27: You ha~~ve~~ inadvertently dropped the sentence, after"to the fore":
"It is ~~not easy~~ to memorize the very large number of generic appellations'
(If this was intended, then add instead ....).
line ~~38: progenitors.~~
p. 10, line 10: ~~better drop the comma between "sort" and "from", it cuts unnaturall~~y.
line 34: underline Seriphidium.
line~~s 43 & 44: ex either underl~~ined or not, not both.
line ~~47: Melica nutans (underl~~ined)
p. 11, line 08: the literature ~~reference is~~ missing for Mentzelia reverchonii.
lines 21~~, 24, 26: remember the ö~~.
~~lines 35, 36: Publ. No. 504:1 - 199. 34 fig. 12 tab.~~
~~line 38: Bot. 21-22: 1 - 1030 (no division for thousands here).~~
~~line 54: Could you avoid dividing Artemisia?~~
p. 12: ~~lines 10 & 11: Á (twice), ö (three times).~~
~~line 14: Polyakov (ya in English, ja in Latin, German, etc.)~~
~~line 26: underline Seriphidium.~~
lines 29 - 32: Weber 1983 before Weber & Löve 1981. And remember Á and ö.

This is all that I could find, and I hope there is no more of these small printing errors if one dares to use that term for these petitessen. One may perhaps dispute if it is necessary to correct it all, though I am inclined to think so. Especially in a fine contribution, which I and certainly many more want to see in print as soon as possible, of course. So you can get to the next number in the series or to something as good or better in other fields, or even to the flora itself, which will become the only one on the continent that will be properly modern and logical...and nevertheless certainly not without something for you and the coming generation to correct, because floras never are completed if they are to be good and scientific.

All the best,

San José, October 23, 1983.

Dear Bill:

Thanks for the October 13 letter that arrived yesterday - they must use mules again for mail service in America - or perhaps the mailmen, who here are paid better than professors in Colorado, simply store letters until they feel fit to deliver them?

Nice that Wilken confirmed the Poae classification of the new genus.

I can see the wisdom in waiting with the Aletes until you know why Ron Hartman wants to recognize Neoparrya. Hope that problem is already solved.

I am in no doubt that you will solve the Seriphidium question satisfactorily even though Leila, who evidently did not know what Polyakov really did since his paper was hidden in my library - and perhaps somewhere in Washington? - is confused, but if she has a clear evidence against separating A. bigelovii from Artemisia and is good enough to understand our generic philosophy, this ought to be accepted, at least until contrary evidence is presented; and I (and I believe you too) was never quite satisfied with it in Seriphidium. But if the fellows, who ever they are, who in 1981 proposed a subgenus Tridentatae, argue logically for the distinction of the American group as compared to the Eurasiatic Seriphidium, and if also Beetle's 1960 reasoning looks sound (I have neither paper to check, and am critical about the abilities of Beetle because of his inability to see certain classical American errors in the grasses, i.e. his accepting of socalled Pleuropogon from California, and his identification of some socalled Agropyron from South America), then I would use their arguments in support for your original opinion on the genus Steppea. And if other and wiser men later may prove you wrong, no harm is done, but much good may come of the more liberal way. In the case of A. vaseyana, it is true that there are reports of both the diploid and tetraploid numbers for it and the main species and several other Artemisiae, which I believe are caused by the inability of little trained floristicians to identify these critical and very closely related autoploids, and there is also no doubt that hybridization occurs, e.g. with A. cana, which only shows that these taxa are related. However, I maintain that there is no valid reason to doubt that the taxa s.str. are either diploid or tetraploid, not both, and that their biological isolation is perfect, as it is in the autoploid series Acetosella, the report on which I hope you can get before the end of the year. Despite the perennial claims by nongeneticists and pheneticists of the Cronquist type (he lists, e.g., uncritically from the literature in the Vasc. Fl. SE U.S. (1980) the numbers n = 8,16,24,32 and 36 for Aster simplex, which according to the meticulous Almut Jones has, s.str. as typified, only 32, of course; the chromosome number of plant species correctly identified vary no more than that of the human species, although bigots would like the latter to do so...and I suppose Cronquist would list 46 and 48 as our chromosome number and ignore the correction of the latter after the invention of more exact methods and the studies of more exact and better trained cytotaxonomists). Both numbers have been reported in the past for both these taxa by doubtfully skilled and careful botanists arrogant enough to make distinct claims against the paradigm of chromosome number constancy, as, e.g., your miserable and little learned former student Dave Murray excels in from his shelter in Alaska. And 14,28,42 and 56 continue to be reported from Acetosella four decades after my cytogenetical treatment of that autoploid series, which bad floristicians, phenetists and creationists ...including the arrogant Hultén and Hylander, who refused to even check!... refuse to acknowledge as a series of closely related and distinct species If you want to be safe, perhaps Hartman could help you find specimens determined by Beetle that he could make pollen studies on...though from my point of view such trouble is not need

You are probably not the only one that Heywood and Jones have confused as to the typification and delimitation of Noccaea...they have done worse things too. I am sorry that I do not have the paper by F. K. Meyer (probably in Feddes Repertorium ca. 1974 - 1976?) and that I have also filed my notes from 1975 too effectively in some box upstairs, so I have only indirect references to it, in Rothmaler's 1976 and 1982 Exkursionsflora for the DDR and BRD, Kritischer Band, and in Czerepanov, 1981: Plantae Vasculares URSS. The former lists three species, including the type N. rotundifolia (L.) Moench and N. montana (L.) F.K.Meyer, whereas the latter lists thirteen other species from Eurasiatic Soviet Union... all listed as Pterotropis in Flora SSSR. We accepted the Moench genus because of its distinction as a perennial, alpine group that never grows as weeds and never crosses to other Thlaspi because of profound karyotype differences. When we transferred the arctic American T. arcticum to Noccaea, we did it because of its morphology, but hesitated to aceept it as a distinct species because we could see its closeness to what here has been called T. montanum. Even Porsild realized this relationship, though he felt it was closer to T. cochleariformis, which we knew has 56 chromosomes contrary to 28 of the European T. montanum...and though we measured pollen grains of both, we could not become sure that arcticum is a diploid. However, now we know, thanks to a report from Alaska by Dawe & Murray (1981: Can. J. Bot. 59:1373-1381), that the arctic American taxon is diploid and thus incorrectly placed by us with N. montana, which is distinctly tetraploid in Europe and not met with here, as far as I understand.

Unfortunately, I have not seen the long paper by Patricia K. Holmgren, 1971: A biosystematic study of North American Thlaspi montanum and its allies, Mem. N. Y. Bot. Gard. 21(2):1 - 106, but according to the Index to Plant Chromosome Numbers 1967 - 1971 (which I ought to have consulted in 1975), she reports n = 7 for T. montanum var. fendleri and n = 7 and 14 for var. montanum and idahoense...I cannot guess who did the cytology, because I doubt that she has done it...more than Rollins did the counts he reports and discusses as a learned man in that field, but her way of reporting this indicates that she has about the same ideas as Cronquist and so may report 14 from the literature (European, or Cronquist's floras?), though there is, of course, even a possibility that some polysomaty may occur in her material? But if the diploid number is the only one correct for the Rocky Mt. plant that evidently have been called T. montanum var. fendleri and idahoense (and other names), then the reduction of T. fendleri A. Gray to a variety of the strictly European T. montanum by whoever did it was a serious mistake that needs to be corrected, as also is the case with our mistake to transfer the arctic plant to N. montana. I think I ought to urge you to make this correction and transfer the American complex to Noccaea as the species N. fendleri (A. Gray), with the two subspecies fendleri and arctica, and with the former split into the more local varieties fendleri, glauca, hesperium and idahoensis. Am I right in guessing that this is just what you had thought would be needed when you asked if I understood your problem?

You have my sympathy in the struggle with the Brassiceae (not Cruciferae, please) key...but others have had the difficulty before you and may perhaps be of some help.

Thanks for the Moss Champion correction...I would never have observed it, because the publisher put the color pictures at the printing time and contrary to my advice, and I never saw or even thought of his mistake when adding the English names to them! Fortunately, most users will never observe this, for evident reasons, only some ardent reviewers who may even read the forword and look critically through the index, in which the names on the color plates were, naturally, not added, since then I was not in control.

Here I ought perhaps to stop, but I cannot let be to tell you that I just got a long letter from Yurtsev, who spent the summer mainly in the mountains SW of Chukotska and close to the shores of the Okhotsk Sea, but also at a meeting of taxonomists and phytogeographers in Lithuania, where they had some workshop for certain problems that never are discussed in America of the molecular buffs and the pheneticists trying to hide their distrust in evolution...remember that my good old friend Bob Sokal, who invented the latter word and also the socalled numerical taxonomy that the computer people later took up for their mechanical approach to biology, is first and foremost an orthodox rabbi, who by religion cannot and will not believe in evolution because it is against the Bible...though he never says anything like that in writing, of course, when have you seen that the bigots admit their bigotry? Yurtsev tells me also that the latter part of Volume 8 of the Flora Arctica SSSR will be ready in October, it is a small fascicle that was inadvertently left out for technical reasons so that even the index is missing from the main volume. He also tells that volume 9 now is complete and will be with the printer late this month, and that when he and his wife have had three weeks vacation in the Crimea, the team working on the volume 10 will get together in Leningrad and try to complete also that part...time certainly has come to complete this fine review of their arctic plants...though now they must start a revision, not least because of the new Icelandic flora and its more exact nomenclature, as he says...he made the remark in his long Bot. Zhurn. review of the Arctic Atlas that among others Saxifraga should have been split. I hope you have all the eight volumes and also the fine checklist by Czerepanov on the Soviet flora as a whole, though he remains rather conservative, though highly revolutionary if you compare with those in America who write floras, except one.

When I wrote you and asked for seeds for Laane of Oxytropis, I also wrote to Boris. Since the species does not occur in European Russia or western Siberia and grows far away from his region in the far northeast, he contacted one of his colleagues in Yakutsia, who is sending seeds to Laane through the very formal channel of their seed exchange. But I suppose Laane has written to tell you that the seeds you sent first germinated, so now he has plants from abroad to cross with this rare Norwegian species, that Nordhagen, for no proper reasons other than geographical isolation, named as the ssp. norvegicum, without ever comparing it with the Rocky mountain or Alaskan material, which to me seems identical. We will see soon what Laane finds out about the morphological and cytological relationship, though I believe that all changes are so slow that nothing can have happened in the isolated Norwegian plant, though it may perhaps have been isolated for up to ten million years (million or more generations?), and come from the Bering Straits area with the ocean current when the straits opened up and let not only plants as this one and Carex lyngbyei and other coastal and alpine species float over the then still open Arctic Ocean on the top of the herring and codfishes and salmons and seals and whales and under all the many ocean birds that we enjoy to have even in Iceland. Unfortunately, "great" men as Gjaerevoll etc. who know everything in phytogeography and cytogenetics without ever have read such matters, have not yet observed this method of dispersal that explains the relationship of the Beringian-Alaskan and Scandinavian-North Atlantic flora that even they talk about, but the new generation will switch over...partly thanks to your help to Laane.

Jack was here on their way west, and we had good discussions. Since I am not sure he wanted me to report details to you, I stop here, though I may mention that we discussed the possibility to get A. Stählin to assist with the contacts. He may need judgements of my qualifications from able Europeans that Laane has, or want to get them himself, we will see. All the best,

Ask

27 Oct. 1983

Dear Askell:

I have received a question from a colleague in Vancouver who asks about the concept of Ciminalis that I am using. He points out that the lectotype selected by Holub is Gentiana acaulis L., which he rather legitimately questions as belpnging to the same group with prostrata, fremontii and the rest. He also points out that Tutin (1972) included includes G. acaulis in Megalantha (I can't comment at all on this because he didn't cite a paper). Of course, the ING doesn't know that a lectotype has been selected! Would I be right in thinking that G. acaulis goes with G. angustifolia G. brachyphylla, G. decumbens, G. djimilensis, and G. ligustica? In making your combination you didn't comment on typification.

This fellow (John Spence, U.B.C.) thinks that Dasystephana is the name applicable to Gentiana douglasiana! That, to me seems pretty wild. But he needs to know particularly why we use Ciminalis.

I happened to meet Phil Becker yesterday, after a month had passed when he didn't answer my letter about your file. He says that he knows that the file exists, and that it is in the office of the lawyer, and even in what drawer of whose sectrtary's desk it is to be found. If you think it would be useful for us to see it, I think the easiest way would be for you to give me your power of attorney in the matter, and I'll go get it. But if you do this, we should get it before anyone is given the information that we know about it.

The Lund exchange closed down, and instead of ordering from the list I simply asked Ingrid to send me what they had, and this week we got seven very big, heavy packages with probably three or four thousand specimens, a real windfall. Of course, they want me to continue to exchange as an institution, and we no doubt will. The exchange has built up our European collections a great deal.

Next week I shall get your book review finished. One thing at a time.

As ever,

San José, November 1, 1983.

Dear Bill:

You can refer your Canadian correspondent John Spence to Holub's good paper of 1973: New names in phanerogamae 2. - Folia Geobot. & Phytotax. Praha 8:155-179. In it Holub points out that whereas Adanson's Ciminalis cannot be properly typified by aid of the information given in his book, typification by aid of the exclusion method has later been accomplished: G. pneumonanthe was separated as the genus Pneumonanthe by Gleditsch in 1764, for which it is the type species; Moench (1794 then adopted the Adansonian generic name for G. acaulis L., without mentioning the other species given by Adanson, thus automatically typifying it. So typified, the genus is taxonomically and morphologically and even karyologically very uniform, and includes, of the species you mentioned, also G. angustifolia and G. ligustica, but G. nivalis, tergestina, orbicularis, pumila, terglouensis and utriculosa belong to the genus Calathiana...I do not know right away where to place decumbens and djimilensis that you also mention, and am too lazy to look them up now, since that matter little in this context. But the section Megalanthe of Kuznetsow, which Tutin accepts in Flora Europaea 3, is synonymous to the genus Ciminalis.

(Yes, it has 2n=26 in the Queen Charlotte Islands, same as Pneumonanthe and some other genera)

I am not familiar with the endemic Pacific coast species G. douglasiana except from pictures and descriptions, and, unfortunately, I have no information about its chromosome number, which is vital for its placing in some of the available genera. However, it does not seem to come even close to Dasystephana, which we, 1976:222, in a paper on The natural genera of Gentianinae.- Recent advances in botany, ed. P. Kachroo. Dehra Dun 1976...typified with D. asclepiadea (L.) Borkh. It is an Asiatic taxon with one other species: D. schistocalyx (C. Koch) Löve & Löve, and its basic chromosome number is 11. I have the feeling, but only feeling, that this taxon may belong to Gentianella, though perhaps it may be closer to Arctogentia, the genus we recently separated for G. aurea and its few relatives that at least may come close to parts of the area of G. douglasiana. But that problem needs much more studies before anybody can say anything with certainty about its correct place in the system...except that it does not belong to Dasystephana, unfortunately.

Have you not missed the deadline for the earliest 1984 number of AAR if you did not complete the review two weeks ago, or did you make an agreement with the editor? Hope so, because otherwise the readers will not know of the book until early in the summer...but does that matter? I have just had a letter from the publisher who wants a slightly revised third edition of the Icelandic text as soon as possible, so I will have to do my best in that soon. That will mainly be for those who read Icelandic, and I plan to keep up the scientific part mainly in the English edition in the future, since it certainly is of more interest outside than inside the country...and for tourists, we hope.

I am shocked to read that even the Lund exchange has closed down, and wonder why the taxonomists let the physiologists kill all Swedish and Nordid botany...not without the assistance of the socalled molecular group? Is all botany to go that way and to be replaced by nothing worth while...as in Boulder?

Thanks for the information from Phil Becker. I wonder if we should not wait with any action on the file until Jack returns soon and something else has been thought of, it is hardly necessary to rush after all the years nothing has been touched?

All the best,

Áskell

San José, November 2, 1983.

Dear Bill:

When I wrote you yesterday about Gentiana s.lat. for the benefit of John Spence, I had the feeling that I ought to know more about G. douglasiana, but my memory tricked me so I forgot even that I should look it up in Kusnetzov, and also felt that I recognized some group on the drawing in Hultén's Alaskaflora, and in the Vascular Plants of the Pacific NW,4. When I looked these pictures up again just after the mailman had taken the letters today, I suddenly observed that this is a species of the Gentiana sect. Chondrophylla Bge, so I looked it up in Kusnetzov (1895) and, alas, there it is listed, of course. I admit never having seen the species myself, but since I have separated the group and united it with section Megalanthe earlier, and later observed my mistake and given it a separate description and name, I ought to have recognized it better yesterday. Though there are some other species that we now know belong to the later described genus Kuepferella Laínz, which has x = 10, the western American taxon clearly belongs to section Chondrophylla s.str., which we in 1975 distinguished and described as the new genus Holubia...and replaced with the new name Holubogentia in 1978, when we discovered that Holubia is long since taken in honor of another and much older Holub. The type of that genus is H. pyrenaica, it has the basic number x = 13, as Taylor & Mulligan (1968) and Pojar (1973) have established for the NW American plant. Unfortunately, we did not transfer all the species of that genus from Gentiana, because we described it in a short paper in a Festschrift for one of our Spanish friends, but I would advise either you or Spence to do this, the faster the better...and so solve a problem that nobody in America may have observed that has at least been partially solved, ~~except for the transfer,~~ because who reads here a Spanish journal that is mainly written in Spanish and preferably about Spanish phytogeography? According to my notes,* you ought to have copies of our papers involved so you could perhaps give him xerox of these, but for the sake of completeness I list here below the texts mentioned above.

Hope you forgive me that I tried to be fast and thus actually misled you...but perhaps it is better to wake up so fast and soon than to forget the entire matter and let both you and Spence believe that nobody actually knows where to place the species in our new system...or even in the old one, because who has Engler-Prantl at hand and looks up the old Gentianinae system of Kusnetzov?

All the best, again,

Áskell

Kusnetzov, N. 1895: Gentiana Tournef. - Engler-Prantl, Nat. Pflanzenfam. 4(2): 80 - 86.
Löve, Á. & Löve, D. 1975: The Spanish gentians. - Anal. Inst. Bot. Cavanilles 32,II:221-232
15169 Löve, Á. & Löve, D. 1976: The natural genera of Gentianinae. - Recent Advances in Botany. Prof. P. N. Mehra Commemorative Volume, ed. P. Kachroo, Dehra Dun:205-221.
Löve, Á, & Löve, D. 1978: Holubogentia, a new name in Gentianaceae. Bot.Not.131:385.
Pojar, J. 1973: Levels of polyploidy in four vegetation types of southwestern British Columbia. - Canad. Journ. Bot. 51:631-628.
Taylor, R. L. & Mulligan, G. A. 1968: Flora of the Queen Charlotte Islands. Part 2. Cytological aspects of the vascular plants. Ottawa.

* Since I am not quite sure that you have the two papers of ours of which I still have some copies, I enclose them so that you may save some time for Spence by copying them even though you may not have them. But the main review is out of print as our reprint.

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

JOSE, CA 951
PM
18 NOV
1983

Robert Morris Patriot
US Postage 13¢

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

San José, November 19, 1983.

Dear Bill:
Thanks for the call. To be sure that my fast reaction and answers do not mislead you, I must mention that since you are using DeC. name as a basionyme, you do not need to correct McArthur's & alii misspelling, though their name must be mentioned as a category deciding reference...the spelling will be yours, not their.
With the ending um in Seriphidium, a in Artemisia, where their combination could be automatically corrected, with "Tridentatae" in parentheses:
Seriphidium sg. Tridentatum (DC.) W.A.Weber, comb. nov.,
based on Artemisia subsectio Tridentata DC. 1837,Prodr.VI:105;
Artemisia sg. Tridentata("Tridentatum")McArthur & alii....
Typus generis: ~~Artemisia~~ Seriphidium cana (Pursh). And the list of species...
Do you know of some Chinese or other new genera split out of Gentiana or Lomatogonium-Pleurogyne since 1970? I know I have one reference but cannot for my life remember under what name it has been filed in my almost 39000 reprints...old age of course
And do not have Kew Index after vol. 14. All the best,

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

SAN JOSE, CA 951
PM
15 NOV
1983

give
The United Way

Robert Morris
Patriot

US Postage 13c

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

San José, November 23, 1983.

Dear Bill:

Thanks for two good cards. I do not know the two prairie Artemisiae, but if Hall & Clements' sect. Dracunculus coincides with that of Besser, then a transfer is needed to Oligosporus. The spelling Tridentatae for the subgenus, borrowed from the sectional spelling by Rydberg, should, acc. to the Sydney Code just received, Art. 73.10, be treated as a wrong use of termination or an orthographic error to be corrected into Tridentata. That, however, prevents the use of the excellent Steppea at this level, so if there are not safe grounds to regard it as an equivalent to Seriphidium as to level, then you will have to find another use for this excellent name.

Sorry about Paul and Diana...and most for Mary.Such is life

And pity that Barkworth missed the Ptilagrostis point, but also that can be corrected so truth prevails, she is learnin

Have you seen Jack and mentioned the file to him? And since you say nothing, I suppose I was right in guessing that Fosberg backed out?

All the best to you all,

Áskell

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

SAN JOSE, CA 951
M
9 NOV
1983

Robert Morris Patriot
US Postage 13¢

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

We do not envy you the [illegible]!

San José, Nov. 28, 1983.

Dear Bill:

Thanks for the Nov. 21 postcard, just received. No, since you include the species <u>tridentatum</u> and its relatives in your subgenus, there is no way around the fact that the the oldest, though recent, subgeneric name is sg. <u>Tridentatum</u> of McArthur & alii, which therefore must be used at this level, such is the Code. But you are free to include in it all the three sections of Rydberg and to widen its circumscription accordingly I suppose Rydberg's name was their basionym so the auctors' names will be (Rydb.) W.A.Weber, but though you must mention their combination because it is rank-deciding, you need not to include their names more than in that reference, which you may correct if you so feel even in their combination. But your combination must be given as comb.nov., not as comb.& stat. nov. because the status they validated.

There are two corrections to be made with old <u>Matricaria</u> in the new Colorado list...but that discussion can wait for now

All the best, Löshell

UNIVERSITY OF COLORADO, BOULDER

Museum

11 August 1983

F. R. Fosberg
National Museum of Natural History
Smithsonian Institution
Washington, D.C. 20560

Dear Ray:

I have finally found time to xerox a few of the pertinent parts of the Askell Love dossier and am sending them to you. Probably the most important piece to start with is the one-sheet Yugoslav embassy statement to Dr. Susnik telling of the proposed intention to replace Love (I appended a translation of this). Then the meat of the matter is in the letter (original and my transcription enclosed) telling about the "arrangement" for getting the money; this was the petard on which Askell was finally hoisted. That letter to Schmertz was never answered. Then I enclose the details of our last official attempt to get some justice through the University. This failed, I feel, because the university attorney who was in cahoots with the Chancellor at the time later became the private secretary of the next Chancellor, Russ Nelson, who reviewed the case.

If I am to believe Askell, and I see no reason not to, he tells me that whenever I contact anyone here about the case, he gets threatening phone calls (indirectly to me too) at his house reminding him about the disappearance of Professor Riha. I have suggested that he get an unlisted number, but in his state of poverty he probably can't afford it.

Incidentally, all records of Askell's presence at the University have disappeared. They were borrowed by the University attorney's office and never returned. There is no record of Mary Kirk having ever been a registered student here.

The only thing that might help us at all, it now seems to me, is to get Dillon Ripley involved and investgate the person at Smithsonian who was responsible for this whole tragedy.

I hope that you have some ideas.

Sincerely,

Bill

William A. Weber
Prof., Curator

Henderson Building • Campus Box 218 • Boulder, Colorado 80309 U.S.A. • (303) 492-6165