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

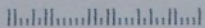
Hunt Institute was dedicated in 1961 as the Rachel McMasters Miller Hunt Botanical Library, an international center for bibliographical research and service in the interests of botany and horticulture, as well as a center for the study of all aspects of the history of the plant sciences. By 1971 the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

5780 Chandler Court,
San José, Calif. 95123.



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

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P.S.: Has Jack seen you?

San José, February 3, 1984.

Dear Bill:

Welcome back, and it is evident that the Galapagos have been something special this time, and that it will take you long time to sort out all the experiences and memories.

Hope the New Combinations paper will be in order, and so is the nice review, except that the book looks somewhat smaller than it really is, since the title says it is only 40 pages, instead of 403! But it is probably too late to correct that now, and most readers will not see the error.

We have been having a warm and dry January since the first cold and rainy week left us...but I and Lóa have been having a long cough after a troublesome cold that I got just when my only uncle and his family were with us for a week in early January. It will go away before the spring, when we expect the proofs from Feddes of the long Triticeae paper that Cronquist seems already to be discussing and degrading in letters to Utah. That is his pleasure, I am not fond of subjective claims in so-called science by pheneticists. All the best, *L. Sheen*

San José, February 21, 1984.

Dear Bill:

Try to excuse my slowness, but I had to spend considerable time on composing a short commentary on our 1956 Icelandic conspectus, which still seems to be mentioned a good lot...though hardly in pheneticism-Lysenkoism America...so the editors of Citation Classics of the hardly great but economically profitable Current Contents wants to include it in his special series. That is not too bad for a work almost thirty years old, but it has also affected many in Eurasia and does so still...even the Swiss are beginning a great study on the same idea...though in America it only irritates the delinquent pheneticists. A pity that none of my great judges at the so-called molecular-cellular branch that impressed the narrow-minded administrations with their judgements of my standing as a scientist so they shot me down have gotten that kind of works published...but why should they do that when they only talk about themselves?

I would not cry over the small printing error in your good review...though those who see it may become pleasantly surprised when they find out that instead of paying much for only 40 pages, they get ten times more for their money! I have heard of reviews coming from some Russians and Czechs, but it may take one more year before we see them, if ever.

I have recently dug out our notes on experiments with *Matricaria* and found that already in Montreal we had a good deal of results that we then could not quite deal with taxonomically. Although these experiments support the division into *Matricaria* and *Chamomilla* by Rauschert, the latter genus still is too heterogenous, since it includes two taxa that certainly are strong genera in their own right, from various biological and morphological points of view that none of the great students in recent decades seems to have caught. Since one of these genera occurs in Colorado and is an erosiophilous mountain plant and a shore plant on this continent, this ought to be publicized...perhaps we should do that together, in case there are sum funds available for *Phytologia*, though the article might also be accepted by *Taxon* or other such journals, despite oldfashioned so-called reviewers selected to prevent progress? Perhaps worth thinking of?

Although I knew your "*Arenaria*", or "*Minuartia*" *nuttallii* of McNeill and had realized that it is fundamentally different from the three eastern taxa that he puts in his highly artificial section *Sclerophylla* that Mattfeld included in as many series, I must admit that I had forgotten all about it recently. If you look up what they call *M. dawsonensis* and *M. michauxii*, you will probably come to the same conclusion as Hultén did in his scientific *Alaskaflora*, that at least the first one is nothing but *Alsinanthe stricta*...and I believe he would have agreed with my conclusion that all the eastern taxa belong to this genus and variable species as subspecies and varieties,...except the western plant. The former have 15 as a basic number, as the other species of *Alsinanthe*, whereas the basic number of *nuttallii* is simply 9...and the morphology distinct. It is possible that a better taxonomist and florist than the then young McNeill might have observed similarities to some Asiatic more restricted genus for this plant, but since he did not do that and we do not know more than he did, we might perhaps do right in finding for it a new generic name...the Russians may then later prove us wrong and replace it with their probably older name...if at all? We already have a valid description in Mattfeld's *sectio Pungentes*...

If you do not dislike it or think it better to separate such matters from the ordinary problems you are solving in the Colorado flora, perhaps you might permit me to make the transfer with you in the next number of the additions? Whatever your decision...and you could do this alone, of course, with my consent to your full use of the following...the taxon must be given a name as a genus...based on Mattfeld's description, of course. Any name would be acceptable, but if you do not have something in mind already, perhaps one could call it *Minuopsis*, by combining *Minuartia* and *Alsinopsis* that have been used for the collective group previously? Then:

Minuopsis Löve & Weber, or simply Weber...based on *Minuartia sectio Pungentes* Mattfeld, 1921, Bot. Jahrb. 57, Beibl. 126:28.

Minuopsis nuttallii (Pax) L. & W. or W...comb. nov., based on *Arenaria nuttallii* Pax, 1893, Bot. Jahrb. 18:30, in obs.

Arenaria pungens Nutt., 1836, in T. & G. Fl.N.Am 1:179, non *Clemente* 1816, in *Legasca*, Gen. et spec. plant.:15;

Minuartia nuttallii (Pax) Briquet, 1911, Ann. Cons.Jard. Bot. Genève XIII & XIV:385;

Minuartia pungens (Nutt.) Mattfeld, 1921, Bot. Jahrb. 57, Beibl. 126:28;
Alsinopsis occidentalis Heller, 1912, *Muhlenbergia* 8:96.

I doubt that the varieties mentioned by Hitchcock, 1964, in *Vasc.Pl.Pacif.* NW 2: 258 - 259 need to be mentioned, since at least as far as informations available to me are concerned they seem to be doubtful. But you may feel otherwise and want to accept one as a major geographical race as indicated by Leo?

That is all that I have to say today, so I stop here, especially since our mailman is coming close as we can hear from the dogs in the neighborhood and even by the interest of our two...Ingela has just added another puppy, black and friendly, to the old one for us to take care of...it will not make it easier to leave the house both at the same time, since the older becomes sick if he gets into a car, even before starting, and the little one chews things as normal for his age...but both seem to understand when other dogs advertize the coming of the interesting mailman!

All the best,

Walter

P.S.: Jack asked me two weeks ago to get certain informations from TIAA regarding my so-called pension and they promised to react at once. The letter still is not here...perhaps they have had to get written permission from Boulder?? I observed certain peculiar language used by the State Department murder organizers for Central America that got me to think that they might call our case an "unlawful and arbitrary deprivation of means to live and work"...they spoke simply about deprivation of life when talking about their murders! You are an unamerican person in more than one respect, perhaps most botanical since in your peculiar ideas you even come close to the despicable Europeans! That also fits Mary Barkworth, who has sent me, confidentially of course, a copy of a letter from Cronquist in which he slanders me as if he was speaking about the devil... did he get us to Boulder and the States to try to prevent us from working, and was he one of the reviewers of our Yugoslavian project at the end? I would like to see the documents the Smithsonian crooks put together, can anybody get copies?

San José, February 28, 1984.

Dear Jack:

When you phoned on February 7 and asked me to contact TIAA, it was too late in the day to reach an office in New York, so I waited until February 8. The 800 number you gave me referred me to a collect call to (212) 490-9000, which is the Annuitant Information Center. I asked them, as you had said, for a letter clearly stating what part, if any, of my monthly TIAA-CREF pension payment derives from the deduction from my salary at the U. of C., and what, if any, from contributions by the U. of C...and repeated this question twice and emphasized its importance. I believe I gave the secretary proper time to write it down. She promised me an immediate answer. When no letter from the TIAA had come on February 23, I phoned the same collect number again and asked for the answer...and was told that they had had to contact the University for this information, which they had just received, so a letter to me would be forthcoming soon. That letter arrived yesterday late in the afternoon, so I could not get this letter written and sent to you until now, because the office where I get my things copied had closed. I am enclosing a copy to show you that this is a long form letter of little significance to the question...until they add on page 2 a paragraph, which evades the question, as I expected, these are businessmen cooperating with the establishment. I do not think I can do more, since they will evidently always evade answering me honestly, as earlier, but if it is the University that is hiding the facts you can find some greater power to force the truth out of them...if you then want to continue without getting straight to the real matter and its astonishing truths that are even worse than the good old political Watergate and no less political if you look closer. Although I realize that you have never discussed the matter with an outsider with power or fortitude, I still hope you and Bill still can find somebody at the hopefully honest new administration, or a politician one can trust that dishonest administrators are nervous for, as we know the Boulder crooks are for Pat Schroeder, or a determined and fortitudious lawyer (contingency of course) ready to look closer at the entire scandal, or even an immutable journalist who feels he could open this Boulder-Washington Watergate. If you are interested in getting help from an independent colleague of mine whom everybody can trust, you and Bill could contact Douglas Dewey at the US Dept. of Agriculture in Logan, Utah, with whom I have been working on the new taxonomy of the wheatgrasses for years, and his colleague Mary Barkworth at the Biology Department, who has recently received a letter of "warning" about me and my opinions from the great Arthus Cronquist, the main power getting (luring?) us into the US in 1964 - a clear blackmail if it had been published...I have read a copy though that should be revealed to nobody for obvious reasons, Mary is young and independent and should be spared the treatment we have gotten...and many others since Rafinesque. No more this time, you read between the lines.

All the best...but time is running out...

as ever,

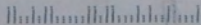
P.S.:

You missed the citation information on the *Acetosella* paper that is in the title of the abstract!



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San José, April 4, 1984.

Dear Bill:

Thanks for the "New names" part of Phytologia, it is good. But the haste with its completion caused at least one minor mistake: on p. 7 the subgenus must be Tridentatum, since the generic name is neuter, but the corrected spelling of the basionym is correct, since *Artemisia* is feminine. No great thing

I have been digging into old material of crosses within the collective *Matricariae* which clearly show biological etc. differences that make the two genera of Rauschert and Flora E untenable: we will have to revive *Cotulina aurea* of Pomel (1874) and *Lepidotheca suaveolens* of Nuttall (1841). I will do it preliminarily when publishing their chromosome numbers in the fall number of *Taxon*, and then hope to find energy to write a short paper about the reasons for this...according to Polyakov and his morphological studies they belong to distinct subsection and we never succeeded to cross them and found the N. Am. taxon to be autogamous, the others allogamous, etc. All the best... but has anything happened with the file and pension... *z*.

Cassin
August 29th.

6 mm.
1. 2 mm (f.)

less than 0.5 mm below
Cassini

San José, April 15, 1984.

Dear Bill:

Although I know that you are busy working on the Galapagos plants, I hope you feel you need some little rest that then might perhaps be gained when you help me with two small matters:

I have been looking closer on old material we have on record for the autopoloid series of *Dactylis glomerata* s.lat., which is rather similar to *Acetosella* in several respects. Then I stumbled over the fact that in our Spanish series we evidently have reported ssp. *hispanica* as a tetraploid taxon, though my notes indicate that the specimen in question does not fit its description as having a root-stock that is stout with bulbous-based tillers; slender, stiff, tough leaves with markedly toothed margins, and slender, wiry culms up to 60 cm tall; heads compact; florets short with the lemma having pubescent keel and lobed apex. Would you be able to help me look closer at the specimen N 0193 from Pantano del Tranco in the Sierra de Cazorla, which I believe is in Colo? Thanks.

We have succeeded in getting the chromosome numbers of the three Australian taxa of what I plan to call *Australopyrum*, from seed material collected by the Canberra officer Roy Patten, who went to considerable troubles to search for them in the mountains; they are all diploid so the genus is as good biologically as it is distinct morphologically and geographically. The numbers will be published in the Taxon list in November, together with numbers and the correct names for *Matricariæ* s. latissimo. I have specimens collected by Patten that are duplicates of what he left in the Canberra herbarium, and list them as COLO, so when you come next you should bring them back to Boulder...as far as I know there is nothing of that kind even in Washington.

The second matter that I wanted to ask about, not report, regards an item I have just seen in Koeltz Scientific Book, Catalog 295: Dorn, Robert D. 1977: Manual of the vascular plants of Wyoming, 2 vols., 1977; 395 DM! Who is this Dorn and what is his background? I had observed earlier in the miserable Flora of Alberta that Packer accepts his new name for one of the, as far as I understand good, *Salix* species, and Breitung had also made that observations but had been unable to locate the reference or original description nor find the author himself? Is this one of the young men who believe there is nothing to writing a flora, or is he perhaps good and better than the older and naturally miserable generation? I admire young men who dare to try to write floras in a land without that kind of luxury...though hardly if they do not understand the Linnaean tradition better than the large and strong-voiced Art and believe in creation as Reagan and other great scientists of this continent and times. Who is he and what is your impression of his expensive work?

Otherwise nothing remarkable from here...but the spring has become established here with warm days last week, and we have never seen as great flowering of our *Rubus* and the citrus trees so we seem to be able to continue to enjoy them next year as this one. And even the snails are less frequent thanks to drought. The aspen has probably started in sheltered localities in the Rockies now?

All the best from us all to you both,

As ever,

San José, May 1, 1984.

Dear Bill:

Many thanks for the good letter of April 22, which arrived today, the snowstorms evidently delay the mail as if it went by horsebuggies still. Especial thanks with your efforts with *Dactylis*; I am not sure if our identification of the Spanish plants is correct, since it is tetraploid, because we also have material from other parts of the southern Mediterranean that are hexaploid and undoubtedly fit the description of hispanica...though I am afraid that this autopoloid series may be as difficult as *Acetosella* so that only the chromosome numbers are decisive? We may perhaps get some solution by time, though perhaps the next generation may have to solve the problems...and if American tendency to ignore genetics in favor of phenetics, or to ignore the influence of nature contra nurture and forget all about Johansen's and the plant breeders understanding of the fundamental differences in the evolutionary significance of the phenotype (which is hardly any) and the genotype (which is basic to evolution from their point of view) and turn back to creation (as Reagan and other wisemen do, including Cronquist?), then this does not matter and taxonomic botany will return to being only an art. We may not see... but the world is turning back into less education and more foolishness and less honesty and absolutely no interest by so-called scientists in looking for the truth.

Since I received the enclosed envelope with a copy of a wise definition by Rollins that he later abandoned because he understood that in this land of his there is little wisdom in following genetics if one wants to get somewhere in botany, I have been expecting to get another letter in which the evidently left out real letter might turn up. Since this has not happened, I am enclosing the envelope that to me looks as if it had been opened and then glued together again? Possible? I have also been waiting to hear from you and Jack about your efforts to find the facts of my story that you thought might be hidden in the eternally lost and refound so-called files at the University...I sent him an authorization to ask for that file early in March but never heard more. Perhaps other letters have been lost and then also the reprints of the Böcher obituary that Katherine promised me long ago and I expected when the journal itself arrived? I hope you also have seen the answer I got at long last from TIAA when Jack forced me to start a discussion with their clerks on my retirement fund money...of course the TIAA clerks had to ask for permission in Boulder that took weeks to get, and then said almost nothing... and I had made a fool of myself by asking them rather than ask you and Jack to contact somebody with authority in Boulder itself, where all the information certainly is met with. Though I still cannot see the importance of such an action in order to find the truth in a matter that the next generation may believe was the same kind of a witchhunt that the "great" and rich Asa Gray organized against the much greater but poor Rafinesque a century ago or more. But perhaps you feel one should not try to solve problems that are based on witchhunts... though there may be others with fortitude and great conscience who would like to help...if they are only contacted by my friends? I know and trust that you do not misunderstand me and that our situation does not disturb you too much when you have to use all your valuable time to complete the new flora...which will be as good as that by Dorn is evidently bad...you know that Packer adored him? Thanks for the information about who he was and is and for the review...where printed?

Our spring is late, as yours, but comfortable and filled with fights against pests that want to destroy some of our trees, as usually. But some of our vegetables are already ripe....but the tomatoes still waiting for getting into the soil.

All the best from us all to you both, *Wick*

San José, May 15, 1984.

Dear Bill:

Thanks for your letter of May 7 that arrived on May 11. But the letter you say you sent before the piece with Rollins' generic definition, still is not here, for some reason that I cannot guess. But it astonishes me that you say in the letter I received that you had been speaking to some "ombudsman", probably some with good contacts with the old gang and probably one of them, as the political professor you once contacted for help, was his name not Garnsey? That was a change from when Jack told me earlier, and when he asked for my authorization for him and you to ask for my files because at that time he said he had a good contact with a friendly chairman of the committee on grievances. But Jack has evidently not even shown you the letter and copy regarding the efforts to get information through the clerks of TIAA, since then you would hardly have felt you ought to continue to let me be harrassed by asking me to contact again the TIAA clerk add thus make a fool out of myself...since it is evident that these clerks can do nothing and have no information more than that the University sends them a final sum without details, whereas the details are naturally found at Boulder. And not through some clerk as Phil Becker or others, but through somebody with authority and fortitude greater than that we have been demonstrating. Perhaps your "ombudsman", whoever he is? No problem has ever been cleared through going through the lowest levels of the bureaucracy, least of all when those higher up have done something criminal, as, e.g., breaking the constitutional rights of even the greatest of criminals, though I am slowly beginning to realize that as an immigrant I have not only any right to a human treatment but also am thought of as an enemy of the state and of its scientific opinions and you add what you feel I mean, so perhaps you are trying to tell me by your way of acting on my behalf that you are afraid that this is beyond your power? Though not your good will...but what about Jack...do not show him this letter as you did Carter when I had mentioned my thoughts that perhaps the matter did not fit into his business, with which I meant nothing bad? And do not let Jack stop you too long from doing something wiser, even asking some of the rare men with trust, sincerity, candor and reliability in Boulder to read all the material and advice you and he if they so feel...I know that you do not share my feelings for Pennak, but of all those superior to me and older that I met in Boulder, he certainly demonstrated to my satisfaction that he is one such man...and I would trust him as strongly as I trust you. But do something else than listen to Jack's peculiar advice, I suppose he advised you to change the sentence around Askellia so that even the truth about the ostracism went out? I have phoned again to TIAA and talked to the same person who clearly was embarrassed as I was myself, and repeated his earlier information that he is powerless and that the information is in Boulder only. But can you see it as something more important than the main points that prove a treatment in the same style as Rafinesque got from the rich Asa Gray...and all the small men until Merrill stood up for him? Could you not become another Merrill for us, ask for files from the CIA in Denver and elsewhere, the Smithsonian, NSF and The University, and even get for me at least a list of my many applications for funds at the NSF that were denied by aid of clearly unqualified and non-genetical "reviewers", with no standing to judge my works? But perhaps you find that too much...though in that efforts you could perhaps get the official help of our good consul? or by some great colleagues, e.g. Cronquist himself who lured us to the rotten place where you have also been mistreated?

We are happy to hear that you are proud of becoming a member or fellow of the British Linnaean Society... a place closed to those who dare to challenge the old Hooker ideas, of course, and sometimes almost like a stamp of conservatism, as one of my great British colleagues once told me. And I envy you the trip to London in the spring, which is something very special, and hope you take the opportunity to spend at least a day at Kew, where you might even discuss our matters with Keith Jones, an old friend, who once told me, in Canada, that he could not understand that we had gone to the States to where he had gone reluctantly at the Seattle meeting. Perhaps he could strengthen your views of our works... or give you an opportunity to change your opinions on such heretics as we are?

Although I once looked somewhat on the giant and complex *Rhododendron* and got the opinion that it needs modernization at least in the northeast, I am hardly familiar enough with *Azaleastrum* to have any opinion. Though my knowledge of your sensibility in taxonomical matters indicates that you are right in separating it from the main genus. But in the case of *Lonicera involucrata* I believe you are right in moving it to *Distegia*, though we know no genetical reason for doing this, only morphological.

The large earthquake shook our house considerably and also the streets close to the post office where Doris was parking... but I and the dogs felt it here in the kitchen where they came running and clearly believed that I had done this to them! Some small things fell from the shelves and the house itself went in waves so it looked as the light in our living room swung almost half a meter... but that may have been an illusion. Otherwise nothing here... but in Morgan Hill, where fools have built over the Calaveras fault and not even fastened the houses properly to their foundation, several even newly built houses were badly damaged. But we prefer earthquakes (which we are used to from Iceland) to twisters, tornadoes and hurricanes, not to mention flooding that we hope you have not had this spring either.

Our garden is better than ever, and we are already harvesting a good deal and have sown some vegetables for the second time this year, and the berries and fruits are slowly coming, the blackberries will be ready after a week and then hopefully for a month afterwards, and then we are close to the apricots and plums. But although our tomatoes are in the ground since two weeks, they grow very slowly, because the weather, though sunny, is so cool that we still have to have some heat on some of the evenings and mornings. So perhaps the usual heat may not come, though that is unlikely.

I realize that some of my words may sometimes hurt you, though this never is intended, and that you may sometimes feel that I am ungrateful for all that you, and you alone, have been trying to do for us. But even friends may sometimes irritate each other to strengthen the connection, and you ought to know that I am more grateful for having you as a friend than for anything else in your peculiar land. Though I still would mention that it would be good if you and others could help us at least to find out what are the facts around the treatment we have experienced for doing just what our conscience and conviction in the scientific methods as a way of looking for the absolute truth... which evidently still is not what our politically thinking so-called colleagues feel one should do.

We hope you will find an excuse to visit us properly and alone sometimes during the summer to lift our spirits... perhaps even with Dewey or others of his quality if you want to have support... but mainly to plainly sit in the sunshine and talk about matters that long have interested all of us. But you may not find time..

All the best to you both from us both,

Lish



20 May 1984

Dear Askell:

Your words don't hurt me, but I think that there is no use to continually tell me the things that I already know just so that you can let off steam. The most part of your letters continue to spend most of their length on bitter diatribes on anybody and everybody, including me; what on earth do you have against my accepting a fellowship in the Linnean? You certainly are not going to be able to bother old J. D. Hooker's sleep after this length of time! I hope that you will some time soon try to write me a letter that talks only of other matters. You should save your energy for using it when it counts.

This leads me to a few necessary remarks, which I have probably made before. First, on the TIAA matter, it is squarely up to you to continue to write to them for the proper answers to your questions; they have to do it. No one else is going to be able to do it for you. There is no reason to believe you have been swindled unless you can cite figures. Just suspecting that you might not be getting enough money, and having no concrete evidence, will not do the job.

Second, I am coming to the conclusion that no one is going to pick up your case except yourself. How on earth can I go back to the administration, having done it once and been given the answer that I got. You yourself, if you expect to have any satisfaction, are going to have to make your own personal appeal to whatever body you think would be appropriate. Whenever I have suggested that you come along with anything I have tried to do, you refused, preferring others to take on the load and do it for you. John Alden was told, in another situation to "Speak for yourself, John." You are the person who can get whatever files you want from whatever place under the Freedom of Information Act. Why must someone run all of these errands for you? I sometimes wonder if you really want Pat, or Jack, or me to help, or whether you would rather prefer to just be a martyr and continue to bombard me and others with vitriolic attacks on them and us?

Also, I would suggest that you choose your confidantes more carefully. You are trying to get a sympathetic ear from a very poisonous woman at Logan who has been causing a great deal of trouble to someone I have been trying to help. Your friend is very insecure, bitter, and generally nasty. I wouldn't trust her.

No one persuaded me to change the wording of the Askellia either. I decided myself that this was not the place for it.

Bill

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

San José, October 5, 1984.

Vaccinium
gundlachii in Japan

Dear Bill:

I owe you a lot of apologies for not having thanked you for the card from Kobe, the letter from early September about the Japan trip and some more findings of good Colorado plants, and for your last letter on Veronica etc. There are some excuses, as always, family affairs and illnesses of the usually milder kinds, with one exception, the long and unbelievably hot September that broke all records, letters from others who know me less than you do and so had to be answered at length, especially when coming from students at institutions of higher learning, so-called, but without libraries, as were Nelson and others who made western botany...and make it still, and other letters that irritated and had to go unanswered, as one from the selfish Arne Strid that confirmed my earlier impression of him as a first-class rascal now trying to get me to join him and the Ehrendorfer clique in their unbelievably malicious slander of one of his Greek students, who may become excluded from their "club" and even from a Greek position that nobody fits better into...because he dares to think differently...I was involved in his contacting Strid at the first point in 1973, when he wanted to join us at Boulder because of his own interest in the alpine flora of Greece that Strid and the Vienna group still think is their privilege. I know you understand and forgive my tardiness, as always before, since your friendship is a true one that I value immensely...as my occasional barbs towards you show.

I am not astonished that you enjoyed Japan when travelling in luxury at the cost of its not too honest businessmen who support the not too great scientist Miyawaki and his German and often former Nazi group that hung themselves on Tüxen and Braun-Blanquet without understanding their evolutionary philosophy. As you know, I was with them in 1974 thanks to Tüxen, an old friend since the Irish IPE, and had the pleasure to thwart some of the political efforts of the condemned Nazi Ellenberg so only he and his closest friends signed a stupid "international" declaration in support of Miyawaki and the Nippon Steel fraud. That and the fact that Tüxen is no more is probably the main cause for that I was not there this time and that you replaced me, fortunately for both of us...if Miyawaki and Ellenberg had invited me, I would have declined, despite the fact, as you say, that Japan is Japan and that there must have been several old and good friends in the group. Do you have a list of the participants and a short itinerary? I am sure you enjoyed it thoroughly nevertheless, and rested from botany.

Then to the first letter and its agenda: Nice that you found *Salix lanata* ssp. *calciicola*, one more of the arctic relics...it is probably hidden elsewhere in the mountains under the species name alone...or simply hidden because nobody with a sharp floristic eye has looked closer before. Also fine that you rediscovered *Baeothryon pumilum*, we always looked for it in vain when we were in places where it could be expected. The *Scirpus multicaulis* that bothers you because this old-fashioned name lures you to think of other groups, is, indeed, given in Hylander 1966, *Nordisk kärlväxtflora* 2, p. 29, as *Eleocharis multicaulis* (Sm.) Desv....though with Sm. as the last author. It is also listed in our 1961 Chromosome numbers book on p. 61, and in *Flora Europaea* 5, pp. 283 - 284.

Thanks for the clipping regarding the refusal by the University and even the Supreme Court to make available the "evidence" in the Ueberoi slander case...this does not astonish me a bit because I have long realized that although the KKK was outlawed pro forma in 1923 from Colorado, it still dominates the bureaucracy and even the politics so that even Hart did not dare to look properly into the Riha case. Not least in Boulder...and where would you place Coors and Ann Burford and Betty Willard? And many more. This may also explain that my letter to the President's office

sent in medio June at your instigation and requesting copies of my "file", has been thoroughly ignored. Why should they acknowledge such matters that then could embarrass these "law-abiding" citizens who would rather see it filed in the round file and amply forgotten...they know that though my friends might ask about it, they would not do much more after their long experience of negativism and lies. In that case, however, the fact~~s~~, told to me by Jack at his return from Iceland, that the crooks have sent the slander to Iceland clearly to hurt me more, may make it more reasonable than some years ago to ask for the help of the Icelandic Ambassador (not his subordinates!) and the Icelandic Government to get copies of the slander both from Boulder and Washington, as you and Bill Carter wanted to do some years ago and I then prevented because I did not want to affect negatively the weakest NATO link by complaining at home about the unbelievably fascistic and unlawful treatment we experienced in this heaven of "democracy" - not to forget all the more or less official slander and lies about us in America itself and likely also in various parts of Europe and Asia? If you both still want to try to help in that way, you now have my consent. And if you want to contact those who have known me and my ethics in Iceland and Sweden, I now would furnish you with names. Though I myself have not seen the "file", a good and old friend and colleague whom I have known longer than others on this continent told me recently that he himself had been permitted to read my file both in Boulder and at the Smithsonian, and was shocked to find that it included clearly political slander both from Iceland and Boulder...even the same nonsense that the Icelandic quisling (who sold Iceland to the US Navy at the beginning of the war) whispered to the Embassy in Reykjavik when it had become known in 1950 that we were leaving for Canada...and since the Canadian Embassy in Oslo that represented their interests in Iceland sought information at the American and not British Embassy in Reykjavik, we were prevented from getting our visa until after months of investigations by the Icelandic Ambassador at Stockholm who laid the slander to rest as far as the Canadian Government was concerned. Or did it rest...perhaps that matter still has found its way into the American files, though hardly the corrections by the Canadian Ambassador and his Icelandic colleague in Sweden in 1950-51? But even that material ought to be available in several places to show that great men looked at it as nothing but malicious slander, among them the conservative Skottsberg and Hultén. Then we felt proud of that these small men at home and abroad found ~~xxx~~ us to be important enough for their tries to hurt us... perhaps we ought to have made the scandal internationally known then to prevent further attacks that came after we arrived at Boulder? From now on, you are free to show or distribute copies of my documents to anybody you feel might want to know and help - not least those American botanists who recommended us to Boulder in good faith who are entitled to know what ~~want~~ wrong and to an explanation of why I was found to be in need of such a treatment that the entire American community would have stood up and protested...if I had been in the Soviet Union at least... where kangaroo courts and official stabbings seem to be no less frequent than at Boulder, look at the Sakharovs. Although that might also hurt others than us and American reputation far away, even in Iceland where the rich and conservatives have spread doubtful propaganda about the paradise in the west so that the fools who want to be free from the "protection" from any army would not protest against it loudly.

No more politics that I doubt you appreciate...we have, as far as I recall, never discussed real politics, though I have the feeling that in general we think very similarly and agree that since God only helps those rich who are able to help themselves, we and the society are entitled to help the others socially. And in botanical philosophy we have rarely differed, thanks to that we share the background of classical genetics from our first years of training.

Cynobolus base 9

Now to the Veronica letter of September 26. The earlier splitting of that much too inclusive genus that we accepted for our Slovenian Atlas took out the austral genera Hebe (basic number 10) and Parahébe (basic number 7), as well as the boreal genera Paederota (basic number 9) and Pseudolysimachion (basic number 17). I gladly admit that my efforts in the small Icelandic flora were highly preliminary, so that I still am in doubt on some points, among which is the status of the Alpine section or subgenus of Veronica s.str. (type: *V. officinalis*, cf. *Species plantarum* Ray Society fascimile), or as a genus of its own - we have made no hybridization efforts and the chromosomes are too small for karyotypic recognition. [I must mention here that I believe your listing of *V. wormskjoldii* from Colorado in your 1979 Inventory is caused by an oversight and the fact that previous American authors including Hultén identified the western taxon with the eastern American tetraploid rather than accepting the correct name *V. nutans* for this diploid...one of the two supposed parents of the tetraploid wormskjoldii, cf. the Arctic Atlas and the Alaska flora, where these names are given as synonyms, which they are not]. I am in no doubt about the distinctness of *Pocilla* as typified by *persica*, to which also *biloba* belongs for your transfer, but the fact that I avoided a formal transfer of *arvensis* should indicate my hesitation caused by lack of pertinent literature. If you, nevertheless, think this was correct, the transfer is your pleasure. I am in no doubt that the widely introduced South American peregrina ($x=13$) is a distinct genus, though I can find no valid name available for it in the literature at hand. Likewise, *virginica*, *japonica* and a few other Asiatic taxa with $x=17$ are best placed in *Keptandra* Nuttall, as you suggest, though already Moench (1794) identified it with *Veronicastrum* Fabr., perhaps a blunder because Fabricius and Heister were listing European plants, or so I understand (however, my copy of the *Regnum Vegetabile* book by Dandy on these and other critical genera is not easily available where it is in a box above our garage). That I accepted the typification of *Veronicastrum* by the 7-chromosome *serpyllifolia*, as did Fourreau, was based on a similar action by Dostál, J. 1982: *Seznam cérnatých roslin kvetany ceskoslovenské*, p. 236, in synonymy, since I am of the opinion that Fourreau's understanding of the necessary generic splitting was considerably more logical, from my points of view, than that of the faceless author of the ING identifying it with *Veronica* s.str. which already at the time of Fabricius was fixed in the minds of botanists as the relationship of *V. officinalis*, as later typified. As always, however, my opinions are ~~always~~ open and ready for revision when new material becomes available, so I trust you make a more logical revision if you feel I was mistaken.

I am not astonished that president Weber is leaving for greener pastures, it must also have been a hell for him to find out what a prison he went into. I hope this does not disturb your dreams for a more permanent home for the herbarium. That dream also needs a special library, so perhaps our collection could fit in, if proper arrangements are made...we have been contacted by some places, including Iceland, though we want it to stay where it will be most useful. Perhaps the Pooled Income Fund mentioned in the September issue of the *Colorado Alumnus*, p. 6, might be a method to accomplish this, if you so feel, though they have not yet reacted to my 27/9 request for their brochure. Doris is more hesitant than I am in this idea, for understandable reasons, though that could be overcome, because we share your concern for a better taxonomical teaching and research in the place where we have been worst treated...thanks to that we believe in true genetics and evolution perhaps, rather than to the general crookedness of the bureaucrats that govern.

Though I could continue for ever because I miss talking to you, this will have to suffice today. Glad that you showed Phipps the file, though he may not even tell the Canadians, who were no better earlier...that is my fate that some envy me my ideas, I dare to suppose. All the best to you both from us all here,

as ever,

L. J. H.

San José, November 6, 1984.

Dear Bill:

Thanks for the October 25 letter and list that reaced me with three days difference. I am as convinced as you are that the basic numbers help in breaking up the complex in a sensible way. Actually, it has already been divided into more genera than you seem to be aware of and fit into the scheme, though some of these remain hazy because of lack of chromosome information and hybridization attempts. I believe that a complete revision requires considerable experiments that may not become available in our lifetime. So perhaps the piecemeal approach still is the most appropriate one until nothing will be left to divide? Therefore, for your purpose, I believe we could come to a reasonable solution by keeping Veronica s.str. as a genus of $x = 9$ species, that are perennial: americana, anagallis-aquatica, catenata, nutans and scutellata; Pocilla for the $x = 7$ taxa, annual: arvensis, persica and biloba, the last of which (and the first?) you may transfer; Veronicastrum for the $x = 7$ perennials around serpyllifolia (as does Flora Europaea at the sectional level...how is it typified, if at all, in the Index Genericorum?), and then split ~~out~~ the South American and presently universal weed peregrina out as a genus of its own with $x = 13$; the South Americans may know more taxa of that relationship. For that taxon you can select a name you feel fit, but perhaps I may suggest the combination Notoveronica, noto means southern? You may even spice the note with various discussions that could be fed reasonably by discussions of other relationships and places of origin, for instance based on what Meusel mentions in his last volume of the magnum opus...though that is not necessary. This is at least a reasonable first solution that may induce those with better facilities and closer to the problems of other parts of the group to do some improvements, and so at infinitum until there is no problem left?

By the way, mentioning problems: there have been suggestions in the past that there may be a close relationship between Leptandra and Pseudolysimachia based on their morphology that indicates some relation to the 9-chromosome Paederota and the 8-chromosome Veronicella. To my mind that may perhaps be supported by our present knowledge of the number 17 as basic to both genera. As far as I am aware, nobody has even tried to cross these groups...perhaps a good project for a graduate student if genetics ever will again be allowed to rise from the ashes in which American botanists evidently want to keep it?...and if they can cross and there are indications ~~of~~ that their allopolyploid genomes are similar, then another conclusion might be that these genera need to be united under the older, American, name? Perhaps such suggestions ought to be kept secret for the time being, but they show that if one has the ability, that many want to suppress, to dream and speculate on basis of available observations and logic, there is no limit to the healthy progress which our unhealthy and unscrupulous colleagues do not want. You are probably the only one who listens to such foolishness...the great Cronquist, Raven etc. listen to the sounds of the shells instead and hear the old Asa Gray songs.

I understand your wish to return to Japan, but you would be foolish if you think of competing with them in research on alpine plants, and I doubt that Shimizu^v, who knows their alpine flora, has the imagination to stimulate your much better mind. So why not keep to the Colorado mountains and complete something that will guarantee your move into eternity? You are of another kind than the earlier and later American so-called botanists who dare not think by themselves, but the Japanese need no help for the time being...except to sink moneymakers as Miyawaki into some of their lakes.

You have probably been taking part in the American farce this morning...you ought sometimes to see how very different the really democratic process is in Scandinavia!

L. van Kesteren

How do I know
that my student
P. Hansen provided
a number of D.A.
Hansen in the
diagnosis of Veronica
at Portland, Ore.
at Tokyo, U.S.

San José, November 21, 1984.

Dear Bill:

Thanks for the November 13 letter with the information on the ING types for Veronica and Veronicastrum. I am sorry that I cannot easily find my packed copy of Dandy's work in Regnum Vegetabile on the critical genera of Fabricius and others from that period, so I cannot see if or what species he listed there under Veronicastrum. Do you have that booklet available, or should I climb upstairs and try to dig it out from among the many filled boxes? Or does it actually matter, since we could maintain, with considerable logic, that when Fourreau transferred *V. serpyllifolia* it could be regarded as good enough typification?

By the way, when I mentioned my boxes, I came to think of that though I have some of the Indexes or whatever they are called, the lists of collectors in Regnum, I do not have the one with my part of the letter L...and so cannot see what herbaria have listed, or not listed, material that we know we have left with them during the years...and Boivin, who was here recently, told me that Montreal definitely had left our large material out...perhaps because we were not French? Could you copy the appropriate page and give it to me?

I can understand your reluctance in making *Notoveronica*...but I know it no better than you do, though perhaps we could cooperate in irritating the "great" small men (as to the smallness of those who have decided to ostracize not only us but even the great geneticist Goldschmidt, cf. the enclosed copy from Science 84 about Ernst Mayr, another small but loudspoken "phylogeneticist" who knows as little genetics as does the much taller though equally small Cronquist...since we were only in Boulder, though with winds from Harvard, Berkeley etc. we can hardly generalize as to the jungle-fighters that even you permit to persist, for some reason...probably they all like you no less than us?). So, if you make some blurr for the taxa you want changed, with as much logic as you can dig out, I will gladly add what I have in my books...That goes not only for *Notoveronica*, but also for "*Arenaria*" *nuttallii* (you probably have the possibility to find a copy of the 1921 monograph of *Minuartia* by Mattfeld, with the exact place and description of his sect. *Sclerophylla* series *Pungentes*, on basis of which we could name the western genus...but what name should it have? And you have the data for *Linum kingii* etc. which are hardly known to me.

I am sorry that I forgot last time to answer your questions about *V. cymbalaria*. It is morphologically so distinct that it has not only been given the "group" name *Megasperma* by Lehmann (1908) and the section name *Megasperma* (Lehm.) Borissova by Borissova (1955, in Fl. SSSR XXII:413), but also...and that is essential for our purposes, the ~~xxxxxxxx~~ generic name *Cochlidiospermum* Opiz, in Bercht. & Opiz 1939, Ökon.-Techn. Fl. Böhem. 2, 2:145, where also the combinations are made for three species, *C. cymbalaria* (Bertol.) Opiz, *C. hederifolia* (L.) Opiz (type), and *C. lappago* (Schrank) Opiz. See Pouzar, 1964, in Preslia 36:338. [Nomenclatorial remarks on some species names of *phloxifera* published by Filip Matoušek, Opiz, - Preslia 36: 338-342]

I hope your enthusiasm as to the herbarium move continues and that the not too honest bureaucrats will not disappoint you once more. Though I would not shout my hurrah until everything is in place...we cross our fingers. More important, however, may seem to be to get a consent of the bureaucrats to replace you by a good man who could even come now to learn the philosophy of this certainly best herbarium in the west...yes, it is better than Laramie despite the much better facilities the latter have had...but the philosophy and energy that has gotten into the Boulder place since you came, is greatly superior to that at Laramie. If I were permitted to make a proposal, then I

would say that the best solution would be to join them both at Boulder under a new department for taxonomic botany with teaching mainly at the graduate level, because the undergraduate teaching in the Rockies is of very little significance thanks to the old attitude of the dominating small zoologists and politicians. But that is perhaps only the second, though important, step. The first is to get the consent to look for your replacement. I know that you want some of your own students, whom you have trained, but since none of these actually is great enough to replace you and carry on the good work you have built up and the fine international reputation you also have carried to the place, I would disregard that possibility and look elsewhere...but you may dislike that. Nevertheless, I even dare to mention a young and energetic man, whom I would trust to take on your shoes and to cooperate with you peacefully as long as you want to stay with him, a young man with a basic education of the classical kind and good field and herbarium experience in the west, though he might have to wait a year or two for his Ph.D. at Claremont: the one I am thinking of is Jim Morefield at Flagstaff, who has worked intensely with Clark Schaack in studies on their flora, including considerable critical investigations of its cytology that have been partially published in recent years in my Taxon column. Why not visit him to look him over, or even invite him to look at Boulder, without telling him about the purpose...his address is NAU Box 6201, N. Ariz. Univ., Flagstaff, Arizona 86011. If you get a permission to let you look at him during a short visit, perhaps you could lure those who decide to give him a preliminary position that could then be changed to a permanent one so that you ceased to be alone during the last few years of your tenure? I am sorry that the crooks at the molecular etc. places at Boulder were permitted to stab us so effectively that the world is as shocked as by the constant attacks on Goldschmidt who has been dead for almost 30 years, because if they had not been permitted to do this, I would gladly have gone out to support you in the efforts to secure the herbarium...even by going to Washington, as when we got Jack [though that I now believe was a mistake that I should have thought of because I already then knew about his tendency for empire-building, at the cost of others. But you probably know ways and means to proceed...I recommend Morefield because I know that he is a good collector and a fine modern botanist with possibilities to grow just in the direction that you have~~xy~~ been growing into for years...so...

It is typical of Landolt to try to get people to write some report on things that they only have seen through windows...but what do some of our colleagues not do to get publications for their institutes...when we were at Montreal, Dansereau travelled with the train and buses to Quebec and wrote on basis of his window-shopping a couple of "phytosociological" account of the flora that the Europeans, and we, laughed at. But perhaps such a book would strengthen Miyawaki and give you all another trip to Japan at the cost of his wealthy supporters... and then hopefully without the former Nazi Gauleiter and famous dinnerspeker?

We can see on the TV that winter is coming to Colorado...it is also here, though the rains are hardly disturbing yet...and no frosts at lower elevations. But all that may come sooner than one can dream of it.

It is too late to wish you both a fine thanksgiving, since the letter will hardly reach you until a week after. But we hope you both are as well as usually at least, and that your energy and imagination never diminish.

All the best from us all here,

L. S. Howe