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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 nonfusion 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. ~~Univ~~ 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?

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Boulder, 9 Sept. 1983

Dear Askell:

Turaniphytum did not check out; what oooked in the figure to be single large heads were clusters of very small ones; other characters do not seem to suggest this to be a reasonable match for the pattersonii group.

Is there a count for A. scopulorum? The two plants do seem to be pretty much related. I wish I had been keeping a catalog of chromosome numbers of Rocky Mountain plants; I could do this if you could provide me with some of the catalogs. Unfortunately this is one area that I never was able to keep up on.

Sincerely,

Bill

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Boulder, 8 Sept. 1983

Dear Askell:

I still don't understand about Sect. Seriphidium. Index Nomina Gener. says: "Artemisia Sect. Seriphidium W.S.J.G. Besser, Bull. Soc. Imp. Naturalistes Moscou 1:22. 1829 ("Seriphida") la invalid. Artemisia Sect. Seriphidium Besser ex W. J. Hooker, Fl. Bor.-Amer. 1:325. 1833 (sero) is typified by A. cana Pursh.

Why is Hooker's establishment of the section better than that of DC.(1837!). Subsection (Series?) Trifida [what does the sign mean?] of course is O.K. But I think you were trying to tell me that DC., not Hook., was the proper author, and that's what I don't get. I will of course go along with Subsection Trifida DC.

B.

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 spp. in nana and then five more species from Asia, according to Babcock, but you may perhaps want to give specific 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 M'Untzing 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 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 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,

TEXTNAME: ncomb4 (R)P: 01

DRAFT ONLY. OK??

NEW NAMES AND COMBINATIONS, PRINCIPALLY IN THE ROCKY MOUNTAIN FLORA--IV

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The third paper in this series was published in Phytologia 53:187-190. 1983.

LINUM AND ITS SEGREGATES

ADENOLINUM GRANDIFLORUM (Desf.) W. A. Weber, **comb. nov.** Linum grandiflorum Desf., Flora Atlantica 1:278. t. 78. 1798.

ADENOLINUM PRATENSE (Norton) W. A. Weber, **comb. nov.** Linum lewisii pratense J. B. S. Norton, Trans. Acad. Sci. St. Louis 12:38, pl.6. 1902. Rogers (1968), in a review of the yellow-flowered species of Linum in western North America, unfortunately did not concern himself with the generic problem in the genus Linum, sens. lat. Linum is based on the type, Linum usitatissimum L., a blue-flowered annual species with linear stigmas and erect flowers and chromosome base number, n=15. In western North America, the blue-flowered group, Adenolinum Reichenbach 1837, has capitate stigmas and recurved fruiting pedicels, and chromosome base number n=9. The yellow-flowered group consists of two well-defined lines: the first, Cathartolinum Reichenbach, 1837 (construed very broadly by Small [1907], based on Linum catharticum L. (Rogers' L. schiedeanum complex), differing significantly in fruit dehiscence, ovule number, pollen morphology, style morphology, and chromosome base number n=8, from the second, Mesynium Raf., 1838 (Rogers' L. rigidum group) with a chromosome number of n=15. Rogers clearly tabulated these important differences but declined to divide the genera. Love and Love recently revived Adenolinum and Mesynium (Love 1982), quite justifiably in my opinion.

... will be added before submitting.

MESYNIUM Raf., Fl. Telluriana 3:33. Nov.-Dec. 1837. A lectotype should be designated. Of the five species mentioned, M. texana was new, three others were nomina nuda, and M. mexicanum (H.B.K.) Raf., was a transfer. I propose M. mexicanum be chosen as the lectotype.

MESYNIUM ALATUM (Small) W. A. Weber, **comb. nov.** Cathartolinum alatum Small, N. Am. Fl. 25:81. 1907.

MESYNIUM ARISTATUM (Engelm. in Wisliz.) W. A. Weber, **comb. nov.** Linum aristatum Engelm. in Wisliz., Tour Northern Mexico 101. 1848.

MESYNIUM AUSTRALE (Heller) W. A. Weber, **comb. nov.** Linum australe Heller, Bull. Torr. Bot. Club 25:627. 1898.

MESYNIUM AUSTRALE ssp. GLANDULOSUM (C. M. Rogers) W. A. Weber, **comb. nov.** Linum australe var. glandulosum Rogers, Sida 1:336. 1964.

MESYNIUM IMBRICATUM (Raf.) W. A. Weber, **comb. nov.** Nezera imbricata Raf., New Flora & Bot. North Amer. 4:66. 1838.

MESYNIUM HUDSONIOIDES (Planch.) W. A. Weber, **comb. nov.** Linum hudsonioides Planch., London J. Bot. 7:186. 1848.

MESYNIUM PUBERULUM (Engelm. in A. Gray) W. A. Weber, **comb. nov.** Linum rigidum var. puberulum Engelm. in A. Gray, Smithson. Contr. Knowl. 3 (Pl. Wright. 1): 25. 1852.

MESYNIUM SUBTERES (Trel.) W. A. Weber, **comb. nov.** Linum aristatum Engelm. var. subteres Trel. in A. Gray, Syn. Fl. N. Am. 1(1):347. 1897.

MESYNIUM VERNALE (Wooton) W. A. Weber, **comb. nov.** Linum vernale Wooton, Bull. Torr. Bot. Club 25:452. 1898.

ALETES (UMB): An expanded concept

Despite the fact that many eminent American botanists have attempted to classify the western North American Umbelliferae, several genera remain to some extent artificial. While one complete treatment (Mathias and Constance, 1944-45) has tended to stabilize and reduce a number of generic names, the submergence of some of the old genera has simply served to hide the fact that large ones like Lomatium and Cymopterus are still very heterogeneous, and unless monographers of some of the allied genera carefully reexamine these large ones for misfits, this situation will likely continue.

The history of classification of the western North American umbels also displays a lack of feeling for the whole organism, its total morphology and habitus, its chemistry, phytogeography and its ecology. Too much emphasis has been placed on one or two characters that are given much weight.

The genus Aletes is based on Aletes acaulis C. & R., 1888 (Deweya acaulis Torr.). A revision of this genus was published very recently (Theobald, Tseng and Mathias, 1963). It was undertaken as a result of my rediscovery of Neoparrya lithophila and my suggestion, which they accepted, that Pteryxia anisata should be referred to Aletes. I have never been satisfied with the maintenance of Neoparrya as a monotypic genus, and recently (Weber 1979) I transferred a second taxon, N. megarrhiza, out of Lomatium, where it was anomalous.

Unfortunately, Theobald et al., while they described a few new taxa, did not examine other genera for possible additions to Aletes. They also deferred study of Pteryxia and implied that they were about to study Cymopterus. They pointed out, however, Cronquist's (1961) expansion of Cymopterus to include two more discordant elements, Pteryxia and Pseudocymopterus.

Theobald et al. described Aletes as "perennials from slender to thickened elongated roots". This is inaccurate. The structures they refer to are caudices, which are covered with marcescent sheathing petiole-bases, a critical difference. I would expand their generic concept to include plants with yellow, pale yellow to whitish and exceptionally (as in Pseudocymopterus) purple, flowers. And I would allow considerable variation in the number, size, and disposition of the vittae, and in the compression and development of the lateral and dorsal wings of the mericarps. I agree completely when they say that "the genus is remarkably consistent in its habit and basic leaf pattern". Their monograph is a good starting point, but more bricks need to be laid in order to make the building complete.

Without seriously altering the circumscription provided by Theobald et al, I regard Aletes is a natural group embodying the following unique constellation of characters:

1. Plants densely caespitose with stout, branched caudices clothed with long-enduring marcescent petiole-bases.
2. Strictly acaulescent; (this eliminates Pteryxia terebinthina, which is always slightly caulescent).

4. Pseudoscapes never developed.
5. Plants strongly scented (anise, citronella, celery)
6. Leaves pinnatifid or pinnate, with pinnae simple or pinnatifid, usually stiff-textured.
7. Bracteoles always well-developed, lance-linear to linear, dimidiate.
8. Involucre never developed.
9. Flowers yellow, pale yellow, whitish, or exceptionally purple.
10. Rays subequal, widely spreading, sometimes the outer ones deflexed.
11. Mericarps with variable development of lateral wings; dorsal ridges often prominent.
12. Mericarps usually trapezoidal in cross-section, not or variably dorsally compressed.
13. Stylopodium none, the styles arising out of the base of a spongy disk (some authors seem to have confused this disk with a low stylopodium).

If, bearing in mind this set of characters, one returns to the standard treatment of North American umbels, several taxa stand out in Lomatium, Pteryxia and Cymopterus discordant elements. Furthermore, these taxa have always been controversial, placed variously in other discarded genera such as Cynomarathrum and Pseudopteryxia.

The following new combinations are proposed to bring these taxa into Aletes.

ALETES EASTWOODIAE (C. & R.) W. A. Weber, **comb. nov.** Cynomarathrum eastwoodiae C. & R., Contr. U. S. Nat. Herb. 7:247. 1900.

ALETES BIPINNATA (S. Wats.) W. A. Weber, **comb. nov.** Pseudocymopterus bipinnatus C. & R., Rev. N. Am. Umbell. 75. 1888.

ALETES HENDERSONII (C. & R.) W. A. Weber, **comb. nov.** Pseudocymopterus hendersonii C. & R., Contr. U. S. Nat. Herb. 7:190. 1900.

ALETES JUNCEA (Barneby & N. Holmgren) W. A. Weber, **comb. nov.** Lomatium junceum Barneby & N. Holmgren, Brittonia 31:96. 1979. Barneby & Holmgren (1979), in recognizing and presenting a key to the "Cynomarathrum species of Lomatium" saw the natural group that I feel is incorrectly placed in Lomatium, but they made no connection with Aletes. They, however, included L. triternatum and L. concinnum, two caulescent species, in the group.

ALETES LATILOBA (Rydb.) W. A. Weber, **comb. nov.** Cynomarathrum latilobum Rydb., Bull. Torr. Bot. Club 40:73. 1913.

ALETES LITHOPHILA (Mathias) W. A. Weber, **comb. nov.** Neoparrya lithophila Mathias, Ann. Mo. Bot. Gard. 16:393. 1929.

ALETES LONGILOBA (Rydb.) W. A. Weber, **comb. nov.** Pseudopteryxia longiloba Rydb., Bull. Torr. Bot. Club 40:72. 1913. Mathias, Theobald & Tseng (1964) did not include this taxon in their monograph of Aletes (despite the fact that Rydberg clearly showed its close relationship to P. anisata), probably because Mathias had earlier synonymized it (incorrectly, we feel) under Pteryxia hendersonii. Mathias et al (1964) declined to discuss Pteryxia. A. longiloba differs from A. anisata chiefly in its more delicate leaf texture and more slender and attenuate leaf segments.

ALETES MEGARRHIZA (A. Nels.) W. A. Weber, **comb. nov.** Peucedanum megarrhizum A. Nels., Bull. Torr. Bot. Club 26:130. 1899.

ALETES MINIMA (Mathias) W. A. Weber, **comb. nov.** Lomatium minimum Mathias, Ann. Mo. Bot. Gard. 25:273. 1937.

ALETES NIVALIS (S. Wats.) W. A. Weber, **comb. nov.** Cymopterus nivalis S. Wats., Bot. King's Exp. 123. 1871.

ALETES NUTTALLII (A. Gray) W. A. Weber, **comb. nov.** Seseli nuttallii A. Gray, Proc. Amer. Acad. 8:287, in part. 1870.

ALETES PARRYI (S. Wats.) W. A. Weber, **comb. nov.** Peucedanum parryi S. Wats., Proc. Amer. Acad. 11:143. 1876.

ALETES PETRAEA (M. E. Jones) W. A. Weber, **comb. nov.** Cymopterus petraeus M. E. Jones, Contr. W. Bot. 8:32. 1898.

ALETES SCABRA (C. & R.) W. A. Weber, **comb. nov.** Cynomarathrum scabrum C. & R., Contr. U. S. Nat. Herb. 7:247. 1900.

ASKELLIA, a new segregate of the genus Crepis

Askellia , genus nov.

Based on Crepis, Sect. Ixeridopsis Babcock, Univ. Calif. Publ. Bot. 22:212. 1047. Typus: Crepis nana Richards. This genus is named in honor of my friend Dr. Askell Love, student of Gote Turesson and dean of the Icelandic flora. His dedication to the Science of Botany, his encyclopedic memory of botanical information, his understanding of biosystematic, especially cytological, techniques and his exposition of its philosophy, his role in developing the concept of the Flora Europaea, and his recent perseverance in the face of controversy, misunderstanding, extreme misfortune and character assassination has earned him lasting recognition amomg the outstanding plant taxonomists of our generation. His kindness and support of colleagues and young botanists is well-known and appreciated by all who have benefitted from knowing him.

Askellia elegans (Hook.) W. A. Weber, **comb. nov.** Crepis elegans Hook., Fl. Bor.-Amer. 1:297. 1834.

Askellia nana (Richards.) W. A. Weber, **comb. nov.** Crepis nana Richards., Bot. App. Franklin, 1st Jour. ed. 1:746. p. 18 in repr.) 1823; ed. 2: 757 (p. 29 in repr.). 1823.

The Western North American Sagebrushes
STEPPEA, a new genus proposed for Artemisia, sect. Seriphidium

The woody western North American sagebrushes centering about Artemisia tridentata form a homogeneous group of similar morphology and ecology, differing from all other Artemisia in having homogamous heads, all but one (A. bigelovii) lacking any ray-flowers whatsovever. The section Seriphidium was proposed by Hooker (1833) and is typified by Artemisia cana Pursh. This group has been treated exhaustively by Ward (1953). Earlier accounts include those of Rydberg (1916) and Hall & Clements (1923).

The philosophical justifications of a conservative generic concept in Artemisia were excellently stated by Hall & Clements, whose discussion of the taxonomic history is a classic. In their maintenance of a Seriphidium as a section of Artemisia, they were influenced by the marginal character of A. bigelovii, where "the ray-flowers, recognized by their peculiar 2-toothed corollas, are usually present, although reduced in number to only one or two, but occasionally entirely suppressed, the head then consisting of only two or three flowers with regular 5-toothed corollas. Perhaps this species represents the beginning of the Seriphidium line, where the evolution of homogamous from heterogamous heads is still in progress."

Hall & Clements used similar logic to submerge Artemisiastrum under Artemisia: "While the presence or absence of these structures [receptacular bracts] is of much value in the classification of the Compositae, their occasional occurrence in a genus whose species are almost universally devoid of them may be looked upon as a possible case of reversion rather than as the basis for a new genus." Yet Hall & Clements maintained Artemisia bigelovii in another subgenus because of the occurrence of a variable number of marginal ray-flowers. Ward followed Hall & Clements' reasoning but treated the species "because of its close resemblance to certain members of Seriphidium and its frequent misdetermination as such."

A diametrically contrasting point of view is delightfully presented by Camp (1940), and this is pertinent here. After showing that Gaylussacia, according to his current ideas, comprised three additional genera, he wrote: "The erection of the genera Buxella, Decachaena, and Lasiococcus to take care of our North American species of huckleberries has met with a great deal of opposition and I, too, have deplored the segregation. But fundamentally it was sound, for the old classic genus is composed of four very definite groups of species.... Had we been able to maintain the species with which we are most familiar in the genus Gaylussacia and erected new genera for those in South America, there would have been little protest. Apparently it is a common reaction among taxonomists--being human--that, so long as a genus is endemic in some remote part of the world it may be split as the student pleases, the splitting being hailed as a brilliant piece of research. But let one among us attempt, phyletically, to rearrange a genus with species in our own local areas--the rearrangement resulting in the necessity of learning new generic names--there is an immediate and loud protest. Even so, Lasiococcus dumosus, Decachaena baccata and Buxella brachycera are names with a strange and unfamiliar sound and I don't like them any more than you do. But, I have been asked, "Then why change them? We have known them as species of Gaylussacia for so many years." There is only one answer. If such an argument is to determine our criteria concerning the status of a generic name, then let us be purists and return those species to the genus Vaccinium, for they were known as Vaccinium dumosum, V. resinosum and V. brachycerum for about a half-century prior to their inclusion in the genus Gaylussacia. The point is, none among us remember the clamor that arose when the botanists of another day

had to learn to think of them as belonging to "that new-fangled genus Gaylussacia." From the standpoint of phylogeny, there is no more reasonableness in retaining these species in Gaylussacia than in returning them to Vaccinium.... Perhaps we should adopt as our motto, not 'Back to Linnaeus,' but, 'Forward to the truth.' Perhaps, if we were not afraid of the puling croaking of certain of our confreres every time we broaden and particularize our concepts, we could put new life into old taxonomic bones, long interred in the musty vault of nomenclatural conservatism."

The fact, whether we like it or not, new concepts in phylogeny deriving from evidence from anatomy, SEM observation, phytochemistry, cytology, genetics and ecology, so-called "generic splitting", once considered taboo because of the uproar raised by laity and applied botanists and blamed for the temporary decline in popularity for taxonomy, continues as it must when justified. It is happening just as massively, or more so, in the fungi, bryophytes and lichens. Delimitation of genera does not necessarily rest on the selection of one or more so-called "generic characters", but upon all of the biological features of a group that set it apart as a monophyletic line separated by barriers of whatever sort, from its near relatives.

Divergent generic concepts represent different points of view, and as alternative treatments they should be tolerated until proved incorrect. Good science should not involve decisions based on personal convenience, likes or dislikes of large or small genera, their names, or the scientists who propose them. It is indeed strange that the practitioners of taxonomy, which through its binomial system, has developed one of the most concise and logical ways of enabling scientists to express their different points of view, should deny their colleagues the exercise of them. In other disciplines this would be considered intolerable.

Hall & Clements argued that raising sections to generic rank caused "relationship and perspective [to be] lost, [producing] results [that] are both unnatural and unusable." Their argument was strongly polemical, and based on personal preference rather than on any genetic basis or consideration of the magnitudes of the gaps or on crossability or ecology. When scientists hold such rigid beliefs, no counter-argument, no matter what the facts are, will change the minds of those who do not like to have their preconceptions disturbed. I would predict that foresters and range managers would prefer to continue to consider the sagebrushes as belonging to Artemisia. But at the same time, for them Artemisia comprises the sagebrushes alone, since they have very little to do with the vast remainder of the genus. Nor would they recognize most of them since most species are so different from the sagebrushes. Yet A. vulgaris remains forever the type species of Artemisia. If most taxonomists are content with the sagebrushes belonging with A. vulgaris, they are of course welcome to their viewpoint. This is why binomial nomenclature exists.

My basis for segregation of the section Seriphidium on the genus level rests on no new evidence, but on my long acquaintance with Artemisia, sens. lat. Its morphological homogeneity, both vegetative and floral, occurrence side-by-side with members of the other sections without any genetic mingling, discrete geographical distribution and common ecology, convince me that this is as "good" a genus as any in the Anthemideae. I also subscribe to the resurrection (Love & Love 1982) of Cassini's genus Oligosporus (1817), based on the type of Artemisia campestris L., for Section Dracunculus, characterized by having sterile disk-flowers (cf. King & Dawson, eds., 1975).

Artemisia palmeri A. Gray, included by Ward, and Hall & Clements under Sect. Seriphidium remains anomalous, differing by its chaffy receptacle, elongate herbaceous branches, bicolored, deeply incised leaf-blades suggestive of A. vulgaris, and nearly equal phyllaries. I lean toward retaining Artemisiastrum Rydberg for this monotype.

Seriphidium cannot be used as a generic name for this group because of its preoccupation by Seriphidium Poljakov (1961), based on Artemisia maritima L. Therefore, I have chosen the name Steppea, which has not been used before, and is particularly apt for a genus of plants which epitomize the vast steppe-desert area of the western United States.

STEPPEA, W. A. Weber, genus nov.

Artemisia, Sect. Seriphidium Bess. ex Hook., Fl. Bor. Amer. 1 325. 1833. TYPUS: Artemisia cana Pursh.

Steppea bigelovii (A. Gray in Torr.) W. A. Weber, **comb. nov.** Artemisia bigelovii A. Gray in Torr., Pacific R.R. Rep. 4:110. 1857.

Steppea arbuscula (Nutt.) W. A. Weber, **comb. nov.** Artemisia arbuscula Nutt., Trans. Amer. Phil. Soc. II. 7:398. 1841.

Steppia nova (A. Nels. W. A. Weber, **comb. nov.** Artemisia

Steppea cana (Pursh) W. A. Weber, **comb. nov.** Artemisia cana Pursh, Fl. Amer. Sept. 521. 1814.

Steppea cana ssp. bolanderi (A. Gray) W. A. Weber, **comb. nov.** Artemisia bolanderi A. Gray, Proc. Amer. Acad. 19:50. 1883. nova A. Nels., Bull. Torr. Bot. Club 27:2674. 1900.

Steppea pygmaea (A. Gray) W. A. Weber, **comb. nov.** Artemisia pygmaea A. Gray, Proc. Amer. Acad. 21:413. 1886.

Steppea rigida (Nutt.) W. A. Weber, **comb. nov.** Artemisia trifida beta rigida Nutt., Trans. Amer. Phil. Soc. II. 7:398. 1841.

Steppea rothrockii (A. Gray) W. A. Weber, **comb. nov.** Artemisia tridentata ssp. rothrockii Hall & Clements, Carnegie Inst. Wash. Publ. 326:139. 1923.

Steppea tridentata (Nutt.) W. A. Weber, **comb. nov.** Artemisia tridentata Nutt., Trans. Amer. Phil. Soc. II. 7:398. 1841.

Steppia tridentata ssp. parishii (A. Gray) W. A. Weber, **comb. nov.** Artemisia parishii A. Gray, Proc. Amer. Acad. 17:220. 1882.

Steppea tridentata ssp. vaseyana (Rydb.) W. A. Weber, **comb. nov.** Artemisia tridentata ssp. vaseyana (Rydb.) Beetle, Rhodora 61:83. 1959.

Steppea tridentata ssp. wyomingensis (Beetle & Young) W. A. Weber, **comb. nov.** Artemisia tridentata ssp. wyomingensis Beetle & Young, Rhodora 67:405. 1965.

Steppea tripartita (Rydb.) W. A. Weber, **comb. nov.** Artemisia trifida Nutt., Trans. Amer. Phil. Soc. II. 7:398. 1841, non Turcz. 1832.

MISCELLANY

ACROLASIA THOMPSONII (Glad) W. A. Weber, **comb. nov.** Mentzelia thompsonii Glad, Madrono 23:289. 1976.

BROMELICA BULBOSA (Geyer ex Porter & Coulter) W. A. Weber, **comb. nov.** Melica bulbosa Geyer ex Porter & Coulter, Syn. Fl. Colo. p. 149. 1874. The articulation of the spikelets above the glumes, the lack of tendency of the spikelets to nod, and the world distribution patterns of Melica typified by M. nutans L. according to Tzvelev (1976), and Bromelica (Boyle, 1945), suggest that these groups represent different phyletic lines.

BROMELICA SPECTABILIS (Scribn.) W. A. Weber, **comb. nov.** Melica spectabilis Scribn., Proc. Acad. Nat. Sci. Phila. 37:45. 1885.

DELPHINIUM RAMOSUM Rydb. var. **ALPESTRE** (Rydb.) W. A. Weber, **comb. nov.** Delphinium alpestre Rydb., Bull. Torr. Bot. Club 29:146. 1902.

NUTTALLIA ARGILLOSA (Darlington) W. A. Weber, **comb. nov.** Mentzelia argillosa Darlington, Ann. Mo. Bot. Grd. 21:153. 1934.

NUTTALLIA REVERCHONII (Urb. & Gilg) W. A. Weber, **comb. nov.** Mentzelia pumila (Nutt.) T. & G. var. reverchonii Urb. & Gilg, Nov. Act. Nat. Cur. [Abh. K. Leop.-Carol. Deutsch. Akad. Naturf.] 76:94. 1900. Mentzelia reverchonii Thompson & Zavortink.

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Weber answered 9/9

UNIVERSITY OF COLORADO, BOULDER

Museum
Campus Box 218 • Boulder, CO 80309

Turaniphytum — poljakovii
Turaniphytum Poljakov (Fl. SSSR 26, 1961), 632
eranthemum (Bunge) Poljak. (Artemisia e.)
kopetdaghense Poljak.

BOULDER COLO. AUG 31 '83
U.S. POSTAGE .37
PB METER 3318676

Artemisia
sect. Seriphidium Bess. 1829, 1834

§ 3. Trifidae, foliis radicalibus trifidis
A. cana Pursh
A. mendozana D.C.

D.C. 6: 105 (1837).

[illegible] 9/9 '83

Askell Love
5780 Chandler Court
San Jose CA 95133

[illegible]

Gen. Cen. 10PD, Fl. N. Am.
Icel + Arctic — Chr. Lists.

Send Poljakov's Artemisia paper to Bill!

Dewey, [illegible]
[illegible] protected by indifference
[illegible]

Flora of Iceland — Mary Barkworth

UNIVERSITY OF COLORADO, BOULDER

Museum

11 August 1983 ltr 15/9.

Dear Askell:

Enclosed is a little of the folder on Leila Shultz. She is still waiting to hear whether she has been chosen to be director of the Rancho Santa Ana Botanical Garden. Actually, she and Tom Elias were the two finalists, and the garden has offered the job to Tom, but in four months he has not accepted or rejected it; he is in the USSR this summer and of course can't be expected to answer until he comes back. I don't think that the Garden would keep a male waiting for a verdict, but Leila is not important enough to treat fairly.

What happened at Utah State is explained somewhat in the enclosure. Barkworth is still there, and still trying her best to get Leila to leave. Leila still wants to stay if the situation can be improved, but I think her only chance is to wait till I retire and try for here. There is still a strong possibility that Tom really does not want to get the Rancho job and is just trying to put some pressure on New York Botanical Garden for improvement to his own position. Leila went to Rancho and got her Ph.D. in anatomical work on Artemisia under Sherwin Carlquist. She is tough and independent and a scientist that we can be proud of having something to do with.

I am sending part of the dossier to Fosberg. We will wait to see whether he has any ideas about where to go from there. I asked the District Attorney's office in Denver about threatening phone calls. They refer us to the telephone company. The difficulty of tracing calls is that when they are so sporadic it is impossible to monitor continuously, as you already know. If you want to stop getting them, they suggest your getting an unlisted number. I don't understand why they seem to be indirectly threatening me by calling you. They could save their money and phone me directly.

I'm struggling with the Potentilla key right now. Unfortunately Barry made a key to sections and didn't tell by whose insights he was using them or what species they contained. He's off in the field until Friday. We have the Space Committee's O.K. on the new space, and next they have to talk to the chairman of thr Phys. Ed dept. to get his approval or objection. If there are objections there have to be plans and money available for the P.E. dept. to carry them out; otherwise we have a pretty clear justification for using the space. Will let you know.

Bill

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

UNIVERSITY OF COLORADO, BOULDER

Museum

16 March 1982

Dr. Ralph M. Johnson
Dean of the College of Science
Utah State University
Logan, Utah 84321

Dear Dr. Johnson:

I am writing you this letter pursuant to our discussion on the telephone last week concerning the annual review for Leila Shultz and the extraordinary criticism of her work by Dr. Barkworth. As I told you, I had spoken to Leila the night before, and now I have available to me copies of Leila's Job Description and the letter of 5 Feb. 1982 from Mary Barkworth and her Evaluation.

I think we can pass over any question of Leila's qualifications for the curatorship. I am sure that she has demonstrated to everyone's satisfaction the fact that she is eminently qualified from the standpoint of her understanding of good curatorial methods, expertise in the Intermountain Flora through field and herbarium studies, and the patience, pleasant personality and helpful nature that makes for good relationships with users. And, Mary Barkworth excluded, I have every confidence that she has made friends for the university in her contacts with herbarium users as well as the institutions which have exchange and loan relationships with UTC.

As you suggest, there are always two sides to a personality clash, which this appears, in a large sense, to be. Although I have had more contact with Leila than with Mary and am seeing this problem from some distance, I have known Art Holmgren and the herbarium at Logan for a long time, and I have been the curator of the University of Colorado Herbarium for over 35 years, earlier assisting in the herbaria of Washington State University and Iowa State University. I also served as Leila's boss when she assisted in my herbarium and I served as her major professor. So I feel I am in a position to give some useful commentary on this situation in particular and on herbarium administration in general.

I think that this personality clash is a natural consequence of the close association of two strong-minded women quarreling about their respective job responsibilities and prerogatives, but I feel that it also is a direct consequence of a serious mistake on the part of the administration, in that it has unwittingly set the stage for it. Leila assumed the curatorship in the waning years of Holmgren's curatorship. The job was set up as a non-tenure track position with little budget for salary improvement. In other words, Leila was not brought in under the same working relationship to the University that Holmgren enjoyed. She had only a masters degree. However, it should have been obvious that she was more than the average herbarium assistant (in many institutions the word "curator" has come to mean this). She went about revitalizing and reorganizing the collections, established an active field program,

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introduced as much mechanization (memory typewriter, for example) that the budget would allow, picked up an exchange program that was heavily in debt, and established rapport with users by her identification skills. Furthermore, she developed a real attachment for Utah, Logan, and appreciated the potential for spending a lifetime developing her, and thus your, knowledge of the Intermountain Flora.

Leila is a scholar, and her intellectual curiosity led to her sitting in on at least one course in the department, which probably whetted her appetite for further work. Her later decision to go after a Ph.D. in botany logically followed. If you were a talented individual, liked your job, but saw little future in it but had faith in your institution and its people, you might gamble on raising your level of accomplishment in hopes of your job improving its status, or toward the likelihood of your obtaining a tenure-track appointment elsewhere. I think it is a tribute to her devotion to Utah State that she tried to accomplish this by going on leave without pay rather than quitting the job. Mary Barkworth evidently strongly opposed and worked against Leila's plan to take this study leave.
Before discussing the personality clash between Barkworth and Shultz, I think I should discuss the administrative question of bringing in an Herbarium Director over an established curator. Three points should be made:

1) the fact that the herbarium is a part of a department whose main concern is not collections, and whose emphasis may change, placing in jeopardy programs involving collections. Herbaria all over the country, and the world, are in deep trouble just at a time when emphasis on threatened and endangered species is growing. This is partly due to the fact that the means for financial growth and security of departments seem to rest with the fields of greatest current popularity--cellular and molecular biology. We have, in many institutions, the incredible phenomenon of one branch of a science actually denigrating another (the polite way of expressing this is to talk of a "classical" biology area) in order to gain power.

The more fashionable parts of the field of biology tend to spread the utterly false doctrine that taxonomy is a finished science, and that all herbaria are useful for is to make identifications. Herbaria, which used to be centers of research, are being placed on the back burner and treated merely as service centers. They are being staffed with baccalaureate or masters degree people, on no tenure track, and with limited financial advancement. Equal employment opportunity works against women in this instance, because while there never used to be women curators, they are now becoming frequent, but at the same time it does not improve the status of women one iota. Because of the lack of career incentive, these people are likely to come from lower levels of academic achievement and promise. To be successful, herbaria and other natural history collections nowadays should be given shelter in an organizational arrangement in which they will be autonomous with respect to the teaching and "modern" research aspects of the university. They must have continuing curatorship and the possibility to accomplish research, and their staffs must be academically professional.

2) The department at Utah State has changed emphasis toward domination by the popular segments of the field. Your departmental chairman is a plant

physiologist and your dean is a biochemist. I suspect that this change of emphasis may be one strong reason why Dr. Barkworth was brought to the University. I have no quarrel whatsoever with this; the university can certainly use the cytogenetic approach to plant systematics. However, I see no evidence of Dr. Barkworth's achievements in connection with herbarium curatorship or management. I would feel that her place would be as a faculty member in the department, with a teaching responsibility in advanced systematics and her research responsibilities in the cytotaxonomy of grasses. I do not understand why the department felt a need to have a Director of the Herbarium. A curator is sufficient, given the proper perquisites.

3) I have only seen the job description for Shultz. I do not know that one exists for Barkworth. My remarks, therefore, reflect my ignorance on this. It is clear, from studying Shultz' job description and Barkworth's evaluation, that there is conflict in understanding of their respective duties and prerogatives. In no way, however, can I be persuaded that it was Barkworth's responsibility to suggest to Leila, even before this so-called evaluation was studied by administration, that she should look for employment elsewhere. I suggest that this is clear evidence of malice and legally constitutes harassment.

I find Barkworth's "evaluation" a tirade against Leila, presenting little useful evidence to the higher administration, but merely releasing a lot of pent-up resentment against a person who obviously would, if she returned to Logan with a Ph.D., present a real or fancied threat to her. If I had been the administrator receiving this, I would have returned it with the suggestion that Barkworth, in modern lingo, "cool it".

Barkworth seems to have chosen a very poor time to "invite" Shultz to send a synopsis of her activities, since I understand that Shultz was in California without access to her records. Barkworth's attack on Shultz seems to omit all specifics. What are the "policies of the Director" that Shultz is inconsistent with? What were the "requests for work on projects related to the herbarium" that have been ignored (would Shultz agree that these were such)? What is the policy on charging for identifications? Is this something that has been thought through so as to be in harmony with the policies of a State University? Obviously there is a conflict here and I would like to hear each side, spelled out. And what on earth is wrong with delegating some identifications to a student--this is part of the educational process. Barkworth seems to forget that the program has two parts-- work, and study. I do not understand the pointed reference, as if in criticism, of Shultz' field work ", with her husband,". I do not understand what is derogatory about verification of identifications made by others. We curators do this all the time.

Barkworth seems to be aware, and correctly so, of Shultz' desire to be independent of the Director. I think that this is the crux of the problem. What, if any, is the real value of having a Director of the Herbarium? As I say, I have not seen the administrative flow-sheet or job description, but I think that all of this trouble has arisen as a result of setting up an wholly unnecessary and, in the case of two strong personalities, patently predictable administrative problem. I hope that the administration will

have the wisdom to salvage the best out of this unpleasant situation, and if given the opportunity, I would like to be able to come to Logan and talk with you at greater length in the hopes of helping you to this end. There is no doubt that Barkworth and Shultz are competent and productive scholars, and both will be assets to the University, but unless they be separated from mutual interaction, the problem will continue.

In view of this intemperate evaluation on her part, I think you would be wise to discreetly investigate whether Barkworth has exhibited any past history of similar kinds of conduct and relationships with her peers elsewhere. Certainly, with her short tenure here she is not likely to have built up any great devotion to the University here, and since she is coming up for tenure you should be very clear about the situation you may be getting into for the foreseeable future. I think it is very significant that in no place in the Evaluation did Barkworth tell of any efforts she may have made to find common ground or to get along with Leila.

I would close by asking you to consider the consequences of Shultz leaving the University. I would suggest that if this happens, you will have to replace her with a more malleable person with little ambition and probably not as qualified. Can Barkworth, in fact, manage an herbarium? Will the new replacement for Shultz be, in effect, a technical assistant to Barkworth? Who will provide the expertise on the Utah Flora (besides the Stipeae)? Will the Intermountain Herbarium continue to exist as a vital part of the University? Universities are composed of three elements: the library, the collections (upon which the libraries are based), and a body of scholars. To lose one of these vital parts of a University may be irreversible tragedy for higher education in Utah.

Sincerely yours,

William A. Weber
Professor, Curator

UTAH STATE UNIVERSITY · LOGAN, UTAH 84322

COLLEGE OF SCIENCE

OFFICE OF THE DEAN

March 22, 1982

William A. Weber
Professor, Curator
University of Colorado
Henderson Building
Campus Box 218
Boulder, Colorado 80309

Dear Dr. Weber:

Thank you very much for your letter of March 16, 1982. I appreciate very much your thoughtful input to the Barkworth-Shultz matter. It will be most helpful; and as you can perceive, we are going to need all the help we can get, plus at least the wisdom of Solomon, in order to resolve this one.

We have written to Leila assuring her that we don't want her resignation, and that we do not suggest it as a resolution of the problem. I concur with you that she is a valuable person, whom I want to see remain in her present position.

Sincerely yours,

Ralph M. Johnson
Dean

RMJ:gr

cc: Dr. Gene Miller

Received 31 Jan. '83.

UTAH STATE UNIVERSITY

UMC 53, LOGAN, UTAH 84322
Phone (801) 750-2485

DEPARTMENT OF BIOLOGY
COLLEGE OF SCIENCE

DATE: January 27, 1983

TO: Leila Shultz, Herbarium Curator

FROM: Gene
Gene W. Miller
Department Head, Biology
Acting Herbarium Director

SUBJECT: Outline of Director and Curator Responsibilities-Direction of the Herbarium

Enclosed is my write-up outlining the operation of the Herbarium utilizing Director and Curator positions.

Input for this document was received primarily from the Herbarium Advisory Council, and faculty from the Biology Department.

My intent in producing this summary is to stabilize Herbarium operations and eliminate disturbing points of contention by clarifying the separate responsibilities of the Director and Curator.

The Departments aim has always been to have a faculty member serve as the Director. Reasons for this are given in the enclosed summary and have been voiced in our previous conversations.

My hope is that you can feel professionaly comfortable with your assignment as outlined and be able to work with the future Director and me to provide services necessary.

GWM/jk

OPERATION OF THE INTERMOUNTAIN HERBARIUM
WITH A DIRECTOR AND CURATOR

Description of Activities

Director - Director must be a faculty member in order to ensure the Herbarium fulfills and enhances its roles as a research, education and service facility. The Director would serve on graduate committees and teach courses utilizing the Herbarium.

The Director has full responsibility of the Herbarium with power to delegate specific responsibilities and functions to others. The responsibilities include all activities necessary for its development, maintenance and function.

The following specific assignments are the responsibility of the Director:

----making policy decisions concerning the operation and use of the Herbarium.

----supervision of all workers in the Herbarium.

----drawing up goals, particularly medium and long range goals and planning how to achieve them.

----ensuring that students and others that use the Herbarium are made aware of the appropriate procedures.

----assist the Curator in answering inquiries.

----preparing the Annual Report. The Director shall consult with the Curator in developing this report.

Curator - The Curator is a professional person and must have the technical background to perform services and assist the Director. Responsibilities are delegated by the Director for the curation of the plant collection. Principal accountabilities are:

1. Maintain and insure accessability of the plant collection for researchers, teachers and students using appropriate curatorial methods.

2. Provide accurate and scientific identification and consulation on species, distribution, and current nomenclatural status of the plant species of the Intermountain Region to the users of the Herbarium.

3. Personally responsible for the daily operations within the Herbarium.

HERBARIUM CURATOR ACTIVITIES

Curation - Management of the collection involves monthly and annual budget accounting, preparation of annual reports for the Director, acquisition of equipment and cases for the collection, and working with the Herbarium staff.

Maintenance of the collection involves keeping of loan records, maintaining the exchange balance, maintaining the library collection, protecting specimens from damage (dust, light, insects), and properly housing individual specimens (labeled folders).

Curation of the collection involves systematic examination and identification of specimens and research of the taxonomic literature in order to know how to classify species of the region, and be able to revise and reclassify specimens in accordance with current taxonomic thought and established policy. Revision of specimens is done through annotation of specimens, reordering of family groups, etc.

The collection growth is primarily through field research by research taxonomists who are responsible for generating exchange specimens sent to other institutions. Specimen acquisition through gifts are usually in return for the Curator's identification of specimens collected by state, federal, and some private agents.

Accessibility of the collection for students, classes and researchers depends primarily on the expertise of the curation and effective manageme The value of the collection as a research tool is enhanced by active grow The value of the Curator as a resource depends in large part on the exter of the incumbent's sensitivity to the needs of researchers.

The Curator is expected to know or be able to obtain access to information concerning the 5,000+ species of the Intermountain Region. The Curator is also expected to know the major vegetation types within the region and is the person perhaps best equipped to produce species checklists of specific geographic areas.

Budgets - Determination of budget priorities and capital expenditures are the responsibility of the Director. The Curator manages the Herbarium budget that is allocated for curatorial assistance, supplies and maintenance of the collection. The Curator is to maintain records for any charges for Herbarium services.

Research - Utilization of not more than 20% of time for research is allowable. Such research must be appropriate to the curatorial assignment as determined by the Director and not involve independent field or laboratory studies on specific taxonomic groups. Research or writing of floras and compilation of checklists is appropriate to the research assignment.

Consulting is a privilege that may be requested by faculty and staff in accordance with established University guidelines. Requests must have the approval of the Director, Department Head and Dean.

Grant proposals from the Herbarium are the responsibility of the Director. Curator involvement in such research is by prior arrangement.

Publications - Publication by the Curator is appropriate in cooperative research with research taxonomists or in the reporting of research consistent with the curatorial assignment.

eaching - The position of Curator does not involve any formalized teaching of students nor direction of graduate students.

As an educational resource, the main collection of the Intermounatin Herbarium is available for teaching specimens and is used in graduate research. A separate collection, duplicates of Utah material from the main collection, are maintained for routine identifications and is especially useful in undergraduate classes.

UTAH STATE UNIVERSITY

UMC 45, LOGAN, UTAH 84322
Phone (801) 750-1575

DEPARTMENT OF BIOLOGY
COLLEGE OF SCIENCE

1 February 1983

Dear Dr. Weber,

The interview at RSA went quite well, but I have no idea how "close" I've come. I left your name as a potential reference. You may or may not be called.

In view of the condition of things here and the "revised" position description which was waiting on my desk when I returned, let's pray that another position does appear. This place is such an abominable mess that I think that all I can do is bide my time. I believe that this whole situation with my position is the result of ignorance on the part of the department head and deviousness on the part of those who use him as a pawn. The head (Miller) is out of town until Feb. 17, so there is no urgency to act now. However, Dean Johnson is willing to call a committee meeting of "neutral" advisors and it may be possible to work through him in Miller's absence.

I have talked with one of our personnel officers who has told me (in confidence) that I would have a good case with Affirmative Action in making a charge of harrassment. This constant changing of position descriptions is absolute nonsense.

As I read the new position description, I will not be allowed to do any fieldwork (notice the exclusion of me from the category of research taxonomist), think about any "specific taxonomic group", or conduct any "independent research". Clever! I have talked myself blue in the face. Now I am going to work like the devil to get as far away as possible. In all conscience, however, I cannot leave this position in such a condition. There should be some clear legal issues here, such as downgrading of a position after 9 years (it is clearly downgraded from the Assistant Curator position described in 1973). Also, the position description signed by Dr. Miller on 12 November was revised after meetings with the Advisory Committee. I knew about the Director nonsense, but I had no idea what this latest document would contain or even that it was coming.

I am worn out.

I was treated graciously at RSA and the Chairman of the Board of Trustees, with whom I met on Thursday, will be coming to Utah this month and will be visiting with me at greater length. I am giving it my best shot, but I have a deep suspicion that I may be a token woman candidate. All I can do is try and hope that the world will change before it is too late.

Thanks for all you do.

Lila

UNIVERSITY OF COLORADO, BOULDER

Museum

7 Sept. 1983

Dear Askell:

The Steppea-Seriphidium question is a bit perplexing. I do not understand (not having ever seen his paper) why Polyakov chose a different type specimen for the new genus Seriphidium. If the type chosen had been A. cana, the type of Seriphidium as a section, it would be much simpler. Then I could accept Seriphidium for all of our species, and someone else would have to justify the maritima side of the group. However, I have a number of specimens of Asiatic species of Seriphidium Polyakov, and, while they are homogamous-headed and have the same type of style branches (flat, with a terminal tuft of trichomes, the Eurasian species still are a good step removed from ours. They are only suffruticose, most of them have finely pinnatisect leaves, and very elongate, stringy inflorescences. My question, am I on solid ground in lumping our things with the Eurasian branch simply on the basis of the homogamous heads? We have no chromosome numbers to help with this, nor experimental studies on the compatibility of any of these things.

I tend to feel that our western American things form a unified set, and to unite them with Seriphidium without better evidence is as shaky as setting up a separate genus for them. After all, the genus Artemisia sens. lat, while it is held together by the common feature of the sexuality of the heads, still contains many discrete genera which will eventually have to be considered. How can you include Artemisia vulgaris and its congeners along with Artemisia frigida in the same genus? The latter would be an ideal candidate for the name Steppea, wouldn't it? Please argue with me about this. Do you have a copy of Polyakov and could I copy it? The only Polyakov paper I have is the big general work on the Compositae.

I don't think Steppea would be very appropriate for A. pattersonii because it is strictly high-alpine. But we may think of somtething. I think you're quite right; O tried to interest Wiens in the generic suituation and suggested to him that one of the Arctic Archipelago species but now as I look at them, pattersonii stands quite alone with its broad many-flowered heads and brown-edged phyllaries.

INSTAAR has asked me for a review of the book, so I shall start on it soon. I'll have some questions for you, and I found what I think is a typographic error (Horrors!). Moss Champion should be Moss Campion. I'm sorry, but I see these even though I don't try to. Dagny's illustrations are really amazing for being so utterly simple and creating the ideal aspect of the plant.

Bill

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

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 Melanthiaceae, 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 notomention 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

16 June 1983

Dear Askell:

Unfortunately I couldn't stop in California on the fare that I was able to pay; this trip was on my own money. I would like to come, because a lot of nomenclatural things are still on my mind, but this summer is un-possible. Maybe in the fall, if I can find some money.

I have been very full of work getting the Hawaiian collections into the herbarium, and new problems constantly arise with the flora. I haven't done anything with Mesynium yet, but I am deep into Aletes. Mathias, Theobald & Tseng's monograph was terribly superficial. They should have been able to find species of Aletes hiding in Lomatium, Pteryxia and Cymopterus, but didn't,. and now I am having to straighten out all that mess. Again, Rydberg knew more than Mathias and Constance on the subject of the Aletes-like umbels.

Have you ever had thoughts about Melica and Bromelica? The separation into two genera for the North American ones based first on the disarticulation of the florets and glumes seems significant to me, and the fact that the Bromelicas never have the secund, flagged spikelets of the real melicas (typified, if Tsvelev is to be followed, by M. nutans). True Melica in America is Tertiary relictual, and most of them are South American. I do wonder whether Melica ciliata L. is really a Melica, it looks unique, but the Europeans haven't done anything about it. Unless anyone beats me down I am going with Bromelica and two combinations are needed for M. spectabilis and M. bulbosa, which I shallmake.

The runoff is so hard here that I am just as happy to stay home and work indoors and just make a few selected forays in Colorado. We're planning to go back to Santiago or Concepcion in December for a month collecting if the political situation doesn't get difficult for gringos.

You differ from Doug Dewey, as I recall, in your treatment of "Agropyron spicatum". I think I prefer to follow you on this rather than expand Elytrigia to that extent.

Leila, I think, will be the new director of the Rancho Santa Ana Botanic Garden. I haven't heard for sure, but she was one of a final list of two, and I have rumors that the other person decided not to pursue the job any farther. I hope this is true, because Leila has done a great job at Logan, for which she has only been harassed by Mary Barkworth, and it will be nice if she can rub their noses in the new development.

Suki had a week of worrisome illness and we just discovered that it was because she has been wearing a tick collar! Of course, Hartz does not have any warning on their products.

Love to Doris. We miss you more all the time. Vladimir has gone back to New York, where he has a job as a clerk in a washing machine repair shop, but he has a "key" to the New York Botanical Garden and I have borrowed a lot of saxifrages for him that I have shipped on.

Bill

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Medical and other benefits of full pension promised orally and in writing to the most prolific and most widely known biologist ever employed by the University. That fact cannot be erased by slander and perjury and worse miscarriages of justice — and even the fact that I have dared to suggest the un-American scientific opinions of genetically and evolutionarily sound taxonomy and phytogeography, continental drift, ... ought to be acknowledged.

I honor you since for the authorities and crooks and your weaknesses in fighting injustice even for yourself — but also your strong ethics.

So [illegible] look for supporters for a good cause, in and outside Buffalo and the country.

Over ten years of agony is certainly inappropriate punishment for un-American ideas.

Pope John Paul II in Krakow, 21/6'83:

"I ask you to call these weaknesses, these sins, these vices, these situations, by name. To fight against them constantly. Not to allow yourselves to be swallowed up by the wave of ... indifference and despondency".

Crime = Corrupt use of power based on lies, bribes, threats, . . .
Panel [illegible] by [illegible]?

Promised pension ...

Shmuck = scoundrel, devil,

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Askell Löve
5780 Chandler Court
San Jose CA 95133

Sorry, nothing in the Transpo syn mentioned. (probably post-1970!)

KEW INDEX SUPPLEMENTS 14, 15, 1961-1970

AEGILOPS:
geniculata Fig. & DeNot. in Mem. Accad. Sci. Torino, Ser. 2, xii (Agrost. Aegypt.) 262 (1852)--Aegypt.
intermedia Steud., Syn. Pl. Gram. 354 (1854).--Syria.
ligustica (Savign.) Coss. in Bull. Soc. Bot. France, xl. 164 (1864): Agropyron ligusticum.
trispiculata Hackel ex Battand. & Trab. Fl. Alger., Monocot., 241 (1895).--Alger
AGROPYRON: See xerox sheets.
CRITHOPSIS: none
DASYPYRUM:
hordeaceum (Coss. & Durieu) P. Candargy, Etude Monogr. Hordees (Arch. Biol. Veg. Athens, Fasc. I)35, in clavi,62(1901)[Triticum hordeaceum]
sinaicum (Steud.) P. Candargy, l.c.: Triticum sinaicum.
villosum (L.) P. Candargy, l.c.: Secale villosum.
ELYMUS: see xerox.
ELYTRIGIA: see xarox.
EREMOPYRUM: See xerox.
HENRARDIA:
hirtella Nikiforova in Opred. Rast. Sred. Azii, i. 165.200.(1968)--USSR (Centr. As.).
HETERANTHELIUM: none
HORDEUM: See xerox.
LEYMUS:
interior (Hulten)Tzvelev in Fl. Arct. URSS, Fasc. 2, 253 (1964): Elymus interior.
chinensis (Trin.) Tzvelev in Akad. Nauk SSSR Inst. Bot. Komarova, Rast. Tsentral. Azii, Fasc. 4, 205 (1968): Triticum chinense.
lanatus (Keng) Tzvelev in Novit. syst. Pl. Vasc. Acad. Sci. URSS, vl. 21 (1970): Elymus lanatus.
ligulatus (Keng) Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral Azii, Fasc. 4. 206 (1968): Elymus dasystachys var. ligulatus.
secalinus (Georgi) Tsvelev, l.c. 209: Triticum secalinum.
PSATHYROSTACHYS:
hyalantha (Rupr.) Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral Azii, Fasc. 4, 202 (1968): Elymus hyalanthus.
caduca (Boiss.) Melderis in K. Dansk. Vid. Selsk. Biol. Skrift., xiv. No. 4 (Symb. Afghan. vi)93 (1965): Elymus caducus.
SECALE: none
TAENIATHERUM: none
TRITICUM:
dicoccoides (Koern. ex Aschers. & Graebn.) Aaronsohn in Verh. Zool.-Bot. Ges. Wien, lix.491 (1910): T. dicoccum var. dicoccoides.
pungens (Pers.) DC. in Lam. & DC. Fl. Franc., ed. 3, Tome v vel Vol. vi. 283 (1815): T. junceum pungens.
(alii vide Xerox)

SAXIFRAGACEAE

GENERA RECOGNIZED: Chrysosplenium, Ciliaria, Conimitella, Heterisia, Heuchera, Hirculus, Lithophragma, Micranthes, Mitella, Muscaria, Parnassia, Saxifraga, Spatularia, Sullivantia, Telesonix.

CHRYSOSPLENIUM TETRANDRUM (N. LUND) TH. FRIES
CILIARIA AUSTROMONTANA (WIEG.) PIPER
CONIMITELLA WILLIAMSII (D. C. EAT.) RYDB.
HETERISIA ODONTOLOMA (PIPER) W. A. WEBER INED.
HEUCHERA BRACTEATA (TORR.) SER.
HEUCHERA HALLII A. GRAY
HEUCHERA PARVIFOLIA NUTT. EX T. & G. VAR. NIVALIS (ROSEND.) LOVE, LOVE & KAPOOR
HEUCHERA PARVIFOLIA NUTT. EX T. & G. VAR. PARVIFOLIA
HEUCHERA RICHARDSONII R. BR.
HEUCHERA RUBESCENS TORR. IN STANSB.
HIRCULUS PLATYSEPALUS (TRAUTV.) W. A. WEBER INED.
HIRCULUS PRORERENS (Sternb.) W. A. WEBER INED.
HIRCULUS SERPYLLIFOLIUS (PURSH) ..... SSP. CHRYSANTHUS (A. GRAY) W. A. WEBER INED.
LITHOPHRAGMA GLABRUM NUTT.
LITHOPHRAGMA PARVIFLORUM (HOOK.) NUTT. EX T. & G.
LITHOPHRAGMA TENELLUM NUTT.
MICRANTHES OREGANA (HOWELL) SMALL SSP. MONTANENSIS (SMALL) W. A. WEBER INED.
MICRANTHES RHOMBOIDEA (GREENE) SMALL
MITELLA PENTANDRA HOOK.
MITELLA STAUROPETALA PIPER VAR. STENOPETALA (PIPER) ROSEND.
MUSCARIA ADSCENDENS (L.) SSP. OREGONENSIS (RAF.) W. A. WEBER ined.
MUSCARIA DELICATULA SMALL
MUSCARIA MICROPETALA SMALL
MUSCARIA MONTICOLA SMALL
PARNASSIA FIMBRIATA KONIG
PARNASSIA KOTZEBUEI CHAM. & SCHLECHT.
PARNASSIA PARVIFLORA DC.
SAXIFRAGA CERNUA L.
SAXIFRAGA HYPERBOREA R. BR. SSP. DEBILIS (ENGELM. & GRAY LOEVE LOEVE & KAPOOR
SAXIFRAGA RIVULARIS L.
SPATULARIA FOLIOLOSA (R. BR.) SMALL (I am dubious)
SULLIVANTIA HAPEMANII (COULT. & FISCH.) COULT. VAR. PURPUSII (BRAND) SOLTIS INED.
TELESONIX JAMESII (TORR.) RAF.

UNIVERSITY OF COLORADO MUSEUM
BOULDER, COLORADO 80309

Dear Askell 16 Jan 1982

Here are the Colorado saxifrages as I think I see them. Is the combination Hirculus made for serpyllifolius? There was, I think, some talk between Vlad and me about crandallii being the right species for the Colorado Taxon.

I think Small should have taken some of the species out of Micranthes and add them to his Heterisia. With the exception of S. rotundifolia, this group seems to be Amphi-Beringian, and I think the vegetative character of odontoloma, nelsoniana etc. fits with the Heterisia group. Do you agree that this group should be separated from Micranthes?

I am not really convinced about Spatularia. S. foliolosa definitely seems to belong with stellaris. Are they really to be separated from Micranthes?

How about adscendens and tridactylites? And its generic position? I am puzzled.

Outside of Colorado, I like Chondrosea, Robertsonia, Antiphylla. Where would you put S. exchscholtzii, in Antiphylla??? Or in another exotic Asiatic group?

Saxifraga nudicaulis doesn't seem to really belong anywhere, so perhaps Small's assinment of it to Ocrearia is right.

I have tentatively gone through the herbarium and most everything seems to fall justifiably into genera; there are a few problems, and of course I don't have enough of the Asiatic things.

Small has to be wrong about Micranthes Geum. This is a Robertsonia. Could it have been introduced into Newfoundland on ballast perhaps?

It will be a long time before I have to write up the saxifrages but if you can comment on my list it will be filed away till then, and I'll get Vlad's opinions too.

B.

ELYMUS:—
burchan-buddae (*Nevski*) *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 220 (1968): Agropyron burchan-buddae.
canaliculatus (*Nevski*) *Tzvelev, l. c.*: Agropyron canaliculatum.
confusus (*Roshev.*) *Tzvelev, l. c.* 221: Agropyron confusum.
czilikensis (*Drobov*) *Tzvelev, l. c.* 214: Agropyron czilikense.
czimganicus (*Drobov*) *Tzvelev, l. c.* 221: Agropyron czimganicum.
fibrosus (*Schrenk*) *Tzvelev in Sched. Herb. Fl. URSS*, xviii. 29 (1970): Triticum fibrosum.
franchetii *Kitagawa in Journ. Jap. Bot.* xliii. 189 (1968): E. cylindricus (*Franch.*) *Honda*.
gmelinii (*Ledeb.*) *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 216 (1968): Triticum caninum var. gmelinii.
komarovii (*Nevski*) *Tzvelev, l. c.*: Agropyron komarovii.
kronenburgii (*Hackel*) *Nikiforova in Opred. Rast. Sred. Azii*, i. 196 (1968): Hordeum kronenburgii.
kronokensis (*Komarov*) *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 216 (1968): Agropyron kronokense.
laevis (*Scribn. & J. G. Smith*) *Hoover in Leafl. West. Bot.* x. 339 (1966): Agropyron parishii var. laeve.
latiglumis *Nikiforova in Opred. Rast. Sred. Azii*, i. 192, 201 (1968).—U.S.S.R. (Centr. As.).
macrolepis (*Drobov*) *Tsvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 217 (1968): Agropyron macrolepis.
macrourus (*Turcz.*) *Tzvelev in Sched. Herb. Fl. URSS*, xviii. 30 (1970), in obs.: Triticum macrourum.
mutabilis (*Drobov*) *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 217 (1968): Agropyron mutabile.
nevskii *Tzvelev in Sched. Herb. Fl. URSS*, xviii. 29 (1970): Agropyron ugamicum.
panormitanus (*Bertol.*) *Tzvelev, l. c.* 27: Triticum panormitanum.
pectinatus (*M. Bieb.*) *Lainz in Bol. Inst. Estud. Astur.*, Supl. Cienc., No. 15 (*Aport. Conoc. Fl. Cántabro-Astur*, ix.) 44 (1970), in adnot.: Triticum pectinatum.
pendulinus (*Nevski*) *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 218 (1968): Roegneria pendulina.
praecaespitosus (*Nevski*) *Tzvelev, l. c.*: Agropyron praecaespitosum.
scabridulus (*Ohwi*) *Tzvelev, l. c.*: Agropyron scabridulum.
subfibrosus (*Tzvelev*) *Tzvelev in Sched. Herb. Fl. URSS*, xviii. 30 (1970), in obs.: Roegneria subfibrosa.
subsecundus (*Link*) *Hoover in Leafl. West. Bot.* x. 339 (1966): Triticum subsecundum.
svensonii *Church in Rhodora*, lxix. 134 (1967).—U.S.A. (Tennessee).
trachycaulus (*Link*) *Hoover in Leafl. West. Bot.* x. 340 (1966): Triticum trachycaulum.
transbaicalensis (*Nevski*) *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 219 (1968): Agropyron transbaicalense.
trinii *Melderis in K. H. Rechinger, Fl. Iran.*, Lief. 70, 225 (1970): Agropyron ramosum.
× vancouverensis *Vasey* pro sp.; *Bowden in Canad. Journ. Bot.* xxxv. 973 (1957).—Canada (Vancouver Isl.).
varius (*Keng*) *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 219 (1968), cum descr. lat.: Roegneria varia.
vernicosus (*Nevski ex Grubov*) *Tzvelev, l. c.*: Agropyron vernicosum.

×**ELYSITANION** Bowden in Canad. Journ. Bot. xlv. 721 (1967). *GRAMINEAE*.
[ELYMUS × SITANION.]
aristatum (*Merrill*) *Bowden, l. c.* 722: Elymus aristatus.
hansenii (*Scribn.*) *Bowden, l. c.* 721: Elymus hansenii.

UNIVERSITY OF COLORADO MUSEUM
BOULDER, COLORADO 80309

Dear Askell:

Santesson's retiring has no effect on me; he has been dead as far as I am concerned for many years; he never answers letters and publishes nothing, although of course he is helpful to people who are close enough to him to get things done where he is; he is a big disappointment to me; I also have not received any exchange from Stockholm in all of his tenure.

Part of the caprock of Castle Rock is cracking and the rich people's houses are in danger. They will go to a lot of expense to remove the rock although they were warned (the city) long ago by the geologists that they should not allow building there. California is not alone. Certainly taking Stenactis, Conyza out tidies up Erigeron a little, but it leaves E. peregrinus and a lot of other distinctive groups. The Russians have different groupings than I can agree with, but it is a very big problem. I have just made my key to Aster, and in going through the herbarium pulling our the species of Eucephalus, which I am recognizing, I found to my delight that you have pulled Tripolium out (Aster tripolium certainly stands alone). Aster is a very big dumping ground but I don't have to do much with it yet.

Thanks for "läkare". I'll enter your note with my copy of the libretto.

Enclosed are all the citations; very little in the way of new taxa, thank goodness!

B.

×**ELYHORDEUM** Mansf. apud Zizin & Petrowa in Der Züchter, xxv. 164 (1955), in adnot.: **Hordelymus** Bachtj. & Darevsk. (Gramin.).
schmidii (*Melderis*) *Melderis in K. Danske Vid. Selsk.*, Biol. Skrift., xiv. No. 4 (*Symb. Afghan.* VI.) 89 (1965): Elymordeum schmidii.

×**ELYMORDEUM** Lepage (Gramin.).
littorale *Hodgson & W. W. Mitch. in Canad. Journ. Bot.* xliii. 1355 (1965).—U.S.A. (Alaska).•

ELYMUS L. (Gramin.).
×aleuticus *Hultén*, pro spec.; *Bowden in Canad. Journ. Bot.* xlii. 564 (1964).—Ins. Aleut.
angustiformis *Pavlov in Vestnik Akad. Nauk Kazak. SSR*, No. 5 (86) 86 (1952).—U.R.S.S. (As. centr.).
donianus (*F. B. White*) *Á. & D. Löve in Taxon*, xiii. 201 (1964): Agropyron donianum.
×maltei *Bowden in Canad. Journ. Bot.* xlii. 575 (1964).—Canada (Quebec; Ontario).
piperi *Bowden, l. c.* 592: E. condensatus var. pubens *Piper*.
rechingeri (*Runemark*, sine ref.) *Runemark in Hereditas*, xlviii. 548 (1962): Agropyron rechingeri.•
subsecundus (*Link*) *Á. & D. Löve in Taxon*, xiii. 201 (1964): Triticum subsecundum.
×ucluelentensis *Bowden in Canad. Journ. Bot.* xlii. 563 (1964).—Canada (Ins. Vancouver).

×**ELYHORDEUM** Mansf. (Gramin.).
arcuatum *W. W. Mitch. & Hodgson in Rhodora*, lxx. 470 (1968).—U.S.A. (Alaska).•
berkeleyanum (*Bowden*) *Bowden in Canad. Journ. Bot.* xlv. 720 (1967): Elymordeum berkeleyanum.
bowes-lyonii (*Melderis*) *Melderis in K. H. Rechinger, Fl. Iran.*, Lief. 70, 243 (1970): Elymordeum bowes-lyonii.
dakotense (*Bowden*) *Bowden in Canad. Journ. Bot.* xlv. 719 (1967): Elymordeum dakotense.
dutillyanum (*Lepage*) *Bowden, l. c.* 718: Elymordeum dutillyanum.
iowense *R. W. Pohl in Brittonia*, xviii. 255 (1966).—U.S.A. (Iowa).•
montanense (*Scribn.*) *Bowden in Canad. Journ. Bot.* xlv. 720 (1967): Hordeum montanense.
piperi (*Bowden*) *Bowden, l. c.* 719: Elymordeum piperi.
schaackianum (*Bowden*) *Bowden, l. c.*: Elymordeum schaackianum.
stebbinsianum (*Bowden*) *Bowden, l. c.*: Elymordeum stebbinsianum.
triploideum (*Bowden*) *Bowden, l. c.*: Elymordeum triploideum.

brunonis (*Endl.*) *George, l. c.* brunonis.•
emarginata (*Lindl.*) *George, l.* emarginata.•

ELYTRIGIA Desv. (Gramin.).
disticha (*Thunb.*) *Prokudin ex Á. Löv* Beih. 3, 83 (1962): Triticum disti
maritima (*Koch & Ziz*) *Tzvelev in* *Vasc., Acad. Sci. URSS*, 196 Triticum repens var. maritimum.
villosa (*Drobov*) *Tzvelev in Fl. Arct.* 2, 247 (1964), in obs.: Brachypod

EMBADIUM J. M. Black (Boraginac
johnstonii *Ising in Trans. Roy. So* lxxxix. 288 (1965).—Austral. (S.
uncinatum *Ising, l. c.* 287.—Austral.

EMBERGERIA Boulos in Hj. Eichle Black, Fl. S. Austral., ed. 2, 332 (*POSITAE.*
grandifolia (*T. Kirk*) *Boulos, l. c.*: S folius.
megalocarpa (*Hook. f.*) *Boulos, l. c.* asper var. megalocarpus.

EMILIA Cass. (Compos.).

Biol., 1966, 92 (1967).—Aegean Isl.
sikkimense (*Hook. f.*) *Chrtek, l. c.* 1967, 104 (1968): Avena sikkimensis.
thospiticum *Chrtek in Bot. Notiser*, cxix. 489 (1966).—Turkey.
turcicum *Chrtek, l. c.* 487.—Turkey; U.S.S.R. (Caucas.).

TRISTACHYA Nees (Gramin.).
bicrinita (*Phipps*) *W. D. Clayt. in Kew Bull.* xxi. 124 (1967): Dolichochaete bicrinita.
viridearistata (*Phipps*) *W. D. Clayt. l. c.*: Veseyochloa viridearistata.

TRISTAGMA Poepp. (Liliac.).
anemophilum *Ravenna in Bol. Soc. Argent. Bot.* xi. 147 (1967).—Argent.•
narcissoides (*R. Phil.*) *Traub in Plant Life*, xxiv. 49 (1968): Stemmatium narcissoides.

TRISTEMMA Juss. (Melastomatac.).
thomense *Ferreira in Garcia de Orta*, xvi. 66, 76 (1968).—S. Tomé Isl.•

TRISTIROPSIS Radlk. (Sapindac.).
ferruginea *Leenh. in Blumea*, xiii. 395 (1966).—Borneo.

TRITELEIA Dougl. ex Lindl. (Liliac.).
guadalupensis *Lenz in Aliso*, vii. 145 (1970).—Mexico (Guadalupe Isl.).•
×tubergenii *Lenz, l. c.* 159.—Cult.

TRITICUM L. (Gramin.).
markgrafii *Greuter in Boissiera*, xiii. 172 (1967): Aegilops cylindrica *Sibth. & Sm.*
neglectum (*Req. ex Bertol.*) *Greuter, l. c.* 171, in adnot.: Aegilops neglecta.
palaeo-colchicum *Menabde in Comm. Georg. Br. Acad. Sci. U.S.S.R. (Mitt. Georgisch. Abt. Akad. Wiss.)* i. No. 9, 686 (1940).—U.S.S.R. (Caucas.).
rectum (*Zhukov*) *Bowden in Canad. Journ. Genet. & Cytol.* viii. 135 (1966): Aegilops triaristata subsp. recta.
syriacum *Bowden, l. c.*: Aegilops crassa subsp. vavilovii *Zhukov*.
×turgidovillosum *E. Tschermak in Ber. Deutsch. Bot. Ges.* xlviii. 40 (1930).—Hab.?
vagans (*Jord. & Fourr.*) *Greuter in Boissiera*, xiii. 170 (1967): Aegilops vagans.
zhukovskyi *Menabde & Erizin in Comm. Georg. Br. Acad. Sci. U.S.S.R. (Mitt. Georgisch. Abt. Akad. Wiss.)* No. 16, p.? (1958); cf. *Bowden in Canad. Journ. Bot.* xxxvii. 672 (1959).—U.S.S.R. (Caucas.).

TRIURANTHERA Backer (Melastomatac.).
hirsuta *Nayar in Kew Bull.* xx. 244 (1966).—Borneo.

TRIVALVARIA Miq. (Annonac.).
kanjilalii *D. Das in Bull. Bot. Surv. Ind.* x. 263 (1969).—India.•

TRIXIS Adans. Fam. Pl. ii. 76, 613 (1763); vide Dandy, Ind. Gen. Vasc. Pl. 1753–74 (Regn. Veg. li.) 87 (1967), non P. Br. *HALORAGACEAE.*

TRIXIS P. Br. (Compos.).
churinensis *B. Herrera in Publ. Mus. Hist. Nat. 'Javier Prado', Lima*, Ser. Bot., No. 25, 14 (1969).—Peru.•
subparadoxa *B. Herrera, l. c.* 6.—Peru.•

TROCDARIS Rafin. Good Book Amenit. Nat., Philad. 50 (1840); cf. Amer. Midl. Nat. iii. Append. (1913). *UMBELLIFERAE.*

EREMOPYRUM:—
prostratum (*L. f.*) *P. Candargy, l. c.* 32, in clavi, 58: Triticum prostratum.
sibiricum (*Willd.*) *P. Candargy, l. c.* 33, in clavi, 60: Triticum sibiricum.

EREMOSTACHYS Bunge (Labiat.).
ambigua *Popov in Fl. Uzbekist.* v. 332, 633 (1961).—U.R.S.S. (As. centr.).
anisochila *Pazij & Vved. in Fl. Uzbekist.* v. 339, 634 (1961).—U.R.S.S. (As. centr.).
codonocalyx *Pazij & Vved. l. c.* 344: E. desertorum subsp. ferganensis *Popov.*

×AGROËLYMUS G. Camus ex Rousseau (Gramin.).
bowdenii *B. Boiv. in Natur. Canad.* xciv. 520 (1967).—Canada (Alberta).
cayouetteorum *B. Boiv. l. c.*—Canada (Quebec).
dorei *Bowden in Canad. Journ. Bot.* xlv. 715 (1967).—Canada (Quebec).
hultenii *Melderis apud Hultén in Arkiv Bot., Stockh.*, Ser. 2, vii. 21 (1968).—U.S.A. (Alaska).
pamiricus (*Melderis*) *Melderis in K. H. Rechinger, Fl. Iran.*, Lief. 70, 225 (1970): Agropyron pamiricum.

×AGROHORDEUM G. Camus ex Rousseau (Gramin.).
jordalii *Melderis apud Hultén in Arkiv Bot., Stockh.*, Ser. 2, vii. 21 (1968).—U.S.A. (Alaska).

AGROPYRON Gaertn. (Gramin.).
calcareum *Černj. in Novit. Syst. Pl. Vasc., Acad. Sci. URSS*, 1966, 304 (1966).—Yugoslav.•
dolicholepis *Melderis in K. H. Rechinger, Fl. Iran.*, Lief. 70, 180 (1970): Roegneria sclerophylla.
festucifolium *Černj. & C. Chase in Novit. Syst. Pl. Vasc., Acad. Sci. URSS*, 1966, 306 (1966).—Albania.•
gentryi *Melderis in K. H. Rechinger, Fl. Iran.*, Lief. 70, 165 (1970).—Iran.
grandiglume (*Keng*) *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 188 (1968), cum descr. lat.: Roegneria grandiglumis.
hajastanicum *Tzvelev in Novit. Syst. Pl. Vasc., Acad. Sci. URSS*, 1966, 292 (1966).—U.S.S.R. (Caucas.).•
kengii *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 188 (1968), cum descr. lat.: Roegneria hirsuta.
kokonoricum (*Keng*) *Tsvelev, l. c.*, cum descr. lat.: Roegneria kokonorica.
koryoënse *Honda in Kôryô-shikenrin-no-ippan*, 78 (1932).—Korea.
kosaninii *Černj. & Soška in Novit. Syst. Pl. Vasc., Acad. Sci. URSS*, 1966, 302 (1966).—Albania.•
lachnophyllum (*Ovczinn. & Sidorenko*) *Bondarenko in Opred. Rast. Sred. Azii*, i. 173 (1968): Roegneria lachnophylla.
macrochaetum (*Nevski*) *Bondarenko, l. c.* 170: Roegneria macrochaeta.

lithophilus.

ELYMUS L. (Gramin.).
abolinii (*Drobov*) *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 214 (1968): Agropyron abolinii.
aemulans (*Nevski*) *Nikiforova in Opred. Rast. Sred. Azii*, i. 197 (1968): Aneurolepidium aemulans.
alaskanus (*Scribn. & Merrill*) *Á. Löve in Taxon*, xix. 299 (1970): Agropyron alaskanum.
antiquus (*Nevski*) *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 220 (1968): Agropyron antiquum.

l. Soc.
osmia
'enez.

HORDEUM L. (Gramin.).
×caespitosum *Scribn.*, pro spec.; *W. W. Mitch. & Wilton in Madroño*, xvii. 279 (1964).—Amer. bor.
intercedens *Nevski in Acta Inst. Bot. Acad. Sci. URSS*, Ser. 1 (*Fl. & Syst. Pl. Vasc.*) Fasc. 5, 222 (1941).—U.S.A. (Calif.).•

69

HORDEUM:—
nevskianum *Bowden in Canad. Journ. Genet. & Cytol.* vii. 396 (1965): H. brevisubulatum *Nevski in Acta Inst. Bot. Acad. Sci. URSS*, Ser. 1 (*Fl. & Syst. Pl. Vasc.*) Fasc. 5, 175 (1941), non *Link.*
procerum *Nevski in Acta Inst. Bot. Acad. Sci. URSS*, Ser. 1 (*Fl. & Syst. Pl. Vasc.*) Fasc. 5, 148 (1941).—Patag.•
roshevitzii *Bowden in Canad. Journ. Genet. & Cytol.* vii. 395 (1965): H. sibiricum *Roshev.*

HORMATHOPHYLLA Cullen & T. R. Dudley in

speciosa.

EREMOPOA Ros.-v. (Gramin.).
glareosa *Gamajun. in Not. Syst. Herb. Inst. Bot. Acad. Sci. Kasachst.* ii. 11 (1964).—U.R.S.S. (As. centr.).

EREMOPYRUM Jaub. & Spach (Gramin.).
bourgaei (*Boiss.*) *P. Candargy, Étude Monogr. Hordées (Archiv. Biol. Vég., Athènes*, Fasc. 1) 33, in clavi, 60 (1901): Agropyron bourgaei.
brownei (*Kunth*) *P. Candargy, l. c.* 34, in clavi, 62: Triticum brownei.
bulbosum (*Boiss.*) *P. Candargy, l. c.* 34, in clavi, 61: Agropyron bulbosum.
confusum *Melderis in Anz. Österr. Akad. Wiss.*, Math.-Nat., ci. 15 (1964).—Iraq.
dasyanthum (*Ledeb.*) *P. Candargy, Étude Monogr. Hordées (Archiv. Biol. Vég., Athènes*, Fasc. 1) 34, in clavi, 62 (1901): Agropyron dasyanthum.
dasyphyllum (*Schrenk*) *P. Candargy, l. c.* 34, in clavi, 60: Triticum dasyphyllum.
fibrosum (*Schrenk*) *P. Candargy, l. c.* 34, in clavi: [Triticum fibrosum].
kotschyanum (*Boiss. & Hohen.*) *P. Candargy, l. c.* 33, in clavi, 59: Agropyron kotschyanum.
olgae (*Regel*) *P. Candargy, l. c.*: Triticum olgae.
patulum (*Willd.*) *P. Candargy, l. c.* 33, in clavi, 60: Triticum patulum.

XIV

AGROPYRON:—
subeglume *P. Candargy*, *l. c.* 32, in clavi, 64.—N. Zel.
sylvaticum (*Moench*) *Chevall. Fl. Paris*, ii. 196 (1827): Triticum sylvaticum.
uninerve *P. Candargy*, *Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 23, in clavi, 43 (1901): Triticum chinense.
virescens (*Panč. ex Aschers.*) *P. Candargy*, *l. c.* 31, in clavi, 55: Triticum virescens.
youngii (*Hook. f.*) *P. Candargy*, *l. c.* 20, in clavi, 39 (younghii): Triticum youngii.

AGROSTIS L. (Gramin.).
agrostidiformis (*Roshev.*) *Bor in Árbok Univ. Berg.*,

AGROPYRON:—
muticum (*Keng*) *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 189 (1968), cum descr. lat.: Roegneria mutica.
nathaliae *Sipliv. in Novit. Syst. Pl. Vasc., Acad. Sci. URSS*, 1968, 13 (1968).—U.S.S.R. (Siberia).
stenachyrum (*Keng*) *Tzvelev in Akad. Nauk SSSR Bot. Inst. Komarova, Rast. Tsentral. Azii*, Fasc. 4, 190 (1968), cum descr. lat.: Roegneria stenachyra.
stenostachyum *Melderis in K. H. Rechinger, Fl. Iran.*, Lief. 70, 175 (1970).—Afghan.
subaristatum *Kitagawa in Journ. Jap. Bot.* xliv. 273 (1969).—Mongol.
tilcarense *J. H. Hunziker in Kurtziana*, iii. 121 (1966).—Argent.
transhyrcanum (*Nevski*) *Bondarenko in Opred. Rast. Sred. Azii*, i. 173 (1968): Roegneria transhyrcana.
transnominatum *Bondarenko*, *l. c.* 172: Roegneria sclerophylla.
troctolepis (*Nevski*) *Melderis in K. H. Rechinger, Fl. Iran.*, Lief. 70, 182 (1970): Roegneria troctolepis.

×**AGROSITANION** Bowden in Canad. Journ. Bot. xlv. 720 (1967). *GRAMINEAE*.
[AGROPYRON × SITANION.]
saundersii (*Vasey*) *Bowden*, *l. c.*: Elymus saundersii.
saxicola (*Scribn. & J. G. Smith*) *Bowden*, *l. c.* 721: Elymus saxicola.

AGROSTANA Hill, Veg. Syst. v. 32 (1763); vide Dandy, Ind. Gen. Vasc. Pl. 1753–74 (Regn. Veg. li.) 24 (1967). *UMBELLIFERAE*.

AGROSTIS L. (Gramin.).
agrostiflora (*G. Beck*) *Rauschert in Fedde, Repert.* lxxiii. 49 (1966): Calamagrostis agrostiflora.
balansae (*Boiss.*) *Tzvelev in Novit. Syst. Pl. Vasc., Acad. Sci. URSS*, vi. 20 (1970), in obs.: Calamagrostis balansae.
clivicola *Crampton in Brittonia*, xix. 174 (1967).—U.S.A. (Calif.).•
emirnensis (*Baker*) *Bosser in Adansonia*, n. s., viii. 516 (1968): Deyeuxia emirnensis.
lenis *Roseng., Arrill. de Maffei & Izag. de Artucio, Gramin. Urug.* 23 (1970).—Urug.; Braz. (Rio Grande do Sul).•
marschalliana *Sereďin in Novit. Syst. Pl. Vasc. Acad. Sci. URSS*, 1966, 9 (1966): A. tenuifolia *M. Bieb.*
meionectes *Vickery in Contrib. N. S. Wales Nat. Herb.* iv. 12 (1966).—Austral. (N. S. W.; Victoria).
nevskii *Tzvelev in Novit. Syst. Pl. Vasc., Acad. Sci. URSS*, vi. 20 (1970): Calamagrostis hissarica.

×**AGROTRISECALE** Ciferri & Giacom. Nomencl. Fl. Ital., Pt. 1, 48 (1950). *GRAMINEAE*.
[AGROPYRON × SECALE × TRITICUM.]

×**AGROTRITICUM** Ciferri & Giacom. Nomencl. Fl. Ital., Pt. 1, 48 (1950). *GRAMINEAE*.
[AGROPYRON × TRITICUM.]

AGLAIA Lour. (Meliac.).
rufinervis (*Blume*) *Bentvelzen in Acta Bot. Neerl.* xi. 19 (1962): Trichilia rufinervis.

AGRESTIS Bubani (Gramin.).
alpina (*Scop.*) *Bubani, Fl. Pyren.* iv. 287 (1901): Agrostis alpina.
canina (*L.*) *Bubani, l. c.* 286: Agrostis canina.
interrupta (*L.*) *Bubani, l. c.* 289: Agrostis interrupta.
polymorpha (*Huds.*) *Bubani, l. c.* 283: Agrostis polymorpha.
rupestris (*All.*) *Bubani, l. c.* 288: Agrostis rupestris.
schleicheri (*Jord. & Verlot*) *Bubani, l. c.*: Agrostis schleicheri.
setacea (*Curt.*) *Bubani, l. c.* 286: Agrostis setacea.
verticillata (*Vill.*) *Bubani, l. c.* 282: Agrostis verticillata.

AGRIMONIA L. (Rosac.).
adscendens *Andrz. in Trud. Kom. Vys. Uchr., Kiev*, No. 1 (*Enum. Pl. Podol.*) 36 (1860).—U.R.S.S. (Ukraine).
godetiana *Andrz. l. c.*—U.R.S.S. (Ukraine).
grandis (*Aschers. & Graebn.*) *Andrz. ex Dobrocz. Fl. URSR*, vi. 166–169 (1954);• cf. *Skalický in Acta Hort. Bot. Prag.* 1962, 101 (1962): A. eupatoria var. grandis.
robusta *Andrz. in Trud. Kom. Vys. Uchr., Kiev*, No. 1 (*Enum. Pl. Podol.*) 36 (1860).—U.R.S.S. (Ukraine).
tokatiensis *K. Ito in Hokuriku Journ. Bot.* ix. 69 (1961).—Japon.•

×**AGROCALAMAGROSTIS** Aschers. & Graebn. Syn. Mitteleur. Fl. ii. 1. 223 (1899). *GRAMINEAE.*
[AGROSTIS × CALAMAGROSTIS.]
stebleri *Aschers. & Graebn. l. c.*—German.

×**AGROËLYMUS** G. Camus ex Rousseau (Gramin.).
mossii *Lepage in Natur. Canad.* xcii. 214 (1965).—Canada (Alberta).•

×**AGROHORDEUM** G. Camus ex Rousseau (Gramin.).
pilosilemma *W. W. Mitch. & Hodgson in Bull. Torr. Bot. Cl.* xcii. 404 (1965).—U.S.A. (Alaska).

AGROPYRON Gaertn. (Gramin.).
aitchisonii (*Boiss.*) *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 20, in clavi, 40 (1901): A. longearistatum var. aitchisonii.
alpinum (*Schur*) *Schur ex P. Candargy, l. c.* 23, in clavi, 43: A. caninum var. alpinum.
altissimum (*Schur*) *Schur ex P. Candargy, l. c.* 27, in clavi, 48: [A. repens var. altissimum].
arundinaceum (*Steud.*) *P. Candargy, l. c.* 28, in clavi, 48: Triticum arundinaceum.
banaticum (*Heuffel*) *Thaisz in Magyar Bot. Lap.* ii. 1 (1903): Triticum rigidum var. banaticum.
bessarabicum *Săvul. & Rayss in Bull. Sec. Sci. Acad. Roum.*, No. 10, 282 (1923); cf. *Săvul. & Rayss, Mat. Fl. Basarab.*, Pt. 1, 42 (1924).—U.R.S.S. (Moldav.).•
boliviacum *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 25, in clavi, 46 (1901).—Boliv.
×brevifolium *Scribn.*, pro spec.; *Bowden in Canad. Journ. Bot.* xliii. 1444 (1965).—Amer. bor.
callosum *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 23, in clavi, 43 (1901).—U.S.A. (Calif.).
cognatum *Hackel apud Kneucker in Allg. Bot. Zeitschr.* 1904, 22 (1905), in obs.—Kashmir.
colorans *Melderis in K. Danske Vid. Selsk.*, Biol. Skrift., xiv. No. 4 (*Symb. Afghan.* vi.) 85 (1965).—Afghan.•
diamesum *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 24, in clavi, 44 (1901).—Turcia; India.
distachyos (*L.*) *Chevall. Fl. Paris*, ii. 196 (1827): Bromus distachyos.
divergens (*Steud.*) *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 21, in clavi, 41 (1901): Triticum divergens.
duvalii (*Loret & Barrand.*) *P. Candargy, l. c.* 56: Triticum duvalii.

AGROPYRON:—
edelbergii *Melderis in K. Danske Vid. Selsk.*, Biol. Skrift., xiv. No. 4 (*Symb. Afghan.* vi.) 87 (1965).—Afghan.•
eglume *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 67 (1901), in. obs., nomen subnudum.—Hab.?
elymoides (*Hochst. ex A. Rich.*) *P. Candargy, l. c.* 22, in clavi, 41: Triticum elymoides.
elymoides *P. Candargy, l. c.* 32, in clavi, 57: Elymus triticoides.
fibrosum (*Schrenk*) *P. Candargy, l. c.* 24, in clavi, 44: Triticum fibrosum.
flaccidifolium (*Boiss. & Heldr.*) *P. Candargy, l. c.* 29, in clavi, 51: A. elongatum var. flaccidifolium.
fragile (*Roth*) *P. Candargy, l. c.* 58: Triticum fragile.
gmelinii (*Trin. ex Schrad.*) *P. Candargy, l. c.* 23, in clavi, 42: [Triticum gmelinii].
gracile (*DC.*) *Chevall. Fl. Paris*, ii. 196 (1827): Triticum gracile.
hippolyti *Sennen, Diagn. Nouv. Pl. Espagne & Maroc 1928–35*, 49 (1936).—Hispan.
humidum *Ohwi & Sakamoto in Journ. Jap. Bot.* xxxix. 109 (1964);• *Ohwi, Fl. Jap.*, ed. rev., 124 (humidorum) (1965).—Japon.
ichyostachyum (*Seidl*) *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 58 (1901): [Triticum ichyostachyum].
japonicum (*Miq.*) *P. Candargy, l. c.* 22, in clavi, 42: Brachypodium japonicum.
karadaghense *Kotov in Journ. Bot. Acad. Sci. Ukraine*, v. No. 1, 32 (1948).—U.R.S.S. (Crimea).•
latronum (*Godr.*) *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 31, in clavi, 55 (1901): Triticum latronum.
lolioides (*Karel. & Kir.*) *P. Candargy, l. c.* 29, in clavi, 49: Triticum lolioides.
maroccanum *Sennen, Diagn. Nouv. Pl. Espagne & Maroc 1928–35*, 169 (1936).—Marocc.
microcalyx (*Regel*) *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 21, in clavi, 40 (1901): Triticum strigosum var. microcalyx.
nardus (*DC.*) *Chevall. Fl. Paris*, ii. 195 (1827): Triticum nardus.
nigricans (*Pers.*) *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 58 (1901): Triticum nigricans.
pendulinum (*Nevski*) *Vorosh. in Bull. Princ. Bot. Gard. Acad. Sci. URSS*, No. 49, 55 (1963): Roegneria pendulina.
pinnatum (*L.*) *Chevall. Fl. Paris*, ii. 195 (1827): Bromus pinnatus.
poa (*DC.*) *Chevall. l. c.* 193: Triticum poa.
psammophilum *J. M. Gillett & Senn in Canad. Journ. Bot.* xxxix. 1170 (1961).—Canada (Ontario); U.S.A. (Michigan; Wisconsin).
pseudo-agropyrum (*Griseb.*) *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 32, in clavi, 57 (1901): Triticum pseudo-agropyrum.
×pseudorepens *Scribn. & J. G. Smith*, pro spec.; *Bowden in Canad. Journ. Bot.* xliii. 1442 (1965).—Amer. bor.
pseudostrigosum *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes* Fasc. 1) 21, in clavi, 40 (1901): Triticum strigosum var. planifolium *Regel*.
pumilum (*Steud.*) *P. Candargy, l. c.* 29, in clavi, 49: Triticum pumilum.
rechingeri *Runemark apud K. H. Rechinger in Engl. Bot. Jahrb.* lxxx. 442 (1961).—Ins. Aegaeae.
richardsonii (*Schrad.*) *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 23, in clavi, 43 (1901): Triticum richardsonii.
×saxicola *Piper*, pro spec.; *F. D. Wilson in Brittonia*, xv. 322 (1963).—Amer. bor.
schrenkianum (*Fisch. & Mey. ex Schrenk*) *P. Candargy, Étude Monogr. Hordées* (*Archiv. Biol. Vég., Athènes*, Fasc. 1) 22, in clavi, 41 (1901): Triticum schrenkianum.
semicostatum (*Steud.*) *P. Candargy, l. c.* 21, in clavi, 41: Triticum semicostatum.
striatum (*Steud.*) *P. Candargy, l. c.* 22, in clavi, 41: Triticum striatum.

United States Department of Agriculture
Agricultural Research Service
Western Region

Arid Southwest Area
Crops Research Laboratory
Utah State University - UMC 63
Logan, UT 84322

December 13, 1983

Dr. Askell Löve
5780 Chandler Court
San Jose, CA 95123

Dear Askell:

I thought you might be interested in the enclosed letter from Arthur Cronquist.

We are not making very many converts in the U.S. taxonomic community.

Sincerely,

DOUGLAS R. DEWEY
Research Geneticist

Enclosure

The New York Botanical Garden

Bronx, New York 10458 (212) 220-8700

30 November 1983

Dr. Douglas R. Dewey
Crops Research Laboratory, UMC 63
Utah State University
Logan, Utah 84322

Dear Dr. Dewey:

Thank you for your reprint on generic delimitation in the Triticeae. You have certainly contributed a great deal to clarifying our concepts of relationships in this group. Clearly, we can no longer cling to the traditional definitions of Agropyron, Elymus, and Sitanion. The question is, where do we go from here?

I am reasonably happy with the thought of limiting Agropyron to the crested wheatgrasses. They form a morphologically and cytogenetically well defined group that anybody can recognize. I am not so happy with Elytrigia, Leymus Pascopyrum, etc. It sticks in my craw to have Agropyron smithii (to use the traditional name) put in a different genus from A. dasystachyum. I don't have the experimental data to back it up, but my recollection is that these two things may even be connected by a series of more or less apomictic polyploids of eventually hybrid origin. I am also not happy to have two of the segregate genera morphologically distinguished by the presence or absence of a long awn, especially since Agropyron (or whatever genus) spicatum includes both long-awned and awnless phases. The plant I learned as Agropyron subsecundum (with long awns) is certainly closely allied (conspecific?) with A. pauciflorum (or whatever the right name may now be), which lacks awns.

Therefore my thoughts turn in a different direction, toward the transfer of most of traditional Agropyron (including all our native species) as well as Sitanion to Elymus. Hystrix can just as well go along with them. The expanded genus Elymus would of course be somewhat diversified, but to a non-agrostologist such as myself the group seems to hang together, and not do any violence to phylogenetic concepts.

There is nothing magical about generic rank, in the grasses or any other group. A genus is nothing more than a group of species sufficiently similar inter se, and sufficiently different from other groups, so that we find it useful to think of these species collectively and have a group name for them. There is no inherent theoretical criterion to distinguish a genus from a subgenus or section. From a practical standpoint, it is well to have genera that can be recognized and conceptualized by botanists who are not specialists in the group, if this can be done without doing violence to phylogenetic relationships and presumably more fundamental similarities and differences. This is where I think the splitting route in this set of genera leads us into a swamp. When we have to recognize Pascopyrum as a distinct genus in order to have a coherent, internally consistent scheme, then I think it is time to reconsider the whole scheme.

Of course, the Lord hasn't whispered the answers into my ear. Ultimately it will be our colleagues and successors who decide what the botanical community will accept. Regardless of how things go, your work is fundamental to a resolution of the problems.

I was sorry to receive a note from Mary Barkworth that the conference on Triticeae was not funded and thus had to be canceled. Maybe another time.

Yours,

Art Cronquist

Arthur Cronquist
Senior Scientist

ac/lk

UNITED STATES DEPARTMENT OF AGRICULTURE
Arid Southwest Area
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20

NEW NAMES AND COMBINATIONS, PRINCIPALLY IN THE ROCKY MOUNTAIN FLORA--III

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

The second paper in this series was published in Phytologia 52:369-376. 1982.

AQUILEGIA MICRANTHA f. MANCOSANA (Eastwood) W. A. Weber, **comb. nov.** A. micrantha var. mancosana Eastw, Proc. Calif. Acad. (3) Bot. 1:77. 1897.

ASTRAGALUS KENTROPHYTA ssp. COLORADOENSIS (Jones) W. A. Weber, **comb. nov.** A. kentrophyta var. coloradoensis Jones, Contrib. W. Bot. 10:63. 1902.

ASTRAGALUS KENTROPHYTA ssp. DANAUS (Barneby) W. A. Weber, **comb. nov.** A. tegetarius var. danaus Barneby, Lfl. W. Bot. 5:95. 1951.

ASTRAGALUS KENTROPHYTA ssp. DOUGLASII (Barneby) W. A. Weber, **comb. nov.** A. kentrophyta var. douglasii Barneby, Mem. N. Y. Bot. Gard. 13:364. 1964.

ASTRAGALUS KENTROPHYTA ssp. ELATUS (S. Wats.) W. A. Weber, **comb. nov.** A. kentrophyta var. elatus S. Wats., Bot. King's Exped. 77. 1871.

ASTRAGALUS KENTROPHYTA ssp. IMPLEXUS (Canby ex Porter & Coulter) W. A. Weber, **comb. nov.** A. tegetarius var. implexus Canby ex Porter & Coulter, Syn. Fl. Colo., Add. 1874.

ASTRAGALUS KENTROPHYTA ssp. JESSIAE (Peck) W. A. Weber, **comb. nov.** A. jessiae Peck, Lfl. W. Bot. 4:180. 1945.

ASTRAGALUS KENTROPHYTA ssp. NEOMEXICANUS (Barneby) W. A. Weber, **comb. nov.** A. tegetarius var. neomexicanus Barneby, Lfl. W. Bot. 6:99. 1951.

ASTRAGALUS KENTROPHYTA ssp. UNGULATUS (Jones) W. A. Weber, **comb. nov.** A. kentrophyta var. ungulatus Jones, Proc. Calif. Acad. Sci. II, 5: 650. 1895.

BRICKELLIA ROSMARINIFOLIA (Vent.) W. A. Weber, **comb. nov.** Kuhnia rosmarinifolia Vent., Descr.... Cels. t. 91. 1803.

BRICKELLIA ROSMARINIFOLIA ssp. CHLOROLEPIS (Woot. & Standl.) W. A. Weber, **comb. nov.** Kuhnia chlorolepis Woot. & Standl., Contrib. U. S. Nat. Herb. 16:177. 1913.

BRICKELLIA MICROPHYLLA ssp. SCABRA (A. Gray) W. A. Weber, **comb. nov.** Brickellia microphylla var. scabra A. Gray, Proc. Amer. Acad. 11:74 (1875-6).

CERASUS PUMILA (L.) Michx. ssp. BESSEYI (L. H. Bailey) W. A. Weber, **comb. nov.** Prunus besseyi L. H. Bailey, Bull. Cornell Univ. Exp. Sta. 70:261. t.l. 1894.

GERANIUM CAESPITOSUM ssp. ATROPURPUREUM (Heller) W. A. Weber, **comb. nov.** Geranium atropurpureum Heller, Bull. Torr. Bot. Club 23:1965. 1898.

HIRCULUS PLATYSEPALUS (Trautv.) W. A. Weber, **comb. nov.** Saxifraga flagellaris var. platysepala Trautv., Fl. Taimyr, p. 43. 1856.

HIRCULUS PLATYSEPALUS ssp. CRANDALLII (Gand.) W. A. Weber, **comb. nov.** Saxifraga crandallii Gand., Bull. Soc. Bot. France 65:30. 1918.

HIRCULUS PROREPENS (Fisch. ex Sternb.) W. A. Weber, **comb. nov.** Saxifraga prorepens Fisch. ex Sternb., Rev. Saxif. 59. 1810.

HIRCULUS SERPYLLIFOLIUS (Pursh) W. A. Weber, **comb. nov.** Saxifraga serpyllifolia Pursh, Fl. Amer. Sept. 1:311. 1814.

HIRCULUS SERPYLLIFOLIUS ssp. CHRYSANTHUS (A. Gray) W. A. Weber, **comb. nov.** Saxifraga chrysantha A. Gray, Proc. Amer. Acad. 12:83. 12887.

HUMULUS LUPULUS ssp. NEOMEXICANUS (A. Nels. & Ckll.) W. A. Weber, **comb. nov.** Humulus lupulus L. var. neomexicanus A. Nels. & Ckll., Proc. Biol. Soc. Wash. 16:45. 1903.

PEDICULARIS BRACTEOSA ssp. PAYSONIANA (Pennell) W. A. Weber, **comb. nov.** Pedicularis paysoniana Pennell, Bull. Torr. Bot. Club 6:46. 1934.

PEDIOMELUM AROMATICUM (Payson) W. A. Weber, **comb. nov.** Psoralea aromatica Payson, Bot. Gaz. 60:379. 1915. The genus Psoralea is typified by a South African shrub with linear, acicular leaves. Rydberg (1919), in my opinion, was quite correct in segregating out the North American taxa into a number of genera which, geographically, ecologically and morphologically stand very clearly as discrete groups.

The American species of Crepis were treated in a now classic monograph by Babcock & Stebbins (Carnegie Inst. Wash. Publ. 504. 1938). The authors seem to have been preoccupied with the species alone, and unfortunately they did not discuss the significance of their cytological findings as having a bearing on the generic level, even though Nuttall (1841) had proposed the name Psilochenia for the American species.

All of the native American species of Crepis, with the exception of two Old World species (C. elegans and C. nana), representing an ancient Tertiary extension of the genus onto western North America, have the chromosome base number x=11. "This is in striking contrast to the Old World species of Crepis, whose basic haploid numbers range from x=3 to x=7, 4 and 5 being much the most common" (Babcock & Stebbins, op. cit.). The authors went on to postulate that the American species may have arisen by amphidiploidy from a cross involving Crepis species with x=4 and x=7. Whether or not this can ever be substantiated, the fact remains that the American species of Crepis form an indisputably monophyletic line, spatially and genetically isolated from the Old World species.

Recently Love (1982, p. 360) transferred Crepis runcinata to Nuttall's genus Psilochenia because of this evidence, but among the rest of the species, only the type, Psilochenia occidentalis Nuttall, has a name in that genus. The following combinations are needed.

PSILOCHENIA ACUMINATA (Nutt.) W. A. Weber, **comb. nov.** Crepis acuminata Nutt., Trans. Am. Phil. Soc., n.s. 7:437. 1841.

PSILOCHENIA ACUMINATA ssp. **PLURIFLORA** (Babc. & Stebb.) W. A. Weber, **comb. nov.** Crepis acuminata ssp. pluriflora Babc. & Stebb., Carn. Inst. Wash. Publ. 504:178. 1938.

PSILOCHENIA ATRIBARBA (Heller) W. A. Weber, **comb. nov.** Crepis atribarba Heller, Bull. Torr. Bot. Club 26:314. 1899.

PSILOCHAENIA ATRIBARBA ssp. CYTOTAXONOMICORUM (Boivin) W. A. Weber, **comb. nov.** Crepis atribarba var. cytotaxonomicorum Boivin, Nat. Canad. 87:31. 1960.

? check this citation, esp polyploid, then replace with current one.

PSILOCHENIA BAKERI (Greene) W. A. Weber, **comb. nov.** Crepis bakeri Greene, Erythea 3:73. 1895.

PSILOCHENIA BAKERI ssp. CUSICKII (Eastw.) W. A. Weber, **comb. nov.** Crepis cusickii Eastw., Bull. Torr. Bot. Club 30:503. 1903.

PSILOCHENIA BAKERI ssp. IDAHOENSIS (Babc. & Stebb.) W. A. Weber, **comb. nov.** Crepis bakeri ssp. idahoensis Babc. & Stebb., Carneg. Inst. Wash. Bull 504:141. 1938.

PSILOCHENIA INTERMEDIA (A. Gray) W. A. Weber, **comb. nov.** Crepis intermedia A. Gray, Syn. Fl. 1(2):432. 1884.

PSILOCHENIA MODOCENSIS (Greene) W. A. Weber, **comb. nov.** Crepis modocensis Greene, Erythea 3:48. 1895.

PSILOCHENIA MODOCENSIS ssp. GLAREOSA (Piper) W. A. Weber, **comb. nov.** Crepis glareosa Piper, Bull. Torr. Bot. Club 28:42. 1901.

PSILOCHENIA MODOCENSIS ssp. ROSTRATA (Coville) W. A. Weber, **comb. nov.** Crepis rostrata Coville, Contr. U. S. Nat. Herb. 3:564. 1896.

PSILOCHENIA MODOCENSIS ssp. SUBACAULIS (Kellogg) W. A. Weber, **comb. nov.** Crepis occidentalis var. subacaulis Kellogg, Proc. Calif. Acad. 5:50. 1873.

PSILOCHENIA MONTICOLA (Coville) W. A. Weber, **comb. nov.** Crepis monticola Coville, Contr. U. S. Nat. Herb. 3:562. 1896.

PSILOCHENIA OCCIDENTALIS ssp. CONJUNCTA (Jeps.) W. A. Weber, **comb. nov.** Crepis occidentalis ssp. conjuncta Jeps. ex Babc. & Stebb., Carneg. Inst. Wash. Bull. 504:134. 1938.

PSILOCHENIA OCCIDENTALIS ssp. COSTATA (A. Gray) W. A. Weber, **comb. nov.** Crepis occidentalis var. costata A. Gray, Bot. Calif. 1:435. 1876.

PSILOCHENIA OCCIDENTALIS ssp. PUMILA (Rydb.) W. A. Weber, **comb. nov.** Crepis pumila Rydb., Mem. N. Y. Bot. Gard. 1:462. 1900.

PSILOCHENIA PLEUROCARPA (A. Gray) W. A. Weber, **comb. nov.** Crepis pleurocarpa A. Gray, Proc. Amer. Acad. 17:221. 1882.

PSILOCHENIA RUNCINATA ssp. ANDERSONII (A. Gray) W. A. Weber, **comb. nov.** Crepis runcinata ssp. andersonii A. Gray, Proc. Am. Acad. 6:553. 1865.

PSILOCHENIA RUNCINATA ssp. BARBERI (Greenm.) W. A. Weber, **comb. nov.** Crepis barberi Greenm., Proc. Am. Acad. 40:52. 1904.

PSILOCHENIA RUNCINATA ssp. GLAUCA (Nutt.) W. A. Weber, **comb. nov.** Crepidium glaucum Nutt., Trans. Am. Phil. Soc., n.s. 7:436. 1841.

PSILOCHENIA RUNCINATA ssp. HALLII (Babc. & Stebb.) W. A. Weber, **comb. nov.** Crepis runcinata ssp. hallii Babc. & Stebb., Carn. Inst. Wash. Bull. 504:104. 1938.

PSILOCHENIA RUNCINATA ssp. HISPIDULOSA (Howell) W. A. Weber, **comb. nov.** Crepis runcinata var. hispidulosa Howell, Mem. N. Y. Bot. Gard. 1:461. 1900.

PSILOCHENIA RUNCINATA ssp. IMBRICATA (Babc. & Stebb.) W. A. Weber, **comb. nov.** Crepis runcinata ssp. imbricata Babc. & Stebb., Carn. Inst. Wash. Bull. 504:102. 1938.

SENECIO FREMONTII T. & G. ssp. BLITOIDES (Greene) W. A. Weber, **comb. nov.** Senecio blitoides Greene, Pittonia 4:123. 1900.

TEUCRIUM CANADENSE L. ssp. OCCIDENTALE (A. Gray) W. A. Weber. comb. nov. Teucrium occidentale A. Gray, Syn. Fl. N. Am. 2:349. 1878.

WYETHIA X MAGNA A. Nels., **hybr. nov.** Putative hybrid, Wyethia amplexicaulis (Nutt.) Nutt. X Wyethia arizonica A. Gray.

TYPUS: COLORADO, U.S.A. Routt Co.: Elk River, high mountain slopes, L. N. Goodding 1664 (RM 52083).

Wyethia amplexicaulis ranges widely through northwestern United States, entering Colorado as a pure population in the northwesternmost counties. Wyethia arizonica occupies the southwestern United States, reaching Colorado in the Four Corners area. Occupying large areas of the western Colorado plateaus is a population of plants which, because of their large stature and similar gross morphology would be called W. amplexicaulis except that the plants are not glabrous but are densely pubescent. Aven Nelson applied the manuscript name, W. magna to such plants. In the northern counties, W. X magna and W. amplexicaulis both occur with intermediates having variable pubescence. In the southwest corner of Colorado, typical W. arizonica occurs along with a plant somewhat larger but more glabrate, in an obvious hybrid swarm. It is noteworthy that plants leaning toward the morphology of W. amplexicaulis are frost hardy compared to W. arizonica (Weber 1952).

Over the major part of western Colorado, however, a population is widespread which seems to be a stable hybrid having the habit and detailed morphology of W. amplexicaulis, differing only in the copious pubescence on all parts. Since these plants continue to be the subject of inquiry by collectors, it seems appropriate to provide a name for them.

LITERATURE CITED

Babcock, E. B., & G. L. Stebbins, Jr. 1938. The American species of Crepis. Carnegie Inst. of Washington Publ. 504. 199 pages.

Love, Askell. 1982. IOPB chromosome number reports LXXV. Taxon 31:342-368.

Rydberg, Per Axel. 1919. (Rosales) Fabaceae: Psoraleae, in North American Flora 24(I):1-25.

Weber, William A. 1952. The glabrate form of Wyethia arizonica. Lfl. W. Bot. 6:223-225.

UNIVERSITY OF COLORADO, BOULDER

Museum

25 February 1983

Dear Askell:

I am about to try to put Linaceae together, and I need to ask you a few things about "Linum".

1. Where does Linum usitatissimum L., the type species, according to some, differ generically from Adenolinum (type not designated, according to Index Genericorum: can it not be typified?). I do not have Reichenbach Handb.

2. You take up Mesynium Raf. (type not designated) for what Reichenbach called Cathartolinum (type not designated). According to Tax. Lit., Reichenbach's Handb. was published Oct. 1-7, 1837, and acc to Index Genericorum, Rafinesque's Fl. Tell. was published Nov.-Dec. 1837. If this is correct, then Cathartolinum would have priority, nicht wahr? Also, wouldn't it be wise to designated types if you pick up these genera?

I am waiting to hear something (anything) from Stanford. They told John Schwartz that it would take about 3-4 weeks before they could touch all the necessary bases. However, so far nobody has grumbled about my format or style.

We got a nice bunch of plants from Erevan last week by way of Jane Bock. They were all composites sent to her as a gift; I suspect that there may be more boxes on the way since only one family was represented in this one.

Possibility of another trip to Altai-Sayan this summer; Krasnoborov hints that they are going to invite me and big Arthur. What fun we two would have together--the modern splitter and the ultimate lumper.

A very good biography has been written about Aven Nelson by a non-botanist historian (Roger Williams); I have read it. It has good stuff on the Rydberg-Greene-Coulter-Robinson correspondence as well as the messy politics of the State and University of Wyoming. It was like reading my autobiography in many places. I am trying to raise some money for a subsidy to have it printed in our press rather than going to Europe or somewhere unlikely.

I finished the legumes this week, and think that my Astragalus key will work. You simply can't identify species in Barneby. He certainly knows them but did not try to make it easy for anyone else. Incidentally, I like the way Psoralidium and Pediomelum work out. Neither of the two species alleged to serve as lectotypes for the genus have anything to do with ours, and Rydberg's genus Hoita is a very nice group of Pacific Coast-southern South American species.

As ever,

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1) The simplest key to differentiate *Linum* L. s. str. ($x = 15$) from *Adenolinum* $x = 9$ (type *perenne*) looks so:

a. Stigmas capitate; heterostylous . . . *Adenolinum*

b. Stigmas linear; homostylous - - - *Linum* s. str. (*usitatissimum*)

Cathartolinum Reichenbach 1837 is a European, W. Siberian genus based on the type species *C. catharticum* (L.) Rchb., which has the basic number $x = 8$; ~~it has~~ despite Greene and other Americans, it has nothing to do with the American *Mesynium* Rafin. 1837, with the basic number $x = 15$, which ought to be typified by either *M. mexicanum* (H.B.K.) Rafin. or, less appropriately, *M. texanum* ~~Rchb~~ Rafin., which were validated when the genus was described. The Californian *L. digynum* A. Gray, which Small also put in *Cathartolinum*, is something else and still nameless as to genus, ~~x=8~~ though $x = 8$, according to Raven. Yes, types should be designated when you pick up such genera, provided that they are properly circumscribed and certainly not heterogeneous; that ~~[illegible]~~ however requires additional space if the transfer is made in my Taxon list, so this ought to be your privilege and add to your list of achievements.

I have a couple of remarks on your 3rd list, the first perhaps serious, because I want to claim that by splitting hopefully properly delimited and widely distributed species of *Astragalus* is hardly to be recommended, because it violates our knowledge of that such variations are only more or less ephemeral pure lines of more or less obligate autogams, which have no geographic or even evolutionary status. To call them varieties is 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., ~~[illegible]~~ northern Scandinavia, or Iceland, or Greenland, or Colorado, or simply the ~~[illegible]~~ southern Rockies, ~~[illegible]~~ they certainly are no subspecies which are best defined as major geographic races of considerable stability and age and with a sizeable regional distribution, ~~[illegible]~~ covering, e.g., eastern or western N. Am., ~~[illegible]~~ or ~~[illegible]~~ Alaska [illegible] Labrador, the prairies, the Rockies from north to south, etc. I do not give such ~~[illegible]~~ pure lines any name, neither in Iceland or elsewhere, because I feel no need for names the more that they are autogamous and may include more or less ephemeral populations of more or less numerous genetically pure lines that may or may not differ slightly in morphology. If you really feel a need for their geographically meaningless identification, why not leave them, still unnecessary, varieties of less ~~stable~~ biologically stabled status untouched?

~~Again, continuing with the list, I wonder if it is not unwise to recognize as distinct subspecies all the varieties from the Rockies of other species. Since they are, admittedly seen, more likely to be minor geographical races, or varieties, only of a Rocky Mt subspecies of each species?~~

The only other remark I can make concerns Humulus lupulus and its ssp. neomexicanus (I would write out Cockerell, for the benefit of non-Americans): Although Mansfeld, R. (1959: Vorläufiges Verzeichnis landwirtschaftlich oder gärtnerisch kultivierter Pflanzenarten. – Die Kulturpflanze, Beiheft 2: 1–659) maintains H. lupulus as a species without subdivision, with H. americanus Nutt. and H. neomexicanus (Nelson & Cockerell) Rydb. as American synonyms, I believe there are indications that the American populations (this is a plant of the N. Am.–E. Asiatic group) differ from the Asiatic ones from which the cultivated hops derive, though perhaps not in earthshaking characters — and they are of course intergradable as all disjunctive races of the Tertiary relic group. But if the western populations differ from the eastern ones, which I doubt, they are hardly more than a variety of the American species. Since we made the transfer of ssp. americanus (Nutt.) L. & L. in Taxon 31(1982):121, I believe you are creating a synonym only, at this level. If you are sure that the western plant differs from the eastern, which we have studied in the east and in Manitoba, then you could place it as a variety neomexicanus of ssp. americanus, which does not need a formal transfer and additional authors, though you may feel differently in that. I would refer to the name as a synonym only — though you may prefer to disagree on that point, hopefully not on the subspecific status.

ssp. americanus (Nutt.) L. & L. Taxon 31(1982):121

I like your discussion of the American Crepis which is Psilochenia of course, except C. elegans and C. nana Richards., as you also say. But also these two taxa (which have x = 7) are not real Crepis, which is typified by C. biennis with x = 5. ~~And I may be believing that the Arctic–North there are taxa of the Arctic–North American genus~~ 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. – Univ. Cal. Publ. Bot. Vol. 22: 212–213) describes this small Arctic-American 7-chromosome group as the section Ixeridopsis Babcock of the genus Crepis. I believe this is a mistake caused by his peculiar generic concept and that this group would be better distinguished as a genus in its own right. Since the name he selected is hardly

an easy or beautiful name, and ~~may even follow to induce others to think of Ixeris~~, ~~which~~ and since I believe it is misleading to name it after the genus Ixeris, with which it has no relationship, it would be wiser to give it an entirely new name but refer for ~~the~~ its validation to the description by Babcock (1947, p. 213), and ~~then transfer to it~~ typify it, as Babcock did with his section, ~~with~~ by [illegible] the species nana, and transfer to it the following taxa:

Crepis cornuculata Regel & Schmalh. ex Regel, 1881, Pl. Nov. Fedtsch., in Fedtsch., Reise Turkest. 18:54;

Crepis elongata H. Krasch., 1933, Acta Inst. Bot. Acad. Sci. URSS, ser. I, 1:182;

Crepis flexuosa (D.C.) Benth. & Hook. f. 1873, Gen. Pl. 2:515;
which was based on Barkhausia flexuosa DC., 1838, Prodr. 7:156;

Crepis nanaeformis Babcock 1947, The genus Crepis, Part 2:536;

Crepis lactea Lipsch. 1937, Fedde's Repert. 42:159;

Crepis nana Richards, 1823, Bot. App. of Franklin, 1st Journ., ed. 1:746;
ssp. nana
ssp. ramosa Babcock, 1947, The genus Crepis Part 2:542;

Crepis elegans Hooker, 1834, Fl. Bor. Amer. 1:297.

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

Otherwise no changes. I am trying to write a paper on the taxonomy of Acetosella to straighten out misunderstandings by especially a nasty little Dutch amateurist, who has been attacking my ~~[illegible]~~ 1943 ~~[illegible]~~ preliminary naming of the four taxa, for wrong reasons. It is ~~[illegible]~~ remarkable that even in Holland botanists do not realize that, although descriptive ~~[illegible]~~ names are nice to have, they still are valid if correctly typified, even though may later be found to ~~[illegible]~~ have been coined by a mistaken anthropism, as when I defined Mueller's ~~R. angiocarpus~~ diploid R. angiocarpus (the type is diploid) by aid of its angiocarpy, which later has been found also in the hexaploid west European race v. pyrenaicus, that is, the weed of ~~[illegible]~~ introduced ~~[illegible]~~ on other continents. But when one lets the bible decide upon ones thinking and writing, perhaps more important mistakes are overlooked, as, e.g., the frequent misidentification, ~~[illegible]~~ even by Hultén and the Russians, of the eastern Asiatic diploids of the otherwise western American Parvifoliae section as R. graminifolius, which is octoploid. If botanists only would try to ~~think~~ [illegible] evolutionarily and genetically and not on basis of various kinds of creationism, which even ~~the~~ some so-called great colleagues of ours educated in Utah and elsewhere seem to follow slavishly, though avoid admitting!

Lol. [illegible]! Completing the [illegible] [illegible] nobody dares to protest [illegible]...

UNIVERSITY OF COLORADO, BOULDER
Museum

15 December 1984

Askell and Doris Love
5780 Chandler Court
San Jose CA 95133

Dear A. & D.:

I do not know a citation for Dandy on Veronica, but probably Regnum Vegetabile is in the library, since we got an institutional membership until recently when they dropped it and the museum had to pick it up. Give me a guess on the date. Index to collectors has this about your things:

Love, Askell (1916-x)
Canada: C. & E. United States; Rocky Mts. (col. 1951-x): CAN, DAO, MIN, MT (orig.) MTJB, NA, NDA, NY, PAC, S, SASK, WIN (orig.).
Iceland (col. 1931-37 & 1946-50): C (370), GB, GH, H, ICEL (orig.), K, LD (298), LISE, MT, MTJB, O, S, UPS.
Sweden (col. 1938-45): LD (orig.), MT, S, UPS, UPSV.
----------, & Doris (1918-x)
Herbarium (cytotax. material of Canada): WIN.
Canada (Manitoba), Sweden: WIN.
Iceland: GB (265 Ph, col. 1947-49), WIN.
Love, Doris (1918-x) (cf. Love, A.).
(same as for Askell above)
Yugoslavia (1971): LJU.
[I don't know why the Yugoslavia is only listed under Doris' entry].

Perhaps after Christmas I'll be able to mull over the Notoveronica etc. There is still a real problem in Pocilla and Veronica. If Pocilla is the x=7 group, then, if V. hederifolia is the type of an Opiz genus, it also has the 7 number and would make the genus Cochlidiospermum Opiz older than Pocilla!. But C. cymbalaria, according to the Chromosome Atlas, is a 9 base number species. This is what I originally asked you about. What now???

The architects plans are finished and the remodelling next has to go out on bid. This probably won't happen before the end of the Christmas break. Actually I don't think the work should take a month; I still have hopes of moving in in April or May. No, there is not much chance of a hitch, since the Regents appropriated the money.

Christmas came early to the herbarium. You will remember that Bill Penland at Colorado College was a Harvard man and that in 1939 he went to Ecuador with my cousin Martin Brown and collected over the tracks of Humboldt in Ecuador. Those specimens have lain unused, but Jack Carter, when he took over from Penland, pulled all of the exotic specimens out about 12 years ago and boxed them up, eventually to come to us here. But Penland first had to pass away, then his son had to get through college there, and Penland's second wife had to be approached. Evidently Missouri Botanical Garden wanted them (along with the subject of the next paragraph) but Mrs. Penland wanted them to stay in Colorado, Bless her! Last week Jack told me

that he was bringing all of it up here, since no one at the College seemed to care about the collection. All of the specimens are mounted, and almost everything was named by specialists (difficult things at least to genus), and there are a bunch of types. Penland made the most beautiful collections and there are very detailed notes on field appearance. It is about 1200 sheets.

But what I was totally unprepared for (Jack said he thought I must have known that Colorado College had this) was a grand set of Pringle Mexico, probably well over 2,000 specimens, containing as you would expect hundreds of types! Just looking at it and checking against the Pringle book, it looks as if this is the best set outside of Gray and Smithsonian. I can see that we already have things that Smithsonian lacks, that Gray lacks, and that herb. Pringle lacks (at least according to the list in the book). At any rate, this is going to help out immensely in the Mexico work, as you well understand!

There are also some things from Colombia etc., evidently traded from Smithsonian to Penland, and a batch of Penland's Colorado things, which we also did not have, mostly El Paso County.

The matter of my replacement is something I will take up as soon as the move is accomplished. It is probably too early to ask Morefield to think about it; what has he done, collected, published? I don't know the name at all. Whether we can get two to replace one is the most critical thing, and how to do this. Contrary to what you say, Laramie is in extremely good hands with Ron Hartman and we get along. There would be no chance to move them to Boulder, but possibly the other way round if it came down to it; they have lots of support for herbarium and library, and a good man who has an open mind; I would say that our herbaria are complementary in the best sense.

Can you send the citation for Mattfeld, Minuartia; I am not sure I know where to go for it, probably in your Lidia paper of course, but everything is in the other building and I seldom get there nowadays. About Lidia, I can't honestly distinguish L. biflora from L. obtusiloba. At least here in Colorado.

God Jul och ett gott Nytt Ar!

Bill

Manfred
Fischer, M. 1967: Beiträge zur Cytotaxonomie der Veronica hederifolia-Gruppe (Scrophulariaceae). — Österr. Bot. Zeitschr. 114: 189–233 (v. m. 25238). Cochlidiosperma Opiz.

V. triloba Opiz 2n=18

V. hederifolia L. s. str. 2n=54 [illegible]

V. sublobata M. Fischer sp. n. [p. 227]. 2n=36 [illegible]?

V. sibthorpioides Deb. & Deg. 2n=?

V. hederoides M. Fischer 2n=18 [syn. nov. p. 228]

~~[illegible]~~ 1969, 1976 (Fischer & [illegible]),

V. cymbalaria Bod. 2n=54 (&36?)

V. lycica Lehm. 2n=18

V. panormitana Tin., V. trichadena Jord. & Fourr. 2n=18

V. stamatiadae M. Fischer & V. Greuter 2n=18 (Pl. Syst. Evol. 125: 249)

[illegible] 8:
[illegible] 8:

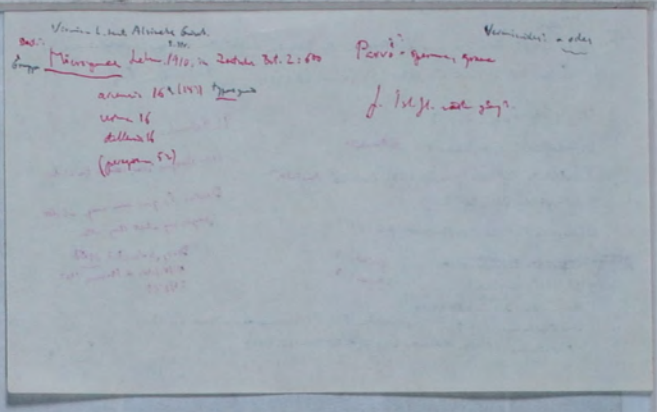
The [illegible]:

[illegible]

[illegible]

[illegible] 27, 1968
[illegible] 1967
23/2 '69

[illegible]



UNIVERSITY OF COLORADO, BOULDER

Museum

13 Nov. 1984

Dear Askell & Doris:

Here is the statement in ING:

Veronica Linnaeus, Sp. Pl. 9. 1 May 1753.
LT.: V. officinalis Linnaeus (vide) N. L. Britton & A. Brown, Ill. Fl. N. U.S. ed. 2. 3:199. 7 Jun 1913.

Veronicastrum Heister ex Fabricius, Enum. 111. 1759.
T.: V. officinalis Linnaeus
=Veronica Linnaeus 1753 lectotypification)

Would you like to make the genus Notoveronica yourself; you know so much more about all this than I do. Also, I hesitate to do the genus for Arenaria nuttallii for the same reason. If you write it I'll be glad to put it on the word processor in good form for Phytologia, and take care of Moldenke's page charge. There is also the problem of a genus name for the American species of Linum kingii etc. I understand that there is a new number of N. Am. Flora by C. M. Rogers on Linaceae, but it won't do anything generically, I'm sure.

Also, you didn't say anything about Veronica cymbalaria. Morphologically it would seem to belong to Pocilla but it has the numbers for Veronica. Of course, such things happen.

The Herbarium definitely is going to move; I have talked with the architect and the plans for the renovation of the space should be ready by the end of the month. Then bids, and if it goes smoothly he says they would be able to start construction in January. I see us moving in April or May. It really is good space, and i can fill it very well but still be able to fill it up more when necessary. But next on the agenda is to try to arrive at a plan for my replacement. will it be done? I wonder.

Landolt has sent around a call for papers based on the Japanese excursion. I can see everybody putting words together based on the chatting in the bus, but certainly there was no work of any kind accomplished on the trip.

I have been able to obtain a copy of the holographic list of plants that Hooker and Gray collected in Colorado, Utah, Nevada and California on the 1877 trip, so at least I can see what impressed Hooker so much.

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

San José, November 21, 1984.

Dear Bill:

Thanks for the November 13 letter with the information on theING types for Veronica and Veronicastrum, I am sorry that I cannot easily find me 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 answere 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 xxxxxxxxxx generic name Cochlidiospermum Opiz, inBercht.& Opiz 1939, Ökon.-Techn. Fl. Böhem. 2,2:145, where also the combinations are made for three species, C. cymbalaria (Bertol.) Opiz, C. dentifolia (L.) Opiz (type), and C. lappago (Schrank) Opiz. See Pouzar, 1964, in Preslia 36:338.

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 change 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 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 dinnerspeaker?

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 diminis.

All the best from us all here,

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Askell and Doris Love
5780 Chandler Court
San Jose CA 95133

21/11/84

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 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 ~~XXX~~ 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: Aakellia: 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, ~~XXXXXXX~~ 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 Scriphidia, 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 haxx of Seriphidium have been treatedxbx 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" Steppea to XXXXX 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. -XXXXX 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, so let us wait with the "political" part and the [illegible] But [illegible]
All the best, L.

Yes, you are evidently somewhat out of touch when you are unaware of that Runner and his group did everything to make my position difficult after I had been elected against his will, and especially after I had refused to join their select group for underhanded discussions that ought to have supported them and weakened us. But it would take too long tonight to write about this, and since I feel you ought to get as soon as possible the scientific notes...even though they may look confused... I wonder if you would not rather wait with that matter? Though I should remind you that when I was refused a faculty fellowship 1969-70, then Prescott had been appointed to chair the committee, by Crowe of course. When I complained to Briggs, my dean, he proposed that he take up the matter with the proper authorities, and then reported to me that both Crowe and Prescott had refused to reconsider...and Briggs regarded that as acceptable, though I was astonished to experience that it is regarded democratic and correct in America to refer judgement on oneself as a judge back to the same person whom we complain against...that is not democracy in the oldest democracy in the world, Iceland. There were more minor things the coming years, but they evidently waited for some better opportunity that at last came from the Smithsonian...though then it was important to force me to resign with threats of deportation etc. and actions from the Justice Department, which I am sure were simple lies.

I think you are correct — The Museum of Natural History. And the Colorado Citizenship Society is the digital

O. [illegible] of L. & L. 1975 (OA, NAI), & L. & H. 1971; although Hall's was confused — they think it actually also has evidence on the scientific name of the Alaska flower. A. [illegible] it — Fairchild explains to which book Fairchild [illegible] for us I inherited it; it will probably be discussed in the last volume of the Mt. Rainier [illegible]

Thanks back to the letter.

I understand you wish to let A. pothecaria wait — little, but hope you solve that problem, too.

Thanks for the flattery — hope it helps in one way or another, if anybody records Phyllodoce.

I am surprised that you had not observed the dominance of the Chem group of the graduate school & its deans, even before Arthur & Crocker, & the fact that the latter's recommendation of Little Chem & Prescott, (and also Briggs!) rather early transformed the fate of the Biology Department over to my person, when I did my best to help my colleagues get facilities & to keep the Department floating — despite all the undermining work by several of the faculty members. You introduced me to Chem, & he & the singular & honest Joe Daniels befriended me the first winter & tried to get me into their group, though they evidently did not trust me enough to fill a vacancy on their "reorganization" committee, ... I may not have sounded enough understanding of the greater importance of chemistry & behavior/exploration; then of genetics, which is the strongest things rarely expressed reason for the religious belief in so-called molecular & behavioral "genetics" that this incompetent political committee was created to express as the basis for the need of molecular biology & behavioral genetics on the campus. During that first winter, they both informed me a good deal about the low quality of the faculty, which of course was in conformity with the truth as to the majority of the members, & they also mentioned several times that Daniels is a drunkard & a former CIA agent in Afghanistan, what he did not conceal himself. But I could not agree about their [illegible] of Corey — [illegible] for gold-fish work, or of Daniels as a biologist, since he was [illegible] Daniels was the only Dahlia biologist I knew of by sight & ever thought of going to the city. This Shaker did not impress me, nor did my evident lack of interest impress Chem, I believe. But I was negatively impressed when he once told me, that he had not only monopolized the Biology office but also gotten the administration to [illegible] him, I believe he said 30.000 square feet of research space, since to me his research was nil, or only fiddling with impressive equipment. When I asked him why he asked for that much space, he smirked & shook his head in the typical Chem fashion & said that he felt it was an impressive figure. When I wondered if he had asked for space for the other faculty, his answer was simply that they needed no space, though he added, that Hasler, Daniels, Alexander, Chem & Dahlberg had gotten some. Why Dahlberg, who never was a scientist, I do not know, but he got got some space that was available in 1964, when he & I came, but despite my evident needs & qualifications, space was not even mentioned. But that was Chem, not Chem, & probably his aversion against all botany [illegible] was the reason. Chem did not impress me as a man of ethics [illegible] my first year in Dahlia, & that first impression never weakened. And I hope I did not impress him either as zealously worthy Seventh-day, because such a thought would taint my ethics. Perhaps, I should have been impressed by him & the group around him, despite the fact that I find them morally decrepit, in order to protect myself & my family from such an unscrupulous collection of crooks?

I am sorry that the contract delay as to Jackson square, but I could not be astonished if the molecular-behavioral genetics people have been advising an inactivity until you [illegible] is out, to the Johnson can be solved?! Then there will be no [illegible] studies opp. & if you publishing successes [illegible] & with the [illegible] Behavioral field out Colorado can easily be put to molecular sleep & the funds used for a distinguished purpose?. But keep them awake as long as you are standing upright.

When I was elected chairman in the spring of 1966, I first [illegible] Dave to support Joe Daniels, but instead he proposed somebody from Utah that none of us had heard of before and voted for him — and apparently he and Dave were the only ones who left without shaking my hand.

I believe it was in the fall of 1966, rather than the spring of 1967, that dean Archer called a meeting in a large basement room of the UMC to discuss or inform on various grant possibilities and application methods. I remember little details, except that I did not get the impression that this was of importance to the Biology faculty in general or to myself in particular. When the meeting was over, however, Meredith Ommer, accompanied by Jack Kelso of Anthropology, cornered me near the exit. Meredith wanted to tell me that now he had gotten the impression that I were ripe for joining their "little" luncheon group in discussions of a modern reorganization of biology — the campus in general and of the Biology Department in particular. I reacted by saying that in such discussions I could not take part alone but would do it accompanied by my associate chairman Charles Norris. Meredith's eyes turned black and he almost shouted that that could not be done, and I said that then I would not take part in any complots behind my colleagues. He spit out something like that "this you will regret in the years to come, you are evidently not the man we thought", and I said: "fortunately not". No more. But he hardly spoke to me ever after except at the few dinners Helen or Doris arranged, since they liked each other.

Some years later, (on May 1, 1969, which I believe was in the middle of the week, I was at the little post office in South Boulder (80302?), where I was helped by a pleasant clerk whom I knew was from North Dakota and often chatted with. We exchanged some words about the weather and having to be inside, so I remarked that if we had been in Europe, then he could have enjoyed the laborers' holiday. I had not observed that next to me in the row was Homer Dall, who smilingly said to me: No communistic propaganda! I turned to him and said: you know that it is unfair to say so to an immigrant from the oldest democracy in the world", and he added something like: I know, but some of your colleagues say so about you, I have heard Drs. Ommer and Prescott discuss your political opinions with [illegible] in the Faculty Club. I believe I convinced him that this was a mean thing [illegible]. I never had difficulties with Homer, who always was helpful, [illegible] he would confirm this and not claim to have forgotten it. Although at that time it meant nothing to me, it now does in view of what happened later — in this country where thoughtless words of that kind easily grow into giant slander, though such a slander I have not heard later myself.

Prescott was the "peer" chairing the dean-appointed committee that in the winter 1969–70 refused me a faculty fellowship, and refused to reconsider, when Briggs asked Crowe to ask for that. And Crowe told Jack Kelso, that the refusal had been, because he does nothing but play with damned chromosomes". [illegible] These were the "biology" judges for Crowe, who knows even less in biology than in his socalled religious philosophy, and I was regarded as less qualified for this honor aimed at scientists — the faculty then Crumpacker, Rogers, Winston, Marr, and you can continue the list.

Others certainly can tell you more about the "voluntary" actions, even Jack Kelso, since you evidently were too innocent to observe their "honest" methods of degrading me and other biologists and upgrading their good friends and relatives, by aid of a dishonest and stupid lower clerk they had helped in order to bribe him and be bribed by him — the story of Crowe would be worthy for a great spoke [illegible]

A little later I met Dave in the Hale basement, where he was shouting obscenities at Helling, and then turned to me smiling and said that he hoped I would tell him, when Helling slandered him. When I said that that he had not done yet, he added "wait and see" and "do not forget", whatever that meant, but I was perplexed as to who he was from the start, although I realized his peculiarities when he told me that he had asked for, I believe, 30 000 square feet for research space when he came, and when I asked him why, he said, it looked impressive; and when I wondered if he had asked for space for the others, the comment was: they did not need any. Should I have been impressed by [illegible] of others — in order to protect myself from this unscrupulous collection of crooks?

I never was impressed by Ommer, and that I hope was mutual, because I do not share his ethics — or lack of it.

I believe my considerable studies in philosophy before delving seriously into biology did give [illegible] me to judge.

No politician or [illegible] administrator likes to discuss shady or fishy matters when witnesses are present

There was much more politics, meetings arranged by non-biologists to criticize biology and those and my attitude and our research, some of these meetings were large and generally more were small and more or less closed, still others more or less secret and in the offices of Crowe or Briggs, though some of those concerned were not unscrupulous enough to let be to tell me. Several of the meetings with Briggs to denounce me were duly reported to me by the conscientious Mary Woods, always first by phone but frequently followed up by detailed letters, confidential so I duly destroyed them to protect her. I specially remember one in 1969, when Bushnell, Bock*, Crumpacker*, Dave Norris*, Rogers, and Windell had formed a committee to ask Briggs for my ouster. This was one too much for Briggs, who treated me as a chairman with a lower salary than Pannell, Smith and Rogers probably because I was an immigrant rather than because of his inability to judge academic excellence, so he wrote a long letter to every member of the biology faculty that I read as a kind of a defence. Because of some slander, which was unknown to me until I got a "report" of the "visit" from Mary Woods. When I confronted Dave Rogers, who used me to further his unscientific-taximanic nonsense, with the Briggs letter, he denied all knowledge of it, but became sheepish when I showed him Mary's report, where his name was prominent, and asked why he did not talk to me about his gripes? No more about this now, but if you are familiar with Lord Snow's Corridors of Power and other books about the [illegible] academic life at Cambridge, you [illegible] with that really good author and journalist delve into the scandalous intrigues of several more at the University of Colorado during the years I was there — and probably during its entire history? Such an author might have a chance to get a Nobel Prize or more!

Sleep of Reason, The Affair etc.

This concerned your Pannell question. As to the proposal you indicate Jack will make that I come with you and him to present the case in full to the new administration, I wonder if you have thought of the fact, that such a meeting is doomed to fail if there is nobody there who carries a big stick? And also if one or more of those concerned have been threatened by them or [illegible] from the country [illegible] not, but Jack? In your innocent and sincere honesty you probably never thought of that the reason for the failure of success when you and Pat visited Russell Nelson for the same purpose five years ago was likely mainly the fact that you were two — in addition to that he brought as an advisor just the same person, Holloway, who had planned the action against me in 1973 with Crowe and so had interest in preventing my action? In addition to the fact that by presenting to him an abridged version of my review, you may have missed essential points that can only be presented in writing by a juridically sharp person, who also would not miss the opportunity to force his way through an indication of nervosity, as you indicated became evident when Pat mentioned to them that Jack was willing to witness that I had been denied legal or any kind of counsel? If you had both had courage and [illegible] of a lawyer or politician, you would not have let that opportunity go, because it provides such an essential point of demanding [illegible] all other [illegible] becomes worthless — but there still is time [illegible] No, I do not believe it would do any good, but could add to my frustration, to let me come to read my report and add explanations to people totally unfamiliar with the affair and its long and crooked intrauniversity and conservative American biological political background,

except if this is done by somebody with a stick so big and strong that the small administrators realize that he means real business. And I think it is unwise and indeed unfair to try to force me, an immigrant who has been mistreated for decennia, to present the case to the successors of the criminals involved, with only you and Jack as kind of supporting witnesses, because it is easy for American crooks to break me down completely after what they have done before and after what my socalled friends have avoided to do — with a single exception. You are honest and trustworthy, but Jack has done things, even to us, that make him vulnerable and he knows it, though he tried to help first but broke down when Crowe stood against him. I still believe that an effective method for getting a complete and honest evaluation and correction of the situation would be to present it for advice and possible action to the honest and effective Congressman Pat Schroeder, who is familiar with some of the scandals at Boulder, and perhaps also to the committee for discrimination of the National Academy of Sciences, rather than to inform some journalist, here or abroad or to ask for action by some international organization, as e.g. Amnesty International, as suggested by a Scandinavian lawyer friend. If you select to go to the new administration, I would suggest that you and other friends familiar with the matter, see the Public counsel and George Williams after studying the documents thoroughly, corner either the new president or the chancellor individually and under conditions safe for their conduct, and free from all witnesses or recording devices, and inform them of the entire story and the possibility that it may suddenly blow internationally open, when your paper is published and ask for their reaction. Though I would believe that seeking the advice of Pat Schroeder before any other action is pondered would be the wisest move, least likely to hurt me and Doris more.

One more thought: Perhaps what you need most before deciding about these things, is an advice from a solid and experienced friend and colleague, who knows the situation except the details of which he should be advised: On the faculty at Boulder there is but one elder statesman who may be conservative and sometimes grumpy, though I have never observed him break the highest of ethical standards and never experienced from him anything base, dirty or mean, (always found him trustworthy and fair, wanting to help, [illegible]) though you may feel differently. I am thinking of Dermah, who always gave me fair and honest advice, and who might even feel inclined to make the first contact to the administration to open up the Pandora's box? Think it over, I trust him as much as I trust you, and more than Jack.

I gather that you have heard nothing from [illegible]?
I enclose a copy of the Boulder obituary that I'm sending to A&R; perhaps you could check the [illegible] [illegible] that I hope to [illegible]?

UNIVERSITY OF COLORADO, BOULDER

Museum
Campus Box 218 • Boulder, CO 80309

DENVER, CO 802
PM
16 SEP
1983

USA 20c

Askell Love
5780 Chandler Court
San Jose CA 95133

San José, February 18, 1985.

Dear Bill:

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

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

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

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

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

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

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

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

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

As ever,

No answer on the question of [illegible] Á. 15 dec. 1984?

UNIVERSITY OF COLORADO, BOULDER

Museum

30 January 1985

Askell and Doris Love
5780 Chandler Court
San Jose CA 95133

Dear Askell:

Our letters must have crossed again. How are we going to get the message of the Triticeae to more people? The Triticeae is only one instance; I hope that this work, so soundly based now, will make people think of other taxonomic groups in terms of a genomic theory, even though we know so much more about the Triticeae than anyone may ever know about the others. However, I think I begin to see an erosion of the authoritarian domination; at least people I talk and write to seem to have more open minds. It was a revelation to hear from Dewey's paper that Nevski accomplished all of his work before age 31!

This week I started to dig a little on the Minuopsis matter. On the one hand there is a strong similarity between M. nuttallii and Alsinanthe macrantha, but this does not bother me so much for writing a key at least; the capsule in Minuopsis is practically globose, and very small and in Alsinanthe m., it is oblong and much larger, and of course the one is glandular and the other totally glabrous, and the lvs. are quite different. But where does Minuopsis stand in relation to Arenaria (what I made into Eremogone probably incorrectly) hookeri? McNeill seemed not to have a place for that, but it has to be considered when we talk about Minuopsis. Perhaps you can write a discussion of all this that I can look at and then we can get together on the whole business. I will need some resolution of these genera for the publication in 1986 of the book. As I said before, you should really handle this in a paper of your own; I don't know half as much as you do about the case. If you write it I would pay the page costs to publish it in Phytologia if you would allow me to so this.

About your library. Absolutely, I would like to get it for the herbarium, and I feel that this would be the very best place for it. I see no likelihood of us being put down the drain now, except in extremis if it ever happened we would move it up to Laramie. Laramie's library needs have been satisfied for all time since they have bought the Meckler microfiche for about $100,000.00 (De Mink (I.D.C.) is furious about this piracy of his things). I don't understand why you think that INSTAAR should even be considered. There is no assurance that they will ever be botanical again; there is no assurance, being an institute supported by grants, that they will survive. Furthermore, I am getting rather out of patience with them because they never cooperate with anybody. For instance, about 5 years ago Tass Kelso spent about a year with Vlad Siplivinsky identifying scraps of plants that Pat Webber collected in eastern Siberia (many worth keeping I guess, but ecological specimens nevertheless), and I have tried the best I could to get his field notes, but he is always too busy. I finally last week sent the cartons back down to him because I can't afford to house them rent-free in my little space. What arrangement (you mention the Foundation) would you consider if you were to pass it on to the herbarium? You know what our financial situation always has been. The interest from the endowment is never more than $6,000 a year and it buys books and supports f[illegible] make it useful for you.

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field work, but we would try to make it useful for you.

Thanks for the Oreophylax information. Do you have the citation for Airy-Shaw? I don't know that work. Incidentally, I worked on Laurie Adams about this group in Australia long ago (1967-8), when we were in the field together in the Snowy Mountains of Australia, so if he has become interested in Oreophylax it is partly because I told him I couldn't see that put in Gentianella, as the Australians did. He's a pretty good fellow, also interested in hepatics. Penland collected a lot of them in the Ecuadorean Andes.

How do you feel about going back to Chondrophylla for the pygmy gentians, rather than Ciminalis. My friend Spence, in BC, argues for it as distinct from the type species of Ciminalis in the Alps. Fremontii seems to be synonymous with aquatica. In the Intermountain Flora they claim that they can't see what I was talking about, distinguishing this from prostrata; evidently they have never seen more than the one species, for they are not easy to distinguish in the herbarium but are amply distinct in the field.

Which brings me to another question. I am trying to find a small cabin to rent at Alma, under Hoosier Pass, or, failing that, to get a campsite on the ranch of a friend of mine in South Park north of Jefferson. I really want to start seriously working that area because of the things that are there but haven't been taken since 1862. For one thing, in Wyoming they have now turned up Arctous alpina, growing with Baeothryon pumilum, and there are similar bogs in the South Park area that should be located and studied. How about you and Doris coming out for a few weeks? Linna and Ludger and their kids want to spend some time with me in the field too, and if I find the right milieu it would be fun. Maybe some of could chip in together and bring you out. Late July-beginning of August.

As ever,

Bill

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UNIVERSITY OF COLORADO, BOULDER
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21 January 1985

Askell and Doris Love
5780 Chandler Court
San Jose CA 95133

Dear Askell & Doris:

On the same day almost I got your big paper on the Triticeae and Doug's paper on the Triticeae and hybridization. I find I still have some trouble although I am happy for the most part.

I don't feel that I can be honest in putting Hordeum brachyantherum as a subspecies under Critesion. This is a conviction based on morphology and ecology.

I shall be happy to go back to Critesion glaucum (Steud.) A. Love, however.

My big question is what to do with Elymus/Elytrigia albicans. I cannot for the life of me see that this thing has anything to do with lanceolatus. With us it is a very clean species with rhizomes, and slender spikes with beautifully divergent awns; the spikelets are spread out along the culm as in Pseudoroegneria, and in fact, it is almost impossible to distinguish from that if you don'e have the basal parts. The spike is not as dense as in E. trachycaulus ssp. bakeri. Your key does not allow Elymus to have rhizomes, If both you and Dewey have similar evidence of the genome composition, how can this plant be included in Elymus by you and in Elytrigia by Deweyt. Wer hat recht?

B.

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

4 Oct. 1983

Dear Askell:

Please look this copy over very carefully and let me know corrections by page and line number. This is getting close to being the final copy. Dieter Wilken has promised me a chromosome count of Argillochloa; he has plenty of fixed material and has living plants in his garden. From what Frederiksen told me it seems pretty clear that we are dealing with Festuca relatives and not Stipeae. I like to have as little as possible to do with Mary Barkworth.

As the oldest member of the Museum faculty I may some time be asked to say a bit about the history of the Biology Department, since I have been away from it sufficiently long to have some perspective. So if you feel like filling me in on Runner I'd really like to know more about that.

I hope that we don't have anything to worry about with the Seriphidium typification. So I am not adding the Lessing part of the story.

Thanks for reminding me of the Oligosporus groenlandicus.

Your review of Boecher is beautiful. I found only two errors: Antiphylla, not Anthiphylla; and in last paragraph, Danish botanist of his generation. If you like I can cell INSTAAR and pass them on to Kathleen.

B.

NEW NAMES AND COMBINATIONS, PRINCIPALLY IN THE ROCKY MOUNTAIN FLORA--IV

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

The third paper in this series was published in Phytologia 53:187-190. 1983.

A NEW GENUS OF GRASSES FROM THE WESTERN OIL SHALES

ARGILLOCHLOA W. A. Weber, **gen. nov.**

Gramen perenne, inflorescentis non secundis, ramis floriferis rigidissimo-divaricatis basalibus 2, spiculis 2-floribus, gluma secundo lemma secundum aequans, lemmatibus sterilibus nullis vel rudimento clavato sterili faciens, a Festuca differt.

Type species: **Argillochloa dasyclada** (Hackel ex Beal) W. A. Weber, **comb. nov.** Festuca dasyclada Hack. ex Beal, Grasses N. Amer. 2:602. 1875. Derivation from Greek, argillos, clay (including shale), + chloe, grass.

Festuca dasyclada, until very recently, was known from the type locality (Wasatch Plateau, Emery County, Utah), but field knowledge was nil. Irvine et al (1978), reporting it from Colorado, wrote: "This plant was listed as "possibly extinct" in the "Report on Endangered and Threatened Species of the United States"... Only two vouchers of this taxon exist in major herbaria (US, NY), and mention of the species last occurs in the second edition of Hitchcock's treatment of the grasses...."

This species was reported from Colorado (Irvine, l.c.) from the Upper Parachute Member of the Green River Formation and the Uinta Sandstone throughout Garfield County, Colorado. Recent activity involving environmental impact research has added a number of localities in Rio Blanco County at altitudes from 2,160-2,580 meters (7,120-9,000 ft.).

The plant is a bunch-grass with a very strong but superficial resemblance to Oryzopsis hymenoides, and occurring near it on the same areas of shale scree slopes. The two grasses seem to have slightly different ecological preferences, however, because stands of Argillochloa are never as ubiquitous as those of Oryzopsis, which commonly colonizes mixed soils of eroding road banks as well as the pure shale slopes.

Argillochloa differs strikingly from Festuca by its rigidly divaricate secondary branches, at the bases of which a strongly-developed convex, often red, pulvinus fills the axils; the spikelets have an unusually long second glume which equals the second lemma; the spikelets have two fertile florets; the terminal rachilla is either naked or sometimes topped by an early-deciduous sterile rudiment; the lowermost branchlets of the inflorescence are paired; the inflorescence is not at all secund as is the case in Festuca; at maturity the flowering culms commonly break away and behave like tumbleweeds. The habitat is extremely unusual for Festuca, at least as it is known in America.

Signe Frederiksen (Univ. of Copenhagen, corresp.) has kindly made a thorough anatomical analysis of Argillochloa and found that, as she expected, "the anatomy is within the variation of the genus Festuca, but the question is how important that observation is." She continues: "According to Metcalfe (1960: Anatomy of Monocotyledons I. Anatomical evidences concerning genera and species: '.... there is a marked overlap in the characters of those genera that are generally accepted as being closely related to one another. This seems to indicate that differences between closely related genera, based on leaf characters alone, would be of little taxonomic value.' I would like to turn it around and say that if the morphological characters are strong enough to separate this taxon from Festuca, then resemblance in the anatomy of the leaf blade is of minor significance." Frederiksen was impressed by the morphological divergence of Argillochloa from Festuca noted herein.

NEW COMBINATIONS IN LINUM, SENS. LAT.

ADENOLINUM GRANDIFLORUM (Desf.) W. A. Weber, **comb. nov.** Linum grandiflorum Desf., Flora Atlantica 1:278. t. 78. 1798.

ADENOLINUM PRATENSE (Norton) W. A. Weber, **comb. nov.** Linum lewisii pratense J. B. S. Norton, Trans. Acad. Sci. St. Louis 12:38, pl.6. 1902. Rogers (1968), in a review of the yellow-flowered species of Linum in western North America, unfortunately did not concern himself with the generic problem in the genus Linum, sens. lat. Linum is based on the type, Linum usitatissimum L., a blue-flowered annual species with linear stigmas and erect flowers and basic chromosome number, x=15. In western North America, the blue-flowered group, Adenolinum Reichenbach 1837, has capitate stigmas and recurved fruiting pedicels, and basic chromosome number x=9. The pale yellow-flowered group consists of two well-defined lines, both with x=8, though otherwise cytologically distinct according to Löve (corresp.): one, Cathartolinum Rchb. (1837), based on Linum catharticum L., with white flowers and yellow claws. The other is Rogers' L. schiedeanum complex, which Small (1907) included in his broadly construed Cathartolinum, differing significantly in fruit dehiscence, ovule number, pollen morphology, style morphology, and basic chromosome number x=8, from Mesynium Raf. (1838) (Rogers' L. rigidum group) with a basic chromosome number of x=15. Rogers clearly tabulated these important differences but declined to divide the genera. Löve and Löve recently revived Adenolinum and Mesynium (Löve 1982), quite justifiably in my opinion. With Adenolinum, Cathartolinum s. str., and Mesynium segregated, the L. schiedeanum complex forms a distinct group.

MESYNIUM Raf., Fl. Telluriana 3:33. Nov.-Dec. 1837. A lectotype should be designated. Of the five species mentioned, M. texana was new, three others were nomina nuda, and M. mexicanum (H.B.K.) Raf., was a transfer. I propose M. mexicanum be chosen as the lectotype.

MESYNIUM ALATUM (Small) W. A. Weber, **comb. nov.** Cathartolinum alatum Small, N. Am. Fl. 25:81. 1907.

MESYNIUM ARISTATUM (Engelm. in Wisliz.) W. A. Weber, **comb. nov.** Linum aristatum Engelm. in Wisliz., Tour Northern Mexico 101. 1848.

MESYNIUM AUSTRALE (Heller) W. A. Weber, **comb. nov.** Linum australe Heller, Bull. Torr. Bot. Club 25:627. 1898.

MESYNIUM AUSTRALE ssp. GLANDULOSUM (C. M. Rogers) W. A. Weber, **comb. nov.** Linum australe var. glandulosum Rogers, Sida 1:336. 1964.

MESYNIUM IMBRICATUM (Raf.) W. A. Weber, **comb. nov.** Nezera imbricata Raf., New Flora & Bot. North Amer. 4:66. 1838.

MESYNIUM HUDSONIOIDES (Planch.) W. A. Weber, **comb. nov.** Linum hudsonioides Planch., London J. Bot. 7:186. 1848.

MESYNIUM PUBERULUM (Engelm. in A. Gray) W. A. Weber, **comb. nov.** Linum rigidum var. puberulum Engelm. in A. Gray, Smithson. Contr. Knowl. 3 (Pl. Wright. 1): 25. 1852.

MESYNIUM SUBTERES (Trel.) W. A. Weber, **comb. nov.** Linum aristatum Engelm. var. subteres Trel. in A. Gray, Syn. Fl. N. Am. 1(1):347. 1897.

MESYNIUM VERNALE (Wooton) W. A. Weber, **comb. nov.** Linum vernale Wooton, Bull. Torr. Bot. Club 25:452. 1898.

ALETES (UMB): AN EXPANDED CONCEPT

Despite the fact that many eminent American botanists have attempted to classify the western North American Umbelliferae, several genera remain to some extent artificial. While one complete treatment (Mathias and Constance, 1944-45) has tended to stabilize and reduce a number of generic names, the submergence of some of the old genera has simply served to hide the fact that large ones like Lomatium and Cymopterus are still very heterogeneous, and unless monographers of some of the allied genera carefully reexamine these large ones for misfits, this situation will likely continue.

The history of classification of the western North American umbels also displays a lack of feeling for the whole organism, its total morphology and habitus, its chemistry, phytogeography and its ecology. Too much emphasis has been placed on one or two characters that are given much weight.

The genus Aletes is based on Aletes acaulis C. & R., 1888 (Deweya acaulis Torr.). A revision of this genus was published very recently (Theobald, Tseng and Mathias, 1963). It was undertaken as a result of my rediscovery of Neoparrya lithophila and my suggestion, which they accepted, that Pteryxia anisata should be referred to Aletes. I have never been satisfied with the maintenance of Neoparrya as a monotypic genus, and recently (Weber 1979) I transferred a second taxon, N. megarrhiza, out of Lomatium, where it was anomalous.

Theobald et al., while they described a few new taxa, did not examine other genera for possible additions to Aletes. They also deferred study of Pteryxia and implied that they were about to study Cymopterus. They pointed out, however, Cronquist's (1961) expansion of Cymopterus to include two more discordant elements, Pteryxia and Pseudocymopterus.

Theobald et al. described Aletes as "perennials from slender to thickened elongated roots". This is inaccurate. The structures they refer to are caudices, which are covered with marcescent sheathing petiole-bases, a critical difference. I would expand their generic concept to include plants with yellow, pale yellow to whitish and exceptionally (as in Pseudocymopterus) purple, flowers. And I would allow considerable variation in the number, size, and disposition of the vittae, and in the compression and development of the lateral and dorsal wings of the mericarps. I agree completely when they say that "the genus is remarkably consistent in its habit and basic leaf pattern". Their monograph is a good starting point, but more bricks need to be laid in order to make the building complete.

Without seriously altering the circumscription provided by Theobald et al, I regard Aletes is a natural group embodying the following unique constellation of characters:

1. Plants densely caespitose with stout, branched caudices clothed with long-enduring marcescent petiole-bases.
2. Strictly acaulescent; (this eliminates Pteryxia terebinthina, which is always slightly caulescent).
4. Pseudoscapes never developed.
5. Plants strongly scented (anise, citronella, celery)
6. Leaves pinnatifid or pinnate, with pinnae simple or pinnatifid, usually stiff-textured.
7. Bracteoles always well-developed, lance-linear to linear, dimidiate.
8. Involucre never developed.
9. Flowers yellow, pale yellow, whitish, or exceptionally purple.
10. Rays subequal, widely spreading, sometimes the outer ones deflexed.
11. Mericarps with variable development of lateral wings; dorsal ridges often prominent.
12. Mericarps usually trapezoidal in cross-section, not or variably dorsally compressed.
13. Stylopodium none, the styles arising out of the base of a spongy disk (some authors seem to have confused this disk with a low stylopodium).

If, bearing in mind this set of characters, one returns to the standard treatment of North American umbels, several taxa stand out in Lomatium, Pteryxia and Cymopterus discordant elements. Furthermore, these taxa have always been controversial, placed variously in other discarded genera such as Cynomarathrum, Pseudoreoxis and Pseudopteryxia.

The following new combinations are proposed to bring these taxa into Aletes.

ALETES EASTWOODIAE (C. & R.) W. A. Weber, **comb. nov.** Cynomarathrum eastwoodiae C. & R., Contr. U. S. Nat. Herb. 7:247. 1900.

ALETES BIPINNATA (S. Wats.) W. A. Weber, **comb. nov.** Pseudocymopterus bipinnatus C. & R., Rev. N. Am. Umbell. 75. 1888.

ALETES HENDERSONII (C. & R.) W. A. Weber, **comb. nov.** Pseudocymopterus hendersonii C. & R., Contr. U. S. Nat. Herb. 7:190. 1900.

ALETES JUNCEA (Barneby & N. Holmgren) W. A. Weber, **comb. nov.** Lomatium junceum Barneby & N. Holmgren, Brittonia 31:96. 1979. Barneby & Holmgren (1979), in recognizing and presenting a key to the "Cynomarathrum species of Lomatium" saw the natural group that I feel is incorrectly placed in Lomatium, but they made no connection with Aletes. They, however, included L. triternatum and L. concinnum, two caulescent species, in the group.

ALETES LATILOBA (Rydb.) W. A. Weber, **comb. nov.** Cynomarathrum latilobum Rydb., Bull. Torr. Bot. Club 40:73. 1913.

ALETES LITHOPHILA (Mathias) W. A. Weber, **comb. nov.** Neoparrya lithophila Mathias, Ann. Mo. Bot. Gard. 16:393. 1929.

ALETES LONGILOBA (Rydb.) W. A. Weber, **comb. nov.** Pseudopteryxia longiloba Rydb., Bull. Torr. Bot. Club 40:72. 1913. Mathias, Theobald & Tseng (1964) did not include this taxon in their monograph of Aletes (despite the fact that Rydberg clearly showed its close relationship to P. anisata), probably because Mathias had earlier synonymized it (incorrectly, we feel) under Pteryxia hendersonii. Mathias et al. (1964) declined to discuss Pteryxia. A. longiloba differs from A. anisata chiefly in its more delicate leaf texture and more slender and attenuate leaf segments.

ALETES MEGARRHIZA (A. Nels.) W. A. Weber, **comb. nov.** Peucedanum megarrhizum A. Nels., Bull. Torr. Bot. Club 26:130. 1899.

ALETES MINIMA (Mathias) W. A. Weber, **comb. nov.** Lomatium minimum Mathias, Ann. Mo. Bot. Gard. 25:273. 1937.

ALETES NIVALIS (S. Wats.) W. A. Weber, **comb. nov.** Cymopterus nivalis S. Wats., Bot. King's Exp. 123. 1871.

ALETES NUTTALLII (A. Gray) W. A. Weber, **comb. nov.** Seseli nuttallii A. Gray, Proc. Amer. Acad. 8:287, in part. 1870.

ALETES PARRYI (S. Wats.) W. A. Weber, **comb. nov.** Peucedanum parryi S. Wats., Proc. Amer. Acad. 11:143. 1876.

ALETES PETRAEA (M. E. Jones) W. A. Weber, **comb. nov.** Cymopterus petraeus M. E. Jones, Contr. W. Bot. 8:32. 1898.

ALETES SCABRA (C. & R.) W. A. Weber, **comb. nov.** Cynomarathrum scabrum C. & R., Contr. U. S. Nat. Herb. 7:247. 1900.

ASKELLIA, A NEW SEGREGATE OF THE GENUS CREPIS

ASKELLIA , genus nov.

Based on Crepis, Sect. Ixeridopsis Babcock, Univ. Calif. Publ. Bot. 22:212. 1947. Typus: Crepis nana Richards. This genus, differing morphologically and cytologically from Crepis and Psilochenia (Crepis, sens. lat., cf. Babcock 1938, see Weber 1983), represents an Old World group with a basic chromosome number of x=7. It is named in honor of my friend Dr. Áskell Löve, student of Arne Müntzing and Eric Hultén, dean of the Icelandic flora, founder and first president of the International Organization of Plant Biosystematists. His dedication to the Science of Botany, his encyclopedic memory of botanical information, his understanding of biosystematic, especially cytological, techniques and his exposition of its philosophy, his

role in developing the concept of the Flora Europaea and, in its earliest phase, the Flora North America Project, and his courage and perseverance in the face of controversy, misunderstanding, character assassination, gross miscarriage of justice, and subsequent academic ostracism has earned him lasting recognition as one of the outstanding plant taxonomists of our generation. His kindness and support of colleagues and young botanists is well-known and appreciated by all who have benefitted from knowing him.

ASKELLIA ALAICA (Krasch.) W. A. Weber, **comb. nov.** Crepis alaica Krasch., Tr. Bot. Inst. AN SSSR, ser. 1,1:182. 1933.

ASKELLIA CORNICULATA (Regel. & Schmalh.) W. A. Weber, **comb. nov.** Crepis corniculata Regel & Schmalh., Izv. Obsc. Ljubit. Estestv. Antrop. Etnogr. 34(2):54. 1882.

ASKELLIA ELEGANS (Hook.) W. A. Weber, **comb. nov.** Crepis elegans Hook., Fl. Bor.-Amer. 1:297. 1834.

ASKELLIA FLEXUOSA (Ledeb.) W. A. Weber, **comb. nov.** Prenanthes polymorpha gamma flexuosa Ledeb., Fl. Altaica 4:145. 1833.

ASKELLIA KARELINII (M. Pop. & Schischk. in Popov) W. A. Weber, **comb. nov.** Crepis karelinii M. Pop. & Schischk. in Popov, Fl. Almat. zapovedn., Addenda 28:757. 1940.

ASKELLIA LACTEA (Lipsch.) W. A. Weber, **comb. nov.** Crepis lactea Lipsch., Fedde Rep. 42:159. 1937.

ASKELLIA NANA (Richards.) W. A. Weber, **comb. nov.** Crepis nana Richards., Bot. App. Franklin, 1st Jour. ed. 1:746. (p. 18 in repr.) 1823; ed. 2: 757 (p. 29 in repr.). 1823.

ASKELLIA NANA ssp. RAMOSA (Babcock) W. A. Weber, **comb. nov.** Crepis nana ssp. ramosa Babcock, Univ. Calif. Publ. Bot. 22:542. fig. 155. 1947.

ASKELLIA SOGDIANA (Krasch.) W. A. Weber, **comb. nov.** Youngia sogdiana Krasch., Bot. Mat. Herb. Bot. Inst. AN SSSR 9(4-12):184. 1946.

THE WESTERN NORTH AMERICAN WOODY SAGEBRUSHES

The western North American woody sagebrushes centering about Artemisia tridentata belong to a homogeneous group of similar morphology and ecology, differing from all other local Artemisia in having homogamous heads, all but one (A. bigelovii) lacking any ray-flowers whatsoever. One additional species was described from southern South America. Related species in Eurasia were segregated from Artemisia by Polyakov (1961) based on the type species Seriphidium maritimum (L.) Pol. Artemisia, section Seriphidium was proposed for this group by Besser (1829) and adopted by Hooker (1833) who incorrectly typified it by Artemisia cana Pursh. Rouy (1903) treated it as Artemisia, subgenus Seriphidium. The American species were treated by DeCandolle (1837) as Artemisia, Sect. Seriphidium, subsect. Trifida. This group has been treated exhaustively by Ward (1953). Earlier accounts include those of Rydberg (1916) and Hall & Clements (1923).

The North American members of Seriphidium form a very natural unit and I propose recognizing them as a subgenus.

SERIPHIDIUM, **Subgenus STEPPEA** W. A. Weber, **subgen. nov.** Based on Artemisia, Sect. Seriphidium, subsect., Trifida DC., Prodromus 6:105. 1837. Typus: Seriphidium canum (Pursh) W. A. Weber.

SERIPHIDIUM BIGELOVII (A. Gray in Torr.) W. A. Weber, **comb. nov.** Artemisia bigelovii A. Gray in Torr., Pacific R.R. Rep. 4:110. 1857.

SERIPHIDIUM ARBUSCULUM (Nutt.) W. A. Weber, **comb. nov.** Artemisia arbuscula Nutt., Trans. Amer. Phil. Soc. II. 7:398. 1841.

SERIPHIDIUM ARGILLOSUM (Beetle) W. A. Weber, **comb. nov.** Artemisia argillosa [as argilosa] Beetle, Rhodora 61:84. 1959.

SERIPHIDIUM CANUM (Pursh) W. A. Weber, **comb. nov.** Artemisia cana Pursh, Fl. Amer. Sept. 521. 1814.

SERIPHIDIUM CANUM ssp. BOLANDERI (A. Gray) W. A. Weber, **comb. nov.** Artemisia bolanderi A. Gray, Proc. Amer. Acad. 19:50. 1883.

SERIPHIDIUM LONGILOBUM (Osterh.) W. A. Weber, **comb. nov.** Artemisia spiciformis var. longiloba Osterh., Muhlenbergia 4:69. 1908.

SERIPHIDIUM MENDOZANUM (DC.) W. A. Weber, **comb. nov.** Artemisia mendozana DC., Prodromus 6:105. 1837.

SERIPHIDIUM PYGMAEUM (A. Gray) W. A. Weber, **comb. nov.** Artemisia pygmaea A. Gray, Proc. Amer. Acad. 21:413. 1886.

SERIPHIDIUM NOVUM (A. Nels.) W. A. Weber, **comb. nov.** Artemisia nova A. Nels., Bull. Torr. Bot. Club 27:2674. 1900.

SERIPHIDIUM RIGIDUM (Nutt.) W. A. Weber, **comb. nov.** Artemisia trifida beta rigida Nutt., Trans. Amer. Phil. Soc. II. 7:398. 1841.

SERIPHIDIUM ROTHROCKII (A. Gray) W. A. Weber, **comb. nov.** Artemisia tridentata ssp. rothrockii Hall & Clements, Carnegie Inst. Wash. Publ. 326:139. 1923.

SERIPHIDIUM TRIDENTATUM (Nutt.) W. A. Weber, **comb. nov.** Artemisia tridentata Nutt., Trans. Amer. Phil. Soc. II. 7:398. 1841.

SERIPHIDIUM TRIDENTATUM ssp. PARISHII (A. Gray) W. A. Weber, **comb. nov.** Artemisia parishii A. Gray, Proc. Amer. Acad. 17:220. 1882.

SERIPHIDIUM VASEYANUM (Rydb.) W. A. Weber, **comb. nov.** Artemisia vaseyana Rydb. N. Amer. Flora 34:283. 1916. This has usually been treated as a subspecies of A. tridentata, but it has a distinctive range and ecology, and is the only one of the tridentata complex that is diploid.

SERIPHIDIUM TRIDENTATUM ssp. WYOMINGENSE (Beetle & Young) W. A. Weber, **comb. nov.** Artemisia tridentata ssp. wyomingensis Beetle & Young, Rhodora 67:405. 1965.

SERIPHIDIUM TRIPARTITUM (Rydb.) W. A. Weber, **comb. nov.** Artemisia trifida Nutt., Trans. Amer. Phil. Soc. II. 7:398. 1841, non Turcz. 1832.

The philosophical justifications of a conservative generic concept in Artemisia were excellently stated by Hall & Clements, whose discussion of the taxonomic history is a classic. In their maintenance of Seriphidium as a section of Artemisia, they were

influenced by the transitional nature of A. bigelovii, where "the ray-flowers, recognized by their peculiar 2-toothed corollas, are usually present, although reduced in number to only one or two, but occasionally entirely suppressed, the head then consisting of only two or three flowers with regular 5-toothed corollas. Perhaps this species represents the beginning of the Seriphidium line, where the evolution of homogamous from heterogamous heads is still in progress."

Hall & Clements used similar logic to submerge Artemisiastrum under Artemisia: "While the presence or absence of these structures [receptacular bracts] is of much value in the classification of the Compositae, their occasional occurrence in a genus whose species are almost universally devoid of them may be looked upon as a possible case of reversion rather than as the basis for a new genus." Yet Hall & Clements maintained Artemisia bigelovii in another section because of the occurrence of a variable number of marginal ray-flowers. Ward followed Hall & Clements' reasoning but treated the species under Sect. Seriphidium "because of its close resemblance to certain members of Seriphidium and its frequent misdetermination as such."

Hall & Clements argued that raising sections to generic rank caused "relationship and perspective [to be] lost, [producing] results [that] are both unnatural and unusable." Their argument was strongly polemical, and based on personal preference rather than on any genetic basis or consideration of the magnitudes of the gaps or on crossability or ecology. When scientists hold such rigid beliefs, no counter-argument, no matter what the facts are, will change the minds of those who do not like to have their preconceptions disturbed.

A diametrically contrasting point of view is delightfully presented by Camp (1940), and this is pertinent here. After showing that Gaylussacia, according to his current ideas, comprised three additional genera, he wrote: "The erection of the genera Buxella, Decachaena, and Lasiococcus to take care of our North American species of huckleberries has met with a great deal of opposition and I, too, have deplored the segregation. But fundamentally it was sound, for the old classic genus is composed of four very definite groups of species.... Had we been able to maintain the species with which we are most familiar in the genus Gaylussacia and erected new genera for those in South America, there would have been little protest. Apparently it is a common reaction among taxonomists--being human--that, so long as a genus is endemic in some remote part of the world it may be split as the student pleases, the splitting being hailed as a brilliant piece of research. But let one among us attempt, phyletically, to rearrange a genus with species in our own local areas--the rearrangement resulting in the necessity of learning new generic names--there is an immediate and loud protest. Even so, Lasiococcus dumosus, Decachaena baccata and Buxella brachycera are names with a strange and unfamiliar sound and I don't like them any more than you do. But, I have been asked, "Then why change them? We have known them as species of Gaylussacia for so many years." There is only one answer. If such an argument is to determine our criteria concerning the status of a generic name, then let us be purists

and return those species to the genus Vaccinium, for they were known as Vaccinium dumosum, V. resinosum and V. brachycerum for about a half-century prior to their inclusion in the genus Gaylussacia. The point is, none among us remember the clamor that arose when the botanists of another day had to learn to think of them as belonging to "that new-fangled genus Gaylussacia." From the standpoint of phylogeny, there is no more reasonableness in retaining these species in Gaylussacia than in returning them to Vaccinium.... Perhaps we should adopt as our motto, not 'Back to Linnaeus,' but, 'Forward to the truth.' Perhaps, if we were not afraid of the puling croaking of certain of our confrères every time we broaden and particularize our concepts, we could put new life into old taxonomic bones, long interred in the musty vault of nomenclatural conservatism."

The concept of genus sometimes seems not to be taken seriously as a scientific one, since "practicality" is so often invoked to sustain traditional usages. Komarov (1968) in the introduction of the monumental Flora USSR, wrote (Israeli translation): "The problem of genera resembles to some extent the problem of species. The contemporary tendency of systematics is to split large composite genera into smaller ones. There is, however, a notable difference. Only in exceptional cases does generic classification reflect the current evolutionary process. As a rule, it related to a more distant past and provides in a way a memorial of an evolutionary process already largely accomplished. This being so, considerations of taxonomic expediency come to the fore. Wherever the professional botanist can use the names with which he is familiar, these ought to be retained. Generic names with mnemotechnical associations represent the most important part, the very basis, of botanical nomenclature...."

While we may take this statement to be one of support of the "practical" genus concept, he goes on to state the principle of generic names used in the flora: "The categories of species and genus are to be conceived in terms of the narrower natural interpretation, reflecting the genetic rather than the formal relationships of kindred organisms; the species being geographic formations, the genera being aggregates arising from divergence in the progeny of chief progenitore of a given group."

A few paragraphs later, however, he champions the notion that generic and specific characters are different in essence: "Confusion of generic and specific characters should not be allowed, i.e. genera ought to be described in such a way that changes in the number of species will not upset the described genus and its determination. It should be noted that generic characters are by nature different from specific characters."

The genus is always going to be a somewhat more subjective unit than the species, but at the same time the genus must always be measured against the razor of scientific method. Genera must not be championed because we like the names or because Linnaeus did, or because sleeping dogs must be let lie. Generic concepts must be allowed to change with scientific thought just as all other categories must.

The fact, whether we like it or not, new evidence from anatomy, SEM observation, phytochemistry, cytology, genetics and ecolo-

gy should cause us continually to reexamine our preconceived notions. So-called "generic splitting", once considered taboo because of the uproar raised by laity and applied botanists, in fact blamed for the temporary decline in popularity for taxonomy, continues as it must when justified. It is happening just as broadly, or more so, in the fungi, bryophytes and lichens. Delimitation of genera does not necessarily rest on the selection of one or more so-called "generic characters", but upon all of the biological features of a group that set it apart as a monophyletic line separated by barriers of whatever sort, from its near relatives. It is the reasonable demonstration of monophylesis and of barriers between groups, more than simply our selection of so-called "generic" characters, that justifies the genus.

Divergent generic concepts represent different points of view, and as alternative treatments they should be tolerated until proved incorrect. Good science should not involve decisions based on personal convenience, likes or dislikes of large or small genera, their names, or the scientists who propose them. It is indeed strange that although we are responsible, by the binomial system, for the development of the most concise and logical way of enabling scientists to express their different points of view, many of us would deny our colleagues the exercise of them. In other disciplines this would be considered intolerable.

I would predict that foresters and range managers would prefer to continue to consider the sagebrushes as belonging to Artemisia. But at the same time, for them Artemisia comprises the sagebrushes alone, since they have very little to do with the vast remainder of the genus. Nor would they recognize most of them since most species are so different from the sagebrushes. Yet A. vulgaris remains forever the type species of Artemisia. If most taxonomists are content with the sagebrushes belonging with A. vulgaris, they are of course welcome to their viewpoint.

Artemisia palmeri A. Gray, included by Ward, and Hall & Clements under Sect. Seriphidium remains anomalous, differing by its chaffy receptacle, elongate herbaceous branches, bicolored, deeply incised leaf-blades suggestive of A. vulgaris, and nearly equal phyllaries. I lean toward retaining Artemisiastrum Rydberg for this monotype.

MISCELLANY

ACROLASIA THOMPSONII (Glad) W. A. Weber, **comb. nov.** Mentzelia thompsonii Glad, Madrono 23:289. 1976.

BROMELICA BULBOSA (Geyer ex Porter & Coulter) W. A. Weber, **comb. nov.** Melica bulbosa Geyer ex Porter & Coulter, Syn. Fl. Colo. p. 149. 1874. The articulation of the spikelets above the glumes, the lack of tendency of the spikelets to nod, and the world distribution patterns of Melica typified by M. nutans L. according to Tzvelev (1976), and Bromelica (Boyle, 1945), suggest that these groups represent different phyletic lines.

BROMELICA SPECTABILIS (Scribn.) W. A. Weber, **comb. nov.** Melica spectabilis Scribn., Proc. Acad. Nat. Sci. Phila. 37:45. 1885.

DELPHINIUM RAMOSUM Rydb. var. **ALPESTRE** (Rydb.) W. A. Weber, **comb. nov.** Delphinium alpestre Rydb., Bull. Torr. Bot. Club 29:146. 1902.

IPOMOPSIS STENOTHYRSA (A. Gray) W. A. Weber, **comb. nov.** Gilia stenothyrsa A. Gray, Proc. Amer. Acad. 8:276. 1870.

NUTTALLIA ARGILLOSA (Darlington) W. A. Weber, **comb. nov.** Mentzelia argillosa Darlington, Ann. Mo. Bot. Gard. 21:153. 1934.

NUTTALLIA REVERCHONII (Urb. & Gilg) W. A. Weber, **comb. nov.** Mentzelia pumila (Nutt.) T. & G. var. reverchonii Urb. & Gilg, Nov. Act. Nat. Cur. [Abh. K. Leop.-Carol. Deutsch. Akad. Naturf.] 76:94. 1900. Mentzelia reverchonii Thompson & Zavortink.

PACKERA OODES (Rydb.) W. A. Weber, comb. nov. Senecio oodes Rydb., Bull. Torr. Bot. Club 33:158. 1906.

VITICELLA ORIENTALIS (L.) W. A. Weber, **comb. nov.** Clematis orientalis L., Sp. Pl. 543. 1753.

CORRECTIONS

In a previous paper (Weber & Löve 1981), inadvertent errors were made concerning the following new combinations and their basionyms. We are indebted to Dr. T. M. Barkley for drawing them to our attention.

Packera cana f. eradiata (D. C. Eaton) Weber & Löve, **comb. nov.** Senecio canus var. eradiatus D. C. Eaton in S. Wats., Bot. King's Expl. 190. 1871.

Packera cymbalarioides (Buek) Weber & Löve, **comb. nov.** Senecio cymbalarioides Buek, Index DC. Prodr. 2:6. 1840.

Packera rosei Weber & Löve, based on Senecio rosei Greenman sine diagn. is a nomen nudum.

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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, lines 15 & 16: Would it not look nicer if comb. nov. were in the same line?
line 21: al. (period missing).
line 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 have 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 unnaturally.
line 34: underline Seriphidium.
lines 43 & 44: ex either underlined or not, not both.
line 47: Melica nutans (underlined)
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,

United States Department of Agriculture
Agricultural Research Service
Western Region

Mountain States Area
Crops Research Laboratory
Utah State University - UMC 63
Logan, UT 84322

February 2, 1984

Dr. Askell Löve
5780 Chandler Court
San Jose, CA 95123

Dear Askell:

I am enclosing copies of the correspondence with Art Cronquist that I mentioned in our telephone conversation a few days ago. Cronquist obviously has his mind made up, and nothing (not even the facts) will change him.

Mary Barkworth (with myself as a co-author) recently published a paper in the Great Basin Naturalist entitled, "New generic concepts inthe Triticeae of the Intermountain Region: key and comments." I will send you a reprint when they arrive. I am enclosing a copy of a letter from a taxonomist in Australia and my response to him. It is encouraging that some taxonomists are actually implementing the genomic system in herbarium collections. We seem to win a few and lose a few.

Sincerely,

DOUGLAS R. DEWEY
Research Geneticist

Enclosure

The University of Adelaide
WAITE AGRICULTURAL RESEARCH INSTITUTE
DEPARTMENT OF AGRONOMY

Chairman of Department:
PROFESSOR C.J. DRISCOLL, D.Sc.

GLEN OSMOND
SOUTH AUSTRALIA 5064
Telephone : 79 7901
Telegrams :
WAITINST Adelaide

19th January, 1984.

Dr. D.R. Dewey,
Research Geneticist,
USDA,
LOGAN. UTAH 84322. U.S.A.

Dear Sir,

I have recently been involved in identifying some Australian native "Agropyron". The species involved are:

A. pectinatum (Labill.) Beauv.

A. retrofractum Vickery

A. velutinum Nees

A. scabrum (Labill.) Beauv. and var. pluvinerve Vickery.

I have found your paper in Crop Sc. 23 (1983) 637 of considerable interest and help and have in fact reorganised our herbarium collection, as far as I can trace species, along the lines outlined by you. However reference to Australian species is conspicuously lacking. I write to ask whether you know of, or have worked with any of these species and can let me know of the genomes present in them. The first two appear very close (if in fact they are distinct) and the third and fourth each distinct. It seems to me that there may be three separate elements. Seed of the first and the last could be made available to you. 2 in Aust.

Yours sincerely,

David Symon

D.E. SYMON

Mountain States Area
Crops Research Laboratory
Utah State University - UMC 63
Logan, UT 84322

January 24, 1984

Mr. D. E. Symon
Agronomy Dept.
Waite Agricultural Research Institute
University of Adelaide
Glen Osmond, South Australia 5064
AUSTRALIA

Dear Mr. Symon:

I am pleased that you find the genomic system of classification has some merit. I, of course, think that it makes biological sense. The enclosed 1982 publication by A. Löve describes the genomic system in more detail. The reason that I neglected to mention Australian species of Triticeae is that much less is known about the genomic relationships of Australian species than species from other parts of the world. I am enclosing pages from a manuscript I am preparing that illustrates how little we know about the Australian species.

I am enclosing a copy of a recent paper by Askell Löve and Henry Connor that deals with New Zealand species, some of which also occur in Australia. I do not agree with all that is reported in that paper, but I concur that Agropyron scabrum belongs in Elymus.

One reason that I know so little about Australian Triticeae is that I have not had access to several of the species. Consequently, I am delighted by your offer to supply seed. I have several collections of A. velutinum and A. scabrum, but I have never seen A. pectinatum or A. retrofractum. So I am especially anxious to get seed of A. pectinatum. I am always looking to expand my collection, so I would also welcome seed of A. scabrum.

Agropyron velutinum is a diploid, 2n=14, but it has never been hybridized with other species. Agropyron scabrum is hexaploid (2n=42) and Löve has hybridized it with several species (see enclosed paper). I agree with your observation that we are dealing with three separate biological elements in Australian Triticeae: 1) A. pectinatum-retrofractum, 2) A. velutinum, and 3) A. scabrum. With your help, we should be able to confirm these observations by hybridizing the various taxa.

I look forward to cooperating with you in untangling the genomic relationships among Australian Triticeae.

Sincerely,

DOUGLAS R. DEWEY
Research Geneticist

Enclosures

Arid Southwest Area
Crops Research Laboratory
Utah State University - UMC 63
Logan, UT 84322

December 16, 1983

Dr. Arthur Cronquist
New York Botanical Garden
Bronx, NY 10458

Dear Art:

I can appreciate your concern about the genomic system of classification as applied to Agropyron and its relatives. All I ask is that you and others maintain an open mind and I will try to do the same. I am concerned that my ideas are being discounted in the North American taxonomic community because I have aligned myself quite closely with Askell Löve, who seems to generate controversy and disdain in North America. I trust that agrostologists will not let personalities stand in the way of objectivity and fairness.

To those who cannot buy the full taxonomic package based on genome relationships, I recommend consideration of N. N. Tzvelev's treatment of the Triticeae in Poaceae URSS. His treatment reflects quite well the cytogenetic facts. One thing that North Americans must keep in mind is that the Triticeae is for the most part an Asian tribe, and the Komarov Botanical Institute houses the most extensive collections, particularly type specimens. North American agrostologists seem to be no more inclined toward Tzvelev's treatment than toward the genomic system. Maybe Tzvelev doesn't belong to the club either.

A meaningful solution to the taxonomy of the Triticeae will come only from a consideration of the tribe as a whole on a worldwide basis. Those who attempt to address general taxonomic issues in the tribe from a provincial perspective are kidding themselves. This is why we had hoped to organize the international conference of the taxonomy of the Triticeae. I suspect that the proposal got torpedoed by agrostologists who can't tolerate more than one viewpoint.

I am pleased that most agrostologists now recognize Agropyron in its restricted (genomic) sense. Psathyrostachys and Leymus also have general acceptance except in North America.

I am not especially concerned whether or not Pascopyrum gains acceptance. That is a relatively minor issue.

I am very much concerned about the sentiment in the U.S. and England to treat Elymus as an expanded genus that combines traditional Elymus and traditional Agropyron (minus the crested wheatgrasses). Elymus in its expanded context is more of a taxonomic nightmare that traditional Agropyron ever was. The British are at least willing to keep Leymus out of this biological amalgam.

For the life of me, I cannot see any useful purpose being served by combining such diverse elements as Agropyron repens, Agropyron spicatum, Agropyron elongatum, Agropyron trachycaulum, Elymus canadensis, Elymus cinereus, Elymus junceus, and Sitanion hystrix into one genus. These species are so morphologically, ecologically, reproductively, and genomically diverse that placing them in one genus simply masks the extensive biological differences inherent in them. If one chooses to define Elymus in this broad fashion, he might as well go the whole distance and put the entire tribe into one genus as recommended by Stebbins.

I liked your comments about the definition of a genus in the last paragraph on page 1 of your 30 November letter. May I quote those comments (minus the last two sentences) in a publication I am preparing? I am enclosing a figure that shows the genomic origin of western wheatgrass (or whatever you want to call it).

Sincerely,

DOUGLAS R. DEWEY
Research Geneticist

Enclosure

The New York Botanical Garden

Bronx, New York 10458 (212) 220-8700

27 December 1983

Dr. Douglas R. Dewey
U.S. Department of Agriculture
Arid Southwest Area
Crops Research Laboratory
Utah State University - UMC 63
Logan, Utah 84322

Dear Doug:

Your recent letter and Mary's about taxonomy of the Triticeae came at about the same time. The enclosed copy of my reply to her should serve also as a reply to most of your comments. I can't myself revise the generic lines in the Triticeae, but I want to have something in the next edition of the Gleason & Cronquist manual for northeastern U.S. that I can be reasonably comfortable with.

You are of course welcome to make whatever use you will of my comments about the nature of genera.

Happy New Year!

Arthur Cronquist
Senior Scientist

ac/lk
encl.

cc: Dr. Mary E. Barkworth

27 December 1983

Dr. Mary E. Barkworth
Department of Biology - UMC 45
Utah State University
Logan, Utah 84322

Dear Mary:

Thanks for your letter of 15 December, which came a little after Doug Dewey's letter of 16 December, both about taxonomy of the Triticeae.

Let me start out by saying that I make no claim to expertise in this group, and that furthermore I am not wedded to the traditional delimitation of genera. I am just a working taxonomist who has to come up with something to use in a flora, hopefully something that anticipates what will be generally accepted in the future.

The difficulties of the traditional (in North America) organization of the genera in this group have become increasingly evident over the last several decades, and Dewey's work puts the last several nails in the coffin of that arrangement. The question is, where do we go from here?

If it were not for the historical precedent and the great economic importance of wheat, I would not be averse to seeing the whole tribe as a single genus, but that is like saying, "Aside from that, Mrs. Lincoln, how did you like the play?" I think we must take it as a given that if there is any reasonable way to do so, we must have a system in which Triticum, Elymus, Hordeum, and Secale are treated as distinct genera. Taxonomy is supposed to provide a general-purpose system, which can be used by all concerned. In this instance the needs of the agronomists must be considered as of first order importance.

I believe it is possible to come up with a reasonable system that preserves the aforesaid genera, but I am not convinced that anyone has done it yet.

Pascopyrum becomes a sticking point for me, not just because it is monotypic, but because of my now rather hazy recollection of problems I had more than thirty years ago at Pullman in distinguishing "Agropyron" smithii from "Agropyron" dasystachyum. I didn't do any experimental work. My observations were purely eyeballing, but they left me with some impressions that are hard to dislodge. As best I can recall, and I think there are some of my own specimens in the herbarium at Pullman to document my observations, there appear to have been some shenanigans between Agropyron (if I may call it that for the nonce) smithii and A. dasystachyum. Some of the things called A. smithii var. molle may actually reflect hybridization (perhaps generations back) with A. dasystachyum; even the plumes are broader and not so sharp as they ought to be in A. smithii (again depending

on my thirty-year-old recollection). Furthermore, there is something fishy about the thing described as A. elmeri. Type material of this at Pullman (if I recall correctly) has scanty and irregular pollen. Maybe it isn't apomictic, but something isn't kosher. These observations and experiences are of course not definitive, but they condition my attitude toward a proposed reorganization of genera. I am not going to be happy with any treatment that puts A. smithii and A. dasystachyum in different genera, and if the logic of the system requires such a separation, then I want a different system. If the only reasonable way to put these two species into the same genus requires that they both be put into Elymus, along with a lot of other things, then that is the way I will be inclined to go unless and until someone convinces me to the contrary.

You are of course quite right, Mary, that the variation in awns in Agropyron spicatum doesn't mean that one can't use that character at a higher level somewhere else. Linnaeus put it, "Scias characterem non constituere genus, sed genus characterem". One of my Soviet friends paraphrases Orwell to say, "All characters are equal, but some are more equal than others". Even so, since taxonomy must first of all satisfy the mind, it is well to minimize rather than maximize the disparity in value assigned to a particular character within a group. The system thus becomes easier to comprehend and to persuade others to accept.

Dr. Dewey makes the point that since the Triticeae are primarily a Eurasian group, we ought to accept the genera as understood by European taxonomists. I would give some, but not overwhelming, weight to that argument. We must balance it against the fact that northern Europeans, having a small flora, are inclined to split things finer than we do in this part of the world. As regards Tsvelev, he undoubtedly knows the taxa over there better than we do, but taxonomy in the Soviet Union is at a different stage of development than here. Partly because of the Lysenko affair, they have been very slow to come out of the exploratory stage (in which the urge is to describe everything and magnify the significance of the differences) and to enter the consolidation stage (in which we reconsider what has been done and try to make sense of it). We began to come out of the exploratory stage here in the 1930's, and by 1940 (Clausen, Keck & Hiesey) the emphasis had switched to consolidation and reconsideration. So splitting goes with small floras and the latter part of the exploratory stage, and one or the other (or both) of these factors have a significant effect on our European colleagues, as seen from this distance.

Dr. Dewey is doubtless right that any perception among American taxonomists that he is associated with or influenced by Askell Love is a negative factor in the reception of his taxonomic scheme. Askell is an old friend of mine, but the world has passed him by in some respects. Although he may

deny it in theory, in practice he maintains that any difference in chromosome number is necessarily of specific importance. Other taxonomists have learned that this approach is not useful. Furthermore, Askell is an extreme splitter at the generic level, and most of us just don't take him seriously. I could give chapter and verse at some length, but I won't do so just now.

Getting back to more practical matters, I must soon face the question of generic organization in the Triticeae as applied to the species in the Gleason & Cronquist manual range. How would you do it? I don't promise to accept your organization, but for starters I want to know what it is. You might also pass on to me anything about Stipa for the manual range that I won't find in your published papers. I understand from Dick Pohl that Stipa avenacea is properly to be assigned to Piptochaetium.

Happy New Year!

Arthur Cronquist
Senior Scientist

ac/lk
cc: Dr. Douglas R. Dewey

United States Department of Agriculture
Agricultural Research Service
Northeastern Region
Beltsville Agricultural Research Center
Building 001, Room 322
Beltsville, Maryland 20705

Telephone: (301) 344-3328

February 27, 1984

rec'd 2/3/84.
ack 2/3/84

Mr. Roy Pullen
Plant Introduction Officer
CSIRO
Division of Plant Industry
P. O. Box 1600
Canberra City, ACT 2601
AUSTRALIA

Dear Roy:

We gratefully acknowledge receipt of your January 16 letter and the Agropyron samples. The material has arrived and been forwarded to Dr. Askell Love, San Jose, California. Your cooperation in providing this material is certainly appreciated.

Sincerely,

GEORGE A. WHITE
Plant Introduction Officer

cc: A. Love ✓

Dr. Love: The material was sent on February 2 under our shipping record, C-13250. Enclosed is a letter which accompanied the seed samples.

CSIRO PLANT INTRODUCTION AND QUARANTINE UNIT

Division of Plant Industry
Black Mountain, Canberra, ACT

A Division of the Institute of Biological Resources

PO Box 1600, Canberra City, ACT 2601
Telephone (062) 46 4911
Canberra PICAN Telex 62351 AUSTRALIA

C 13250

Dr Askell Löve

5780 Chandler Court
SAN JOSE
CALIF. 95123
USA

16.1.84

Dear Dr Love,

This is to let you know that Agropyron pectinatum and A.retrofractum are alive and well following an excellent spring and summer season of rains.

I enclose some samples of the dry heads that were present when collected last week in the mountains near here. The herbarium specimens will be following very soon, and I also have live material potted up for further observation. The latter are in new head,at anthesis(A.pectinatum) and immediate post anthesis (A retrofractum).

The populations were very small in extent and seemed to be exploiting disturbed sites. If they are native species I intuitively suspect relict populations from a colder climatic period. This would make sense in respect of the present occurrence of A. velutinum. Our representatives of the dicot genus Gentiana would belong to a similar time niche on the same reasoning.

An examination shows me that the heads of the A.pectinatum are without caryopses. They were dry on tufts which were in active green head, so are probably empty or male precursor heads.

I didnt investigate the A.retrofractum heads except to note that some rather poor caryopses were in nsome of the spikelets.

Hoping to put the herbarium vouchers in the mail by air during the next two weeks.

Regards and best wishes for the New Year. Thanks for your greetings via Mr Laurie Adams.

Roy Pullen

David Symon in Adelaide has A. pectinatum

PLANT INTRODUCTION / QUARANTINE OFFICER

UTAH STATE UNIVERSITY

UMC 45, LOGAN, UTAH 84322
Phone (801) 752-4100 Ext. 7771

DEPARTMENT OF BIOLOGY
COLLEGE OF SCIENCE

Feb 20, 1984

Dear Ashell:

Thank you for the reprint on Acetosella. And I thought I was looking at a lot of material! I agree with you, however, that I must start getting some cytological and hybridization data on the Stipeae. Unfortunately, my black thumbs did not make for a successful experimental garden. With some guidance and help from the USDA (Doug's group), the garden is now on its way.

I enclose the Intermountain Juncaceae reprint. The only reason it is so slow in coming was that I was going to mail you S. lemmonii at the same time.

Life has been what is sometimes called interesting. Leila filed a grievance, naming me and two others (dept. chairman & the other faculty member in plant taxonomy). I decided legal advice was essential (knowing Leila was taking it), so I contacted the lawyer I had consulted earlier in the year. Oh boy. Fur has been flying. Fortunately, my lawyer is an extraordinarily pleasant and rational person - as well as a good lawyer so far as I can tell. The kind of lawyer one wants to

have – not the kind that Warren Burger described.

Then, to add to the joys of life, it was determined that the floor on which my office and labs (& the herbarium) were located was only strong enough to support itself. So everything has had to be packed up and out – and new locations found. The herbarium is now several buildings away. I am still not completely moved out – and may never be if I can get away with it.

I regret I have not located the Rode paper. To be honest my contacts with Ottawa are no longer that great. John McNeill is no longer at DAO ~~that~~ and Bernard and I are not on good terms. A request from you has a better chance of success than one from me. I will give another try this week.

Sincerely

Thurs

AT 24/2/84

San José, December 4, 1983.

Dear Dr. Adams:

Although I long ago looked it up in Endlicher's (1838) Genera Plantarum, which I later sold, I cannot remember longer if the name Oreophylax was aimed as that of a genus or a section, though at least Kusnetzov (1895: Engler-Prantl, Pflanzenfamilien IV,2:85) seems to have believed the former to have been the case. That, however, was evidently not the opinion of Airy Shaw (1966, in Willis, Dict. 7th ed.:805), who instead felt that Kusnetzov had validated it as a generic name by indirect reference to Endlicher's name and to the description of Gentiana sect. Andicola by Grisebach (1845, in DC Prodromus 9:87), which in itself also could be a validation, cf. the Sydney Code, Article 41,2:"In order to be validly published, a name of a genus must be accompanied by (a) a description or diagnosis of the genus, or (b) by a reference (direct or indirect) to a previously and effectively published description of a genus or a subdivision of a genus". Either are, in my meaning, sufficient, and we may permit the nomenclature buffs to select between them in whatever way they want. I believe this to be a wise rule that prevents a foolish rejection of numerous established genera so validated in the past, though some of the presently required conditions for normal valid publication (e.g. condition (a)) were not strictly in order. To me, Oreophylax is a strong biological genus that may or may not be distantly related to the arctic-boreal Gentianella with which it shares a basic number 9, though their karyotypes are evidently different enough to prevent all crossings: despite efforts in the greenhouse, I have at least never succeeded in crossing them, and natural hybridization is hardly likely between plants so distantly distributed.

You may have observed that I and several American colleagues have for some decades been working on the Triticeae tribe of grasses and their genomic relationships. For years we have been trying to get some help with finding viable seeds of the critical (and perhaps tetraploid?) Agropyron (Australopyrum) pectinatum (Labillardière) and retrofractum (J. W. Vickery), but sofar without success. We have, however, thanks to the help of Hansjörg Eichler and Mr. Roy Pullen of the Plant Introduction Service of your Organization, been able to confirm the diploid number for the related A. velutinum and to add that taxon to our extensive genomic analyses program, though still none of our hybridization efforts have been successful. Some of the Australian botanists whom we have contacted have either given us the silence of the sea or declined our request on basis of the claim that these taxa are extinct, an absurd idea in light of the many localities from the mountains of N.S.W. and Tasmania listed by Joyce Vickery in her 1951 monograph. Would it be possible for you to help us find the necessary contacts to some skilled and energetic botanist working in the areas concerned this austral summer so that we may at least try to find a genetical explanation of the origin of these interesting grasses? I am not the only one who would be grateful for such a help, and I hope we also will be able to reciprocate with some similar assistance to the individual helping us, or at least to somebody else on your continent who needs such a help.

With the very best regards and all good wishes, also to Hansjörg Eichler and Roy Pullen.

Yours sincerely,

Åskell Löve.

CSIRO

DIVISION OF PLANT INDUSTRY – HERBARIUM AUSTRALIENSE

G.P.O. BOX 1600, CANBERRA CITY, A.C.T. 2601
TELEPHONE 46 4911, TELEGRAMS PLANTINDUSTRY CANBERRA, TELEX 62351

25 November 1983

Dr. A. Löve,
5780 Chandler Court,
San José,
California 95123,
U.S.A.

Dear Dr. Löve,

I note that in a recent issue of Taxon (August, p.511) you make a number of new combinations in the genus Oreophylax. I would be most grateful to know the ref. where this name was first validly used at generic level.

Yours sincerely,

L.G. Adams

Oreoglyphe, Endl. gen., p. 600, of [illegible] DC. Prodr. 9: 87 (1845)

[illegible] Australia

[illegible], N. 1895: Engler-Prantl, Die natürlichen Pflanzenfamilien IV, 2: 85; Sect. XII Australis [illegible] (= Oreoglyphe Endl.)

[illegible] 1903: Genera Siphonogamarum, p. 402, No. 6509: [illegible] 12: Australes [illegible]. Genus et [illegible] 1839: 600. Oreoglyphe Endl.

Genera (1838): 600 (sect. [illegible])

Genera plantarum part 8 (Aug. 1839)

Dr. A. Löve,
5780 Chandler Court,
San José,
California 95123,
U.S.A.

AIR MAIL
PAR AVION

COMMONWEALTH SCIENTIFIC AND INDUSTRIAL RESEARCH ORGANIZATION
IF NOT DELIVERED WITHIN 7 DAYS, RETURN TO
P.O. BOX 1600, CANBERRA CITY, A.C.T., 2601
AUSTRALIA

[illegible]

§ 41, 2: In order to be validly published, a name of a genus must be accompanied by (a) a description or diagnosis of the genus, or (b) by a reference (direct or indirect) to a previously and effectively published description or diagnosis of the genus or subdivision of a genus.

H.K. Airy-Shaw, 1966 in Willis Dict. Fl. Pl. ed. 7: 805.

Oreoglyphe (Endl.) [illegible]

CANBERRA 28.11.[illegible]

AUSTRALIA 0.75 POSTAGE PAID PB 266

U.S. DEPARTMENT OF AGRICULTURE
ANIMAL AND PLANT HEALTH INSPECTION SERVICE
PLANT PROTECTION AND QUARANTINE PROGRAMS

NOTICE TO STATE PLANT QUARANTINE OFFICIAL OF SHIPMENT OF IMPORTED PLANTS AND PLANT MATERIAL

1. COUNTRY OF ORIGIN	2. REPORTING STATION	3. REF. NO.
Australia	Beltsville	KE1436

4. NAME AND ADDRESS OF CONSIGNEE
Dr. Peter Lee
5400 Miller Ct
San Jose, Calif 95123

5. TYPE OF PLANT MATERIAL
Agropyron pectiniforme Nondorm. Seed
A. retrofractum
.018 kg

6. CONDITION ("X")
☒ Good ☐ Fair ☐ Poor ☐ Dead

7. NAME OF PEST REQUIRING QUARANTINE ACTION

8. TYPE OF TREATMENT
DICE FAM

9. ARRIVED VIA ("X")	IF REGISTERED GIVE NUMBER	IF FREIGHT ("X")	10. DATE OF ARRIVAL	11. DATE IN STATION	12. DATE INSPECTED	13. DATE RELEASED	14. DATE FORWARDED
☒ Mail ☐ Freight		☐ Air ☐ Surface ☐ Baggage ☐ Other (Specify)	1/30/87 215pm	1/30/87	2-1-87 1250	2-1-87 1300	2-1-87 1300

15. FORWARDED VIA ("X")	IF FREIGHT ("X")
☐ Mail ☐ Freight	☐ Air ☐ Surface ☐ Picked up ☒ Other (Specify) To It. R Hall

16. SIGNATURE OF OFFICER IN CHARGE OF STATION

INSPECTION STATION WORKSHEET

17. NAME OF CARRIER	18. B/L NUMBER	19. NO. OF CONTAINERS	20. NAME OF BROKER	21. CUSTOMS ENTRY NUMBER
		1		

22. PERMIT NUMBER	23. POST ENTRY ("X")	24. DOCUMENT(S) TO BE COMPLETED ("X")
	☐ Yes ☒ No	☐ PPQ 280 ☒ PPQ 220 ☐ PPQ 236 ☐ Other (Specify)

REMARKS (Indicate any insects or diseases found)

2 pkts.

FEB 01 1984 WTD

PLANT INTRODUCTION RECORD

1. PI NO.	2. DATE	3. COUNTRY	4. PQ NO.
	2/2/84		C 13250

5. MATERIAL Agropyron pectinatum-1-pkg. seed
Agropyron retrofractum-1-pkg. seed

6. PERMIT NO.

7. FROM: CSIRO-Plant Introduction and Quarantine Unit, Division of Plant Industry, P.O. Box 1600, Canberra City, AUSTRALIA.

8. TO: Dr. Askell Love, 5780 Chandler Court, San Jose, California 95123.
Ref: Roy Pullen's 1/16/84 letter to Dr. Askell Love, San Jose, California.

A. pectinatum, N.S.W. Gourock Range (SE of Cowra) No. R.P. 1078. Div. of Pl. Ind. (Cowra Nursery) 12-1-'84

A. retrofractum. N.S.W. Nimmitabel 12/1-1984

BE-8836

Roy Pullen

USDA, ARS, Plant Germplasm Quarantine Center, Bldg. 320, BARC-East, Beltsville, Maryland 20705

NER FORM 162
OCT 1977

(See reverse side for plant quarantine clearance)

PACKAGE COPY

AEROGRAMME

BY AIR MAIL . PAR AVION

CANBERRA CITY ACT
17.1.84

AUSTRALIA
0.40
PB2266
POSTAGE PAID

Dr Askell Love
5780 Chandler Court
SAN JOSE

CALIF. 95123

U.S.A.

COUNTRY OF DESTINATION

CSIRO

DIVISION OF PLANT INDUSTRY

PLANT INTRODUCTION
AND QUARANTINE UNIT

P. O. BOX 1600, CANBERRA CITY, A. C. T. 2601, AUSTRALIA, TELEPHONE 46 4911, TELEX 62351

AUSTRALIA

16th January 1984

Dr Askell Löve
5780 Chandler Court
SAN JOSE
Calif. 95123
USA

Dear Dr Löve,

Some dried heads of Agropyron pectinatum and A.retrofractum have been sent to you via the Germplasm Quarantine Center,Beltsville,Maryland. I hope these reach you O.K.

Herbarium material is following. These I will address to you direct at your above address.

All the best for the New Year,

Sincerely,

RoyPullen

PLANT INTRODUCTION / QUARANTINE OFFICER

If you wish to contact Beltsville, ask for Dr George White who is the Plant Introduction Officer.I hope they keep the heads intact for you and dont go about extracting the caryopses before you can check out the morphology.

A. pectinatum, Australia, N.S.W., Gourock Range, Stony Grown Forest Soil. RP 11076

A. retrofractum, Australia, N.S.W. Nimmitabel, Heavy basalt soil. RP 11079