UNIVERSIDAD DE SEVILLA

DEPARTAMENTO DE BIOLOGIA VEGETAL
Y ECOLOGIA

Apartado de Correos 1095
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Drs. A. & D. Löve
5780 chandler ct.
SAN JOSE
CA 95123
USA

March 9th, 1993.

Dear Doris,

Thank you very much for your nice letter, I am very sorry to know about Askell physical situation, and I fully understand he is not able to develop any scientofic work, but I can see you are still active with your translations of scientific papers.

Askell was, and still is, one of the most important cytotaxonomist we have had; his ideas are taken into consideration, and his work mentioned to my Plant Taxonomy students. Of course, his and your papers are continuously consulted and included in the literature in our papers. You both have been working hard indeed. Please give Askell my best greetings.

With very best wishes for both of you.

Yours,

Benito

Fdo. Benito Valdés.

UNIVERSITY OF COLORADO
BOULDER, COLORADO

DEPARTMENT OF BIOLOGY

Boulder, March 14, 1965.

Dear Jack:

I am so moved by the good friendship that goes through your letter which I got at the beginning of the week that it has taken me several days to find words to write to you. And still I would have preferred that you could read my own language, because my feelings cannot be put properly into any other language than the one I learned first and still and always will know best.

It did not astonish me to hear that Ralph Erickson and his committee had decided to ask Billie Turner to replace you. I am marked with the not always comfortable ability to read the eyes and between the words of not too intelligent people when they are not sincere, and so I understood when I spoke to Erickson last time that he was far from being more impressed with me than I was with him, and that he would not recommend me. This was before Allan Brown, who was sincere, told me that he would appreciate to hear from me after I had been with you at the Arboretum and returned home if I then still might be interested in letting them consider me for this position. I was sincere, as I always am (not without getting into trouble for it many times, fortunately), and wrote him that this I was, including the informations about my publications etc. I knew they did not have. When Allan a little later wrote to tell me that he had got this, adding that I were not the only one being considered (that he did not say before), then I did not need more to put two and two together. So your good letter did not astonish me at all. But I am sorry that Billie Turner did not accept this position, because I doubt that there is anybody better and more stimulating than he is in the different fields of modern botany....although I understand that he must have seen that the amiability in the department does not go very deep down and that taxonomy and what follows it continues to be stepmotherly treated in Philadelphia; he has a perfect situation in Texas in the very best botany department on the entire continent, with young and stimulating people all around him and no mediocre people descending from the meagre Småland which has fostered innumerable strong and stubborn people but only one Linnaeus. But I am somewhat astonished that your physiologists and chemical morphologists have dared to think of Billie for this position, because they stand so far below him that he would have had much less difficulties in overshadowing them than I would do myself. And nevertheless also have been as close to you.

I must say that I am pleased and proud of hearing that you feel I might be able to step into your shoes at the Arboretum, although I myself have many doubts about my own abilities in these fields, because of much too limited knowledge. However, it would make considerable difference to know that you would be working at my side as long as you have health to this, and I would do my best to see to that your facilities would improve rather than impair; it is my opinion that when one becomes emeritus one ought to get everything first-class, so such a cooperation would, if one look closer on it, be just an indication of how I would like to be treated myself....in addition to the fact that it takes a whole lifetime to gather all the great experience you can give to the Arboretum and its people now and later, and that our generation must do what it can to get the last one to write down all it knows, for us and others. Selfish, but....

I told Allan in December that you had done so much good for the Arboretum that it would be the perfect place for many kinds of scientific work, and I am convinced that it could become an outstanding place for all kinds of studies in biosystematics, including chemotaxonomy which Billie Turner has done so much to build up in Texas. It could also be the perfect place for one of my pet ideas, the Flora Boreali Americana project, which must come soon, and if the taxonomic and geographic part of the University Library, or perhaps the botanical part of the Academy could be moved to the Arboretum when it gets its new building, then there would be no other place in America better for certain very much needed service works in biosystematics we have been working to establish. Still, the most attracting thing in the Arboretum is the one getting this letter, you do not need to blush for the fact that you have spent most of your time to make it fine and to pull it up from the rather dull condition in which it was when you took over its directorship, all botanists who want to see this know it, and several have even told me that, this winter and earlier....but nobody tells you or thanks you for what you have done and are doing.

It was implicit in my letter to Allan that I would be available for this post if offered to me, I am used to be sincere and mean what I say. If they had acted fast and not left the idea to look for something better, then we would have been able to come this spring. However, things are changing now, because I have already accepted one of two NSF grants for our work here, am getting equipment from the University, and have accepted four graduate students and at least one and perhaps two postdoctorates, so I feel somewhat more bound at this time. In addition, Doris has been given a position here, paid at one-third by the NSF, so even she feels tied down. However, all such matters can easily be arranged if one only wants to do it....although I must admit that Doris is so fond of the climate and people and academic life at Boulder that it may be difficult to convince her that something in the east could be better. She has never seen Philadelphia properly, only gone through it once in the early spring in not too nice weather, never seen its culture or met its friendly people, never seen the Arboretum and never met you and Helen. But I do not think she would object much if she sees a really advantageous prospect and learns to know the place and people involved. Because the fact that half the faculty here will be on leave next winter, I would have some conscience to tell them we would also leave, but even that could probably be arranged in some way or another, perhaps even so that I would not leave them for good until everything could be arranged. But another point may interfere with any change during the summer and fall: we are going to try to complete a good deal of our collecting work for next winter during the early summer months, because in July we are going to Iceland where Lóa will marry her Gunner, who now is studying Fine Arts at the Academy in Copenhagen, and she hopes to be allowed to do the same next year. We will follow them to Denmark and there take part in the Flora Europaea Symposium, but return home alone early enough to make it possible for Doris to give an invited paper at the INQUA Congress here in Boulder at the end of August. But despite of this all things could be arranged, and if Doris should find out that Gunner could perhaps find some museum work or teaching work in Philadelphia when he is ready with his studies, then this could easily soften her heart and get her to become as positive as I am. But it is not necessary to cross the bridge before one comes to it, and even your optimism and enthusiasm may not be able to move the powers to be into the direction you want.

I am sure they never have got anybody from the outside to give any judgement on me or my abilities. Billie Turner is one who could do this, he is responsible for our being in Boulder thanks to an unusually positive recommendation, although he then told me this might be a good place only until something better turned up. Another who could help you move them is Harlan Lewis, and old friend, and certainly also Karl Sax. I am also thinking of Dr. Frans A. Stafleu of the IAPT, a good friend of ours, who will be coming to the States and travelling somewhat to lecture at some seminars, even here in Boulder, in April and May. He will probably be much at Pittsburgh, as usually, but I am sure he would gladly come to Philadelphia for a seminar, if he only were asked to do this. If you could then get him prepared before the others could speak to him, he would be the very best help you ever could find in these matters, since nobody in the entire world did as much to help us when we needed it last year. If you could get him invited for a seminar, you could easily invite him to live with you so that you could get properly acquainted, and I am sure you would find him to be among the very most interesting people you have met - and reciprocally.

I forgot, naturally, to mention in connection with Doris' reaction towards another big city that she has become thoroughly tired of all the discrimination against learned women that dominates in the so-called academic world in America where employing wifes without qualification is compared with those very few women, who happen to be married but have the highest academic qualifications. She knows very well that she is superior to many of the men (here she has published more, over 70 papers, than the oldest man on staff!, and they do not have a Ph.D. and D.Sc. from one of the best universities in the world), but says she has become tired of moving and loves nice climate and has found so many good friends at Boulder already, etc., etc. But I am sure she can be convinced by friendly and nice people even elsewhere and allowed to work without discrimination by aid of the NSF and other funds not only here. I know she is considerably better than I am for a position like yours, whereas I dare to believe I may be as good for certain scientific work because of my other kind of mind!

I am sure you read much more between the lines than I write directly, and when you have done this, you ought to do what you feel is best and most promising for the Arboretum, never think about us and our feelings which you cannot and will not hurt whatever you do. And whatever comes out of your friendly efforts, I will forever be grateful for the fact that this all got me to find two of the very few sincere souls of the kind I appreciate - it is only a pity that this did not happen decades ago and that we still have almost a whole continent between us. I know you will not mind kissing Helen from us both and hope you feel how strongly I would like you to feel my gratefulness for your good friendship. Even a letter in my own language would not have been able to show my sincerety and admiration in a proper way. And so I cannot select anything better to end this letter than the good old Latin

Semper tuus,

UNIVERSITY of PENNSYLVANIA

THE MORRIS ARBORETUM
9414 MEADOWBROOK AVENUE
PHILADELPHIA 18

JOHN M. FOGG, JR., Director

March 2, 1965

Dr. Askell Love
Dept. of Biology
University of Colorado
Boulder, COLORADO

Dear Askell:

You simply can not possibly know how many times I have been on the point of writing to reply to your wonderfully friendly letter of December 8. On each occasion I have refrained for reasons which I hope will be evident to you with your deep understanding of human beings and the forces which motivate their thinking and actions.

I believe you know that I am not a member of the Committee which has been appointed to choose my successor. I had supposed that I would be consulted in this matter but such has not proved to be the case.

Some weeks after you were here Billie Turner, of Texas was invited to give a seminar. Since it had to be scheduled at an eleven A.M. hour only a very small handfull of persons attended. Nevertheless, a few days later, at a meeting of the full staff, his name was proposed as a candidate for a professorship in Botany and Directorship of the Morris Arboretum. I had no knowledge whatever of this in advance of the meeting and registered a violent protest with Ralph Erickson (Chairman of the Committee) at what I considered to be an unethical procedure.

Turner was offered the position but, as I suppose everyone now knows, turned it down in order to remain in Texas.

As to the next move, you know quite as much as I do. I had lunch with Erickson a week or two ago and told him (which I would have done earlier, had he asked me) that I thought you would be a superb person in this job. He asked when I thought you might be available and I answered that I did not have the least idea what your present commitments were at Boulder. It would be a help to me if you would care to comment on this matter. I am, as you know, willing to continue as Director as long as necessary, until a

March 2, 1965

successor can be found, but I have many matters claiming my attention to which I would be delighted to devote a fuller proportion of my time.

I am sure you realize why I have been reluctant to write to you and even now I have the feeling that I should be keeping entirely silent and awaiting developments. There comes a point, however, in the interests of friendship, when one must say what is on his mind and I know that you will accept my remarks in the spirit in which they are offered.

We speak so often, Helen and I, of your visit with us and look forward to the time when we can welcome both of you to our home.

Sincerely yours,

Jack

John M. Fogg, Jr.,
Director

JMF:am

He loves his country best who
strives to make it best.

Americana Notes

150G9042
MADE IN U.S.A.

Dear Doris + Askell,

Thanks for your card and wishes. No cards were sent this past season. Last year was a bad one. Herb was in the hospital in March, April and May. He never came home from the last admission and died on August 20th. It was a second bypass surgery that got him down. The doctors were misleading, inaccurate and inefficient.

Then in Nov. + Dec. I was diagnosed with a 95% clogging of the left carotid artery. Wound up with an endarterectomy on Dec 14th. I am much improved.

Between trying to wind up the highly exorbitant medical bills for Herb + assuming the overlapping ones for me, my time has been extremely tied up. Thank goodness my training has enabled me to assume + execute all this responsibility.

This estate business profits no one but the lawyers. And it is time-consuming.

I think of you and Askell as I always have with genuine affection. You two are honest and real and I appreciate your qualities. Very few possess them.

My regards and respect to you both. I wish you well.

Love

Goldie

Aven Nelson

Cronquist ... 38418

15 November 1984

Ans 27/11 84

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

Dear Dr. Löve:

Enclosed is yet another list of counts for the IOPB Chromosome Reports, along with a return envelope for acknowledgement. These are perhaps less interesting than the last list, as we saved our ca. counts and some of the more common species for this list. There are probably some you will want to delete. The inexact counts were exhaustive efforts on material that simply would not yield good preparations, though the material and our techniques were the best we could ask for. We include only those for which only one interpretation was possible from the figures we were able to obtain; more ambiguous attempts were omitted entirely.

Regarding our last list, I have finally found the older name for _Penstemon bridgesii_: it is _P. rostriflorus_ Kellogg. If it is not too late, we would appreciate your making the appropriate change; otherwise, _bridgesii_ will be acceptable.

I appreciate the letters I received from you last summer, and wish I had been able to make some sort of response before now. Apparently, you have been under the impression I am a Ph.D. I am not, though if Claremont Graduate School/RSA be willing, this should change before too many more years. These chromosome counts are accumulating primarily from ongoing floristic work, Dr. Clark Schaack's in Arizona, and my own in Arizona and in the White Mountains of California/Nevada. The third member of our "group", Michael Windham, has moved on to the University of Kansas, and is fast becoming a remarkable pteridologist. Clark and I also have biosystematic problems in various stages of completion, but the associated cytological results are, of course, being reserved for separate publication. We are thus a very small, but (from what I gather) unusually serious group of plant systematists who labor out of love, and whose education and experience in general far surpasses the standard academics. Unfortunately, we are tucked away in a department and an institution that has no appreciation for what a plant systematist does. We do the best we can.

As for myself, I presently exist in that rather broad gray area between being an amateur or a professional. I feel quite fortunate, during these "formative years," to correspond with people such as yourself, who are considered to be in the forefront of taxonomic and phylogenetic philosophy. I place both you and Dr. Cronquist among that group, though your respective positions differ greatly. I am presently in agreement with what I believe to be your basic position, that subspecies and varietas are two different (though perhaps overlapping) biologic phenomena, based on degrees of genetic divergence within an (at least potentially) interbreeding species. The two phenomena, though I don't fully understand them yet, seem intuitively obvious, both in the field and in the herbarium, and I have run across few instances where I could not distinguish between the two. But I certainly think it is time that these phenomena were better defined, so that we can begin to counteract the dogma that a variety and a subspecies represent the same thing. I am also beginning to believe that the concept of a genus goes beyond the mere "taxonomic convenience" that Cronquist would propose, to an underlying biologic reality that cladistics is only beginning to define (and imperfectly, at that). I know that studies of the folk-taxonomies of primitive peoples have revealed widely differing "generic" concepts amidst a remarkable corellation of species concepts. To me, this only indicates the imperfect state of our knowledge, and the fact that a species is a genetic phenomenon (and therefore readily visible), while the genus is a phylogenetic phenomenon (much less visible to us mortals). Everytime I go into the field, I see genera as well as species, which tells me there is something more than a taxonomic convenience there, even though the genera I think I see may not be the ones that really exist. If nothing else, then, I am at least optimistic that there is plenty of pioneering work left to be done in my chosen field.

On the other hand, I must agree with Cronquist that a practical taxonomy must be based upon readily observed data, with the more cryptic information being used as evidence to support or reject different macroscopic interpretations. And we are beginning to discover that clear-cut genetic diversity often corresponds with a very ill-defined morphological diversity, as in *Gilia* sect. Arachnion or the *Mentzelia albicaulis* group. As a result, while I recognize the hard work and good genetic sense that went into the naming of all the "microspecies" in these two groups (and others I am not mentioning), when it comes to placing names on herbarium specimens, or even live ones, I tend to be sympathetic with people like Cronquist who submerge all the microspecies under the oldest valid name. Perhaps this is a laziness I will grow out of, but I still see a basic conflict between human practicality and natural reality which may, in the end, be unresolvable. Or, perhaps we simply need a different taxonomic system for application in rapidly evolving groups such as those above.

Indeed, then, it is past time for a review of the Southwestern Flora with reference to new genetic and phylogenetic evidence. Unfortunately, we could not possibly muster the resources for such a project here. I fear such an effort must finally come from one of the Californian or Eastern institutions (as did Kearney & Peebles' original Arizona Flora), and genetic evidence often does not do well in those places. But we at least need some sort of solid modern work on which to base taxonomic improvements. (Martin & Hutchens' 1980 New Mexico Flora is nearly worthless, in my opinion.). The Intermountain Flora is presently providing such a work for that region, but even more interesting to me is the portion of Nevada to the immediate south. Thanks to the work of Mary DeDecker, the adjacent portion of Eastern California is now quite well known. Together with S. Nevada, this area is probably the most diverse for its size in North America, yet it has been relentlessly excluded from all the modern floras. The flora of my own White Mountains has reached nearly a thousand taxa, and it is perhaps one of the least isolated of those desert ranges.

This summer's work saw the addition of nearly 80 of those taxa, some of which may be undescribed. But I could not help taking out much time, as you suggested, to rest and reflect upon my surroundings - especially when the summer rains finally arrived. The Sierra Nevada may be spectacular, but the desert ranges to the east are beautiful in a much different way, to me. In all it was a most relaxing summer, and I too wish you could have been there - perhaps we could arrange to meet someday.

I sympathize with your remarks on the lack of classical and philosophical education on this side of the Atlantic these days. My first two years of college were spent at Deep Springs College, a little-known private institution about 30 miles East of Big Pine just inside Eastern California, at the South end of the White Mtns. (thus, my local interest!). It is isolated in a valley by the same name, and has been there since 1917. There are 24 students at any one time, and the emphasis is on a rigorous, classical and philosophical curriculum. The students and 7 faculty together essentially run the entire program, as well as the working cattle ranch on which the college is situated. The basic idea is that only on a solid foundation of classical education, coupled with hard physical labor and the responsibilities of self-government, is it then safe to build a more specialized knowledge. Although the opportunities there were more than anyone could possibly take advantage in a two-year program, I came away with much more than most of that age: a solid foundation of inestimable value to me. I only wish such programs were more prevalent; I feel most fortunate for having had that opportunity. Though I will probably be called a scientist in the years to come, I will not feel the limitations that term often connotes.

I am curious about that written reference you mention, in which Cronquist disavows evolution in plants - would you tell me where to find it? Again, thank you for sharing some of your thoughts over this past summer -- I hope to hear more in the future. The chromosome lists will continue to come for the forseeable future, and there should be some interesting counts from this summer's work.

Best Regards,

Jim Morefield

James D. Morefield

NAU Box 6201, Northern Arizona University, Flagstaff, AZ 86011

UNIVERSITY OF COLORADO, BOULDER

Museum

3 Oct. 1985

Askell and Doris Love
5780 Chandler Court
San Jose CA 95133

Dear Askell & Doris:

I thought I had sent you the entire story on Lellinger's return to Athyrium alpestre. In case I did not, here it is. I am afraid I cannot reject an argument simply because I don't know the person or dislike things American. I can't see any hole in Lellinger's argument. Perhaps you can and will tell me what it is.

"Fuchs (1974) adopted the specific epithet distentifolium Tausch ex Opiz. rather than alpestre (Hoppe) de Claireville. He claimed that de Claireville (1813, p. 301) published a new species, rather than a transfer of Hoppe's name, which was not cited. Fuchs also thought that Aspidium alpestre Hoppe could be considered a superfluous name because Hoppe thought it might be the same as Athyrium rhaeticum Roth. However, this is incorrect, for a superfluous name requires that the author include the type of the name being replaced, and this Hoppe did not do. According to R. M. Tryon, Jr. (pers. comm.), de Claireville's work is an excursion flora in which the introduction states that it is not a technical work. De Claireville's intent was not to name a new species, but to transfer Hoppe's name. Therefore, it seems clear that the epithet alpestre (Hoppe) de Claireville should be reinstated."

More later.

B.

Polypodium r. L. 7899

de Claireville 1811, Manuel d'herborisation en Suisse et en Valais, etc.

TL 2, Vol. 1: 505

Áskell Löve, Oct. 12, 1985: On the name of the Mountain Lady-fern.

1. Linnaeus (1753) included the two species of boreal _Athyrium_ in his wide _Polypodium_, as _P. filix-femina_ L. of the northlands and _P. rhaeticum_ L. of the Alps _P. rhaeticum_ was said to "habitat in Gallia, Helvetia", i.e. in the Alps; by including in it both the "Filix rhaetica tenuissime denticulata" of Bauhin, Hist. 3, which is the species long accepted as _Athyrium alpestre_ (Hoppe) Milde, and the "Filicula fontana major s. Adiantum album, filicis folio" of Bauhin, Pinax [which is _A. filix-femina_ (L.) Roth], The Linnaean _P. rhaeticum_ was a nomen confusum that soon was to be discarded.
2. Roth(1799) erected the genus _Athyrium_, and transferred to it the type species _A. filix-femina_ (L.) Roth. A year later, (1800), he also transferred the Alpique taxon as _A. rhaeticum_ (L.) Roth.

3. Hoppe (1805) described _Aspidium alpestre_ to replace the Alpique Linnaean name, which is mentioned as a synonym, thus rendering the new name as an invalid "nomen superfluum", as pointed out by Fuchs (1956; his 1974 paper referred to by Lellinger is only a translation of some kind of a summary made for Englishspeaking authors that had shown a negative attitude to the thorough paper of 1956...because of language difficulties, he surmised). Despite the fact that the name _alpestre_ was superfluous when published, this was not observed until by Fuchs (1956) and it was correctly transferred to _Athyrium_ as _A. alpestre_ (Hoppe) Rylands in Moore (1857), and again in Milde (1867); though the latter combination was superfluous and thus illegitimate, the most frequent author combination used for more than a century has been that of (Hoppe) Milde.

4. de Clairville (1811) [note the spelling of the name and the year 1811 rather than 1813, cf. Stafleu & Cowan (1976), Taxonomic Literature Vol. 1: 505] in his perfectly acceptable Manuel d'herborisation en Suisse et en Valais [cf. Code Article 29 as an explanation why the remarks by Tryon are completly nonsensical] described the Swiss material as the new species _Athyrium alpestre_, _with no reference to Hoppe or even Linnaeus_, thus making this name only a new and stillborn nomen superfluum of the Linnaean Alpique complex.

5. Tausch ex Opiz (1820) described the species in question unequivocally as _Athyrium distentifolium_ Tausch. The type specimen is still available and was verified by Fuchs (in Janchen, 1956, Catalgus Florae Austriae I:71, footnote).

6. The reasoning above, which is based on that by Fuchs (1956), and his conclusion that the oldest and only legitimate name of this taxon is _A. distentifolium_ Tausch ex Opiz (1820), has been accepted as correct and logical by all critical German-reading European botanists, most recently by Rothmaler (1963 and by the highly critical Jermy in Flora Europaea (1964), as well as by Lid (1974), and Czerepanov (1973, 1981). Therefore, I urge you to acknowledge that there is no reason even to think of the discussion by Lellinger (and Tryon?), that is most appropriately left unmentioned.

7. As a conclusion, I repeat that the name of the Mountain Lady-fern ought to be: _Athyrium distentifolium_ Tausch ex Opiz, with the essential synonym _A. alpestre_ (Hoppe) Rylands, non Clairv...as in Flora Europaea I:18.

No, I do not dislike American botanists because of their origin, though many of you think so about us for that reason...though not you, naturally...but I cannot like socalled scientists who know too little even in the languages they prefer to refer to, and show a total lack of training in the logic that is essential to all scientific advances. I do not understand how you could differ so much from them.

UNIVERSITY OF COLORADO, BOULDER

Museum

17 Oct. 1985

Askell and Doris Love
5780 Chandler Court
San Jose CA 95133

Dear Askell:

Your letter was one hundred per cent the old Askell, and that is why I miss you so much. You have all of this stuff in your head but I have to sometimes wring it out of you! Of course du hast recht.

There is another small matter and that is that I notice that Lellinger has resurrected Asplenium trichomanes-ramosum L., as an older name for A. viride Huds. His argument is that if you accept other compound epithets in Asplenium you should acccept this one as well as A. trichomanes-dentatum.

I think that there may be a chance for me to come and see you in January. I have an invitation to go to Santa Cruz Island off Santa Barbara and after that is finished I could come up around January 11 for a few days. What do you think? I hope that the manuscript of the book will be finished by then and we may have some chance to look it over once more.

As ever,

Bill

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

JENNIFER LUMSDEN
109 MAIN STREET S.
NEWMARKET, ONTARIO
L3Y 3Y8

(416) 895-6884

Dear Askell: Mar 9/87 Ans 21/3

Many thanks for your long and interesting letter of Nov 18/86 which arrived while I was in Denmark. (You write a good letter.) It was a rather long but productive trip which, among other things, gave me a firmer knowledge of the Danish language.

Yes, Sten is the youngest of the 4 Porsild children. He was born in Greenland and was delivered by my Greenlandic great-grandmother. Normally Johanna Porsild would have gone to Denmark to have her baby but she realized her condition shortly after the last boat of the season had departed. Bob (Thorbjorn) was born 1899, Erling in 1901 and Tulle (Asta) in 1903. Morten was very ambitious for his sons, especially the eldest. Bob was slated for a career as a marine biologist but he was no academic and 'flunked out' of his first year at København University. I think Erling resented the way Bob wasted this opportunity, for Erling would have dearly loved to have gone to University. However, family finances would not allow for this.

Erling's first 'wife' was a greenlandic nurse and midwife who bore my mother in June of 1923. From all reports, they were very much in love but she refused to marry him because she realized that he had no future in Greenland. Reluctantly, Erling left Greenland in 1925 without her. The plan was that he would establish himself in North America and then send for her and their child. However, she contracted typhoid and died in the fall of 1925. My mother spent the following 7 years with Erling's parents at the Danish Arctic Research Station. In the beginning, she spoke only Greenlandic but Sten was there to help and they became quite close.

My mother is the only (known) child of Erling. In 1929, he married Asta Köfed-Hansen, a Danish nurse whose father was an Admiral in the Danish navy. In 1930, Erling, Asta and Edith (my mother who later changed her name to Karin) went up to the mouth of the Mackenzie River to establish the Reindeer Station with the help of 3 families of Laplanders. That isolated and wind beaten country was no place for an upper class city girl. Erling was devastated when she left 3 years later. She took Edith to a boarding school in southern Ontario and returned to Denmark.

In the spring of 1948, Erling remarried. Elizabeth was a very proper Englishwoman who had had 2 previous and tragic marriages. Her first husband, the father of her child, was an RAF flight officer killed before Toni was born. She remarried a Canadian officer who was also killed. His family brought her and the child to Ottawa wehre she got a job in the mapping section of the Geological Survey. Erling met Toni first and became enchanted by this sweet little girl. My mother says it was not a good marriage and it ended in 1955 with Elizabeth's death. As you know, Erling was in Europe at the time.

You are correct that Margrit, the Austrian, is "of a different kind from Erling". I could never understand what he saw in her, but I do know that she made him very happy.

Your Taxon review of Erling's Rocky Mountain Wildflowers was very interesting and I am very happy to have it. Thank-you! I have made photocopies of the preface to your Cytotaxonomical Atlas Vol 2 which you so kindly sent. I found the section "Plants of the Arctic" to be the most useful.

Just before Sten left in October, we sat down to discuss the most important topics to be included in the book. A funny thing happened after he left. I discovered an envelope and inside was a list, in Erling's handwriting, of "Material to be preserved for Autobiography". This list coincides very closely with the list Sten and I compiled.

Erling's list

1)Correspondence with Morten P. Porsild 1925-56
2)diary of summer 1921 (Grøn. trip with A.C. Seward)
3)papers related to seperation and divorce - Asta
4)papers and plans for colonization of Scoresby Sound
  (from what I gather, this was Morten's idea stolen by Mikkelsen)
5)Autobiographical notes of Morten
6)Morten's plans and Correspondence related to the establishment of DARS
7)Erling's establishment of Reindeer Station
8)AEP as Canadian Consul to Greenland during the war

I was very interested to hear Johannes Grøntved's story about how MPP read his newspapers while in Greenland. I vaguely recall hearing the same story many years ago. Do you know if Grøntved was a member of the 5th Thule Expedition? x/

I should very much like to come to San José to meet you but unfortunately the cost is an unsurmountable obstacle. However, this may change if I am sucessful in getting a Canada Council grant.

Sincerely
Jennifer

x/ No. But he reported on it in 1936 on basis of the collection in ~~K~~ Copenhagen.

Royal Ontario Museum
Musée royal de l'Ontario

100 Queen's Park
Toronto, Ontario
Canada M5S 2C6

ROM

Office of the Director
Bureau du directeur

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26 November 1991

Dr. Áskell Löve
5780 Chandler Court
St. José, CA 95123
U.S.A.

Dear Áskell:

Belated congratulations on your 75th birthday. I discovered quite by chance that you celebrated this just over a month ago and I am sorry not to have taken note of the occasion more timeously.

As you will see from the letterhead, I am now back in Canada. My giving up the Regius Keepership at the Royal Botanic Garden, Edinburgh in 1989 after only two years directing the Botanic Garden was not due to any dissatisfaction with the position itself or failure to readapt to life in Scotland – in fact, I enjoyed it all enormously – but, as you may have read in *Taxon,* because of the U.K. government's policy of mandatory retirement of senior staff in the public sector at age 60. I am not quite there yet but it was looming on the horizon and I did not like the idea.

At any rate, I transferred here as Associate Director in charge of collections and research in November, 1989 and took over as Acting Director some 10 months later and, after the usual international search, was confirmed as Director in March of this year. The ROM acquired the University of Toronto's vascular plant herbarium (TRT) some 10 years ago and we are committed to taking over the cryptogamic herbarium (TRTC) at sometime in the future – when we have space. We have two botany curators. One whom you may remember is Jock McAndrews who has been with us for many years and who works on Quaternary ecology, and the other, a more recent appointment, Dr. Tim Dickinson, who works on the biosystematics of *Crataegus* and other genera.

Most of my own time is, I am afraid, taken up with administrative matters, having given up my biosystematics lab when I left the University of Ottawa in 1987. I keep my hand in as nomenclatural advisor to the Flora of North America project now going actively ahead from its centre at the Missouri Botanical Garden and in other matters relating to the *International Code of Botanical Nomenclature* and the governance of IAPT. In this context, you may have heard that Frans Stafleu was hospitalized recently, but has got back to work actively on a new edition of TL-2, even although the long term prognostications are uncertain.

So much for news. A 75th birthday is a time for congratulation and celebration. Let me take this opportunity to put on record my personal appreciation of your lifetime's outstanding contributions to plant biosystematics. There is visible evidence of this in my research office upstairs where the capacious reprint boxes are labelled "A-Am", "An-Az", etc. but there are three whole ones that just say "Löve". It is true that, in the breadth of your work, some conclusions were reached perhaps too hastily that later data proved wrong, and I, like others, have complained about not being able to find some of your voucher specimens, but all the same the stimulus that you provided for research, particularly on the use of chromosome data in systematic studies, was immeasurable. This was particularly true in Canada, where today, with our active work in plant biosystematics, it is easy to forget the pioneer work that you accomplished in your all-too-brief stays in Winnipeg and Montreal.

I was made acutely aware of this contribution by the support that I received when I was looking at the possibility of a position for you at Agriculture Canada after you left Boulder. Alas, the fact that you had not acquired Canadian citizenship during your time in Canada dashed any prospect of that.

I heard that you have, alas, had to retire from active botanical work and that is a great loss. Nevertheless, I wish you all the very best in your second retirement. We only met on a few occasions but on each I remember your spirited and sparkling conversation and your keen determination. These are qualities from which the world has benefitted, and has thereby been made richer. Thank you.

Yours sincerely,

John McNeill

John McNeill
Director

Royal Ontario Museum
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ROM

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26 November 1991

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

Dear Doris:

Mary Barkworth sent me a copy of your recent letter to her and I was very sorry indeed to read that Áskell's health had declined in the way it has. It seems only the other day when I was getting letters from him seeking help with references relating to his conspectus of the *Triticeae*. His encyclopedic knowledge was phenomenal and for all that I have criticized some of his work, and, if I ever get back into Botany, may have occasion to do so again, does not in the least detract from my admiration of him as an intellect, as a person and as a stimulator of others.

Hearing that he was disposing of his library and reprints, makes me realize that he has indeed come to the end of his botanical work. As you say, Koeltz will sell things at inflated prices. Whichever university gets his reprints will have a marvellous treasure. Alas, all that we could offer you would be a U.S. tax receipt and that probably does not do very much for you.

I was glad to hear that you are still enjoying your translation work. I am not taking away anything from the letter that I wrote to Áskell when I say that the "& D. Löve" was a major factor in the success of "A. Löve".

With best regards.

Yours sincerely,

John McNeill

John McNeill
Director

BOTANISK HAVE, KØBENHAVNS UNIVERSITET
Motiv fra [illegible]

The First International Conference on the Triticeae is now history - and was evident that you have had a very profound effect on thinking about the tribe. One workshop was devoted to the question of genomic classification - & it was brought out that you considered the genome the best reflection of genetic relationship - + that was why it was important. ~~Not~~ everyone agrees with your ideas - ~~but~~ at least not totally BUT everyone does consider ~~them~~ worth serious consideration + all would agree you have helped us see the tribe more clearly - a big step forward from 40 yrs ago - + an amazing step given how [illegible] taxonomic [illegible].

My best wishes to you and Doris [illegible] Many [illegible]

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Dr A. Löve
5780 Chandler Court
San José CA
USA

San José, May 10, 1985.

Editorial Office of the Index Kewensis,
Royal Botanical Gardens,
Kew, Richmond, Surrey, England.

Dear colleagues:

I would greatly appreciate it if you could inform me about genera that may have been named in honor of Dr. Aleksandre Melderis (and the correct spelling of his first name) since I am considering to name for him a new genus of grasses that he has worked with in the past. Also, if available, an uptodate brochure on the Index Kewensis, of which I lack all volumes after the 14th supplement, as well as prices of these and the entire collection, if easily available to you.

Yours sincerely,

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

San José, May 10, 1985.

Dr. Richard R.-C. Wang,
Crops Research Laboratory,
Utah State University - UMC-63,
Logan, Utah 84322.

Dear Dick:

I have heard nothing from you since I wrote you in early February, but hope that I did not offedd you with my remarks, which were meant well. I hope you continued with the paper, with or without notice of my remarks or proposals, since it is essential that the world of Triticeae workers learn about your skillful studies of the relationship of the E and J haplomes. I would appreciate to hear about its progress and to receive a reprint when it has been published, since I have been digging up my unpublished data for reevaluation in light of your results; I believe that may result in a somewhat more exact genomic definition and limitation of <u>Elytrigia</u> and the splitting from it of two west-Asiatic and South and west European complex-genome genera that I hope will solve the confusion Doug could have avoided if he had reacted when I sent him my original manuscript eons ago...but he then had no time, of course. Perhaps we will always continue to have unsolved problems so that the time never will come when our nepotes or grandchildren can decide if our solutions were reasonable or not, as Linnaeus hoped for in his preface to the much more controversial Species Plantarum two centuries ago?

With the very best regards and all good wishes, also to Doug and Mary,

Sincerely,

P.S.: Doug mentioned that he might go to China again this summer, hope you will join him. In that case he may be interested in that a suspicion that I had about some confusion in the use of Roegneria by our Chinese colleagues that I accepted uncritically when transfering their names, may be solved by the mentioning by a colleague in Peiping of the misuse of that name for taxa that belong to Pseudroegneria? That would explain why Doug mentioned that somebody in China had told him that there was a diploid Elymus there.... the area of Pseudoroegneria clearly passes over China on its way from Central Asia to North America.

San José, February 8, 1985.

Dear Dick:

Sorry to have let you wait, but I wanted to try at least to advice you reasonably as to the improvement of your already good paper, and that took more days than I expected. I enclose the results in the hope that you will find my suggestions to be acceptable, though you may want to change the wording here and there. But not the logic, hopefully, since I am in no doubt that it is closer to perfection than yours was, when you tried hard to favor an oppinion that was originally based on misunderstanding by a fine colleague with more limited training both in general and evolutionary logic than one who started his studies in philosophy of logic and metaphysics long ago! You may find some of my remarks unacceptable, of course, and perhaps you feel strongly about keeping some of your remarks on the usefulness of your work that I believe are of less importance and sometimes perhaps naive, in the good meaning of that word of course. But do as you find wisest.

Otherwise nothing more, since I hope the enclosed pages are selfexplanatory.

With the very best regards to Doug Dewey and Mary Barkworth. They and you would be astonished to see all the hundreds of requests for reprints of the thick Feddes Repertorium paper that still keep coming from almost all the corners of the globe...though I understand some who evidently think that a reprint I pay for and mail is less expensive and more handy than if they themselves xerox or copy a 96 page paper. And since most of those asking have never been connected with work of this kind and those from America at least frequently are graduate students who may be forming their fields of interest, I hope I do not discourage them when I only write them a letter recommending that they xerox the paper in their University library?

Thanks for the reprints...and hope you remember me when you send out new products in the future since what you do evidently interests me. And do not hesitate to ask me for help whenever you think I could give this to you, by telephone or letter or both. One such recommendation that I came to think of when observing that you have similar difficulties with English as I always have, though your original language is not the source of 60 percent of the English language, as mine is, concerns a lexicon that I found late but enjoy no less than others in my family, not least my granddaughter in our house: It is a lexicon of English words that we need when writing to avoid repeating the same simple word that foreigners tend to know when those brought up with the language have ten or twenty at least:

Roget's International Thesaurus, revised by Robert L. Chapman.
Thomaz Y. Crowell Company, New York, 1317 pages.

All the best,

GENOME ANALYSES OF *THINOPYRUM BESSARABICUM*, *T. ELONGATUM*, AND THEIR F_1 HYBRIDS

Richard R.-C. Wang

United States Department of Agriculture
Agricultural Research Service
Crops Research Laboratory
Utah State University - UMC 63
Logan, UT, U.S.A. 84322

Received ______________.

Key words: Genome analysis, mitosis, meiosis, karyotype, karyogram, hybrid.

____

Cooperative investigation of the USDA-ARS and the Utah Agricultural Experiment Station, Logan, Utah 84322.

Approved as Journal Paper No. _________.

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ABSTRACT

Thinopyrum bessarabicum (2n=14;JJ) was successfully crossed with *T. elongatum* (2n=14;EE) but the reciprocal cross failed. Five of the nineteen F_1 plants headed in a greenhouse without being vernalized. Spikes of F_1 hybrids were intermediate to those of the parents for number of florets per spike, glume length, and the first rachis internode length, but similar to those of *T. bessarabicum* and *T. elongatum* for spike length and number of spikelet per spike, respectively. Karyotypes of mitotic chromosomes in the parental species revealed that three of the seven chromosomes in J and E genomes were similar in length and arm ratio. Meiosis in F_1 hybrids substantiated the conclusion from karyotype analysis that the other four chromosomes had undergone considerable structural rearrangements such as reciprocal translocation. Metaphase-I cells in hybrid plants averaged 2.68^{I}, 4.68^{II}, 0.27^{III}, 0.27^{IV}, and 0.01^{V}. Although 10% of pollen grains were stainable with I_2-KI, F_1 plants of *T. bessarabicum* X *T. elongatum* did not set seed~~s~~ upon selfing. It is concluded that J and E genomes are closely related though clearly distinct. The impacts of the findings in this study on taxonomy and breeding are discussed.

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INTRODUCTION

Genes in the genus Thinopyrum, which includes the diploid T. bessarabicum (Savul. and Rayss) Löve [=Agropyron bessarabicum Savul. and Rayss and Elytrigia bessarabica (Savul. and Rayss) Dubovik] having the J genome and the diploid T. elongatum (Host) D. R. Dewey [=A. elongatum (Host) Beauvois, E. elongata (Host) Nevski, and Lophopyrum elongatum Löve] carrying the E genome (Dewey, 1984), have been transferred into wheats (Triticum species) via the hexaploid T. intermedium (Host) Barkworth and D. R. Dewey and the decaploid T. ponticum (Podp) Barkworth and D. R. Dewey (Cauderon, 1979). Five of the E-genome chromosomes are homoeologous with those of the three genomes of bread wheat, Triticum aestivum L. (Dvořák, 1971, 1980).

No homoelogous relationships have been demonstrated between any diploid wheat and T. bessarabicum. However, the karyotype analyses (Cauderon and Saigne, 1961; Heneen and Runemark, 1972) and genome analyses in triploid and tetraploid hybrids (Cauderon and Saigne, 1961; Dvořák, 1981; McGuire, 1984) had led to the conclusion that J and E are closely related genomes. Dvořák (1981) and McGuire (1984) changed the genome designation of T. bessarabicum from J to E^b. Nevertheless, they admitted that direct evidence to support the change would have to come from the diploid hybrid between T. elongatum and T. bessarabicum.

This paper reports the successful hybridization of T. bessarabicum with T. elongatum and meiotic chromosome analyses of the parents and hybrids. In addition, the J- and E-genome chromosomes were compared in an analysis of mitotic chromosomes of the parental species.

MATERIALS AND METHODS

Two accessions of *Thinopyrum bessarabicum* were used. They were designated as Jaaska and Jaaska-11 because they were acquired from Dr. V. Jaaska of Institute of Zoology and Botany, Estonia, U.S.S.R. The ~~T.~~ L. Lophopyrum *elongatum* was received from Dr. Y. Cauderon of I.N.R.A., France.

Hybridization procedures of these diploid species were reported earlier (Wang, 1984). Plants in these diploids varied in vernalization requirement. Lophopyrum ~~*Thinopyrum*~~ *elongatum* generally requires four weeks of vernalization at 5° C under 16 hours of dark period. The two accessions of *T. bessarabicum* consisted of both spring-type and winter-type plants. The latter required approximately eight weeks of vernalization. Before anthesis, spikes were emasculated and enclosed in glassine bags. Twenty-four hours after hand pollination, a 75 ppm gibberellic acid solution was injected into the florets. Half seeds with embryos were plated on slanted orchid agar medium to ensure germination. Seedlings were transferred from test tubes to pots at two-leaf stage and maintained in greenhouse under long days (18 hours of photoperiod).

Spikes were fixed in Carnoy's (6:3:1) solution and stored in 70% ethanol. Pollen mother cells (PMCs) were squashed in acetocarmine for meiotic analyses. Root tips were collected from germinating seeds of ~~T.~~ L. *elongatum* and *T. bessarabicum* (accession Jaaska). Mitotic squash was prepared according to the procedures of Mujeeb-Kazi and Miranda (1984). Ten good preparations were photographed for karyotype analysis with the aid of a microcomputer (Green et al., 1984).

RESULTS

No seeds were obtained in crossing 138 florets of ~~Thinopyrum~~ Lophopyrum elongatum with T. bessarabicum as the pollen parent. However, four seeds resulted from 77 florets and 17 seeds resulted from 79 florets of T. bessarabicum accessions Jaaska and Jaaska-11, respectively, when L. elongatum was used as the pollen donor. All seeds from these crosses were large and plump. Nineteen plants were established from 21 germinating cultured seeds. Three and two plants of the F_1 hybrids involving Jaaska and Jaaska-11 as the female parents, respectively, headed without being vernalized.

Although having more resemblance to those of T. bessarabicum, many spike characteristics in the F_1 hybrids were intermediate to those of the parents (Fig. 1 and Table 1). The hybrids, however, lacked the glaucus-blue color which was present on all plants of both T. bessarabicum accessions. The F_1 plants were vigorous but set no seeds due to nondehiscence of the anthers.

Microcomputer-generated karyotype data of T. bessarabicum and L. elongatum revealed the similarities and differences between the J and E genomes (Table 2). When the data were plotted in a karyogram (Fig. 2), it became obvious that chromosomes 3, 4, and 7 in the two genomes were similar. Significant differences between the two genome were found in chromosomes 1, 2, 5, and 6 for arm ratio. Differences for chromosome length between the two genomes were insignificant. Relative chromosome lengths, ~~that is~~ or the proportion of each chromosome to the total length of the genome, were relatively similar within each of the seven pairs of the J- and E-genome chromosomes (Fig. 2).

Meiosis in the parental species was regular (Table 3), resulting in stainability in over 90% of the pollen grains. Most metaphase-I (~~MI~~) cells showed seven ring bivalents (Fig. 3). The _T. bessarabicum_ accession Jaaska had a higher average of univalents and rod bivalents than the accession Jaaska-11 and L. ~~T~~. _elongatum_. This difference was also present in the hybrids involving the~~se~~ two accessions of _T. bessarabicum_. Hybrids having Jaaska as the female parent had more univalents and rod bivalents (Fig. 4) than those having Jaaska-11 as the female parent. Both crosses revealed the presence of two quadrivalents (Fig. 5), or two trivalents (Fig. 6), or one pentavalent (Fig. 7). Only a single ~~one~~ ring quadrivalent was observed (Figs. 8 and 9). The presence of a heteromorphic bivalent (Fig. 8) and two univalents of different length (Fig. 4) was also noted. Few laggards were observed at anaphase-I ~~(AI)~~ (Fig. 10) but up to four micronuclei occurred in the tetrads ~~quartets~~ (Table 3). Approximately 10% of the pollen grains in the F_1 hybrids were stainable in an I_2-KI solution, although about 23% of metaphase-I ~~MI~~ cells had no univalents.

DISCUSSION

In the greenhouse, Thinopyrum bessarabicum was partially sterile due to the sensitivity of its anthers to the environmental conditions leading to nondehiscent anthers. Even though its pollen grains appeared normal by the staining test, very few seeds resulted from selfing. In the field, the same accessions of this species set abundant seeds upon selfing. ~~However,~~ this hardly ~~could not~~ account for the failure of hybridization between L. T. elongatum and T. bessarabicum using the former as female parent. It it is possible ~~was more likely~~ that difference the size of florets and anthers in these two species has something to do with the ease of a cross between them. those ~~The florets and anthers~~ of L. T. elongatum they ~~are~~ smaller ~~than those of T. bessarabicum.~~ ~~Therefore,~~ Crosses using T. bessarabicum as the female parent gave large seeds whereas no seeds resulted from the reciprocal cross.

The spring-type hybrids were progenies of one plant each of the accessions Jaaska and Jaaska-11. Both of whose ~~These two parental plant~~s are spring type. Therefore, the spring habit appears to be governed by a dominant gene(s). Conversely, the glaucous-blue color of T. bessarabicum is apparently a trait controlled by a recessive gene(s).

The Karyotype analysis of this material ~~in this study~~ generally agrees with ~~those reported~~ previous~~ly~~ reports (Cauderon and Saigne, 1961; Evans, 1962; Heneen and Runemark, 1972; Dvořák et al., 1984). However, the chromosomes in this study are arranged by the length whereas those by Dvořák et al. (1984) were arranged by their homoeologous relationships with wheat chromosomes. Chromosomes of L. ~~T~~. elongatum here numbered 1 through 7 ~~in this study~~ corresponded to 7, 2, 4, 5, 6, 3 and 1 in the study by Dvořák et al. (1984). Since comparisons between the chromosomes of T. bessarabicum and L. T. elongatum revealed that three of the seven chromosomes (numbers 3, 4, and 7 in Table 2) were similar, it was assumed that little structural change had occurred to these three chromosomes. Differences among the other four chromosomes suggested extensive structural rearrangements, especially translocations. Meiotic pairing in the F_1 hybrids ~~supported these conclusions~~. Two quadrivalents in hybrids indicated the occurrence of two reciprocal translocations involving four chromosomes. The heteromorphic bivalent (Fig. 8) ~~suggested~~ indicated that one translocation might have led to two homoeologous chromosomes of significantly different length. If so, the homoeologous relationships between the chromosomes of T. bessarabicum and L. elongatum could not be inferred by their length, at least for those four chromosomes having undergone rearrangements during the evolution of these species. The karyogram (Fig. 2) ~~suggested~~ indicated that chromosomes 1 and 2 of T. bessarabicum might be homoeologous to chromosomes 2 and 1 of L. elongatum, respectively.

Nevertheless, the evidence presented here supports the conclusion that J and E genomes are closely related. Whether the designations of these genomes should be changed is still controversial (Löve, personal communication). Since J was designated earlier than E (Östergren, 1940; Cauderon and Saigné 1961), the change from J to E^b as advocated by Dvorak (1981) and McGuire (1984) may not be universally accepted. Dewey (1984) placed these two species into one genus Thinopyrum but under two separate sections maintaining the two genome designations. With the successful hybridization of these two diploid species and the high frequency of chromosome pairing in the hybrids, his decision is now ? supported by experimental results.

Both T. bessarabicum and L. elongatum had been crossed with Triticum (Jenkins and Mochizuki, 1957; Alonso and Kimber, 1980). The homoeologous relationships among the A, B, D and E genomes had been established (Dvořák, 1971, 1980; Dvořák et al., 1984). Now the relationships between J- and E-genome chromosomes are demonstrated, genes in the J-genome chromosomes should be as readily utilized by the wheat breeders as those in the E genome.

Although F_1 hybrids of *T. bessarabicum* X *T. elongatum* could not set seeds by selfing in the greenhouse due to nondehiscence of anthers, the stainability of 10% of pollen grains suggested partial fertility. These hybrids, therefore, should be capable of setting seeds by backcrossing with the parental species. By doing so, substitution lines could be developed for gene-chromosome relationship studies. When amphiploids are obtained from F_1 hybrids, the meiotic pairing may provide us clues as to whether these diploid species already possess a diploidizing mechanism to prevent homoeologous pairings in tetraploids.

ACKNOWLEDGEMENT

The author acknowledges Catherine Hsiao's work on mitotic squash and photographic printing. Appreciation is also extended to Drs. D. R. Dewey and Á. Löve for their helpful suggestions.

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Table 1. Mean spike characteristics of *Thinopyrum bessarabicum* (6 spikes), *T. elongatum* (6 spikes) and their F_1 hybrids (13 spikes). Range is given in the parenthesis.

Species or hybrids	Spike length cm	Spikelets/ spike	Florets/ spike	Glume length mm	1st rachis internode cm
T. bessarabicum	19.0 (15.4-23.3)	9.3 (8-12)	37.7 (29-51)	11.2 (9.5-12.5)	3.1 (2.7-4.1)
T. elongatum	11.6 (9.4-14.4)	10.3 (9-11)	61.2 (54-69)	5.8 (5.0-6.7)	2.0 (1.6-2.4)
T. bessarabicum X *T. elongatum* F_1	19.1 (16.1-22.8)	11.4 (9-13)	56.3 (42-74)	8.5 (7.5-9.5)	2.2 (1.5-2.8)

Table 2. Mean chromosome length and arm ratio of chromosomes in *Thinopyrum bessarabicum* and *T. elongatum* (10 root tip cells each species). Standard deviations are included in parentheses.

Species	Chromosome characters		Chromosomes 1	2	3	4	5	6	7
T. bessarabicum	Length -	satellite				0.90	1.90		
		short arm	4.59	4.92	3.74	2.12	2.01	3.85	3.30
		long arm	5.91	5.35	5.74	6.18	5.14	4.68	4.98
		TOTAL	10.50 (0.44)	10.27 (0.57)	9.48 (0.51)	9.20 (0.47)	9.05 (0.47)	8.53 (0.37)	8.28 (0.49)
	Arm ratio (L/S)		1.29 (0.02)	1.09 (0.02)	1.53 (0.03)	2.04 (0.10)	1.31 (0.11)	1.21 (0.09)	1.51 (0.05)
T. elongatum	Length -	satellite				0.28	1.62		
		short arm	4.53	4.02	3.28	2.40	2.22	3.03	2.86
		long arm	4.81	4.91	4.70	5.30	3.88	4.59	4.24
		TOTAL	9.34 (0.74)	8.93 (0.55)	7.98 (0.64)	7.98 (0.36)	7.72 (0.55)	7.62 (0.59)	7.10 (0.48)
	Arm ratio (L/S)		1.06 (0.01)	1.22 (0.01)	1.43 (0.04)	1.98 (0.15)	1.01 (0.06)	1.52 (0.09)	1.48 (0.06)

Table 3. Meiotic behavior in pollen mother cells of <u>Thinopyrum bessarabicum</u>, <u>T. elongatum</u>, and their F_1 hybrids.

Species or hybrids	No. plants	No. cells	Metaphase-I I	Metaphase-I oII	Metaphase-I rII	Metaphase-I III	Metaphase-I oIV	Metaphase-I cIV	Metaphase-I V	Anaphase-I laggards cell	Quartet micron. quartet
<u>T. bessarabicum</u> (Jaaska)	1	102	0.14 (0-6)	5.66 (0-7)	1.27 (0-5)	-	-	-	-	0.02 (0-1)	0.03 (0-1)
<u>T. bessarabicum</u> (Jaaska-11)	1	102	0.02 (0-2)	6.51 (4-7)	0.47 (0-3)	-	-	-	-	0.01 (0-1)	0.02 (0-2)
<u>T. elongatum</u>	1	102	-	6.72 (5-7)	0.28 (0-2)	-	-	-	-	0.00	0.03 (0-2)
<u>T. bessarabicum</u> (Jaaska) X <u>T. elongatum</u>	3	322	3.05 (0-12)	1.16 (0-5)	3.42 (0-7)	0.29 (0-2)	0.05 (0-1)	0.16 (0-2)	0.01 (0-1)	0.48 (0-3)	0.73 (0-4)
<u>T. bessarabicum</u> (Jaaska-11) X <u>T. elongatum</u>	2	204	2.11 (0-10)	1.86 (0-5)	2.98 (0-7)	0.23 (0-2)	0.11 (0-1)	0.26 (0-2)	0.01 (0-1)	0.42 (0-2)	0.95 (0-4)

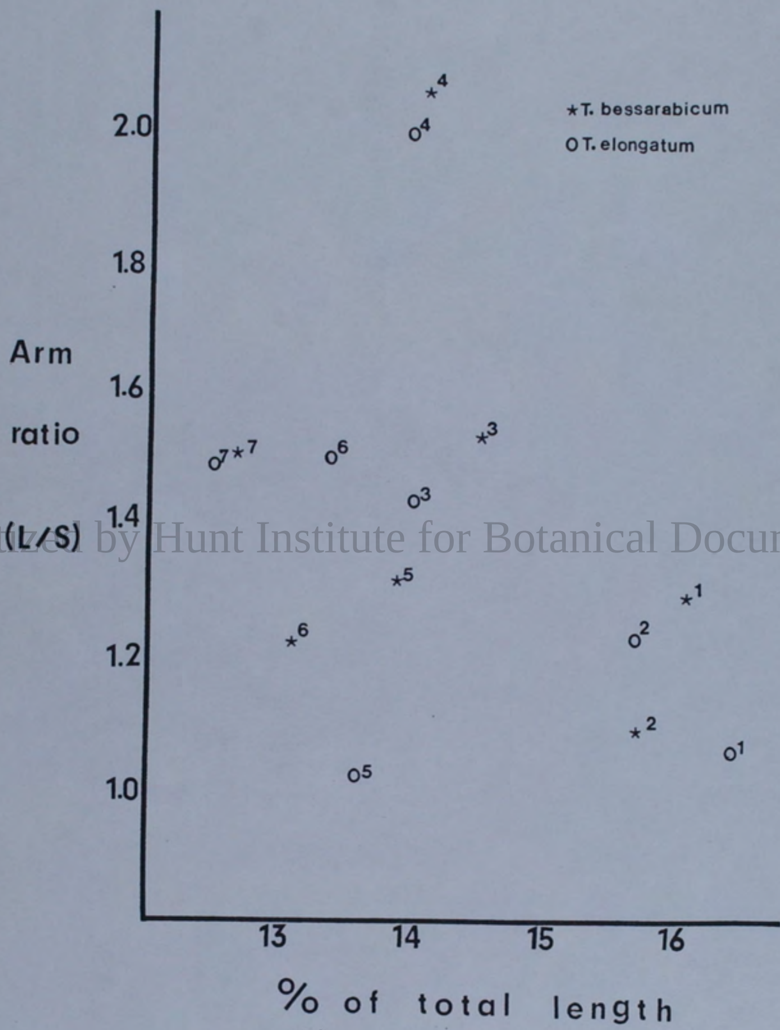
Note: oII, ring bivalent; rII, rod bivalent; oIV, ring quadrivalent; cIV, chain quadrivalent

T. bessarabicum F1 Hybrids T. elongatum

Margins
1" 1 1/2"

Margins
1 1/2" 1"

Fig. 2. Karyogram of seven chromosomes in Thinopyrum bessarabicum (J genome) and T. elongatum (E genome).

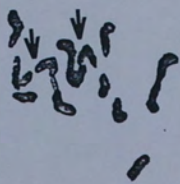


★T. bessarabicum
O T. elongatum
Arm ratio (L/S)
2.0
1.8
1.6
1.4
1.2
1.0
13
14
15
16
% of total length

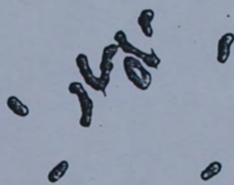
Figs. 3-10. Meiosis in *Thinopyrum elongatum* (3) and F_1 of *T. bessarabicum* X *T. elongatum* (4 to 10). Fig. 3. Seven ring bivalents. Fig. 4. Two ring bivalents, four rod bivalents and two univalents of different length. Fig. 5. Two univalents, two rod bivalents, and two chain quadrivalents (arrowed). Fig. 6. Four univalents, one ring and one rod bivalent, and two trivalents. Fig. 7. Seven univalents, one rod bivalent, and one pentavalent. Fig. 8. Three ring bivalents, two rod bivalents (heteromorphic one is indicated by the smaller arrow), and one ring quadrivalent (large arrow). Fig. 9. Five univalents, one rod bivalent, one trivalent, and one quadrivalent. Fig. 10. Anaphase-I showing one lagging chromosome.

3

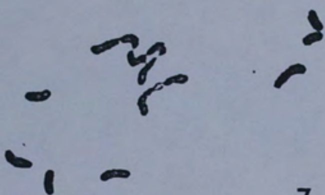
4



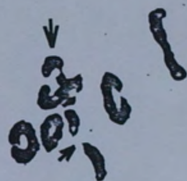
5



6



7



8

9

10

Sent 8/2'85

Some suggestions by Áskell Löve on a manuscript by Richard R.-C. Wang:

p. 1: The title "Genome analyses of Thinopyrum bassarabicum, T. elongatum and their F_1 hybrids seems inappropriate since this analysis (singularis!) of the two species requires such hybrids as its basis. A better title may be: "Genome analysis of Lophopyrum elongatum and Thinopyrum bessarabicum".

p. 2: line 1: T. elongatum changes to Lophopyrum elongatum. [cf. reasoning later].
line 7: T. elongatum changes to L. elongatum.
line 10:...in the J and E haplomes [basic genomes] were....
[I wonder why you prefer to follow non-conformist-agronomists in rejecting the more logical and scientific terminology that accepts the term "genome" as a general one as originally defined and traditionally used but avoids using it also in the very special cases of "basic genome" or "analysers" as does Kihara in his later papers as well as agronomist-cytologists here and there. The more exact term haplome is, as mentioned in my 1982 paper, presented and defined by Heilbronn & Kosswig, 1938: Principia Genetica. - J. Unif. Sci. (ERkenntnis) 8:229-255, the second edition of which was published as the little book Principia genetica, 1966, referred to in my 1982 paper. Before rejecting its proposals, these ought to be fairly evaluated, at least, as since I am sure that you soon will,become the world leader in this field, I urge you to accept this excellent and exact term rather than the confused one used because of ignorance. Other certainly will soon follow].

exact term

line 11: Meiosis in the F_1 hybrids...
line 12: have undergone considerable structural...
line 16: T. bessarabicum x L. elongatum ...set seed. [seed here pluralis-singularis]
lines 17-18: ...that the E and J haplomes are related though clearly distinct as based on the traditional definiton of Triticeae haplomes...
[Omit the last sentence: ...The impacts of...are discussed (cf. later)].

p. 3: replace lines 1 - 21 with:

In the genomic classification of the Triticeae tribe of grasses recently proposed by Löve (1982, 1984), the genetically homogenous genera are characterized either by a single basic genome, or haplome, conventionally designated by a capital letter and sometimes forming an autoploid series, or by an alloploid combination of two, three or even four such haplomes forming complex genera. It is evident from considerable observations of these chromosome complexes that each is distinguished by particular taxonomical and genetical characteristics that are kept intact by strong incompatibility based on cytological differentiation, though occasionally that barrier has been upset by rare hybridization as indicated by the occurrence of the alloploid complexes and by the rare appearance of sterile hybrids. The distinction of the haplomes seems to be upheld by various but intricate rearrangements of the linear organization of the individual chromosomes that may reach from partial homeology to apparent heterology of all the chromosome pairs of the haploid complement that counteracts pairing in possible hybrids. Although these changes are best studied in the meiosis of hybrids, they are also discernable as karyotypes at mitosis (cf. Chennaveeraiah 1960). Since at the lower end of this differentiation are taxa of which only one or a few chromosome pairs may have become homeologous, it has been found convenient to define as distinct basic genomes or haplomes in the Triticeae only those taxa that have at least four pairs of their monoploid chromosome set so differentiated that synopsis at meiosis is disturbed, whereas homeologous disturbances of up to three pairs are tolerated as intergenomic (cf. Kihara 1954, 1963, 1975; Löve 1982). In the Triticeae there have been identified twenty-three taxa with single haplomes differing in four to seven chromosome pairs, and at least fourteen alloploid complex genomes based on some of the former. These taxa of the Triticeae tribe, which Löve (1982, 1984) classified as genera based on the classical definition of this category, are grouped in the four subtribes Agropyrineae, Henrardiineae, Hordeineae and Triticinae, of which the first two include a single very distinct genus each, with three and one biological species, respectively, that seem incompatible to other taxa. Neither are natural hybrids known between genera of the other two subtribes, although chromosomes have been experimentally transferred to hexaploid <u>Triticum</u> from some perennial grasses of the E-haplome genus Lophopyrum Á. Löve and the J-haplome genus Thinopyrum Á. Löve, either directly from the diploids (Jenkins & Mochizuki 1957; Alonso & Kimber 1980) or via the allohexaploid Elytrigia intermedia (Host) Nevski or the autodecaploid Lophopyrum ponticum (Podp.) Á. Löve (cf. Cauderon 1979; Dvořák 1971, 1980; Dewey 1984; Tsitsin 1975).

This paper reports the successful hybridization, etc. unchanged to line 25...

p. 4: Material and methods [material is singularis-pluralis in this connection, not materials, which is for sewing, building etc.].

line 3: because the seed were obtained from...[singularis-pluralis].

line 4:...The lophopyrum elongatum seed were...

line 8: Thinopyrum elongatum...should be L. elongatum.

line 20: last letter: L. [not T.].

p. 5: line 1: Thinopyrum, should be L.

line 7: cultured seeds...should be: germinating seeds...

line 11:...in the F_1 hybrids...

line 16:...L. elongatum...[not T.]

line 23: [should begin with]: lengths, or the proportion of...

line 24: ...of the J-.....

line 27:..90% of the pollen grains. Most metaphase-I...[omit MI].

p. 6: line 3:...and L. elongatum.

line 8:...Only a single ring quadrivalent was...

line 11: [omit AI].

line 12: quartets, should be tetrads, which is the classical botanical term. [Quartet is used in bad taste only by American non-conformists].

line 13: ...pollen grains in the F_1 hybrids...[these are certain, not some, hybrids]

line 14: for MI write metaphase-I, always.

p. 7: lines 6 & 7: upon selfing, so this hardly accounts for the failure...

line 8: L. [not T.] elongatum and T. bessarabicum...

line 9: [better]: ...is possible that difference in the size of....

lines 10-12: [better]:...cross between them, these of L. elongatum being smaller. Crosses using T. bessarabicum as the female....

line 15: ...and Jaaska-11, both of which are spring type...

lines 16 - 17:...governed by a dominant gene...

line 18: ...by a recessive gene...

lines 19-2o:. The karyotype analysis of this material generally agrees with previous reports (Cauderon...

line 24:...of L. elongatum here numbered...

line 26:...between the chromosomes of T. bessarabicum and L. elongatum.....

p. 8: line 1:...essentially homologous [similar is wrong here].

lines 2-3:...especially segmental interchanges, an observation supported also by studies of meiotic pairing in the F_1 hybrids. Two...

lines 5-6:...reciprocal translocations...[better]: segmental interchanges...

line 7: ...(Fig. 8) indicates that one segmental interchange might have...

line 12:...(Fig. 2) indicates that chromosomes 1 and 2...

lines 15 to 27 and page 9: Omit and replace with:

Although the evidence here presented supports the earlier conclusion of relationship between the E and J haplome taxa which also was indicated by the purely morphological observations by Nevski (1936) and Tzvelev (1976, that relationship is close only for three of the seven chromosomes. Therefore, following the traditional definition of what constitutes different haplomes, the conclusion by Dvořák (1981, McGuire (1984) and Dewey (1984) as to the identity of these haplomes cannot be logically sustained. Therefore, the results of the present observations clearly favor the taxonomical conclusions by Löve (1982, 1984) as to the generic distinction of both the E-haplome genus Lophopyrum and the J-haplome genus Thinopyrum and supports the rejection of the combination of these genera advocated by Dewey (1984 as well as his transfer of the complex genome section Trichophorum from Elytrigia to Thinopyrum, an action that would inevitably result in the replacement of the genomic clarity of the distinctly described and defined genus Thinopyrum by a confusion no better than has been typical of the collective genus Elytrigia since it was revived by Nevski (1936), though it would result in a natural Elytrigia defined by only its single complex genome taxon s. str. A solution of the Thinopyrum problem created by Dewey (1984), however, requires not only the retaining of both Thinopyrum s.str. and Lophopyrum sensu Löve, but also the genomic clarification of both Elytrigia s.str. and its section Trichophorae (Nevski) Dubovik, allopolyploid complexes that morphologically seem to consist of some combinations of the S-haplome and the E-haplome and of the S-haplome and the J-haplome, respectively so the confusion may be the result of the fact that E and J are closely related, though distinct, as shown in the present study.

Additions to References:

CHENNAVEERAIAH, M. S. 1960: Karyomorphologic and cytotaxonomic studies in Aegilops. - Acta Horti Gotob. 23: 85 - 178.

KIHARA, H. 1954: Considerations on the evolution and distribution of Aegilops species based on the analyser-method. - Cytologia 19: 336 - 357.

KIHARA, H. 1963: Interspecific relationships in Triticum and Aegilops. - Seiken Zihô 15: 1 - 12.

KIHARA, H. 1975: Origin of cultivated plants with special reference to wheat. - Seiken Zihô 25: 1 - 24.

LÖVE, Á. 1982: Generic evolution in the wheatgrasses. - Biologisches Zentralblatt 101: 199 - 212.

LÖVE, Á. 1984: Conspectus of the Triticeae. - Feddes Repertorium 95: 425 - 521.

NEVSKI, S. A. 1936: Conspectus Loliacearum, Nardearum, Leptureaurum, Hordeumque florae Unionis Rerum Publicarum Sovieticum Socialisticarum. - Trud. Inst. Bot. Akad. Nauk SSSR, Ser. 1, Fasc. 2: 33 - 90.

TSITSIN, N. V. 1975: Origin of new species and forms of plants. - Proc. 12th Int. Bot. Congr.:3 - 10.

TZVELEV, N.V. 1976: Zlaky SSSR. - Nauka, Leningrad, 788 pp.

San José, Septe.ber 28, 1984.

Dr. Richard Wang,
Crops Research Laboratory,
Utah State University - UMC 63,
Logan, Utah 84322.

Dear Dick:

Thanks for the call an hour ago. I have looked up the matter of the J and E haplomes and find, that the former, J, was so designated by:

"Östergren, G. 1940: Cytology of Agropyron junceum, A. repens and their spontaneous hybrids. - Hereditas 26: 305 - 316.

He worked with the plant we called junceiforme, the tetraploid. On p. 313 he stated: " If we ascribe A. junceum and A. repens the genome formulae $J_1J_1J_2J_2$ and $R_1R_1R_2R_2R_3R_3$, the formula of the heptaploid hybrid will be $J_1J_1J_2J_2R_1R_2R_3$....."

The E-haplome seems to have been first recognized by:

Cauderon, Y. & Saigne, B. 1961: New interspecific and intergeneric hybrids involving Agropyron. - Wheat Information Service No. 12: 13-14.

They mention the E-haplome (and J) on pp. 13 and 14, and add the remark on p. 14: "From the high frequency of trivalents at MI, it can be concluded that there is close relationship between the J_1, J_2 and E genomes. Similar conclusions can be drawn from karyotype analysis".

Personally, I have never been in doubt, that these are related haplomes, one likely having originated from the other, though the direction remains obscure, and that they differ in several segmental interchanges, some inversions and other less clearly defined chromosomal rearrangements, as your observations on your new F_1 hybrids seem to confirm. The success in hybridizing these taxa, which I never could get to set seed myself, however, only indicates such a relationship, no more, as far as my logic tells me...but that may change when you get F_2 and later generations, if thay can be produced. The variable frequency of bivalents indicates to me only relationship, not homology, as perhaps one ought to use the word homeology in such a case, since I believe you, and even Dvořák, have clear indications of homeology between the two haplomes, no more so far, as the morphology and some other factors indicate only relationship, not identity. Logically, differences are essential, whereas similarities require much more studies before one can logically mention identity or homology that frequently are impossible to prove. If you need to publish these first results, do it with caution and avoid rash conclusions, but the best would be if you could wait with even a preliminary publication until you have gotten F_2 plants to study...or made so many attempts to make these that you feel safe that they cannot be produced. And in your paper after that work, you could avoid erroneous conclusions by simply describing your observations and leaving nothing out so the hopefully logical reader may make his own taxonomical conclusions himself.

There is no doubt that your results are remarkable and that they may lead to some drastic conclusions as to the evolution of the annual from the perennial wheatgrasses...and much more. And I am in no doubt that whatever will be your final conclusions they will affect thinking in the field drastically.

All the best,

San José, January 25, 1985.

Dear Dick:

I am sorry to let you wait so long for my reaction to your manuscript you sent, but as I told Doug in a recent letter, I do not know how to express myself so as not to hurt your feelings, so I have hesitated too long to send you a line. There is no doubt that your success in making the hybrids that others -including myself - have not had the skill to produce, is highly commendable, and so is also your description of your observations. But I am less satisfied with your introduction and discussion which clearly show your inexperience in writing such papers, so you fail to give the not too well initiated reader information on the historical and scientific background, and even discuss what you believe are characteristics of what you call genomes (and I name , more correctly I am sure, haplomes), without even mentinning their definition as given by Kihara and many others, cf. my 1982 paper p. 200, so the introduction may seem cryptic to some. I am also somewhat disappointed with your timidity to draw conclusions that are not contrary to common logic, because your results, in my opinion, show definitely that my conclusion as to the distinction of the J and E haplomes is correct, so the proposal by Dewey to unite not only Lophopyrum and Thinopyrum but also the sect. Trichophora part of Elytrigia should be rejected in light of your observations. I even dare to observe that because of your observations we now may understand how the old Elytrigia complex that I left undivided for the polyploids can be more reasonably divided into two homologous genera in the correct genomic style, though I doubt that you are yet aware of this: the genus Elytrigia s.str. that is composed of the EES genome, and the sect. Trichophora that Dewey transferred to Thinopyrum and likely is composed of some JS combinations. As a genus we cannot name the latter Trichophorum, because that generic name is preoccupied in Cyperaceae, but the description by Dubovik can be used also under another name, so I am thinking of the possibility of validating a genus Trichopyrum and transferring to it the taxa I list under the section in my Conspectus. Since I am not, at least not at present, planning to do this, perhaps I may propose that you make these transfers in your paper? Naturally, I am ready to help you with the formalities in this if you so feel. Also, if you think you would like to see my proposals as to some improvements in your paper's first and last parts, I am ready to try, though you would have to polish my proposals further and so make them your own. Perhaps we could discuss this per telephone rather than by letters, but if so, it would be best to try to catch me around 1 p.m. (your 2 p.m.) rather than early in the morning to secure that I am properly awake!

I have some remark to make as to where your good paper ought to be published most effectively both for you and your reputation and readers. I do not think that the Canadian Journal of Genetics and Cytology that Dvořák uses because he has a channel to the editor since he lived in Saskatchewan, is the correct place for work on plants of European relationship. That journal is very little known in Europe and much more provincial than international, so you would be burying your reputation as a good observer in evolutionary botany and genetics. A paper of such an importance for the understanding of European complexes ought to be printed in some of the great and old European journals, though many of these, as, e.g., Hereditas, are closed to non-European authors, or, as e.g., Heredity, publish too slowly. You may not be aware that you could print rather fast in the more than century old Biologisches Zentralblatt (of which I am on the editorial board and so could review your paper at once and recommend it for a speedy publication). But there are also other appropriate journals that you may prefer. One such is the Japanese Wheat Information Service published from the Kihara Institute in Yokohama, but since it comes only once a year, you may not like to wait for it...especially since I believe the number for 1985 already may be filled or on its way. There is always time to discuss such matters before an important paper is buried in an inappropriate place, and you and your ideas need good exposition rather than hiding.

I hope Doug has copied for you the Triticeae conspectus that I sent to him on January 11. I received only 30 copies because of the size of the paper, but he proposed that he might help me to copy it and disperse it to more colleagues. That is like him - I envy you to have the opportunity of discussing with him daily in a stimulating place, because that I have not been able to do with socalled colleagues since I was forced to move to places like Boulder where nobody is interested in anything but his own politics and economics at the cost of others, and certainly not in any real scientific discussions.

Hope the eevere winter is not cutting down on your activities. In California it is also unusually cold and foggy, though our oranges slowly ripen outside our windows and the preparations of the gardens around us are slowly starting and soon will be in full operation.

With the very best regards and all good wishes,

Sincerely,

Áskell Löve

MANUSCRIPT PEER REVIEW

MANUSCRIPT TITLE
Genome analysis of Thinopyrum bessarabicum, T. elongatum, and their F_1 hybrids

AUTHOR(S)
Richard R-C. Wang

REVIEWER'S NAME	FOR PRESENTATION AT
Askell Love	
TITLE	PROPOSED PUBLISHER/JOURNAL
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San Jose, CA	Canadian Journal of Genetics and Cytology
SIGNATURE / DATE	PUBLICATION RECOMMENDATION
	☐ Acceptable as is ☐ Acceptable with revision ☐ Unacceptable

COMMENTS *(Attach additional sheets as needed)*
Authors must respond to specific comments on technical content and quality, by notation in the margin of this form or by attachment of a written response.

Changed chromosome races to "~~different~~ "ploidy levels."

San José, February 15, 1983.

Dear Mary:

Excuse me for the delay in thanking you for the manuscript on Leymus ambiguus etc. that I have enjoyed reading. Although I must admit that you have not convinced me yet that these taxa are worthy of recognition at any level, if you are right in that e.g. salmonis is also found in Newfoundland. Is it possible that the diagnostic characters selected are ones of a rather general occurrence in L. ambiguus and so ought to be recognized only as an indication of its variability? Also, your somewhat slippery way of avoiding to define your concepts makes me sometimes feel that you do not tell all that you know...or you do not mean that you, when working on taxa distinguished by their cytology, feel you could accept the far from logical socalled species concept of the big body of the not so courageous Cronquist, who in my opinion is a creationist since he has declared that he does not believe in evolution? To work on that kind of taxonomy must be regarded a step backwards into the middle ages...but I doubt that it is fair to assume that you have too limited a background in cytogenetics yourself to stand firmly in evolutionary taxonomy? I am sure the pheneticists in Britain and Ottawa have confused you as they have confused themselves by adding a new term for what in my youth was called lamarckism and later lysenkoism and always creationism, rather than simply read their good old Wilhelm Johansen and his clear definition of the fundamental difference between the genotype and phenotype that since the beginning of genetics has been the dominant factor in all good plant breeding, at least in Europe outside Britain. And a geneticist who does not define his species by reproductive isolation that is nowhere more strongly expressed than in differences in chromosome number between distinct populations is unbeliebably confused. Therefore, I believe you are not doing right in separating the two western taxa salmonis and salinae in such a way that at least one and perhaps both include two chromosome numbers that indicate a reproductive barrier inside each. And wonder if you are not forcing the matter before all the basics are known? Despite of these remarks, I enjoyed reading it.

Now to the points that perhaps could be improved, taken page by page:

p. 1: Nevski did not accept Leymus, but Pilger did. And both Melderis (1978 is a better reference than the Flora Europaea itself) and Tzvelev were following us in our 1961 NW European chromosome list where it was first accepted in such a general work after Pilger had brought it up again...so for fairness exchange Nevski with L. & L. 1961. Nevski, by the way, accepted Elymus for what we now regard as Leymus, and divided the remains into several genera that we now call Elymus.

Instead of talking about a J genome and an X genome, as Dewey continues to do, I would not name them at all but say:"a genome (actually a haplome!) derived from the Eurasian genus Psathyrostachys and another from the genus Thinopyrum (or if you want to ignore my ideas, which I understand, then "the Junceae section of Elymus". And refer not to Dewey as a source, but to Melderis 1978, which bases his conclusion both on the fact that Leymus hybridizes with E. junceus in the latter sense, and on experiments he made in his garden in London. Chemistry supports this, try to smell the leaves of A. junceum and Leymus, which grow together on the European coasts!

↑ have modified accordingly. Incidentally, my guess is two different X haplomes — a karyotyping apparently supports this - info hot off the press from Cathy Hsiaou at Crops Research.

we would end up recognizing – was what [illegible] would be

Done

p. 2: You ought to explain the meaning of the cryptic OTUs that non-numerical taxonomists are not likely to understand. That goes for all abbreviations, of course.

p. 4: You talk about "phenetic overlap" as if this was something remarkable in groups that evidently have a genetic overlap and then also include more than one chromosome number. This only indicates insufficient observations or some flaw in the planning of the study of the material, or even in its identification. Perhaps you are selecting the wrong characters for comparison? Or perhaps distinct taxa should not be recognized. Some of the characters are better than others – but all had looked reasonable initially. To select only those characters that distinguish the taxa would be to predetermine the result.

This is mainly not come across, we really were not sure how many taxa

p. 6: Slips of the pen: Juniperus...no L...spp. Pseudotsuga menziesii...Franco... D. Don, not Dons.

p. 7: Have you critically observed such material from Newfoundland, or are you just accepting observations by someone who may not be a good taxonomist or even a good floristical botanist? If it is from Bowden, he belonged to the latter.

What is the difference between rocky and volcanic in this connection? And are not all tolerances supposed to be physiological, more or less?

One can't allow one's student some freedom – actually I got tired of arguing.

p. 8: There is something fishy about what is said about "multiple chromosome races" in this place. A race must have a distribution, and it must also be interfertile with other such races of the same species, thus an inappropriate term caused by misunderstanding. If what is meant is the rare, very rare, occurrence of triploidy or tetraploidy caused by very occasional autoploidy in perhaps 2 - 5 promille of all populations, then this is not expressed clearly enough. If that is meant, then the references to Stebbins & Love and to Bowden are inappropriate, because their claims are caused by taxonomical confusion or uncritical identification, as perhaps even your case of salmonis and salinae tetraploids and octoploids? If you do not understand what I am trying to say, then perhaps our little book on Plant chromosomes, published by Cramer in 1975, may help, especially p. 12, bottom line; though I am of the feeling that all this talk about chromosome races is 19th century philosophy caused by the fact, long since acknowledged by good philosophers, that ~~the~~ obscure talk is caused by obscure thinking?

The plasticity is ill-named – variability w/in a population – Sometimes – not ~~genetic~~ ~~[illegible]~~

I am not sure how plastic the others are – no expt'l data

p. 9: The first sentence of the conclusions is obscure and could be expressed more clearly. Reproductive isolation is internal, never geographical, therefore it justifies specific recognition, whereas geographical isolation results in the development of races at the varietal or subspecific levels, and is reversible. You do not accept the muddled concept of Cronquist that is even illogical as he expresses it this time...least of all when you at the same time are discussing chromosomal differences? And if you have observed "plasticity" in your diagnostic characters, you have selected characters that are of no diagnostic value, or so even Linnaeus would have told you in his famous Critica Botanica, which I doubt is even mentioned by the not too literate teachers of taxonomic botany at present in Britain and America...and probably even in Sweden? This chapter is otherwise very little genetics and very much phenetics, unfortunately I believe.

Chickenness.

What do you actually mean by talking about a conservative taxonomic approach? You are, I hope, not trying to tell people that such a point of view is more correct than others, and that by saying that someone is not conservative you are actually indicating that he is no good? I thought that even young people would realize that conservatism never has resulted in any changes in points of view nor in the development of any science and certainly never in any progress, so I would drop the word.

No. But I do like to make sure people can see ~~their~~ why I am doing something – a comment made with my problem in Stygeae in mind more than T. ticeae.

Corrected to 56 in my Taxon copy!
Vol. 29: 168 – [illegible]
Seeds [illegible]?

May I borrow the voucher please?

p. 10: 56 chromosomes for salinae have not only been reported by Jensen and your study, but also by me in Taxon 1980, from near Elk in Wyoming. I am, however, not sure that my identification of the material was exact...but that is also my opinion as to Jensen's two numbers and their intermediate, which most likely is a hybrid between the taxa that he so identified...on what basis? What is the number of the holotype, or the topotype from that locality?

p. 11: Again, I published, in the Taxon list 1980, the chromosome number 28 for L. ambiguus from the Uinta Mts, Mt Agassiz, in Utah ...and of that identification I am in no doubt. But how can you be sure that your 56 chromosome material of this taxon is not identical to the same numbered "race" of salinae? Have they been crossed and meiotically analysed? – no

p. 12: Do you have a copy of Cronquist's 1978 paper that you could help me copy?

p. 13: A better reference for Melderis would be 1978: Taxonomic notes on the tribe Triticeae (Gramineae), with special reference to the genera Elymus L. sensu lato, and Agropyron Gaertner sensu lato. - Bot. Journ. Linn. Soc. 76: 369 - 384.

have dropped

The reference to Nevski, 1933, which ought to be dropped because it is irrelevant, see above, is more correctly: Agrostologicheski etyudy. IV. O sisteme triby Hordeeae. Observe that Hordeeae is the correct spelling that he uses; one e shows only that the writer has not known his Latin and the fact that the root of the name Hordeum is Horde...and with the additional ending it becomes Hordeeae. Most American botanists evidently know little Latin, even less than I do, unfortunately. And the pages of Nevski's paper in Ser. I,1 are: 9 - 32.

The reference to Tzvelev is to his book, not only to a chapter in it, and so ought to be either: Zlaky SSSR, Leningrad, Izdatel'stvo Nauka (or simply Nauka), or Poaceae U.R.S.S. (which is the Latin abbreviation of SSSR or USSR).

I hope you accept these remarks as friendly ones, as they are meant, and realize that I have had a great pleasure of reading the paper, although I feel it could be improved, especially its philosophical part and matters concerning critical taxonomy and terminology and definition of concepts. That probably is a remark one can always make on any paper, not least my own, but only shows the good old observation that eyes see better than an eye. And we all always need some help in seeing the flaws of our works...even when they actually are no flaws but simply differences in the point of view that we must learn to agree to disagree about if there is to be a free science and free thinking...which I have experienced many of our colleagues on this continent want to break laws and even commit murder to prevent others from doing, especially uncomfortable immigrants who do not understand that to climb the stairs of the ivory tower and get honors and acknowledgements you must crawl for those who regard themselves as the only great and swallow all their ideas uncritically. I dare to say all this to you because I believe I have seen that you just do not think in that way.

By the way: I would send you a copy of Plant chromosomes if I had an extra, but if you are interested in it, it is probably found by Lubrecht & Cramer, Books on Botany, RFD 1, Box 227, Monticello, N.Y. 12701, and certainly by J. Cramer, Publisher, In den Springäckern 2, D-3300 Braunschweig-MA, West Germany. It may be worth its price.

All the best wishes and regards,

Yours sincerely,

Löskell

San José, October 8, 1983.

Dear Mary:

Many thanks for your good letter of the end of September; I should have written at once, but had several things that took more of my time than they should, so my energy was not sufficient for even one more letter! Or perhaps it was laziness, the old principle of doing to morrow what you could have done today...you are still filled with energy that does not permit that, but it comes even to those whose energy at younger years is boundless.

I am glad that my small remarks were found to be useful, and appreciate your discussion of those few points on which we may agree to disagree. Since you doubt that the L. ambiguus from the Uintas was correctly identified...by me and Bowden... I am writing to Montreal and asking them to try to find the specimen in one of the many boxes of unmounted vouchers that I left with Marcel Raymond in 1963-64...and hope they have mounted this all or at least kept it available. You will hear from me about that as soon as I hear from them, though I must admit that at that time I was hardly aware of the other variations around that species in the west and knew the complex mainly from Bowden's eastern material. And I have a tendency to trust your judgement in such matters better than my own.

Sorry that the NSF is so slow with its decisions, but since you have contacts with some of those that are regarded as kings in botany here, you ought to be safe. But their "peer" reviews are of the kind that got me to doubt their honesty...every time I applied to them there were socalled reviews by people who in no ways could be regarded as my peers because they knew neither cytogenetics nor modern European taxonomy and nothing in taxonomical philosophy, still less in phytogeography. That is what can be expected when young and inexperienced socalled scientists are asked to review matters they known nothing about and have been told may be contrary to the religion of the land....in a country that still lives on Hooker's once good idea that time has left stranded in the far past. But though it may be difficult for you to make your arrangements if the decision comes late, though positive, there is always another year. If you will need reprints of my Triticeae paper, late in the summer may perhaps be better than early, because I understand that it may perhaps have to wait until the middle of the summer. We will see.

I am glad to know that you are leaning towards more experimental work in the wheatgrasses, since that may help us to solve some of the problems that we have just opened up for discussion, and then to discover others that we do not know of yet. The chemical approach will be helpful mainly with variations that are not indicative of specific differences, I believe, though even at the generic level they can be very useful to those who know to read them. But the essential cytogenetical work on some of the specific and generic differences will require studies that compare meiosis in great detail and even banding and other finesses that only few good laboratories master in America. If you need a good cytologist for that kind of help...also with Stipeae...Pat McGuire may be the man, though I believe both he and Dvořák were disappointed in the selection of a much less qualified person for the position he applied for. Though he may not be interested in Utah if he can stay in California?

Have you heard that Bill Weber is separating Festuca dasyclada as a new genus Argillochloa...for good reasons I believe? It resembles Oryzopsis superficially and so may belong to the Stipeae...but we have no cytological information which is essential for the tribal decision. Do you know this rare plant and can you get it cytologically studied even at such a late date?

All the best,

Dear Askell:
Please excuse this yellow paper and handwriting. It is handy. I am enclosing your comments on the salinus paper – with my replies. You will find that I really did benefit from and accept most of your suggestions. The chromosome "races" have been replaced with ~~of~~ plants at different ploidy levels. The cytological material was not vouchered, other than at the local population level. Consequently we could not determine how or whether different levels differed morphologically.

Would it be possible to borrow your voucher for L. ambiguus from the Uintas? According to Riley & I, it does not grow there.

There is, what I think should be regarded as a subspecies of L. salinus in California & Arizona – growing on steep, north-facing slopes. I am hoping to get chromosome counts from seedlings this fall but the seeds ~~look as~~, seem very poorly developed.

I think I told you that I won't know about the NSF conference until late in Oct. I am busy preparing another proposal for them – for a rather broad attack on the Triticeae – I am even thinking of getting a cytogenetic post doc to check some of Jackson's predictions. I'd rather mess with chemicals myself. Otherwise life goes on as usual. ~~than~~ My efforts at present (apart from the grant proposal) have to be put into the Stipeae – after all, the NSF paid some money into that. -

Doug says you will be coming out next summer in anycase. ~~E~~ Great! After you have been made to look at the Triticeae I shall show you the beautiful Stipeae farm.

Thank you for all your help —

Mary —

UTAH STATE UNIVERSITY

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DEPARTMENT OF BIOLOGY
COLLEGE OF SCIENCE

October 15, 1982

Dr. Askell Love
2780 Chandler Court
San Jose, CA 95123

Dear Dr. Love:

Dr. Douglas R. Dewey and I intend to organize a Conference on the taxonomy of the Triticeae, to be held in the summer of 1984 here in Logan. The intent of the Conference is to bring together individuals who are actively engaged in biosystematic and floristic research on the tribe. The purpose is to examine alternative taxonomic treatments in terms of all the data available. We do not anticipate coming up with a single scheme that will be universally acceptable, but we hope it will be possible, by reviewing the data available from a wide range of studies, to come to a far greater measure of agreement than presently exists concerning the taxonomy of the tribe.

We are enclosing a very preliminary draft of our proposal for the Conference. If you would be interested in participating in such a Conference, please send us, by November 30, a brief outline of your research interests in the Triticeae and a list of your publications. We would also like to receive any suggestions you have as to additional topics or emphases so that they may be considered when we prepare the final program.

Our planning is still in the preliminary phase, but if we are to obtain the necessary funding, we must demonstrate that there would be an interest in such a Conference and that it would serve a useful purpose. Thus, your prompt reply will not only be greatly appreciated but is also essential to our success in organizing the Conference.

If you know of others who might be interested, please draw their attention to this request. We are anxious to bring together all those with active research interests in the tribe as a whole.

Your sincerely,

Mary Barkworth

Mary Barkworth
Assistant Professor, Biology

Enclosure

PROJECT SUMMARY

Logan, Utah — <u>Conference on the Taxonomy of the Triticeae</u> — July, 1984

The goals of the conference are:

1. Stimulation of broadly-based research on this economically important and taxonomically controversial tribe.

2. Exchange of information bearing on the taxonomy of the tribe between individuals working in different fields and in different parts of the world.

3. Development of a consensus concerning the appropriate taxonomic treatment of the tribe and a better understanding of the bases for alternative treatments.

4. Development of a common system for designating different genomes in the tribe.

5. Improved ability to exchange information, taxonomic and otherwise, about taxa in the tribe. W4S?

The Triticeae is extremely important for it includes wheat, barley, and rye, as well as many important forage grasses. Exchange of information concerning its members is, however, seriously impeded by the lack of a widely accepted taxonomic treatment. Even recently published treatments differ substantially from each other. The goal of the conference is to develop a better understanding of the taxonomy of the tribe and to clarify those areas in which consensus is possible by reviewing the information available on a global basis. Because taxonomic treatments are reflected in nomenclature, the results of the conference will have substantial benefits for workers in many fields as well as serving to stimulate those concerned with the taxonomy of the tribe..

THE PROBLEM

The taxonomy of the Triticeae has always been controversial (Bowden 1957). Suggestions as to the number of genera to be recognized have varied from 1 (Krause 1898; Hylander 1945; Stebbins and Snyder 1956) to 36 (Love 1982). The controversy reflects, to a large extent, the widespread auto- and allopolyploidy in the tribe, the extensive natural and artificial hybridization among species, and the ever present reduction and convergence that plagues all grass taxonomists.

The inadequacies of systems such as that of Hitchcock (1951), which are based on Bentham's (1881) treatment, are widely acknowledged (Dewey 1982; Stebbins and Snyder 1956; Estes and Tyrl 1982; Melderis 1978) but they are still widely used because no alternative treatment has achieved widespread acceptance. Nevski (1934) prepared a revision of the tribe that was generally accepted by some Soviet, European, and Chinese taxonomists (Pilger 1954; Keng 1965) but which never won widespread acceptance in the rest of the world. Nevski's treatment, like that of Bentham, was based primarily on the morphological characteristics of the various species. It differs in the very much narrower generic concept he adopted and his misapplication of the name *Elymus*.

During the last thirty years a vast amount of biosystematic information concerning the Triticeae has been accumulated. The extensive genomic data is particularly noteworthy (see, for instance, Lilienfeld and Kihara 1961; Waines et al. 1982; Dewey 1982) but there is also considerable information available on anatomy, micromorpology, crossing relationships, enzyme variation, and immunochemical relationships (see, for example, Bothmer 1979; Konarev et al. 1981; Johnson et al. 1967; Jaaska 1974; Baum 1978). Most of this information suggests that neither Bentham's nor Nevski's treatment effectively summarizes the observed variation in these characters. Since such characters are often considered to present a clearer picture of evolutionary relationships than morphological characters, there is widespread dissatisfaction with both traditional treatments on the part of those who consider that taxonomy should reflect phylogeny.

Recently, several new treatments of the Triticeae have been proposed, in greater or lesser detail. Among the more detailed treatments are Tsvelev's (1976) account of the Soviet species, and the accounts in Tutin et al. (1980) of the European species. These two treatments concur in many respects but differ in their treatment of rhizomatous perennial species with lanceolate glumes which Tsvelev includes in *Elytrigia* but Tutin et al., following Melderis (1978), include in *Elymus*. More extreme treatments have been suggested, although not fully developed, by Estes and Tyrl (1982), who suggested recognizing only two genera (Hordeum and Elymus) among the North American perennials, and Love (1982) who would recognize about 8 genera in the same group of North American species.

The existence of several differing taxonomic treatments is confusing to those who are primarily interested in the nomenclatural results of a taxonomic study for information retrieval. This is particularly serious in an economically important group such as the Triticeae for such groups are studied by individuals in many different countries, many of whom have only a minimal background in taxonomy. When several different taxonomic treatments are in use, such individuals may not recognize all the different names that have been applied to the taxa with which they work. Thus the exchange of information is seriously impeded by the lack of a consensus as to the most appropriate taxonomic treatment for a group.

An increased degree of consensus must, however, be based on a thorough review of all the data available on a world-wide basis if it is to find widespread acceptance. Unfortunately, the size and wide geographic distibution of the Triticeae, as well as the amount of data available, make it unreasonable to expect an individual taxonomist to acquire the necessary breadth of knowledge and understanding to achieve a satisfactory synthesis. Consequently, we are seeking funds to hold an international conference on the taxonomy of the Triticeae at Utah State University in the summer of 1984, during which those actively engaged in research on the tribe can share their knowledge.

The conference will be restricted to those who have an active interest in the taxonomy of the tribe. We have already announced our intention of trying to arrange such a conference to several research workers (Appendix 1) and have submitted a notice for publication in Taxon, the official journal of the International Association of Plant Taxonomists. Our goal in doing this is to give interested individuals the opportunity of modifying the direction of their research, if they wish, in such a way that would enable them to contribute more effectively to the conference.

The conference will be held at Utah State University so that the participants may have the opportunity of examining the worldwide collection of perennial species and hybrids of Triticeae that Dr. D. R. Dewey has established on the Evans Experimental Farm of the University. This unique collection will enable many of the participants to see a far wider taxonomic and geographic sampling of the tribe in living condition than would otherwise be possible. The conference itself will be held in the Eccles Conference Center on the University campus.

We are scheduling the conference for the summer of 1984. This provides sufficient lead time for us to arrange the conference and for the potential participants to modify the focus of their work, if they desire, so that it better suits the nature of the conference. It will also be an excellent time in which to hold such a conference in terms of several major floristic projects that have recently been started. These include Flora Australensis, the Flora of Meso-America and the revised Flora of North America. Such major regional floras exert great influence on the taxonomic systems used in the regions concerned. Thus it would be particularly advantageous to hold the conference before the relevant sections of these floras go to press. This is particularly true of the Australian and North American floras because the Triticeae are well represented in both regions. Incorporation of the findings of the conference in these two Floras would undoubtedly be one of the most effective means of disseminating its results beyond the realm of those working on the tribe to the users of taxonomic treatments, including both other taxonomists and such individuals as agronomists, range scientists, geneticists, and nutritionionsts. The proceedings will be published, possibly in the Annals of the Missouri Botanical Garden. The proposed date would also fit in well with the, admittedly very tentative, plan of holding a symposium on the taxonomy of the grass family as a whole at the International Congress on Systematic and Evolutionary Biology to be held in the summer of 1985.

In summary, therefore, we maintain that the proposed conference on the taxonomy of the Triticeae would benefit research workers in many disciplines by providing an opportunity to revise the generic taxonomy of the tribe on the basis of knowledge gained by many individuals working in different fields and in different parts of the world. We consider that 1984 is a particularly desirable time to hold such a conference because it will enable the results to be incorporated into major floristic works that are in the planning stages will aid in disseminating the findings of the conference.

TENTATIVE LIST OF TOPICS

1. Overview of the tribe and current taxonomic treatments.
   In connection with this introduction and to assist the participants in subsequent discussions, a synopsis of the tribe will be handed out. This will list most of the species currently recognized, together with the genera in which they have been included and their geographic distribution. A "comments" column will also be included for information presented during the conference.

2. Cytogenetic relationships.
   Two major talks are planned, one on the perennial species and another on the annual species. We would also include some shorter

talks on particularly interesting taxa or on karyomophological studies.

3. Chemotaxonomic data
   The primary emphasis will be on data from electrophoretic and immunological studies since these seem to be the areas in which most work has been done. Consideration will, however, also be given to data from other chemical compounds e.g. flavonoids, leaf waxes.

4. Morphology, micromorphology, and anatomy.

5. Phytogeographic and phylogenetic considerations.

6. Summary

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Boulder, 3 May 1983

Dear Askell & Doris:

Just back from Hawaii; had a good trip, got lots of collections, including a good phanerogam set since they did have good drying equipment. Have pictures of the volcano too, from the air in a small plane.

I sent the Rubia to your friend but never heard whether he got it. Will I ever see it again? I have to make combinations in the Linaceae (unless you will), and I need to know if you or anyone ever typified the genus Mesynium Raf. That ought to be clear before other names are changed. I saw an interesting Persicaria in Hawaii, the introduced "Polygonum capitatum", but can't find that it has ever been moved to Persicaria. Jim Hickman at Berkeley seems to believe in Persicaria. But he puts sagittatum in there.

I've been reading Cassini on Oligosporus, but what do we do about the woody American sagebrushes. No one seems to have suggested a genus for them.

Bill

? Polygonum.

Persicaria capitata (Hamilton) H. Gross, 1913, Bot. Jahrb. 49:277

(cf. H. Hara, 1966: The flora of eastern Himalaya, Tokyo, p. 70)

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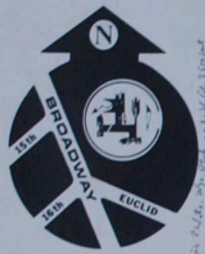

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Askell & Doris Love
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Open Monday - Friday, 9-5 ▪ Saturday, 9-4 ▪ Sunday, 10-4

Boulder, 6 May 1983

Dear Askell:

$x = 10$
(Sugiura [illegible])

I have to start on the Polygonaceae next. A note on one of your lests is that Tracaulon should be Truellum Houtt. (based on T. japonicum Houtt., 1777), which the Japanese call Polygonum senticosum Gross. I note that a Californian colleague puts T. sagittatum into Persicaria. If this were right, Persicaria would have to fall and be replaced by Truellum, unless conserved. Right? What do you think of the generic lines here.

B.

$x = 11$

P. senticosum (Maxim.) Fr. 2 Sav. (in Ohwi 1965)(p. 408). ($x = 10$, $2n = 20$)
= Chylocalyx senticosus Maxim. Persicaria senticosa (Maxim.) Gross
= Truellum japonicum Houtt.

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San José, May 18, 1983.

Dear Bill:

Weloome back from your extended Hawaiian holiday...and we are sorry that you continue to pass California without taking time to visit us properly. But you certainly have your reasons, though they are hardly a good excuse.

Thanks for your two cards that I left unanswered because I was completing a taxonomical paper on Acetosella for a Festschrift for Favarger and then also revising my long manuscript on the Triticeae, which seems to have been accepted not only in Folia Geobotanica that wants to divide it and in Feddes Repertorium that takes it in one part, despite its almost 200 typed pages, and adds directly the index. I believe it will become useful to the plant breeders everywhere, even in America, and that good taxonomists will appreciate the efforts, though I do not expect the creationists bunch here to listen to me more than they have done before...and some may feel cheated that their action that they still get impunity for has not silenced me, not yet. The paper will be printed next year.

I received a long letter from Holub that had been a month on its way, because when you mail matters from his Institute it goes surface mail, as before their socalled "revolution". He is working on his next New names list for Folia in which he transfers more Packera. He has evidently never received a copy of Phytologia that I sent him, and found out about our paper from the Compositae Newsletter which I do not know. So I listed our combinations in my letter to him and promised to ask you to send him a copy of the paper, or Phytologia...and also of your other good taxonomical and nomenclatural papers that he ought to know about, or perhaps even your good Flora, if still available. Since such literature sent to his institutional address will automatically be confiscated for their library, it ought to be sent to his Post Office Box 25, 11121 Praha 1, Czechoslovakia. Naturally, I asked him to send you a set of his taxonomical and nomenclatural works that are still available, and to try to wake up Soják not only for you but also for me, because several of his papers are certainly also of interest. But I believe Holub and Soják are not exactly friends, since the latter once stole a list of new combinations from the former, but hopefully he then has some other way of reaching out for the papers at the Museum.

To your questions: The Spaniard you sent Rubia to is not even an acquaintance of mine but a student of Contandriopoulos, who is working with the floras of the Spanish islands. I suppose it takes time for her to react to the fact that she already has identified the specimens and should return them, but a small reminder would not hurt. Even I am interested in the verdict about the correct name, because the R. peregrina has been such a collective dump that it is high time to clean it out...and it seems appropriate that a Spaniard does this...provided that the philosophy is right.

I do not know if Mesynium Rafin. has been typified, but that ought to be a simple matter, because of the five names he mentioned only one was a new description, M. texana, which is the var. berlandieri of B. rigidum (Pursh) Löve & Löve, the species of which most or all the Colorado taxa may be varieties, three taxa were only nomina nuda, and one was a transfer: M. mexicanum (HBK) Rafin. I believe that must be the type, and the basic number is 15.

It does not astonish me that Jim Hickman at Berkeley, or any American botanist except you, would mix Persicaria with other taxa of the Polygonum group and ignore the facts of basic chromosome numbers as the essential sign of generic status, because if there ever was a bastillon of creationism it is in botany at Berkeley.

Persicaria s. str. has x = 11, but *Truellum* has 10, and that is also the number known for *T. sagittatum*, of course. So ignore that kind of "advice". As to *Persicaria capitata* (Hamilton) H. Gross from the Himalayas and introduced in Hawaii it was transferred, on p. 277, by H. Gross, 1913 in the classical review of Polygonum s. lat.: Beiträge zur Kenntnis der Polygonum. - Bot. Jahrb. 49: 234 - 339. It may have been caused by the first world war that this paper was never reviewed for Kew Index, and the fact that so few have later worked on the group may have prevented its being called to their attention later...I never did, though I observed the omission already in Lund, where we cultivated a great sample of the collective genus and other genera of Polygonaceae and Eriogonaceae, which are not even remotely related though morphologists still keep both as subfamilies of the same group, despite the lack of ochrea and embryological distinctions of the latter. Perhaps you could write a note to Kew and point this out?

You seem to be right that nobody has suggested a generic name for the very distinct woody American *Tridentatae* section of *Artemisia*, but why should we expect such logical conclusion since no proper and good and critical taxonomist with biological philosophy has ever looked at the group here, because that breed has long been either absent or represented by foreign-educated botanists who normally are either starved of stabbed because their accent and smell...with full impunity, of course, because why should anybody show his ethics to prevent such cleansing of the field? If Rydberg had known the Asiatic-European taxa outside Scandinavia he would have reacted in that way, and so would Polyakov, who unfortunately does not live where herbaria with good American collections are situated. So, logically, you ought to give these plants the generic recognition they deserve, and at the same time gather vouchers of cytological determinations by Americans and Canadians as a basis for a proper revision of the species themselves, because the reports of more than a single chromosome numbers for the socalled species is either caused by ignorance of morphological distinctions at the species level or simply by misidentification by sloppy or incompetent taxonomists who are only bad florists. Or who have the same kind of inability to see or understand modern biological philosophy, as, e.g. Keck and Beetle. It is a shame to let this problem stay unsolved any longer, and you are the one to solve it properly...we planned to do it but never got sufficient material because of lack of travel funds and funds for technical assistants at the inferior institution that still has not succeeded in suffocating you or chase you away with invented "witnesses". Only one of many problems that you still can solve...but just because of that I am always somewhat wary when you spend your limited time for pleasure travel to Hawaii, Galapagos or even the Siberian mountains...though I understand your need for such recreation. But sometimes it almost looks as when Hultén had let the Stanford businessmen lure him away from his important world maps...though he nevertheless succeeded in completing them. So I am confident that you also will do the revisions nobody else will be able to here for the next century of creationism in botany and stabbing of those with other ideas, an American pastime?

We have been hearing about the snowstorm you suffered, but hope it has not destroyed too many trees or broken houses and other artifacts. We have at long last gotten summer weather this week after a cold and wet winter, which may be only the first one in a new row of "normal" winters in California. Arctic Europe still has a strong winter, and the ocean now is too cold for hatching the fish in southwestern Iceland, we are told...and central and southern Europe still are looking for the spring, and so is Britain. But Russia and Scandinavia have spring... sometimes I would like to have funds to visit them again and to enjoy spring where it always will remain most pleasant in my memory.

All the best to you both from us all here in the far west,

As ever,

San José, June 29, 1983.

Dear Bill:

Thanks for two good letters, one card, and the first installment towards a New names paper IV. I am sorry that I had to spend considerable time for an evaluation of a couple of fine younger colleagues who have applied for a professorship at an evidently good Nigerian university. Hope you forgive the delay.

It surprised me to see that the third paper in the good deries has been published, because when I saw the first blurr of it I had a reason to expect it to include not only some new combinations in Psilochenia but also a new generic name of the also biologically distinct North American-Asiatic Crepis sect. Ixeridopsis for which you had asked my permission to use my name. I gave you the nomenclatural details in early March, in case you do not have Babcock's monograph. You must, however, have abandoned the idea since now you propose to put my name on the endemic Utah-Colorado Festuca dasyclada, which in my opinion seems to be a good endemic biological species, although still even cytologically unknown, but certainly not generically distinct. So that honor I hope you permit me to decline.

To other details...not to forget to ask for a copy of III, when you get it.. Your Aletes observations do not astonish me, since I have seen on Constance's arrogant treatment of some European Apiaceae that his concepts are muddled and his taxonomic eye absent...just opposite to what I know about your...thanks to your early genetical training that prevents you to mix with creationism-pheneticism that he adores. The same for Melica and Bromelica, which are genomically distinct in a way similar to the diploid genera of the wheatgrasses; the latter is close to the Californian Lophochlaena, which Bentham and still all Californians insist to identify with the arctic Pleuropogon of the Glycerieae (basic number 10). In connection with a study of Lophochlaena (did I send you the reprint?) I came to the, unpublished, conclusion that Meliceae (basic number 9)also ought to include as genera some of the other socalled sections of Melica, which clearly differ in their haplomic constitution, i.e. Husnotchloa (Maire ex Tzvelev 1973), and Beckeria (Bernh.) Ascherson, but not Penicillares Hempel, which to me seems to belong to Bromelica. And perhaps some austral genera as well, though these taxa I do not know enough yet. So you have my sincere encouragement.

In the New names iV, you refer to us as Love & Love. Please, avoid that; if you cannot add the dots over the o which is essential as you know so well, you could transcribe the ö with oe...I never cease to wonder what kind of wisdom it is when ignorant or arrogant American inventors of important printing material and equipment work on the basis of the incomplete Latin alphabet....but why not when they also continue to use the Fahrenheit and inches and pounds! It is not wisdpm that gives the Americans world dominance, but arrogance, money and force.

The division of the Eurasiatic Linum was correctly made by Reichenbach, who followed the evolutionary definitions by DeCandolle and Lamarck, who were, naturally, ignored for two, and probably many more, generations by the English-American arrogance that later gave up and forgot them for their Darwin, of course. Exactly as they fought the German idea of continental drift and still try to "replace" it with a newer explanation that is an improvement. Rafinesque, who followed the same European principles (as Rydberg did later) and so was persecuted by the rich American botanists of his, and later, time, recognized the distinctions of the eastern American genera. But Small was gravely mistaken in "widening" the perfectly defined small Eurasiatic Cathartolinon to include the certainly very remotely related American taxa. As far as I know, your "Linum schiedeanum group" shares the basic number 9 with Adenolinum. I do not know it well enough to guess if it may be genomically distinct, but then it is nameless. Perhaps the most reasonable procedure for the time being may be to recognize it as a section of Adenolinon and transfer the taxa to that genus, at least for the time being?

I am still waiting for my first copy of the English edition of the Icelandic flora, and have asked the publisher to send you a copy immediately after its publication. Perhaps the severe economic situation in Iceland is delaying the printing, they are now feeling the sting of the stupidity of accepting endless American bribes or sovalled loans to keep up a "free enterprise" standard, in addition to the fact that the ocean around Iceland has cooled considerably, because it does not realize the effect of the "greenhouse effect" of some important and wise Washingtonian scientists. A week ago, yes around June 20, there was a severe snowstorm all over the northland, so the numerous tourists who came with the autoferry from Norway to eastern Iceland could not get further for a week. Even the codfish has not hatched for at least two years now, and the newborn lambs in northern and western Iceland succumbed in masses. The new conservative government of the rich for the rich started with a drastic devaluation and cutting of index payments on salaries and of the support to the poor...in a perfect Reaganian style. and with taking nwe loans to secure the selling of the country as a permanent base for the aggressive conservatives here...no more politics, it is everywhere rotten.

We continue to play with the grasses to get the time to go, but the little work that Lóa and Doris have had to keep the wolf from our door together with our sonstantly dwindling small savings, has dried up almost completely towards a complete unemployment. As could be expected. Though I admire your perseverance in trying to modernize American plant taxonomy by some silent aid from me as a kind of an encyclopedia, I still hope that sometimes, not too late, you will put that same perseverance towards helping us since that you asked to be permitted to do, with tears in your eyes, almost five years ago, when you got me to give you the material that was aimed to the ethically very strong Jack Fogg. Now he is no more so you stand alone as our hope. Excuse that I sometimes tend to forget your sensitivity when I mention such matters that are increasingly important to us in this country of miscarriage of justice and dominance of justice by money that we do not have for the greedy and lawless lawyers...excuse the outburst.

I hope you will soon find a publisher for the good floras that must get printed to show that at least some American botanists remain scientific rather than creationists. I have mention this again to Carver, who is powerless when his superiors take the advice of conservatists as John Thomas etc., but he had no new proposal even for another publisher. But somewhere some such must be found, perhaps even the University of Toronto may be interested, since they are printing the new Alberta flora and have published an arctic flora earlier... or the Cambridge University Press, which has an office in New York and a good circulation service in the United States? Or some smaller publisher in Denver, -s the one that published Harringto's large flora years ago? I wish I were rich so I could offer such support that any publisher would come forward....

We read and see about the Colorado floods, though these do not seem to be at the eastern slope this time. Here we enjoy a cool summer, though our fruit grows well and also our vegetables, so Doris and Lóa have been canning extensively. And I caring for the plants, just now I am working on taking away the old canes of our Rubus cultures and replacing them with next year's canes...this year we got very much blackberries of the most delicious kind and want them next year too...

With the very best regards and all good wishes from us, who miss your visits.

As ever,

Comments? This is my parting paper. More to be added.

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.

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.

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.

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.

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

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Boyle, W. S. 1945. A cyto-taxonomic study of the North American species of Melica. Madrono 8:1-26.

Love, A. 1982. IOPB Chromosome number reports LXXV. Taxon 31:342-368.

Rogers, C. M. 1968. Yellow-flowered species of Linum in Central America and western North America. Brittonia 20:107.-135.

Small, John Kunkel. 1907. Linaceae, in North American Flora 25(1):67-87.

Tzvelev, N. N. 1976. Poaceae URSS. Editio "Nauk", Leningrad. 788 pages.

I only wish that you were as energetic in helping us get a ~~[illegible]~~ more energetic and courageous [illegible] to solve our problems and secure us a freedom from the long miscarriage of justice and cheating of ~~[illegible]~~ my promised University [illegible] and [illegible] — and that I be treated at least as Heiberg, Rogers, Shushan ~~[illegible]~~ ~~[illegible]~~ or simply as a scientist and not as ~~[illegible]~~ an American criminal based on American perjury. I do not know the judicial ~~[illegible]~~ word for what the Smithsonian, embassy and Yugoslavs did to us, but Prof Schroeder would know, if you contacted her correspondingly. With love, if you want to be told in the end.

at least so that I get the promised University [illegible] and cease to be treated by those crooks as a common American criminal, much worse than ~~[illegible]~~ [illegible] who kept me scientifically [illegible] at Boulder.

Boulder, 21 June 1983

Dear Askell:

$2n=36, x=9$, (hexaploid)

Help!!! Our western "*Linum schiedeanum* group" which includes the Colorado *L. kingii* can't be related to the European *Cathartolinum*, can it? So it probably is an endemic southwest-Central and South American group, probably without a name. The European *Linum* look as if they can be broken apart, but I'm not about to open that can of worms. I have just put together a discussion of *Aletes* and am going to transfer a number of them out of *Lomatium*, *Cymopterus* and *Pteryxia*. Will send you more copy when I hear from you on *Linum*. Also, *Festuca dasyclada* is a fine monotypic genus, and maybe this is the one I want to make *Askellia*. It will stand up. Want to get to putting words on paper about it soon.

Bill

B.D. Harris 1968: Chromosome numbers and evolution in N.A. species of Linum. — AJB 55: 1197-1204

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Askell Love
5780 Chandler Court
San Jose CA 95133

Do not have the third paper in the series; only the first copy that I received was incomplete, since [illegible] was of the impression that it should have included not only Psilochaenia (~~[illegible]~~ 9 spp. [illegible] x 7) but also the 7 genera of the [illegible] American other American Crepis for which you asked the permission of using my name, ~~[illegible]~~ since it is a perfectly distinct genus of American-Asiatic distribution. Now you want to use my name instead for a highly doubtful genus of an endemic Utah-Colorado Festuca dasyclada — ~~that I decline~~ please, permit me to decline that.

United States Department of Agriculture
Agricultural Research Service
Western Region
Arid Southwest Area
Crops Research Laboratory
Utah State University - UMC 63
Logan, UT 84322

February 7, 1983

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

Dear Askell:

I hate to admit it, but only now have I gotten around to reading your paper on "Relationships and taxonomy of New Zealand wheatgrasses." I must say that I was really impressed with the thoroughness of the paper. At the end of the Introduction you suggest that you resumed your work on the taxonomy of the Triticeae because of my instigation. That is certainly a credit to me if indeed I helped renew your interest in the tribe. It would have been a great scientific loss if you did not publish the vast backlog of information that you have accumulated over the years.

I had no idea that you had made so many interesting hybrids between the New Zealand species and species from other parts of the world. How many other hybrids have you made and analyzed but have not published? If you have data on other hybrids, you should publish them. There is no point in repeating the hybridizations that you have made already.

Maybe I have asked this before, but can you tell me why you think the taxon that I have carried previously as Agropyron leptourum is really A. stewartii? Bor lists both A. leptourum and A. stewartii in Flora Iranica, but he states that A. stewartii "has not yet been reported within the borders of this flora but as it occurs in neighboring countries it is more than likely that it will be found." I collected A. leptourum in Iran, where it grows quite abundantly. Bor himself identified several PI accessions that I used in my studies as A. leptourum. All of our A. leptourum accessions are 2n=42. I know that Tzvelev lists A. transhyrcanum (=A. leptourum) as 2n=56, but I have doubts about that.

I observed the type specimen of A. stewartii while I was at Kew in 1972 and I have a photograph of it. While at Kew, I took some notes on each species and I am enclosing a copy of my notes on A. stewartii and A. leptourum. At that time I thought that A. stewartii belonged to the caespitose species of Elytrigia (your Pseudoroegneria). After looking at the photograph of the type specimen, I think that A. stewartii is an S=genome species.

I also have a photograph of the type specimen of A. leptourum (=Elymus transhyrcanus) and it looks exactly like the plants I am calling A. leptourum. Furthermore, the type specimen of A. leptourum (Borisova 725) comes from Askhabad U.S.S.R., which is just across the border from Iran, where I collected A. leptourum. So I have every reason to think that our 2n=42 plants are indeed A. leptourum (=E. transhyrcanus). I suppose the correct name is Elymus transhyrcanus (Nevski) Tzvelev. Unless I can be convinced otherwise, I will use the name E. transhyrcanus for the plants I had previously called A. leptourum.

I still have hopes that someday we can bring you to Logan for a lecture and consultation. I am sure that we could resolve a lot of taxonomic questions if we spend a few days together.

Sincerely,

Doug

DOUGLAS R. DEWEY
Research Geneticist

San José, February 15, 1983.

Dear Doug:

Excuse my tardiness, but I am trying to complete a difficult paper on Acetosella taxonomy for a Festschrift to one of our European colleagues so I waited a little to look the matters closer up. But since I know you also cannot wait, I took time off for it today and found the following:

As to the authors of Agropyron cristatum, all the Russians seem to use (L.) P. Beauv. following Tzvelev 1970. Although I am not quite sure of his reasoning for this selection, I believe it is connected with the opinion that Gaertner actually did not see Siberian material and so transferred the name as if it was the European plant, which is pectiniforme. If I am right in this assumption, then Tzvelev also was mistaken, because when Gaertner transferred the name, it included also pectiniforme, because Linnaeus used it collectively. The correct nomenclatural reference then must be (L.) Gaertner, as clasdically done, and I will, of course, correct that in my manuscript...and expect that even Holub will observe that mistake and insist upon its correction.

In connection with my use of the name E. stewartii based on A. stewartii Melderis, this is a mistake caused by my simplicity or gullibility and my sometimes trust in others so I do not check all the way back to the original description. When I got the seeds under this name in Montreal 1961 from Bor, I was of the understanding that the material from which the seeds had been collected had been thoroughly checked by Melderis and again by Bor himself. Therefore I did not go back to Bor's good Grasses where the description is found....and I see very clearly now that it fits only some plant that belongs to Pseudoroegneria. My material, however, grew into plants that according to my notes was characterized by the diagnostic characters of Elymus, or Roegneria in the meaning of Nevski I believe, and I also noted that most of the characters were similar to what I then called A. leptourum, disregarding the older name transhyrcanum. According to Tzvelev, this is also synonymous to Elytrigia vvedenskyi from Uzbekistan, which I have not seen. In other words, I will try to express through synonymy in my synopsis that this mistake has been made in our New Zealand paper, though I do not know, and have no possibility to check, what species of the transhyrcanum complex actually was involved in my crosses. But even your leptourum with 42 chromosomes must have been something else, perhaps another species of the same taxon that I got from Bor as stewartii? since the real transhyrcanus from Turkmenistan has 56 chromosomes, as published by the cytologist Chopanov and the good taxonomist Yurtsev in 1976: Bot. Zhurn. 61:9) and that number I have checked myself on seeds from the same locality sent to me XXXXXXXXXXXXXX by Yurtsev himself in 1975. Perhaps this should only be taken as a confirmation of that this is a complex that still needs a thorough taxonomical revision as so many other groups of Elymus from Asia do, and an indication that we ought to accept each other's identification with a grain of salt? The future colleagues will do better. But this is one of the reasons for that I have been hesitant to make a list of all my crosses...though even uncertain list may be valuable.

I appreciate your thought of bringing me to Logan for a talk, but perhaps the best thing would be some Socratesian discussion that students could enjoy also, mentioning especially the philosophy and practice of taxonomy in genetical philosophy? Let us think.

All the best,

Agr. cristatum Gandar. R.D.
R.D. ~~St.~~

A. leptourum — Stavatis
56: 42

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Tzvelev's 1960 paper (Or. Mat.)
Nadis 1983 Stipa paper.

NOTES TAKEN AT THE KEW HERBARIUM

May 17 to 27, 1972 D. R. Dewey

EURASIAN AGROPYRON

CAESPITOSE SPECIES OF THE SUBGENUS ELYTRIGIA

AWNLESS SPECIES OF THE A. TAURI-LIBANOTICUM GROUP

1. A. tauri Boiss. & Bal. Fifteen specimens from Turkey, Iraq and Iran. Several look like PI 228389, but most have less pointed glumes; others have squarrose glumes. One specimen from Iraq had 3 determinations, all by Melderis: 1) A. elongatum 2) A. cognatum, 1962 and 3) A. tauri, 1966.

The type folder contained 3 specimens: 1) Th. Kotschy 536, 1836-relatively long acute glumes, 2) Th. Kotschy 536a, 1836-squarrose glumes; and 3) Balansa 826, July 11, 1955 (Melderis cites this as the type in Flora Iranica). Its glumes have hyaline margins and are relatively long and acute. Not greatly different from PI 228389. I photographed Balansa 826; it comes from Turkey. I also photographed an Iranian specimen collected near Aligudarz (2600-3000 meters) in 1969 and determined by Melderis. Melderis in Flora Iranica places A. pertenue in synonomy with A. tauri.

2. A. libanoticum Hack. Twelve specimens from Iran, Turkey and Lebanon, includes our specimen of PI 228389 plus PI 228390 and PI 228391. The glumes are acute-acuminate and unequal. The type specimen, Hartman 480 from Lebanon is very similar to PI 228389. I photographed it. Also photographed Davis 44707, 10 June 1966, Hakkari Prov., Turkey.

3. A. caesnitosum C. Koch. Fourteen specimens from Iran, Iraq, and Turkey. All have squarrose glumes. The spikes look like miniature A. intermedium. I photographed Davis 46692, 17 July, 1966, Kars Prov., Turkey, determined by Melderis; its glumes are not truncate. Melderis in Flora Iranica places A. angulare Nevski, A. armentum Nevski, and A. firmiculmis Nevski, in synonomy with A. caespitosum.

4. A. nodosum (M.B.) Nevski. Two specimens from the coastal area near Yalta in the Crimea. I photographed Davis 33619 collected near sea level at Yalta; determined by Melderis.

5. A. scythicum Nevski. A very close relative to A. tauri. The co-type specimen--H. Poplawska 150, 21 June 1929, from the Crimea was orginially labeled A. tauri. I photographed the co-type and also--Davis 33234, 1 June, 1959-near Yalta at 100 m. Jaaska (letter) says this is the awnless form of A. strigosum.

6. A. ferganense Drob. One specimen from USSR photographed. Similar to A. tauri but apparently taller. Melderis in Flora Iranica says A. ferganense is a synonym of A. cognatum.

7. A. pruiniferum Nevski. Photographed one specimen from USSR collected July 13, 1951. Its glumes are rounded.

8. A. dshungaricum Nevski. Photographed a specimen collected July 16, 1959 in Kazakstan USSR. Melderis in Flora Iranica says this is a synonym of A. cognatum.

9. A. cognatum Hack. Photographed one specimen from western Tibet. Determined in 1961 by Melderis. It has awn-tipped glumes and lemmas. This taxon is apparently an eastern relative of A. tauri. Melderis in Flora Iranica places A. ferganense and A. dshungaricum in synonomy with A. cognatum.

10. A. stipaefolium Czern. PI 325181 from the USSR was determined by Melderis in 1970. I photographed this specimen.

11. A. kosaninii Nab. Photographed one specimen collected June 3, 1969 around Abadeh at 3000 meters in Fars Prov., Iran. Determined by Melderis in 1970. Two other specimens were observed.

12. A. geniculatum (Trin.) Korsh. Photographed one specimen from the type folder from Siberia. It has awn-tipped glumes and lemmas. Jaaska (letter) says it is the awnless phase of A. aegilopoides; or the equivalent of A. inerme.

13. A. stewartii Meld. Photographed type specimen--Stewart 20704, Aug. 15, 1940, western Kashmir. This species may not belong with this group. Melderis in Flora Iranica puts it in with some Roegneria species.

Other taxa that apparently belong to the above group include: A. gracillimun Nevski, A. sinuatum Nevski, A. setuliferum Nevski, and A. sosnovskyi Hack.

SELF-FERTILIZING ROEGNERIA SPECIES

49. A. caninum (L.) Beauv. Very large collection from England to Asia. Considerable variation in awn length and spike density. The "A. caninum" collections from China and Japan look like A. ciliare or A. tsukushiense.

50. A. donianum F. B. White. From Scotland. Looks like our A. donianum. Melderis (pers. comm.) considers this to be a mutant of A. caninum, possibly a subspecies.

51. A. biflorum (Brign.) Roem. & Schult. Looks like an awnless A. caninum. It occurs in Europe and Asia. Photographed 2 specimens. Close to A. donianum.

52. A. violaceum (Hornem.) Lange. From northern latitudes of Scandinavia. A purple broad-glumed awnless or awn-tipped species with compact spikes. Some sheets were labeled A. latiglume, A. violaceum var. latiglume or Roegneria borealis.

53. A. latiglume (Scribn. & Smith) Rydb. Photographed one specimen from Lappland. Bowden places it in synonomy with A. violaceum. Nevski puts it in synonomy with Roegneria scandica.

54. A. mutabile Drob. Similar to A. violaceum. Photographed one specimen from USSR. Melderis in Flora Iranica puts A. angustiglume in synonomy.

55. A. angustiglume Nevski. A robust species with relatively broad awn-tipped glumes and lemmas. Photographed a duplicate type specimen from USSR determined by Nevski. Nevski places A. mutabile var. scabrum in synonomy.

56. A. fibrosum (Schrenk) Nevski. A broad-glumed species with slender flexuous spikes. Our material appears to be properly identified. Observed 2 specimens from Finland and photographed 2 from USSR. Observed 3 specimens at the British Museum.

57. A. leptourum (Nevski) Grossh. Sixteen specimens from Iran and Turkey including PI 229520, 229927, 229910, 229922. Photographed co-type, Borissova 725, Aug. 25, 1931. Our material is properly identified.

58. A. brachyphyllum Boiss & Hausskn. No specimens at Kew, only photographs of the types, both from Iran. The glumes are awnless and the lemmas have awns to 1 cm. This appears to be an awned formed of A. leptourum.

59. A. praecaespitosum Nevski. PI 314622 was determined by Bor from an SCS specimen. I photographed it. Melderis in Flora Iranica puts A. ochense in synonomy.

60. A. ugamicum Drob. PI 314631 was determined by Bor in 1969. Two other USSR specimens looked like PI 314631. Nevski puts part of A. dentatum in synonomy.

Boulder, 21 Aug. 1983

Dear Askell:

I am hoping to get back to the new combinations soon now, and plan to deal with the two little Crepis species. But a bigger thing is what to do about the shrubby Artemisia. Since you have taken up Oligosporus Cassinii (with O. campestris the type), we must also take out the Subgenus Seriphidium of Besser ex Hooker, Fl. Bor. Amer. (typified by A. cana Pursh). Cana and the Tridentatae form as clean a group as one can find in the composites. What for a name? I am thinking it should be short and easy to remember, and with a meaning if possible. Since these woody things are absolutely characteristic of the western American desert-steppe, what would you think of Steppea? It has not been used.

Bill

also, A. bigelovii really belongs with "Steppea".

P.S. Cassini's Abrotanella doesn't seem to be related altho Hooker listed it as synonym of Sect. Seriphidium!

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Boulder, 26 Aug. 1983

[illegible]

Dear Askell:

[illegible] J.L. Strother [illegible]

The Iceland book came this morning; I shall be interested in the reviews you get from the dinosaurs. We should call it "Love's White Paper"! Reviwers' comments might give me some ammunition. Maybe someone will ask me to review it. If so, I can say some things. I find that H.M.Hall gave a very thorough defense of the conservative generic point of view, and that will be a good starting point for my discussion of "Steppea". Do you have a better name? Still no resolution of the space problem, but we are still working. As usual, they try to find inadequate space that costs them nothing. Yes, we have had Wolf for a long time, because Barry had to have it to do his work on Potentilla.

B.

A. [illegible] A. Gray: [illegible] Antheridia! [illegible]

[illegible] Fl SSSR 1 ([illegible])

[illegible] Suppl. 14.

[illegible] E. [illegible], [illegible],

[illegible] (Ross) [illegible] (60 [illegible] As.: [illegible] Willis) 1961, [illegible] 1972, p. 94. [illegible] Fl. SSSR [illegible] (L.) [illegible] Fl SSSR 11: 171

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A. [illegible] x=?

P.P. Polyakov, 1961: Materialy k sistematike roda polyni - Artemisia L. - Trudy Inst. Bot. Akad. Nauk Kazakhskoi SSR, 11:134-177.

[illegible]

Open Monday - Friday, 9-5 ▪ Saturday, 9-4 ▪ Sunday, 10-4

San José, September 2, 1983.

Dear Bill:

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

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

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

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

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

San José, February 21, 1984.

Dear Bill:

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

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

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

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

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

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

<u>Minuopsis nuttallii</u> (Pax) L. & W. or W...comb. nov., based on Arenaria nuttallii Pax Pax, 1893, Bot. Jahrb. 18:3o, in obs.
Arenaria pungens Nutt., 1838, in T. & G. Fl.N.Am.1:179, non Clemente 1816, in Lagasca, Gen. et spec. plant.:15;
Minuartia nuttallii (Pax) Briquet, 1911, Ann. Cons.Jard. Bot. Genève XIII & XIV:385;
Minuartia pungens (Nutt.) Mattfeld, 1921, Bot.Jahrb.57, Beibl. 126:28;
Alsinopsis occidentalis Heller, 1912, Muhlenbergia 8:96..

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

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

All the best,

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

Citation Classics, Who's Who in the World.

Boulder, 8 Feb. 1984

Dear Askell:

I'm terribly sorry about the pagination error, but you know that kind of thing is the hardest error to try to catch. I called Kathleen right away, and she went to work and inquired, but it was too late. You should get credit for the most concise flora ever written if Iceland. People who buy it should be pleasantly surprised.

Yes, we are talking and trying to get hold of information.

We now have "Arenaria" nuttallii in Colorado, so I am going to have to have a name for it. Are you planning to do something about that one? I would rather you do it than I, since you know so much more about the groups. Somewhere I have McNeill's table of relationships but I don't know if I can find it easily.

Sincerely,

Bill

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State Department human rights language (S.J. Mercury-News 2/12/84, A2):
(for killing): Unlawful and arbitrary deprivation of life
(or of facilities, house, salary, livelihood)

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