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Nauðsynlegar breytingar á 2. prentun 2. útgáfu:

s. 42: Dryopteridaceae — undir: Aspidiaceae: 70

s. 50: Víxl á L. clavatum og L. annotinum: leiðrétt — sé síðan undir L. clavatum, [illegible]: [illegible] L. clavatum [illegible].

s. 52: 2n=272: H. selago

s. 74: D. austriaca → D. dilatata

D. assimilis → D. expansa

s. 78: Čelak. → Čelak.

s. 80: E. palustris ssp. [illegible] (Braun) C. Hartm. 2n=24.

s. 88: E. [illegible] [illegible].

s. 93–94: [illegible]: [illegible]

s. 96: G. fluitans var. [illegible] A. Löve?

s. 98: Hádæli

s. 108: Freravorfgras

P. [illegible]: Sjaldg. á NV, N og A.

s. 114: [illegible] [illegible].

Elytrigia → E. (repens) + [illegible] A. Löve ([illegible])

s. 118: L. [illegible] (Sjaldg. [illegible]) [illegible] Ak og NV.

s. 122: [illegible]

Evolution is the process of diversification of living beings, but it is also the process that decides about the future of any new combination of living matter and selects what is to be discarded of the endless trials of life. Its basic law at any level is the law of mere chance, and that law also decided when the chemicals of life were formed and united eons ago. We have a reason to believe that the first living being had only a single combination of DNA, or of what we are used to call genes, but by duplication and certain changes, which we prefer to call mutation, this was enlarged and long later resulted in ~~one chromosomes~~ ~~a chain~~ ~~one~~ a chain of DNA, which we call a chromosome. ~~a long time~~ The DNA molecules, or their chromosome chain could soon mix and combine in what we call the sexual process, which made it possible for them to increase the variability of the organisms tremendously within a short period of time, and before very long the viruslike original organisms had developed into cellular organisms, which then united and formed the multicellular organisms all the way to what we have in this room and all around us in the living world.

We can speak about the processes of evolution at every level of life, but they have been best studied among higher plants and animals. We can describe them in somewhat different words as being based on gene mutation, which increases the variation by reshuffeling or duplicating the DNA material on the chromosomes; genetic recombination, which through the sexual process combines the new genes with the old and tries all kinds of gene combinations every generation; and natural selection, which decides, by aid of environmental influences, what combinations are to survive, because the new combinations are much too many to get space on the earth. These processes, which have been termed subspeciation, require complete interfertility of all the participants in what we like to call ~~a~~ a gene pool, and they are basic for ~~all~~ the production of all kinds of variability.

If only one gene pool had been created, then all evolution had stopped when the variability within that pool had reached an equilibrium with the environment in which it lived. Further evolution was made possible by the creation of new gene pools by aid of what we term reproductive isolation, which conserves gene combinations and prevents beneficial combinations from being mixed away and lost again. We must admit that we still do not know how this was accomplished at the lowest levels of life, before chromosomes were arranged and sexuality accomplished, but after that stage was reached, reproductive isolation, or speciation in the strict sense of the word, has been produced in one of two ways, gradually or abruptly.

Gradual speciation, which characterized the differentiation of species at the same level of ploidy, benefits from the fact that at meiosis corresponding chromosomes pair at their entire length to make crossing-over and gene-exchange possible. Any small change in the arrangement of genes on the chromosome, or in their amount, affects this pairing adversely, and if large segments of the chromosomes are afflicted, for instance ~~by~~ through ~~inversion or~~ socalled inversion or translocation, duplication or deletion, all pairing may be prevented, with sterility as a result. Frequently this kind of speciation ~~~~ requires the accumulation of such chromosomal changes, thus its name gradual, but it is ~~~~ speeded up by hybridization ~~within gro~~ between groups that are heterozygous for such changes.

Abrupt speciation, on the other hand, is effectuated by the duplication of the entire chromosome set of an individual, which, thus, suddenly gets not only twice as many chromosomes as the original gene pool, but also twice as many genes. In this case, reproductive isolation is caused by the uneven number of chromosomes in a hybrid between the diploid and polyploid populations, resulting in an uneven distribution of vital genes, and the inability of such hybrids to reach a genetical equilibrium at ~~the~~ either level of ploidy. Contrary to gradual speciation, which requires several generations, abrupt speciation is instantaneous and extremely effective, and since it also results in a duplication of all the genes of the old species, it immediately is followed by changes in morphological and physiological characteristics that influence survival and dispersal and, thus, distribution of the new ~~[illegible]~~ ~~[illegible]~~ species.

This carries us to the subject of distribution of polyploids, which is only a small part of all the various kinds of phenomena that are affected by this condition, but a very important part. It was observed by the early students of polyploids, that they tend to have areas that are distinct from those of their diploid relatives, and also grow under somewhat different ecological conditions. However, the real significance of this was not observed until when the Swedish botanist Täckholm, in 1922, pointed out that in the genus Rosa the species growing in the far north are high polyploids, and also when the Danish botanist Hagerup, in 1927 and 1928, observed that in the genus Empetrum and in the Ericaceae species growing in northern regions tend to be polyploid, whereas diploids are typical of more southern lands.

It was also Hagerup, in 1931, who brought out the hypothesis that since species with higher chromosome ~~are usually~~ numbers are usually the ones growing farther north, and, thus, more exposed to extremes of temperature, an increase in the frequency of polyploids should be observable within the floras as a whole with an increase in the extremeness of the climate. That hypothesis was the beginning of the constantly growing literature on the geobotanical significance of polyploidy, which, I am sure, has only taken its first steps as one of the basic methods of historical biogeography.

~~(Let us first look upon the evidence)~~ Since the literature in this field has become too extensive even for a short review, I have compiled it into a list, which is being distributed to the audience. Also, because it would be difficult for you to remember long lists of numbers that show how the frequency of polyploids changes with latitude and altitude, I have also compiled tables of these frequencies and distributed them to the audience, in the hope that you will find this to be more convenient.

Let us look a little at this evidence. Even a fast look at the tables clearly supports the generalization that the frequency of polyploids in any given flora increases with an increase in latitude and altitude. This will be even more convincing if the data are subjected to statistics of any kind, simple or sophisticated. Although the phenomenon is complex and far from being simple, evidence from both the northern and southern hemispheres ~~are~~ is in support of the generalization. Without going into the details that led to this conclusion, I would like to emphasize that the frequency of polyploids in the angiosperms of the floristically rich Tropics is around 20-25%. In the deserts where the species number is much lower, the frequency ~~[illegible]~~ increases to about 37-38%, but decreases again to around 35% in the ~~[illegible]~~ again enriched floras of the Mediterranean climates. From there on the gradual decrease in the number of species in the floras is followed by a gradual increase in the frequency of polyploids with an increase in latitude, up to the drastic 75-85% of polyploids in the cold deserts of the high-arctic regions. As a glance at the tables shows, this is apparently the case in Africa-Europe, which is the best known region in this respect, but it is also evident on the Atlantic islands, eastern, central and western North America, and eastern Asia.

Although much fewer observations are available from southern regions, the report of about 35% polyploids in the Peruvian lowlands and of about 86% on the Macquarie Island in the Antarctic Ocean seem to allow us to the preliminary conclusion that similar conditions should prevail also in austral ~~[illegible]~~ floras.

Dear Bill:

Sorry about Turaniphytum, but A. Potaninii waits. A. scoparia has 18 chromosomes according to Vines & Richter (1966), who correct 16 given by Vines & Hollick 1962, so it must have a crossability barrier towards A. potaninii. Lack of the paper, I do not have it, but remember they discussed similarities, unaware of the fact that in taxonomy only differences count.

I do not have the originals of Besser's papers, neither do I have Hooker's Brit. Flora, or still less Index Nom. Gen., so my references are second hand, except those of Polyakov. It seems to be true, that there was a printing error in the name of the section in Besser's 1829 paper, Seriphida for Seriphidium, the latter being a grammatical variation, as, e.g., his Dracunculi and Abrotanum, which are simply to be corrected to Seriphidium, Dracunculus and Abrotanum, as he seems to have done 1834, though Lessing (1832) used the form Seriphidium, according to Dalla Torre & Harms 1900–1907: Genera Siphonogamarum, p. 560. To reject the name but permit Hooker (1833) to use it for what the editors of the Index select as a new lectotype of a species certainly unknown to Besser and certainly belonging at least to another subsection, as DC (1837) felt, cannot be correct or logical, though it is typical of the arbitrary methods of the group forcing their way through the Index. Since no types were actually selected until when Polyakov (1961) lifted the section (or subgenus Seriphidium (Besser) Peterman 1848) to the level of a genus typified by S. maritimum (L.) Polyakov, which certainly was the (Linnaean) species best known to Besser. If the American and Dutch teachers continue to force such decisions upon the taxonomical public, the result is likely to be an uprising against the otherwise good concept of the type causing its demise. But I am fairly sure that moderate and logical men as J. McNeill at Ottawa and Dan Nicolson of the [illegible] will agree, that the Polyakov selection must be followed and the Ind. Nom. Gen. proposal rejected, and also their rejection of the Besser original spelling in favour of the Hooker [illegible] of the corrected spelling, because both are absurd. You could put the problem before McNeill and Nicolson if you so feel, but it would astonish me if they threw out Polyakov's selection.

In case Polyakov stands, there is no problem with either subgenus (cf. my last letter). Otherwise, I would forget about the typification, and also only mention that the American plants belong to another section than the European ones, described as Artemisia sect. Seriphidium Besser 1829 & 1834, & Tridentatae by DC. (1837). p. 105. But if you want to reject the Index Nom. Gen. typification downright, adding the remark that Hooker only corrected a misspelling but did not realize that the two American species listed do not belong to Seriphidium s. str. — that De Candolle was the first to observe. Hope I am not confusing you, I could discuss it more sensibly eye to eye, but have lost the well-known ability of certain Icelandic-Irish fairies to see through the mountains.

Summing: transfer the American species to Seriphidium (Besser) Polyakov, but mention that they may belong to another section or subgenus as observed by DC (1837), and that the arbitrary lectotype selection by the ING authors, ignoring sensible correction of a printing error by Besser (1834) and the correct selection of S. maritimum as the generic type by Polyakov (1961). Typification should never be done by other than specialists on each genus, and certainly not by young turks — or little trained Americans.

By the way, according to Gyorgyomu (1973), Additamenta, p. 94, Handrych, 1966, has written something about the problem in Novit. Bot. Inst. Bot. Univ. Carol. Prague 1966, p. 32. I know I have this series, but have been forced to pack it away and cannot easily find it. Probably unnecessary.

1. The appropriate page in Hooker
2. The card(s) from ING with sectional (etc.) division of Artemisia ([illegible]) (who is the compiler and judge?), ~~[illegible]~~?)
3. The card for Seriphidium Poljakov (his name in Latin with j, in English with y), including typification.
4. Newest rules and recommendations for typification (I have the 1972 Code).
5. Preamble page
6. Article on spelling and typographic errors (Art. 73 in 1972 Code)
7. Explain and refer to article that makes Seriphidium invalid because of its typographic error.
8. When was A. cana selected as the type and by whom?
9. Any other type selection mentioned and rejected?
10.

United States Department of Agriculture
Agricultural Research Service
Western Region

Mountain States Area
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February 15, 1984

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

Dear Askell:

I am enclosing a copy of the manuscript I have prepared for the Stadler Genetics Symposium. This paper and the others will be published by the Plenum Press in a book entitled "Gene Manipulation in Plants." It should receive wide distribution.

I would be grateful if you would review the paper and fill out the peer review form and return it to me. I need to have the manuscript revised by early March so that I can carry the final version with me to the Symposium. You may make your comments directly on the manuscript. I am too far into the manuscript to make drastic changes. However, I am especially anxious that you point out technical errors to me. I have used your Greek and Latin definitions of the genera. On page 24 I say that <u>thino</u> = a shore weed. Is this at least roughly correct?

[illegible]

You will note that our treatments of <u>Elytrigia</u>, <u>Thinopyrum</u>, and <u>Lophopyrum</u> differ substantilly. I have combined <u>Thinopyrum</u> and <u>Lophopyrum</u> and recognize three sections within <u>Thinopyrum</u>, i.e. section <u>Thinopyrum</u>, section <u>Lophopyrum</u>, and section <u>Trichophorae</u>. In the Appendix, pages 67 and 68, I make new combinations for the sections and several species. Are these done correctly?

I hope you don't get too bored reading the manuscript, and I look forward to your comments.

Sincerely,

Doug

DOUGLAS R. DEWEY
Research Geneticist

cf. p. 239 in Woods, R.S. 1944: The naturalist's lexicon. Abbey Garden Press, Pasadena

Enclosure

THE GENOMIC SYSTEM OF CLASSIFICATION AS A GUIDE TO INTERGENERIC HYBRIDIZATION WITH THE PERENNIAL TRITICEAE

Douglas R. Dewey

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INTRODUCTION

Of the approximately 325 species in the tribe Triticeae (= Hordeeae), about 250 are perennials that include many of the world's important forage grasses. Although more than 75% of the Triticeae species are perennials, they have received far less attention from cytogeneticists and plant breeders than have the annuals, which include three major cereal crops--wheat, barley, and rye. In addition to being important in their own right as forages, the perennials form a vast genetic reservoir that might be used to improve the annual cereals.

Hybridization between annual and perennial Triticeae species has been a relatively common plant-breeding practice since the early 1930's, especially in the U.S.S.R. (Tsitsin, 1960; 1975). However, only a few perennial species were involved in those early programs, whose goals usually were to transfer disease resistance or the perennial habit to the annuals. Annual cereal X perennial grass hybridization remained at a more or less static level until the 1970's when advances in hybridization techniques (Kruse, 1973), embryo culturing (Murashige, 1974), and control of homoeologous pairing (Riley, 1974) stimulated a renewed interest in wide hybridization in the Triticeae. Over the past 10 years I have noticed heightened interest and activity in hybridization between annual and perennial Triticeae species as evidenced by a substantial increase in requests by cereal breeders and cytogeneticists for seeds from the U.S. Living Collection of Perennial Triticeae Grasses, which I curate (Dewey, 1977).

San José, February 19, 1984.

Dear Doug:

Many thanks for the excellent Stadler Symposium paper that I have read and reread several times and enjoyed increasingly, for the simple reason that it is a masterpiece of concentration and clarity. It is also one of these few papers that I would have liked to write myself, though only you could have written it, and also filled with ideas that are as clearly genetical as I would like to believe most of mine are, since you follow beautifully the genetical paradigm that very few botanists here seem to understand or dare to mention to those many who do not understand that kind of biological logic. Therefore, I have had to make great efforts to put my finger on some places to show you that I have read it critically, though I hope you realize that that has not been done because of my nasty nature but only to try to help you to avoid mistakes even of the minor kind and to bypass some illogical thoughts that none of us can get rid of. And though I do not let be to mention some disagreements, you realize that these are small and likely to change by time, from both sides. If I have used harsh words somewhere on the sheets enclosed with my minor remarks, I hope you will overlook them as coming from one who does not know enough English to avoid selecting just the wrong expressions now and then, because as a whole I realize that you are one of the very few botanists here who think similarly to me and who has practically the same kind of philosophy. Can I express my satisfaction better than by repeating that I would like to have been the author of this masterpiece of yours...though it at the same time must be regarded as very personal and typical of its author?

I enclose also the peer review, though I must admit that in this field you have no peers so the paper ought to be accepted with acclamation.

I see in your references that the Barkworth, Dewey & Atkins paper is out and that another of their papers is coming soon in Amer. J. Bot. Hope to be remembered with reprints when they become ready so that I can correct possible mistakes in references in my own long paper, which I am told is getting into the hands of the printers soon. I wait impatiently for these reprints and for the reprint of your magnum opus, of course.

Did you get time to get the retrofractum seeds to germinate? And am I right in understanding that you found the Petrova paper you asked me to try to locate...I have not yet heard from my correspondents about it and hope they have not had difficulties in locating some copy...though this now looks unimportant. Petrova is not a great cytologist, but she may have written several other reports that none of us has heard about.

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

As ever,

P.S.: I hope you permit me to keep the copy until the reprints replace it.

D. R. Dewey; 1984: Genomic system, etc.

Some observations and suggestions by Áskell Löve, February 19, 1984.

p.2: Splitter is a degrading American term fit for the hardly logical men who at the beginning of the century worked on taxonomy mainly without proper training in common taxonomical logic...and as free from tolerance of the opinions of others as even many American scientists still are. This way of discussing is foreign to most Europeans and to all educated people elsewhere. The term would never have been used about Nevski, the young and unusually able agrostologist who rose to greatness despite that he died so young...except by some less civilized Americans, not even the German Nazies used such expressions in their science. I would, therefore, propose that you drop the term and the sentence it is in, or, if you still feel it is worth being rude, even friendly, that you insert the words "in America" before "Earned Nevski"...I know Stebbins used it about his works, but who else in print? If Nevski had lived longer and gotten the cytogenetical training that we have, I am in no doubt that he would have found our system acceptable....and the same I expect from Tzvelev, despite the fact that he was brought up on Lysenkoism (which is the same as American-British botanists now call pheneticism).

p. 3, 11th line:
My genera are now 38, cf. addition in your Table 1. The additional Eigopyrum genus includes E. vavilovii, a BDM taxon, a case similar to what you suggest later that may be required even for Elymus and Elytrigia, when our knowledge has increased. Kihara pointed this case out to me, I had missed the formula for the variety in question and been mislead by an interpretation by Chennaveeraiah.

p. 5, in the Löve column:
Add Eigopyrum before Elymus; and for the alphabetical order, move Gastropyrum one line up.

p. 7:
Add in Table 1 under Elytrigia: 84 (the number for E. varnensis) which I believe is a simple autoallotetraploid of the hexaploid E. intermedia.

p. 6, 7th line from below:
I believe "quite" is too strong, "slightly" would be closer to the truth. But cf. my discussion below for pp. 24-26.

p. 8, 22nd line from above:
Drop one "the".
5th line from above:
I see no reason to doubt that Leymus arenarius is a simple autoalloploid (autoploid if you will) of the allotetraploid L. mollis, from which it differs only in some hairiness on and in the spike, so these two species are frequently difficult to identify except by an experienced eye.
8th line from above:
After "Australian perennials", add: "and the Mediterranean Festucopsis."
22nd line from above:
2nd line from bottom:
It would be better if "genus" were replaced by "complex", for style and consistency.

p. 9, 13th & 14th lines:
The first part of the sentence on Nevski might be improved to:
"Nevski (1933) was the first modern botanist to return to the original concept of *Agropyron* in its restricted sense, and he did this by aid of correct typification and without the benefit of cytogenetic information."

p. 13, 25th line:
Better: *Roegneria*, a genus redefined by Nevski [He made the mistake to widen its concept from that of the single species of Elymus sect. Clinelymopsis of the Caucasus to that of the complex boreal circumpolar *E.* sect. *Goulardia*, which violates typification rules].
33rd line from above:
There is no reason to credit Kimber (1973) with the acceptance of the letter S for *Sitopsis*, since this was originally done by Kihara (1949). Therefore, I add that reference in the bibliography (below) and propose that Kimber's reference be deleted there also.

p. 14, 25th & 26th lines, etc.:
When using a genetic definition of genera, it is hardly logical to continue to reject the strict biological (or even classical Linnaean) definition of species based on intersterility, as done here and somewhere else in the paper. Therefore, for biological consistency, I would like to see you drop references to the certainly imaginary "intraspecific polyploidy" of the antigenetical lumpers and pheneticist-Lysenkoists around us, and simply change these lines to: "Some of the *Pseudoroegneria* species (no references needed, since more than you mention may be involved) are tetraploids that behave cytologically as autoploids or near-autoploids, which I represent genomically as SSSS (Dewey, 1975a)." [As far as I understand from the Ukrainian taxonomists and their grass flora edited by Prokudin, tetraploid *stipifolia* has been described as *cretacea*; and you advised me once that tetraploid *spicata*, or what I believe is *arizonica*, is a species of *Elymus* in our sense. Is either of us mixed-up or perhaps I am nodding?). The mix-up of diploid and tetraploid *spicata* and *stipifolia* extends into the following pages].

I may add here, that in our long 1949 paper on the Geobotanical significance of polyploidy. I. Polyploidy and latitude. Portug. Acta Biol. (A) R.B.Goldschmidt Vol. Vol.: 273 - 352 [one more paper that Stebbins & Co. ignored because it clearly demonstrated his and their foolish stand against a geobotanical theory by Scandinavians, a theory he did not understand, of course], we proposed a clear terminology that takes better care of these two terms and the "segmental alloploids" by Stebbins: panautoploidy and hemiautoploidy and panalloploidy and hemialloploidy. This, however, angered the great god and his many slavish followers so they simply ostracized the terms and the good paper! I am sure you never have seen that paper nor our little "cookbook" on Plant chromosomes from 1975, which our "honest" colleagues also ostracized. I am sure you would have liked both...and disagreed on some opinions as do honest men now and then...but am sorry that we have only a single copy left of each, though the book may still be available from the German publisher?

p. 15, 10th line from below:
readiy...for ready, a simple typing error.

p. 18, 8th line from below:
Is SHY not a mistake for SH, cf. two lines below?

p. 21, bottom, and p. 22, top:
Cugnac's guess and that of Douval-Jouve are not reliable and could and should be ignored, as should several other such "hybrid" proposals of the past. Agrohordeum rounii of Cugnac (I do not have that paper, but others of his) must be the hybrid A. rouxii, which is more likely to be correctly identified by Kerguelen, a good taxonomist, cf. p. 22. Therefore, the chapter on p. 21 could be dropped, and that on p. 22 perhaps modified.

p. 23, 9-10th lines from below:
interspecific...should be intergeneric.

p. 24, 17th line from below:
"shore weed" is a mistake in the Triticeae manuscript; it should be: "thino, a combining form of this, the shore", cf. Woods, R. S. 1944: The Naturalist's Lexicon. Abbey Gordon Press, Pasadena.

pp. 24 & 26...and p. 6:
You are technically correct in your transfers of Lophopyrum and parts of Elytrigia to Thinopyrum, though you clearly replace one possible confusion with another that is more certainly an illogical induction of heterogeneity into an otherwise distinct genetical group. I am convinced that you are biologically wrong in both transfers...and that you evidently realize yourself... since the three complexes you unite are doubtlessly not homogenous and no closer related to each other than they are, e.g. to Psathyrostachys to pick one of many possibilities. I find it illogical to make such a conclusion from apparent morphological similarity of karyotypes for the simple reason that though differences in karyotype (as in logic) are strong indications of distinction, subjectively decided similarity proves nothing...and then especially in groups with as much similarity in karyotypes as the Triticeae. Only meiosis of hybrids between diploid , or alloploid?, karyotypes, perhaps supported by critical studies of banding, may solve that identity problem, but references to incidental and shallow experiments by enthusiastic agronomists little familiar with natural populations of the taxa in question, as are, e.g., Cauderon and Dvořák, can only mislead...as you are experiencing. As a matter of fact, even we did not succeed in producing hybrids between diploid E and J haplomes despite of considerable efforts several times since 1942, though we got highly sterile triploids from crosses between tetraploid JJJJ and diploid EE...though rerely did we find any bivalents or trivalents, sontrary to Cauderon & Saigne, why we do not know. And although we have had opportunities to study mixed populations of both groups at various parts of the Mediterranean abd Black Sea and on the Asia Minor coast of Turkey, last time for two summers (1971 & 1972) when we were stationed near the innermost part of the Adriatic Sea from where the diploid EE was described, we never met with any natural hybrids. You should, of course, not regard this as a protest, only as a mild disagreement caused by the wish to clarify matters that otherwise may become confusing for too many who do not know the little experience you and Dvořák have with these Eurasiatic plants, because I know from experience that when your fine mind has had more time to digest the facts, you will again end up in my camp also on these matters...and so will reasonable European taxonomists living with these taxa. I share your boubts on the Elytrigia complexes, but this is not a solution, only added confusion.

And it mars an otherwise excellently logical treatmentwhen one clutters up some clear groups of s system rather than leave the strict genera untouched and accept Elytrigia as I left it as a smaller and less heterogenous though still too wide a group...but it does not become better through your handling, unfortunately and unusually, though this certainly is caused by your lack of familiarity with these critical groups. Which probably none of us know properly yet, but therefore I have picked out what could be defined and left the other but lesser mess where it was in Elytrigia, as sections or only as undefined groups. Do what you feel, I would spend time even at this stage to clean the act properly, or to get it back to the last stage, which is much more logical and biologically less disturbing. Though you can, of course, blemish your reputation as a very logical scientist and leave the new mess as it is and plan to clean it up in another paper...such has been done by reasonable men before. But since I leave my manuscript as it is in these matters, the minor disagreements may mean little, when others evaluate our conclusions, probably already next year.

A better solution for your second paragraph on p. 26 of the socalled confusion of diploid E. elongata with the decaploid E. pontica might be simply to abbreviate it - and drop all reference to Dvořák's confidence that the type specimen is diploid, because that fact ordinary European taxonomists have never doubted, not even the French agronomists Simonet (1935a,b) and Cauderon (1962, though the original confusion came from the sloppy (I knew him) Canadian agronomist-businessman Peto and from the Tsitsin group that never was an example of exactness. I think the paragraph could be improved considerably by rewriting it, for instance as this: "The nomenclature of the diploid (2n = 14) T. elongatum and its decaploid (2n = 70) relative has for a long time been badly confused among American and Russian agronomists because of misunderstandings by the Canadian Peto (1930) and the Russian Tsitsin (1933) and their followers, but European taxonomists have long since clarified this problem. The species T. elongatum as typified from the Adriatic coasts is a diploid. The epithet ponticum (no need to give the female ending!) has been correctly applied to the 70-chromosome species known in North America as "tall wheatgrass" (Holub 1973). The epithet turcicum was applied by McGuire (1983) to the 56-chromosome taxon, which has been confused with the 70-chromosome ponticum. [No more, Melderis' mistakes are unimportant here and should be ignored].

Thinopyrum [add: as here circumscribed] is a genus of [unchanged continuation]". Do with this as you like, and I understand that for the present paper it may be too late to leave again the faulty Dvořák bandwagon to jump back onto mine...you will survive either if they sink! But I am confident that you will soon return to logic even on these perhaps unimportant points, so I will not bother to add your nomenclatural deviations,that will create unnecessary synonyms, to the proofs of the Triticeae conspectus.

Before I leave the subject I want to mention that on p. 25, you have missed the 84-chromosome Elytrigia varnensis, which seems to be a rather local duplication of E. intermedia; it is easily added here and in the first paragraph on p. 26. And I propose you simply ignore Melderis and his lack of understanding of the biological concept rather than claim some "concurring" and thus break your own biological logic, which you have no reason to do. If you mention him at all in this connection, say simply: "Melderis (1978) considers the octoploid and decaploid taxa as intraspecific". As a matter of fact, he hardly knew them, neither do many others.

p. 34, 10th line from above:
Just to play irritated, which I am not: If Dvořák, who immigrated to Canada and the States less than ten years ago, is acepted as "North American", what then about us, who arrived in 1951 and acepted and made propaganda for Nevski's Elytrigia and his entire system from the beginning and used Elytrigia both in writing and ~~XXX~~ discussions from the first month in Canada, and naturally also in our NW and Central European chromosome list from 1961, which was for Europe, but completed in Winnipeg and Montreal, distinctly American cities? To avoid the barb that may irritate some other immigrants, why not simply drop the reference to Dvořák and McGuire so the sentence will be similar to the following, where "most" is not so qualified? Those who want to find out who were first can spend their time in searching, without your or other advice; we know you intended nothing negative, but we have become much too sentitive after decades of of being ignored and mistreated by small minds...of which you are not one...last time by the selfsecure and vain new god Peter Raven in a nonsensical paper discussing what he claims to be "biosystematics" in the Canad. Bot. Ass. Bull. Suppl. to Vol. 13, 1980, pp. 3 - 10. Stebbins criticized, unfairly and unlearnedly, the Elytrigia concept of Nevski even before we arrived, but if we had not reintroduced it already 1951 and 1952 for concrete examples, it would have been ignored entirely, not least by our good friend Dvořák, who may have seen or heard it used in Czechoslovakia, though then his taxonomical interest probably was nil, he was then a student, good one, of cytology in an agronomical department, not connected with grass taxonomists of whom his country of origin nevertheless has and has had some of the most outstanding.

p. 37, 11th line from below:
"T. timopheevii (AAGG" is probably in order as a reference to Cauderon, though this use here of G may mislead; actually it ought to be AAB'B'.

p. 38, 18th line from below:
Should the reference to Löve 1982 not be 1984, or both years? A pittance, of course

p. 45, 9th line from above:
amphiploiids...one i would suffice.

p. 46, 11th line from above:
"manuscript"...perhaps better paper or report....since you refer so to it.

p. 50, 13th line from above:
"This same genome may occur in the Australian species of Elymus." Though I understand that you continue an old idea of X and Y haplomes from China, there is no substance yet to such a wild guess, and since it is not needed, and might confuse those who know less, I propose that you drop this sentance. Notwithatsnding that you may be right, though that is for the future to judge. But phytogeographically I dare to believe that it is highly unlikely.

p. 54, 5th line from above:
française.,.the cedilla, or accent cedille on the c, is important for pronounciation and should not be dropped even in a reference in an American paper.

p. 56, 32nd, 34th, 41st, and 45th lines:
Dvorak ought to be written correctly as Dvořák, ahatever uneducated and lazy printers may say or have done. And this spelling should also be corrected in various places in the text, for simple politeness, it is his important name.

p. 57: the two papers by Hansen:
In German, nouns begin with capitals. Therefore, the titles of Hanen's papers are:
1959a. Die Gras-Hybriden in der Flora Frankreichs. Kritik und Ergänzungen.
Bull. Jard. Bot. Bruxelles 29: 61 - 68.
1959b. Die Elytrigia-Arten und -Hybriden an der polnischen Ostseeküste.
Fragm. Flor. Geobot. 5: 181 - 189.

p. 57, bottom:
Since Hitchcock is the author of the entire book, I would not refer only to a chapter in it, but to the book as a whole:
Hitchcock, A.S. 1951. Manual of the grasses of the United States. Second adition revised by Agnes Chase. U.S.Dept. Agric. Publ. 200. U.S. Govt. Printing Office, Washington, D.C.

p. 58:
Hochstetter, C.F. 1848. Nachträglicher Commentar zu meiner Abhandlung: "Aufbau der Graspflanze etc.", Flora 7: 105 - 119. [a journal no place needed].
Holmberg, O.R. 1926. Skandinaviens flora, col. 2. P.A.Norstedt & Söners Förlag, Stockholm.
Keng, Y. L. (ed.) 1965. Flora illustrata plantarum primarum sinicarum. Scientific Publishing Co., Peking.
Kerguelen, M. 1975. Les Gramineae (Poaceae) de la flore française. Éssai de mise au point taxonomique et nomenclature. Lejeunia, n.s. 75:1 - 344.

add: Kihara, H. 1949. Genomanalyse bei Triticum und Aegilops. IX. Systematischer Aufbau der Gattung Aegilops auf genomanalytischer Grundlage. Cytologia 14: 135 - 144.

omit: Kimber, G. 1973, the entire reference text.

p. 59:
1st line from above: Dvořák (spelling)
10th line: Lepage (no capital P!)
13th line: Lepage...genéalogique...
15th to 16th lines: Species plantarum (no volume, though two parts, Triticeae in bpt
Stockholm should be: Holmiae.
25th line: (Löve); Feddes Repert. 95 (in press).
35th line: (Lyubimova): no comma after Roem. et Schult.

p. 60, 12th line:
Period missing after Dumort. (abbreviation of Dumortier).

p. 61:
The first title by Nevski should be (spelling and reference corrected):
Nevski, S. A. 1933. Agrostologische Studien. IV. Über das System der
Tribe Hordeeae Benth. Trudy Bot. Inst. Akad. Nauk SSSR, Ser. I, fasc.1:9-32.
In the second title by Nevski, correct Hordeae to Hordeeae.
26th line. Popiloidiya, should be: Polyploidiya
29th line: Pflanzenzücht., (period missing)
31st line: (Hordeae), should be: (Hordeeae)
33rd line:...System... (capital S).

p. 62:
8th & 9th lines: accents are missing:..États-Unis...d'Amérique...

p. 63: Tzvelev 1976. The reference should simply be:
Poaceae URSS, Nauka Publishing House, Leningrad (no citation marks, Nauka
simply means scientific).

p. 66:
Is not a simple asterisk enough to denote the annuals?
s. lat. means: in a wider sense, indicating a collective species, not
one with subspecies. Should therefore be omitted for C. brevisubulatum.

p. 67, 5th & 6th lines: Săvul. & Rayss (period missing after Săvul, which
is an abbreviation of Săvulescu, and the accent missing).
14th line: P. Beauv. (P. is important here).

p. 68, 17th line:
Thinopyrum intermedium.. omit s. lat. (cf. remark above).

p. 69, 14th & 15th lines:
Dumort. (for Dumortier), P. Beauv. (for Palisot de Beauvois)
21st line: Trautv. (not Troutv.)
23rd line: P. Beauv.

p. 70:
11th, 13th & 25th lines: omit s.lat. (cf. above).
22nd line: Drop the E. stewartii - Agropyron line and add in the synonymy line
under Elymus transhyrcanus: Elymus stewartii Löve, non Agrop. stewartii Melderis
10th, 12th, 24th lines: omit s. lat. (cf. above)
12th line: (Hook.) [period missing]
14th line: (Drobov) [as in line above, for consistency]
21st line: (Scribner & Smith) [as in line above, for consistency].

p. 71:
7th line: Hultén [accent omitted]
9th, 10th, 12-15th, 21st, 23-25th lines: Trin. or Trinius, not both spellings!
16th line: Turcz., rather than Turczaninov

p. 72:
3rd line: Rydb. rather than Rydberg, for consistency with lines 3-5.

Febr.19,'84 XX XX

This paper by the foremost specialist in its field is a masterpiece of clarity, learning, logic and scientific tolerance as it reviews critically and logically the immense material on intergeneric hybridization and classification of the large grass tribe Triticeae that encompasses some of the most important grain crops and forage grasses of the world. The author reviews not only his own numerous and remarkable contributions but also those of the many other specialists in the cytogenetics and taxonomy of these grasses who have recently worked with these grasses, and evaluates the significance of the observations for plantbreeding also. The paper is likely to become a classic and to influence both geneticists and taxonomists in the wide regions the world over that are the homelands or adopted regions of these grasses. The paper ought to be published without further comments.

CSIRO

DIVISION OF PLANT INDUSTRY

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

23 May 1984

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

Dear Dr. Löve,

Thank you for your prompt reply of Dec 4th last, and please accept apologies for my lack of response till now. In fact, the information you supplied at my request sparked off considerable activity on my part, hence the delay. The end result is enclosed for your interest and possible comment.

You will see that I fully agree with you about the desirability of generic status for Oreophylax; the main thrust of the paper (which I hope to submit to Taxon) is to tidy up the long-standing misconception surrounding its nomenclature. I hope you don't take offence at my conclusions; you will note that I have refrained from "remaking" your previously-published NZ combinations, preferring to leave it to you at some future date.

I was interested to hear from Roy Pullen that he has managed to get for you the Agropyron seed you were lacking, and hope that chromosome counts were successfully forthcoming.

With best regards,

Yours sincerely,

Laurie Adams

(Mr.) Laurie Adams

San José, June 9, 1984.

Mr. Laurie Adams,
Division of Plant Industry,
C.S.I.R.O., Canberra.

Dear Mr. Adams:

Many thanks for your good letter of May 23 and for the copy of the fine review of the nomenclatural history of the austral gentians. As a matter of fact, your earlier letter also induced me to look closer into the problem and dig into the literature available to me and the numerous notes on our experiments that I found to have been more extensive than I remembered when answering your letter in December. Although that scrutiny was not sufficient to convince me that I ought to publish the as always incomplete evidence, either alone or through the help of some others interested in the problems, I still must admit that I am not yet quite free from some confusion on some nomenclatural points, though during the writing of this letter I may clarify even for myself some of the critical points. We will see. Perhaps I am more confused than before after reading about your dismissal of the oldest and certainly valid name Selatium, because my logic had led to the conclusion that the species that probably would best fit as its lectotype could be S. foliosum (H.B.K.) D. Don, material of which I have once cultivated and studied cytologically; Grisebach believed it belonged to his section Andicola, whereas Nilsson (1963) agreed that it belongs to Antarctophila palynologically. However, I doubt that Pfeiffer (1874) can be credited as having selected S. thyrsoidea as the lectotype of the genus Selatium, as you mention, since at that time the type concept was hardly invented and certainly not accepted...in addition to that this taxon had already been legitimately removed from the group, cf. below. Though in this all I may be mistaken and confused.

Admitting that I do not understand why Selatium has to be rejected because of some earlier muddle, I like your reasoning on behalf of Oreophylax and so would like to urge you to send your paper to Taxon in the hope that it be accepted and printed as soon as possible, or rejected because some awake reviewers may agree with my doubts as to the rejection of Selatium, and thus induce you to make a more permanent solution. Whatever you select to do, perhaps the following remarks may be of some help in improving and reshuffling an already well-composed report:

Abstract (p. 12): I believe the reference to the generic key should not mention Gentiana, which is a complex of more than a dozen good genera as I see it, since it might be taken as an implication that you reject all its boreal splits which you do not know and are immaterial in the context.

p. 1: "erroneous supposition" I would make milder by saying only "doubtful". "infrageneric" in the second paragraph ought to be dropped, no such
p judgement should be made prior to the reasoning.
"matter of weeks" perhaps better "months": August to October, cf TL of Stafle
"spp." or similar abbreviations, also later, should be spelled out.
"G. Don (1837) published a description by his brother David, so better "D.Don in G. Don (1837)".

p. 2: 5th line: Philipson (1982) for (1972).
12th line:...,without comment but cited it as an accepted genus as, e.g. on p. 95, where Anthopogon, Endotricha etc. are listed under sect. IV. Amarella - perhaps an indication that Grisebach regarded Endlicher's taxa as genera?
13th line, etc.:"Pfeiffer (1874) established the rank of section for Oreophylax and the nine infra-generic taxa of Endlicher, an action subsequently overlooked, whereas Willis (1925) cited simply 'Oreophylax Endl. = Gentiana L.', giving the impression that in his view, Kusnetzov (1895) regarded Oreophylax as a generic synonym. The name is, however, not mentioned in the main Gentiana work by Kusnezov (1904). Under the then accepted Code and long after names so cited in synonymy were generally accepted as validly published, although under Art. 34.1(d) in the present Code (Voss & alii 1983) they are rejected." [Perhaps one may wonder if it is legal or logical or democratic to judge things after rules or laws made after the fact, though this was done after the war by the American judges in Nürnberg?].
28th line: Kusnezov' work on the subgenus Gentiana is dated 1904 only, on the title page of a photocopy I received from Leningrad.

p. 3: 2nd line, after species, add and change:..."and Smith (1936; and in Hylander, 1945, and in Nilsson, 1967) made a clear distinction between Gentiana and Gentianella at the generic level, split out some new genera, and rearranged and refdefined the sectional components. Some other workers apparently felt...."
5th line, etc.: Change to: "Despite undoubted cytological, morphological and palynological affinities, other..."
13th line should be: "combinations he failed to cite..."
16th line...not to be so..replace with: "to be disputable" (or doubtful).
18th etc. lines: Replace with Selatium etc. if reasoning below for p. 7 is accepted.

p. 4: 2nd line: "m. or F." not to be abbreviated...write male or female...
6th line: "p.p.maj." perhaps better as "p.p.m."?

p. 5: Drop the first part of the key, it is irrelevant and indeed misleading when the genera are split, as now accepted in boreal regions at least.

p. 6: 4th line, add: "and also the few New Zealand and South American taxa transferred in an Appendix II at the request of Áskell Löve, who had transferred them to Oreophylax in 1983 because of expediency prior to the present validation of the generic name." [I hope you agree to add here your own Appendix I, much longer than mine, in which you make the needed wholesale transfer of all the species concerned, from Grisebach to Fabris, etc. in which you may credit me (in part and in cooperation?) with the transfer of G. wislizenii of Mexico from Arctophila since it seems to belong to the austral group both palynologically and morphologically and cytologically. Perhaps it is the step between the boreal and austral genera?

p. 7, second paragraph: When Grisebach (1845) placed Selatium and Ulostoma species in Gentiana section Andicola and excluded from it G, thyrsoidea as the monotypic section Dasystephana, this restricted the Selatium genus to the remaining species. Therefore, Pfeiffer (1874) could hardly select that species as the lectotype of Selatium [a similar case is the required change in typification of the grass genus Elymus, when some of its species were separated in the genus Leymus]. I also was of the impression that the type concept was a much later invention and not accepted at that time? I am also of the belief that you may be mistaken when claiming that "no classification is possible until authentic material of all species of Don's genera can be examined", since the only thing needed is a typification that follows the Code. I dare to think that it would be correct to select any of the seven species left when S. thyrsoidea has been removed, and that the most appropriate selection would be S. foliosum (H.B.K.) D. DON, which even Grisebach (1845) placed in Andicola. You may disagree - but if you agree with this reasoning, then it ought to be your privilege to revive that name rather than permit others to do so (and then change my Appendix I accordingly). I do not know more than the description of Ulostoma, but am inclined to follow Grisebach (1845) and sink it into the section Andicola, or the genus Selatium. as we see it now.

name

p. 9: 22nd line: Tokoyuni is a printing error for Toyokuni, so I would correct it.

p. 10: 9th line: Although Englers names were Heinrich Gustav Adolf, he usually (and on the title pages of Pflanzenfamilien) preferred to abbreviate it to the single initial A. Engler...so in all the volumes of Engler-Prantl.
12th line: 1894-1904 should be 1904 only, as in my photocopy from Leningrad.
13th line: Tourn....should be Tournefort, not abbreviated in the title.
19th line: Do you really refer to both these parts of the Nomenclator?

Add: Appendix I by L. G. Adams

Appendix II, by Áskell Löve. (Change names to Oreophylax if appropriate)

Selatium antarcticum (Kirk) Á. Löve, comb. nov., based on Gentiana antarctica Kirk, 1895, Trans. N.Z. Inst. 27:339.
Selatium bellidifolium (J.D. Hooker) Á. Löve, comb. nov., based on Gentiana bellidifolia J.D. Hooker, 1844, Ic. Plant. t. 635.
Selatium grisebachii (J. D. Hooker) Á. Löve, comb. nov., based on Gentiana grisebachii J. D. Hooker, 1844, Ic. Plant. t. 636.
Selatium lineatum (Kirk) Á. Löve, comb. nov., based on Gentiana lineata Kirk, 1895, Trans. N. Z. Inst. 27:334.
Selatium magellanicum (Gaud.) Á. Löve, comb. nov., based on Gentiana magillanica Gaudichaud, 1825, Ann. Sci. Nat. 5:102.
Selatium montanum (G. Forster) Á. Löve, comb. nov., based on Gentiana montana G. Forster, 1786, Prodr.:21.
Selatium saxosum (G. Forster) Á. Löve, comb. nov., based on Gentiana saxosa G. Forster, 1777, Sv. Vet. Akad. Handl. 38:184.
Selatium serotinum (Cockayne) Á. Löve, comb. nov., based on Gentiana serotina Cockayne, 1915, Trans. N. Z. Inst. 47:113.
Selatiumvernicosum (Cheeseman) Á. Löve, comb. nov., based on Gentiana vernicosa Cheeseman, 1906, Man. N. Z. Fl.:1144.

Additions to bibliograbhy...if changes accepted:

Hylander, N. 1945. Nomenklatorische und systematische Studien über nordische Gefässpflanzen. - Uppsala Univ. Årsskr. 1945, 7: 1 - 337.

NIlsson, S. 1967. Pollen morphological studies in the Gentianaceae-Gentianinae.- Grana Palynologica 7:46 - 145.

Smith, H. 1936. Gentianaceae. - Handel-Mazzetti, Symb. Sinica 7:950 - 988.

I hope you find at least some of these suggestions helpful in adding to the value of your already good paper and in avoiding making preliminary adjustments of a critical nomenclature rather than permanently settling this long dispute caused more by sloppiness than anything else, I feel...including my own contribution. And hope you accept my remarks as they are meant, with no malice.

Yes, we got seeds from Roy Pullen, many of Agropyron retroflexum and only a single one that germinated of A. pectinatum. This, however, sufficed to establish that the diploid chromosome number is characteristic for these taxa and A. velutinum that he sent earlier, and I trust that after some few years we and our colleagues at Logan, Utah, will have made crosses that give us some information as to the relationships to the boreal groups of the wheatgrasses. I validate thses taxa in my Conspectus of the Triticeae later this summer in Feddes Repertorium as the species velutinum and pectinatum of the endemic genus Australopyrum, and transfer retroflexum as a subspecies of the latter. Perhaps future studies will reveal that they are actually equivalent subspecies or even varieties (or major and major geographical races, if you so want them defined) of a single strictly autogamous species? We may see later...and hopefully we and the Australian colleagues will find in them some valuable genes for breeding of grasses or grains. Please, give the gentleman Pullen my very best regards.

You must excuse my curiosity about your age and experience and work and interests...you probably know already that I am born and brought up as a very nationalistic Icelander, born in Reykjavík in 1916 when the middle ages still dominated my island...my father was a sea captain who risked his and his crew's lives fishing for the British during the first world war. Then I was educated at the University of Lund in Sweden and at the famous Swedish Plant Breeding Institute at Svalöv, where I started as a geneticist with great interest in taxonomy that slowly led to attempts to employ the former for the latter, a task that has been far from pleasant in mainly creationist-pheneticist America where I thought we would get peace for work and encouragement for new ideas. I do not know whom other than ourselves to blame for the fact that though we tried hard to find professional contacts in Australia in the late 1940's when we planned to emigrate no such contacts could then be found, so we came to the wrong side of the world... despite the fact that for two or more years we enjoyed propaganda contacts and the Pacific mailbag in the radio with the laughter of the cookaburro at noon when we had our main meal in far-away Iceland! Things sometimes go otherwise than we dream.

I hope you send your paper...preferably with all the transfers you now avoid... to Taxon and get it printed in some of the early numbers a year from its mailing... and that you do not hesitate to ask me to read your revision in case you think that might still improve it...nothing ever is complete in this world. And there is still so much to do for those with interest in improving science and other human phenomena in this world...if the crazy believers in might do not destroy it.

With the very best regards,

<u>OREOPHYLAX</u>: A VALID NAME FOR THE SOUTHERN-HEMISPHERE GENTIANS

L.G. Adams

<u>Introduction</u>

Under Article 35.2 of the International Code of Botanical Nomenclature (Voss et al. 1983) names published prior to January 1953 without indication of rank can be valid, but are not operative in questions of priority. In his Genera Plantarum, Endlicher (1838) created a large number of infra-generic taxa, in the great majority of cases without specifying a precise rank. Despite Art. 35.2 the usual assumption has been that they were intended, and should be accepted, as sub-genera, sections or subsections (depending on the number of hierarchical levels in a particular case), but Brizicky (1969) argues convincingly that these views are untenable. One such erroneous supposition involving a taxon in Gentianaceae, coupled with subsequent imprecise citation of the name, has created misunderstanding lasting to the present day.

The [infra-generic] taxon <u>Oreophylax</u> was the last (and the only original) one of nine listed by Endlicher in Genera Plantarum under the genus <u>Gentiana</u> L. He based it on the only six illustrated of the thirteen closely-related species described by Kunth from S. America in Nova Genera et Species Plantarum (1819). Just a matter of weeks after Endlicher's work appeared, Grisebach (1838) published his world monograph of the Gentianaceae. In the genus <u>Gentiana</u> L. he circumscribed 38 perennial spp. to form sect. <u>Andicola</u> Griseb., including two Australasian and all the thirteen described by Kunth. He later expanded it (Grisebach 1845) by transferring a further seven spp. already described by G. Don (1837), mostly in the genus Selatium G. Don (see p.). Concurrently (Grisebach 1838) he erected the closely-related sect. Antarctophila Griseb. to accommodate four 'annual' spp., two of which were also Australasian. Together these two sections form the basis of a distinctive, circumpolar group of species which Philipson (1982), in an interesting discussion of the difficulties of dealing with generic claims within Gentiana sens. lat., refers to as the 'Southern Hemisphere Gentians'.

The nomenclatural problem

At the time of publication of his monograph Grisebach was obviously unaware of Endlicher's infra-generic name. In later works, however, both Grisebach (1845) and Kusnezov (1895) cited 'Oreophylax Endl.' as a synonym of sect. Andicola Griseb., without comment ~~or~~ clear indication of rank. Meanwhile stability was achieved when Pfeiffer (1874) listed all the above nine of Endlicher's infra-generic taxa as sections under Gentiana L.; this listing clearly established the rank of Oreophylax for nomenclatural purposes. Even so it appears to have been subsequently overlooked, and the case reached the ultimate in misinterpretation with the erroneous generic citation by J.C. Willis (1925): 'Oreophylax Endl. = Gentiana L. p.p.'. In a later edition Shaw (1966) tried to clarify matters by modifying the entry to read: 'Oreophylax (Endl.) Kusnez. = Gentiana L.', giving the misleading impression that Kusnezov (1895) regarded Oreophylax as a generic synonym. ~~It would seem from~~ other work (Kusnezov 1896-1904) that this had never been so, and furthermore under Art. 34.1(d) (Voss, op. cit.) names cited only in synonymy ~~are not~~ validly published.

Gentianella Moench (1794) was first published as a monotypic genus based on the European Gentiana campestris L. It was later reduced to subgeneric rank in Gentiana L. and broadened in circumscription (Kusnezov 1895, 1896-1904) to accommodate inter alia sect. Andicola Griseb. and sect.

and Smith (1936) and by Hylander (1945 and in Nilsson (1967)) made a clear distinction between Gentiana and Gentianella at the generic level and regrouped (in Nilsson 1967) their sectional components

Antarctophila Griseb. Later still, generic rank was restored (Schustler 1923) for a group of N. Hemisphere species, [but since then] some other workers [have] apparently felt justified in transferring to Gentianella many of the constituent species spp. of the above S. Hemisphere sections also (J.H. Willis 1957; Fabris 1958, 1959, 1960; Holub 1967, 1968). Despite undoubted cytological and morphological and palynological affinities, other opinions regard this as an overly-broad and/or premature step (e.g. Philipson 1972), preferring on the evidence of floral morphology (see key below) to restrict Gentianella Moench to mostly N. Hemisphere taxa. Löve (1983) following Airy Shaw (l.c.) has attempted to regularize the situation by taking up Endlicher's old name Oreophylax at generic level, and broadening its circumscription to include several of the New Zealand species contained in sect. Andicola and sect. Antarctophila. Unfortunately in making the new combinations he has failed to cite a reference to the basionym (in contravention of Art. 41 (Voss, op. cit.)), assuming (personal communication) ~~erroneously, as shown by Airy Shaw (1966)~~ that the name Oreophylax had already been validated as a genus. The latter having been shown to be doubtful not to be so, it becomes necessary to rectify the situation:

Oreophylax (Endl.) L. Adams, gen. & stat. nov.

Basionym: Gentiana L. 'i.' Oreophylax Endl., Gen. Pl. 1: 600 (1838) = Gentiana L. sect. Oreophylax (Endl.) Pfeiffer, Nom. Bot. 1(2): 1429 (1874).

Lectotypus: Gentiana corymbosa Kunth in H.B.K., Nov. Gen. Sp. Pl. 4: 167, t. 224 (1819); = Oreophylax corymbosa (Kunth) L. Adams

[Oreophylax (Endl.) J.C. Willis, Fl. Pl. & Ferns Ed. ~~6~~ 5, P. 472 (19~~31~~25), pro syn.]

[Oreophylax (Endl.) Airy Shaw, ibid. Ed. 7, p.805 (1966), pro syn.]

[Oreophylax Á. Löve, Taxon 32: 511 (1983), nom. nud.]

Etymol.: <u>oreon</u>, of mountains; <u>phylax</u>, guard, sentinel; (Gr.). <u>phylax</u> can be either m. (male) or f. (female) in Greek; <u>Oreophylax</u> is here chosen as feminine, thus avoiding any changes in termination of epithets on transfer from <u>Gentiana</u> L.

<u>Gentiana</u> L. sect. <u>Andicola</u> Griseb., Gen. Sp. Gent. p. 213 (1838), p.p. maj.

= <u>Gentianella</u> Moench sect. <u>Andicola</u> (Griseb.) Holub, <u>Fol.</u> <u>Geobot</u>. <u>Phytotax</u>. 2: 116 (1967).

Lectotypus: <u>Gentiana</u> <u>diffusa</u> Kunth (fide Holub, l.c.)

<u>Gentiana</u> L. sect. <u>Antarctophila</u> Griseb. l.c. p. 235.

= <u>Gentianella</u> Moench sect. <u>Antarctophila</u> (Griseb.) Holub. l.c.

Lectotypus: <u>Gentiana</u> <u>montana</u> Forst.f. (fide Holub, l.c.).

<u>Taxonomic considerations</u>

Despite Holub's (1967) retention of Grisebach's two sections covering the S. Hemisphere species and the fact that there do seem to be annual and perennial taxa involved, I agree with Philipson (1972) that they are largely artificial, in particular geographically (both occurring on both sides of the Pacific), and are better considered as a unified, generic entity closely related to <u>Gentianella</u>, as also confirmed by their cytological similarity and crossability, albeit low ([illegible] in litt.). ~~but which~~ They can be distinguished from previous circumscription by the following key:

1. Calyx membranous between lobes; corolla plicate between lobes (except in the European <u>G</u>. <u>lutea</u>); anthers basifixed; nectaries at the base of ovary: <u>Gentiana</u>
1. Calyx membranes absent: corolla plicae absent; anthers versatile; nectaries at the base of corolla: 2.

2. Corolla lobes short relative to the tube; throat usually fimbriate or ciliate (nectaries never tomentose); corolla usually blue, violet, purple or pink, rarely whitish or yellow: <u>Gentianella</u>
2. Corolla lobes long relative to the tube; throat not fimbriate or ciliate (nectaries and filament bases often tomentose); corolla usually white, greenish or yellowish with greyish violet veins, occasionally violet, pink or orange: <u>Oreophylax</u>

at last [illegible] transferred [illegible] appended at the request of [illegible]
and also the New Zealand & South American taxa, some of which were transferred to it by Löve (1953) prior to the present validation of the generic name (i.e. to the need of revalidation [illegible])

As a number of taxonomic questions remain, it would be premature at this stage to transfer to it all the published taxa which ostensibly belong in Oreophylax. There is no doubt, however, that it includes the only species of 'southern' gentian at present recognised in Australia:

Oreophylax diemensis (Griseb.) L. Adams, comb. nov.

Basionym: Gentiana diemensis Griseb., Gen. Sp. Gent. 224 (1838)

Gentianella diemensis (Griseb.) J.H. Willis, Vict. Nat. 73: 199

Typus: 'Insula Van Diemans Land (Gunn!); (holotypus: K (n.v.) (1957).

One of the taxonomic problems of the group involves the misapplication of the name Gentiana montana Forst.f. (1786) by Grisebach (1838, 1845). The latter included this name in his sect. Antarctophila on the assumption that it should be applied to an annual Tasmanian and New Zealand sp., specimens of which he had examined in herb. Hooker (K) annotated (for reasons at present unknown) by J.D. Hooker as ' G. montana'. Interestingly enough G. Don (1837) applied the name correctly, but Kusnezov (1895) and Kirk (1895) perpetuated the mistake. Hooker himself (1844) later described the same species (presumably from different material) as G. grisebachii Hook.f. (Cheeseman 1906), while still believing that G. montana Forst.f. was a related (annual) species! The error seems to have sparked off general taxonomic confusion regarding the Australasian elements of the group, as demonstrated later by Hooker (1853, 1857), when he synonymised all published names under the two New Zealand spp., Forster's G. saxosa and G. montana. Although he seems later to have had second thoughts (Hooker 1864), the situation was taken to the extreme when all the described Australasian taxa were reduced to a single entity, by Mueller (1864) under G. saxosa, and Bentham (1868) under G. montana! The G. montana anomaly was finally resolved by Cheeseman (1906) with the help of

N.E. Brown, who examined Forster's type material at BM and K. As Cheeseman states, true <u>G</u>. <u>montana</u> is a robust perennial; it is regrettable therefore that Holub (1967) has chosen it as the lectotype sp. for a section which Grisebach characterised as being, among other things, annual! It should be added that the specific and infra-specific levels of the Australasian component of <u>Oreophylax</u> are still somewhat in need of taxonomic overhaul, particularly the inter-relationships between the New Zealand and Australian taxa.

Another serious (and at this stage insoluble) problem concerns the relevance of the two S. American genera <u>Selatium</u> and <u>Ulostoma</u> (G. Don 1837). These genera have not been accepted since they were published, their constituent eight and one spp. respectively having been included in <u>Gentiana</u> L. by Grisebach (1838, 1845). He placed most of them in sect. <u>Andicola</u> but for one, <u>S</u>. <u>thyrsoideum</u> (Hook.) G. Don (syn. <u>G</u>. <u>thyrsoidea</u> Hook. (1831)), he created the monotypic sect. <u>Dasystephana</u> Griseb. (1845). Grisebach does not appear to have seen any actual specimens, apart from <u>G</u>. <u>thyrsoidea</u> in herb. Hooker (K), which was chosen later by Pfeiffer (1874) as the lectotype sp. of <u>Selatium</u> G. Don (perhaps because it was the earliest species name). Thus if it can be shown that any spp. of <u>Selatium</u> or <u>Ulostoma</u> are relevant to the 'southern-hemisphere' gentian group, then one of the two names may have priority over <u>Oreophylax</u> at generic rank. No classification is possible until authentic material of all species of Don's genera can be examined. However, judging by Don's descriptions, and the fact that all of Forster's and Kunth's species which appear to fall naturally into <u>Oreophylax</u> were placed by Don (1837) in <u>Gentiana</u>, not <u>Selatium</u> or <u>Ulostoma</u>, suggests that the latter names do not compete with <u>Oreophylax</u>, and that Grisebach was precipitant in placing the majority of their species into his sect. <u>Andicola</u>. One is led to suspect that if more concrete evidence were to hand there might even be a case for the retention of one or both of Don's genera, in addition to Oreophylax.

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Abstract

Adams, L.G. *Oreophylax*, a valid name for the southern-hemisphere gentians.

The chequered history of the infra-generic basionym *Oreophylax* Endl. (1838) (Gentianaceae), published without indication of rank, is surveyed and the name shown to have been stabilized at sectional level by Pfeiffer (1874). Although erroneously treated as a generic name in the past, it is here validated by circumscription of *Gentiana* L. sect. *Andicola* Griseb. and sect. *Antarctophila* Griseb. (1838) (the 'southern-hemisphere gentians') and lectotypified by *Oreophylax corymbosa* (Kunth) L. Adams. A generic key to *Gentiana* s.lat., *Gentianella* and *Oreophylax* is given and a new combination, *O. diemensis* (Griseb.) L. Adams, made for the only recognised Australian species. Past confusion within the Australasian component involving the misapplication of *G. montana* Forst.f., and the latter's unsuitability as lectotype for sect. *Antarctophila*, is discussed. The question of the (unlikely) relevance of *Selatium* G. Don and *Ulostoma* G. Don to the circumscription of *Oreophylax* cannot be resolved at this stage.

Sokolovskaya, A. P. & Probatova, N. S.
Chromosome numbers of some grasses (Poaceae) of the U.S.S.R. flora. III.
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Agropyron tanaiticum 2n - 28
A. cristatum ssp. pectinatum 2n - 28
(andra 14,28,42)
Aegilops triuncialis ssp. triuncialis 2n - 28
Secale sylvestre 2n - 14
Hordeum bulbosum 2n - 28 snú!

Pienaar, R. de V.
Meiotic associations in a Triticum aestivum L. em. Thell x Agropyron distichum (Thunb.) Beauv. hybrid.
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36786

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Untersuchungen am konstant intermediären additiven Rimpau'schen Weizen-Roggenbastard. –
Züchter 7: 228-233.

(Triticale Tschermak)

over →

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Kritik av 'endangered list', of.

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v.n. 36895

Tracaulon (perfoliatum etc.): Reed 1979, a.s. v.n.

arifolium (L.) Small, sagittatum (L.) Rydb. perfoliatum (L.) Greene: : Maryland. (cf. E. Asia, of Fernald 1950)

Australian Triticeae

Flora of the Atlantic wetlands, (Greenland, Iceland, Faeroes, [illegible])

Swallow the whistle, [illegible] [illegible].

Colombo, P., Colombo, R., Marcenò, C. e Pavone, P. 1978:
Numeri cromosomici per la flora Italiana: 517-525.
Inf. Bot. Ital. 10: 406-413.

524. Secale montanum Guss. 2n = 14

525. Agropyron panormitanum (Bertol.) Parl. 2n = 28

San José, July 25, 1985.

Dear Dr. Simon:

Many thanks for your letter of July 15 regarding the Triticeae question. The Psammopyrum genus will be published in the Fwstschrift for Elias Landolt which will come in a still unnumbered fascicle of the Veröffentlichungen des Geobotanischen Institutes der Eidg. Techn. Hochschule, Stiftung Rübel, in Zürich sometimes early next year, 1986. That is also the place where I validate the generic name Trichopyrum for the former section Trichophorae, or the specific names T. intermedium and varnense, and mention how Elytrigia s.str. is restricted.

I am aware that not only in the key in the Feddes Repertorium paper are there several unfortunate faults, often caused by my inability to keep awake when constructing complicated keys that are more formal than essential and were, in this case, requested with short warning by the Feddes editorial staff, who in addition let several of the printers errors survive...about half of those latter your friend observed, but also numerous printing errors that I had pointed out but missed the attention of the editors, who were in such a hurry after almost two years of waiting that they hardly could make any corrections at all. However, more recently they have evidently gone through the paper to look for mistakes, as your good friend so nicely did also, because I got also from them a list of more or less innocent errors that they want to correct when they get space, they say, and asked for additions if any. So I am sending them your remarks...and several of my own...in the hope that they will correct them in the journal, though I am afraid that such corrections will rarely be observed by those who need them most. Printing errors one can never avoid, least of all in important papers or books...so that even the Bible, in any language, never has been free of this nuisance. Give Trevor Clifford my very best thanks and tell him that I hope my mistakes will be forgiven also by others, even those who never read a paper without seeking the faults of the author in the mistakes by the printer...as was my experience of two of my colleagues when I published my first paper on Acetosella in 1940! They never got their papers printed and never completed their exams, though I dared to risk printing errors and critics in more than 750 publications....

The name Eigopyrum you picked up from Dewey's good paper, into which it came from an earlier manuscript of mine, where I accepted Kihara's results from Aegilops vavilovii as indication of that three haplomes were involved, or B, D, and M, instead of DMM as shown in the printed paper. I did correct this when my manuscript was completed, but forgot to mention it to Dewey, so this is my fault...hope others who follow Dewey will look up what I did...though this is of less importance of course.

I am sorry that I have not been informed of the Washington symposium next year for reasons that you would hardly believe if I told you, so I am sure that we will not be there. But when you pass through California, I would appreciate to get an opportunity to meet you, so if you have time, let me know when, so I can at least try to pick you up at the airport, either in San Francisco or in San José, both are international, socalled.

With the very best regards and all good wishes,

Sincerely,

Form No. 701

Department of Primary Industries

In any further correspondence
refer to No.

Queensland Herbarium,
Meiers Road,
INDOOROOPILLY. QLD. 4068

15th July, 1985.

Dear Dr Löve,

Many thanks for your letter with updated information on the Triticeae. I have added the information for Psammopyrum. Where is this genus to be published?

Trevor Clifford from the University of Queensland was here yesterday and he had been looking at your revision and come across a few discrepancies between the keys and generic descriptions and we thought it desirable to draw them to your attention.

Hordeum — This genus is not perennial yet couplet 25b leads to Hordeum. The rachis in 29a is keyed as tough, yet the description(p. 435) has the rachis as tough or fragile.

Pascopyrum — On p. 483 the spikelets are described as 2 to a node and the anthers 5-6mm long, yet in the key the spikelets are solitary at each node(couplet 22b) and the anthers 3-4.5mm long (couplet 34b).

Hordelymus — On p. 441 the central spikelet is rudimentary and in the key the central spikelet is either perfect or male (couplet 28b).

Triticum — On p. 498 spikelets have 3-6-florets with one frequently rudimentary, yet in the key the spikelets have 3-5 florets, 2-3 being perfect.

Crithodium — The rachis is tough in the description(p 490) yet fragile in the key(couplet 7a).

Cylindropyrum — The spikelets are 1/node on p 500 but there is a mention of lateral spikelets in couplet 10a.
Couplet 6a/6b. The distinction is not clear-cut ie not mutually exclusive choices.

Dr Áskell Löve,
5780 Chandler Court,
San Jose,
CALIFORNIA 95123 U.S.A.

Somehow I have picked up the name Eigopyrum. Does it mean anything to you?

Do you plan to attend the Symposium in Washington on Grass Systematics and Evolution next July. I am trying to organize the possibility of going, so there is a possibility of meeting up there, if not then en route as I would have to pass through California.

With best wishes.

Yours sincerely,

Bryan Simon

(B.K. Simon)
Senior Botanist

THINOPYRUM, LOPHOPYRUM, TRICHOPYRUM, ELYTRIGIA, AND *PSEUDOROEGNERIA*: MORPHOLOGICAL SIMILARITIES[1]

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A reassessment of genome relationships between *Thinopyrum bessarabicum* and *T. elongatum* of the Triticeae[1]

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JAUHAR, P. P. 1988. A reassessment of genome relationships between *Thinopyrum bessarabicum* and *T. elongatum* of the Triticeae. Genome, **30**: 903–914.

Chromosome pairing and chiasma frequency in diploid ($2n = 2x = 14$; JE genomes), amphidiploid ($2n = 4x = 28$; JJEE), and triploid ($2n = 3x = 21$; JJE) hybrids between *Thinopyrum bessarabicum* ($2n = 2x = 14$; JJ) and *T. elongatum* ($2n = 2x = 14$; EE) were analyzed. The diploid hybrids (JE) showed a mean pairing of $<0.01\,V + 0.30\,IV + 0.28\,III + 4.98\,II + 1.97\,I$ with 8.36 chiasmata per cell. The pairing was rather poor, most bivalents being rod-shaped; some were clearly heteromorphic and loosely paired (probably pseudochiasmate). The diploid hybrids were sterile, showing the reproductive isolation of the parental species. The JJE triploid had a mean chromosome configuration of $<0.01\,VI + 0.06\,IV + 1.53\,III + 5.46\,II + 5.20\,I$ with a chiasma frequency of 13.45 per cell. Chromosomes of the duplicated genome JJ showed preferential pairing, forming mostly ring bivalents with two or even three chiasmata each, as in the *T. bessarabicum* parent; most chromosomes of the E genome remained as univalents. Thus, the E genome chromosomes offered little synaptic competition to the chromosomes of the duplicated JJ genome. The degree of preferential pairing was even stronger in the JJEE amphidiploids, which predominantly showed bivalent pairing with up to 14 ring bivalents in some cells. They had a mean pairing of $0.01\,VI + 0.55\,IV + 0.26\,III + 11.75\,II + 1.42\,I$; the mean quadrivalent frequency per cell varied from 0.10 to 1.53. Thus J and E genomes essentially maintained their meiotic integrity at the $4x$ level. This pattern of chromosome pairing in hybrids at different ploidies and the sterility of diploid hybrids show that J and E are distinct genomes and that there is little justification for merging them, as suggested by previous workers. The J and E are homoeologous at best. The merger of *Lophopyrum* (E genome) with the genus *Thinopyrum* (J genome) would be improper. Although the J and E genomes are close enough to permit some intergenomic gene flow, which may be exploited in plant breeding, they are certainly not close enough to have the same genomic designation. The JJEE amphidiploids are meiotically stable and may be a useful source of genes for wheat improvement.

Key words: genome, meiosis, chromosome pairing, phylogenetic relationships, *Thinopyrum*, interspecific hybrid, autoallotriploid, amphidiploid.

JAUHAR, P. P. 1988. A reassessment of genome relationships between *Thinopyrum bessarabicum* and *T. elongatum* of the Triticeae. Genome, **30** : 903–914.

L'appariement des chromosomes et la fréquence de chiasmas ont été analysés chez les hybrides diploïdes ($2n = 2x = 14$; génomes JE), amphidiploïdes ($2n = 4x = 28$; JJEE) et triploïdes ($2n = 3x = 21$; JJE), entre *Thinopyrum bessarabicum* ($2n = 2x = 14$; JJ) et *T. elongatum* ($2n = 2x = 14$; EE). Chez les hybrides diploïdes (JE), la moyenne d'appariements fut de $<0{,}01\,V + 0{,}30\,IV + 0{,}28\,III + 4{,}98\,II + 1{,}97\,I$, avec 8,36 chiasmas par cellule. L'appariement s'est avéré être plutôt faible; la majorité des bivalents avaient la forme de bâtonnets et certains bivalents étaient clairement hétéromorphes bien que faiblement appariés (probablement des pseudochiasmas). Les hybrides diploïdes se sont révélés être stériles, conduisant à l'isolation reproductrice des espèces parentales. Chez les hybrides triploïdes (JJE), la configuration moyenne des chromosomes fut de $<0{,}01\,V + 0{,}06\,IV + 1{,}53\,III + 5{,}46\,II + 5{,}20\,I$, avec une fréquence de chiasmas de 13,45 par cellule. Les chromosomes du génome dupliqué JJ se sont appariés de façon préférentielle; pour la plupart, ils ont formé des bivalents en anneaux avec deux ou même trois chiasmas chacun comme chez le parent *T. bessarabicum*, alors que presque tous les chromosomes du génome E sont demeurés univalents. Il s'avère donc que les chromosomes du génome E ont été peu compétitifs avec les chromosomes du génome JJ sur le plan synaptique. Le degré d'appariement préférentiel s'est même avéré supérieur chez les amphidiploïdes JJEE, lesquels ont présenté des appariements bivalents de façon prédominante, atteignant jusqu'à 14 bivalents en anneaux dans certaines cellules. Leur appariement moyen fut de $<0{,}01\,VI + 0{,}55\,IV + 0{,}26\,III + 11{,}75\,II + 1{,}42\,I$; la fréquence de quadrivalents par cellule a varié de 0,10 à 1,53. Donc, les génomes J et E ont essentiellement maintenu leur intégrité méiotique au niveau de $4x$. De tels patterns d'appariement des chromosomes chez des hybrides de ploïdies différentes, de même que la stérilité chez les hybrides diploïdes, démontrent que les génomes J et E sont distincts et qu'il existe peu de justification pour les fusionner, tel que suggéré antérieurement par d'autres chercheurs. Les génomes J et E sont, au mieux, des homéologues. La fusion du genre *Lophirum* (génome E) avec le genre *Thinopyrum* (génome J) ne saurait être appropriée. Bien que les génomes J et E soient suffisamment rapprochés pour permettre un certain flux génique intergénérique, qui peut être exploité en amélioration des plantes, ils ne le sont certainement pas suffisamment pour avoir la même désignation génomique. Les amphidiploïdes JJEE sont méiotiquement stables et peuvent constituer une source génique utile pour l'amélioration du blé.

Mots clés : génome, méiose, appariement des chromosomes, relations phylogéniques, *Thinopyrum*, hybrides interspécifiques, triploïdes, amphidiploïdes.

[Traduit par la revue]

[1]Cooperative investigation of the USDA Agricultural Research Service and the Utah Agricultural Experiment Station, Logan, UT 84322, U.S.A. Approved as Journal Paper No. 3582.