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

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

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30 December, 1990

Dr. Jan Jansonius
Esso Resources Canada Ltd.
Esso Plaza - Room 1415
237 Fourth Ave. SW
Calgary, Alberta, Canada
T2P 0H6

Dear Jan:

Thanks for response to my queries, etc.

This responds to you only where something important to your cards seems at stake. I was amused that you found my reference "just to card number" annoying--I remember writing you about the problem I have with the numbers and being informed by you that you have a numerical key. However, in reacting to your responses, a numerical list would now help me! The misspelling of "diagnosis" is in the next to last line of the card for Fususpollenites. The number seems to be 3888, just as I wrote it.

Th card I listed as 3886 is Fillaeopsidites. Looks like that number on my copy--could be 3686. That's the one that I note on my index has a type-species new combination not validly published (non-explicit designation of basionym).

Puri 1963 ref.: Puri, G.S., 1963. Some plant-micro-fossils from the Cretaceous and Paleocene of Nigeria. University of Ibadan Botanical Studies No. 10: 142 pp., 8 Plates. It's too long for me to xerox.

"Anulus" is just plain wrong in English (UK or USA).

Spelling is determined by usage. Chagrenate is the spelling used!--cf. Kremp's Encyclopedia, AGI Glossary of Geology. Your explanation of the etymology is good and useful, and I've hooked it into data for my book revision. But it really is pretty close to laevigate (cf. Kremp)-psilate. It would be best to drop it.

Presumably you have access to Andrews' generic list (U.S.G.S. Bull. 1300). That's my source for Polypodites Goeppert 1836. Xeroxes of Bolkhovitina's (accidental?) publication of Polypodites are enclosed. But remember it is a homonym of P. Goeppert.

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Re card 2439--spheroidal. I don't think sphaeroidal is right, even in UK.

I marvel that you did so well with my handwriting!

Thanks for the xeroxes.

Best to you for 1991.

Yours very truly,

Alfred Traverse

AT/et
encl: xerox

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Jansonius

23-XI-90

90-12-11 !

Dear Jan:

Phil Holmes, of the Boulter group, is a nice fellow, and he asked me to check the Permian, spore names in their data bank. It's educational — I'm using my generic list, which I've kept up over the years, plus cards, & other data. I've looked at 500 generic names. I hope you can read my scrawl, as I can't and this check generated these questions: Best to type this! A.E.T.

1. Aumanci sporites (card 191): where are you getting "upper Pennsylvanian"? Artinskian is Permian.

In the 60's there was argument about the placement of the Artinskian.

2. Balmeella PH has it as an "acr. fand." Best you & I treat as a spore. Latest info?

Best err on the safe side. I have no later info

#386 (xerox enclosed) 3. Why is there no card for Capillatisporites Lyuber in Pakhorshaya 1966?

4. ~~How is your (card 1070) misspelled the name of the type species of Cladaitina? (C. bernae, not C. bernae?)~~ Card 1004 I'll. from Boulton, seem to have at least a word missing: should be considered, or some such.

maybe my grammar is in error, or antiquated; I mean the same thing, but tried to express it more strongly

5. Card 1210 has specific epithet of type species misspelled — should be varius.

ouch! Thanks.

yes, ok; but not an "objective" (= nomenclatural) Synonym.

Card 1894. I suspect this was not validly published because Balme gave a footnote in saying it was probably a synonym of C. [illegible] var. [illegible]

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7. Card # 3262. Would you share with us how you determined that (last "1963" I wrote it on my copy; I think Hart must have told me, we were actively writing back + forth at that time. was or eff. published until 1965-? ("early" when = 1965-?)

Although announced as "Dec. 78", the foreword by the chief is dated as 21 Dec.. We rec'd our copy 20 Aug. 1979; I think it to be effectively 1979, but have no further proof at hand. Card # 3845 (not 3845, 3045, but 3645!). When do you set 1977? I have 1978 ("1976").

9. Card # 262. Betula-pollenites seem to be OK as of 1934 — monotypic when pub. because microexcelsus was not published validly in 1931, Combination of characters available for generic-specific description in 1934.

See POLLENITES #3745; I accept this genus as valid in 1931, and hence microexcelsus as valid, too (in Poll.). Thus, not monotypic in 1934 their combined descr. 1.7: "1931" for "1934"

10. Card # 403 to we you may have discussed, I regard Carya-pollenites Bot. & Zool. 1934 as validly published — based on combined description

Same circumstances as previous, if Pollenites valid per 1931

11. Card 408. For similar reasons to the above, I accept Caryapollenites for Raatz 1937, as I regard Pollenites for reasons not ind. pub. in 1931. It was therefore a "new" species.

I beg to differ, see above. All these hinge on when Pollenites was validly published.

~~12. Card 467. I have to pub. date as 1932~~

~~12. Card 509 Perhaps you already noted that spec. epithet needs to be changed to jacobii (Watson) Thus K. Jacobi in a way)~~

Also card in 1979, but

I went with the nominal Sept. '79 (Part 3 is nominal X/80!)

12. Card 3223, I wonder if it really was ~~1979~~? I got my copy on 22-IX-80.

13 Cards 3824, 5042. I concluded that the type sp. should be spelled rhizoporus, interpreting "rizophorous" as an obvious error. What about it?

"rhizoporus" would mean "root-shaped pore", which means nothing to me. "rhizophorus" = "beset by rootlets", which agrees more with the illustration. I'll stick with original spelling.

Card 3024, 3dl. from Upton, ~~prior~~ would be Vacuoli-, ~~rhizo-~~ (more" can only be if 3 or more) touché!

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14. Card 600. In my opinion the original
Yes, ok, as species imparmularis was from P.T. 1934, though
1934. However, its first use
as type sp. for a genus is for
Tricolpopollenites, q.v. ✓
15. Card 610, para. 4, l. 6: ...inasmuch as ...
ok.

16. Card 614. Emended diag — disregard
Corr. portion. for val. pub. in 1934 —
here you are right; I did Comb. gen. + sp. descriptions (Became
1st comphaeus on #3745 as 16
not validly published in 1931. C. comphaeus was a new sp. at that
(Same as Ithipollenites) point. You card #616 seems to
accept this point of view.) +

Thank you! 17. Card 658. Is the specific epithet
megaforaminis?

illuminated + elucidates 18. Card 3669. Last line: elucidatus
(must have been a late night!)

19 Card 670 Are you sure this wasn't OK in
Kremp 1949? I consider that Kremp published (validly) Pollenites largus, which
he compared with Cryptomeria. The capital P in Pollenites leaves me no choice. I don't
know that he really intended to publish "Cryptomeria pollenites"; we can't double guess anybody.
20. Card 3673. Is there a chance the species is
named for a person. It should be corrected
to tripakshii? (for some reason I apparently
Maybe a given name, not family name?
once thought so.) Authors used —ii in several other names, but here
only —i. I took their lead.

20 Card 752 Shouldn't the specific epithet
be corrected to minutus? Oh yes!

21. Card 760 I've got a problem here — apparently
OK in 1953 (no holotype required before '58). I thought original gen.-sp. description could
Potonie is first author to specify the holotype, and must be followed
stand, and disregarded it as ex Pot. 1960.
Comment?

22. Card 777 What do you do a Gert Dicolpites
Vimal 1952? (Proc. Ind Acad. Sci. 36:139)
Vimal has "Dicolpites (Erdtman) Sp.nov." : nom. nud., nothing
to it (not even if reference to Erdtman is in error)

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23. Card 778. Is the generic description OK? In a non-monotypic genus can a combined generic-specific description — from one species — be used to found a new genus, as it by definition excludes the other species.

We're screwed whatever we do! Srivastava explicitly did not propose a new genus (with one species "ancoris" in it), but raised an existing and typified subgenus of Krutzsch. The latter's type remains the type in generic rank. However, Srivastava at the same time, "corrected" the orthography (-i- instead of -ae). I stick with that in the generic rank, so as not to muddle things even more. Maybe we should undo the arrogant "err. orth."; the ICBN allows such connective vowel corrections only for adjectival epithets of species rank. (Art. 73.8)

23 Card 826. Maybe I wrote about this before. But, it now has been -i- for so long

As Srivastava had only one species, D. ancoris, I don't see how the type can be anything else. The new genus was monotypic when published. It's not necessary to designate a type species of a monotypic genus, thus D. ancoris was the type — not so?

Yes, it should go back to -ae; it will in the next Supplement

24 Card 863 shall edit specific epithet be corrected to spinosus? Yes

25 Card 861 I guess you're already corrected this 3→4 pts —? Yes; Correction was issued earlier (I made the correction by hand)

26 Card 883 last line: Dyadosporites Clarke

I agree that it is not incorrect to give "ex" after the rest of the reference; but in the discussion (three) it seemed to me to be clearer.

27 Card 886 Am I wrong that the transfer of the type species is not validly published — If so, what insufficient citation of basionym? Nothing, because holotype identified in 1965.

I'd leave well enough alone; Vdh + Garcia did explicitly give the ID of the holotype in their new specific description. In 1961, the description is ± in code in a table. Let me know if you differ (n. comb. only is at stake).

28 Card 899 shall edit alfonsi be corrected to alfonsii → Prob. not; Leidelm. apparently named for his close friends, using their given names, not family names (not "patronymicus"); thus alfonso → alfonsus → alfonsi

29 Card 901 I believe the citation at top is wrong.

Yes, you are right; if card is wrong. I carry it as Echiricadopites (Van der Hammen) Germeraadelm. however, it is not a simple raise in rank; there is also the attempt to exchange a new fossil genus for the old Recent pollen which was nomen nudum. I tried to express that "new sense" by "ex", but I only muddled things.

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Cand 1207.

38. Were here around a thi before. It is clear to me that Poljarkina 1949's organ[illegible] is an indep- dichotomous key does not mean there is no descr.- the descr. now, it is true, consists of pieces that must be reassembled, but the wording is IC&N that specifically forbids that mode of presentation. As of 1949 no type species was required. Thus, though Byssella may be acceptable, it seems to me to have been (tragically) validly published.

With Maljavkina, because her work provides no firm image of her conceptual taxa, I refuse to bend a little; I thrown out as much as I technically am allowed. Anyway, the present Code requires a holotype for a genus after 1957.

I won't spend more time on this, and suckers people into thinking that there might be something there.

you're right!

39. Cand 1223. Same old problem — I accept ✓ He deiroidites as val. pub. ✓ in BTI 1950 (why ~~1951~~)

40 Cand 1241. Para 3, l. 3: "hy[sic]late"? (= hilate?) = hyaline?

41. cand 1255, last line: Chaetidae (cf. 1278, where it is correctly spelled)

42. cand 1278. diaeresis is "permitted", but not required Pe diaeresis on Azollaënsis (cf. Art 73.6 ll. 5-8) should have been retained. Wasn't Azollaënsis subsp. "Azoriaënsis" provided inter var., forma etc.? If not, I believe it should be provided on the card next generation marks.

43. Cand 1297. Same old story — I accept No! Liciopelenites Wolff 1934 on grounds of monotypic publication, arguing that neither Pocknites nor P. margaritus were val. pub. in 1931.

I cannot agree with you, since it is not "monotypic": Wolf gives descriptions for 3 distinct forms: margaritatus Pot., of margaritatus Pot. and of propinquus Pot.

44. Cand 1307. My interpretation is same as yours, but I have a disquieting thought: is it possible that Van der Hammen's "tipos" were not intended as nomenclatural types? If so, the generic (pre-1958) names must be kept as formally published fossil species as substitute types for his old Recent tipos, we must assume that they were intended as nomenclatural types.

Since van der Hammen + students

I always thought it 'sneaky' that fossil plants can carry separate names for parts, but not Recent ones; however, who needs names that can be avoided?

OK.

45. Cands 1337 & 1339. Krutzsch's preference for [illegible] Not Art. 34.1; indeed, both names are validly published. They are distinct; 1959 name will be re-issued, with emend. diagnosis? Cand 1337 should be kept

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Engelhardtiapollenites

30. Card 927. Perhaps we've already joined about this. In my view, the type species was not valid. pub. in 1931, even if Lewis was validly published 1937, and genus is acceptable as Canb. and earlier.
I won't buy this one sp. — gen. doesn't.

See under 3745 POLLENITES &

31. Card 929. For reasons cited just above, I'd reject this as synonym of Engelhardtiapollenites. You can never "reject" it; you say make it a synonym. However, #927 was not Senior.

32. Card 992 I would have thought that forma typica was not validly published (Art. 24.3). If so, might the species with appleata - appleata etc. is incorrect?

See Art. 26.1, ex. 1

33. Card 3886? I have a note that the type species new combination not validly published because of not-explicit designation of basionym.

3886 PARASPORA is ok
are you talking about another genus?
(I hate you writing down only numbers, not names of genera)

3888? 34 Card 3888 next to last line: diagnosis
can't find what you are on about.

35 ~~Card 3887~~ Crassipollenites I don't find a card for this one, but I considered it validly published — Potonié was wrong in thinking that the ? invalidated it.

#1151 xerox enclosed

36. You apparently rejected all of names in Newer heard of Puri 1963. Puri 1963, such as Granadicolpites? Can you make me a copy? It is pretty iffy — the illustrations are I'd accept a printed text with photographs, not printed matter — but the original photos, provided it was text is printed. Does the illustration requirement properly distributed.
(If I have no card, I did not mean that they have to be printed with see/know about. If I reject, ~~note?~~ the text of the page must be I still would make a card and printed.
state my decision)

37. Card 1175: These Indians were ahead of their time! — designated a specimen as "genotype." I missed that one, but it is not material, is it?

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46. Card 1424, next to last l. ; micron (or μm)

You are correct; but I'll let it go by. These cards were typed when we had no μ, and no easy way to correct text other than retype (for which there was no time)

47. Card 1437. Acc. to me the nov. comb. for the type species was not val. pub. — two pub. cited, therefore no explicit direct citation. Am I wrong? (Yes, I think)

error in bibl. ref. under generic name is no cause for considering invalid the full + correct bibl. ref. under descr. of n. comb.

para. 2, l. 2

48. Card 1446, Cyrillaceapollenites misspelled. (same mistake on card 4274)

49. card 1496. Same old problem. An alternate view (my comment) is that L. was val. published in 1938 by Roots in combined description because Portalites squinerae was not val. published in 1931 and characters not in 1938. If this is correct, it is Neogloecenites which is the obligate junior synonym.

You are right

I'll reserve

50. card 1528. Here you reveal puzzle me. You apparently regard ~~Say that~~ Luckenovia ~~was~~ not validly published in 1963 because "no holotype" was designated (for its type species) ~~in 1963~~, but that concept did not come in until Monotype & Combined descr. in two decades later. It seems to me that "based on new sp." ; if there is no holotype (a very marginal, a combined species, there cannot be a generic description provided, the type of based on it. The genus is "indicated" viz. L. sahnii. Now, of course, the Code would explicitly reject validity of Luckenovia. (See ICBN 1961, Art. 42, or current ICBN Art. 42.1 : "if all other requirements are fulfilled") I believe that L. sahnii was not validly published for lack of a holotype, but in 1963 that did not invalidate the generic name. Am I wrong?

51. Card 1581, last line ; Columbia

52. Card 1634, para. 2, l. 2 ; annulus

The British/Americans use a corrupted spelling with -nn-, whereas the Germans, correctly, use the Latin spelling (little anus = annulus)

(I have seen "anulus" (1 scarlet, but I find no support for it, even in Spanish publications)

However, in an English translation, the -nn- is accepted and preferable.

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LO is better, but both have been used; I'll let things go.

53. a. cand 16:2, para 3, l. 4: LO analysis. by.

b. same cand, last line: would it be better not to discuss romina and da in comparison? Yes, but I didn't know if it might be validated

54. cand 35:76, 2nd column, para. 3, l. 1, Nichols (no apostrophe)

55. cand 1607, para. 2, l. 4: Huxaceous

56. cand 1706. I regard Monoporopollenites as not val. pub. in Meyer — no generic descr., and therefore cite it as "Meyer ex Potonié" — not right? (I can't find my copy of Meyer's paper) Very poor work, without any value, but still names validly published.

57. cand 1708, para 2: "a pollen grain" or just "pollen" — not "a pollen"! a pollen = a kind of pollen. I'll keep it as is.

58. cand 1713. Is the specific epithet in need of ending-correction? no, it is an (looks like an) early = "eo" Spathiphyllum.

59. cand 1716, para 3, l 1: canvulus

60. cand 1738, near to last line: canvulus

61. cand 1739, para. 2, l 2, Eucalyptus.

62. cand 1740, last line: baculum = backla, but not baculae! should be "columellae"

h. I don't see why M. would not val. published in 1951 — can you explain? looks o.k. to me. in 1951 no generic diagnosis, just "M. thiergarti n. spm", but without description, and an imprecise reference to "a form already described by Thiergart 1940, herewith is given a name" — no indication which form, just an ill.

63. cand 1752, para. 2, l. 4 Margins

L. margo, pl. margines
Fast line pl., margins

near colpi = close to colpi, or along colpi.

64. cand 1759, =3878 refers to ~~Some areas~~ near colpi" mean?

Elsik has "from some areas along colpi".

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Subsp. obtusangulus had been raised to species level by Krutzsch 1954, then made type of new subgenus by Krutzsch 1959; this raised to genus by Pocock. However, Pocock did not validly publish n. comb.,

65. card 1800. I don't understand why the type species isn't Obtusisporis neddamii instead of which was done O. obtusangulus. Can you explain? by J. + H.

66. card 1805. why isn't Octacolpites Vimal not validly published? (pre-1958!) Have you looked at Vimal 1952? There is nothing there!! ?? don't get what you are referring to

67. card 1806. specific epithet should be brevicolpus

68. card 1807. Para 3, l. 5. cudanulus; chagranate [see cards 1814, 1772, 4023 for that annulus is often spelled that way in the cards.]

See note on loose sheet

69. card 1818, specific epithet should be both forms allowed corrected to oculusnoctis? (Art. by 3.9; as they stand 3.9). If not, then how about card I'll leave things 6.13? At least, these should agree!

70. card 1836, para 2, l. 2 annulus; l. 3 chagranate.

71. card 1858. Should specific epithet be corrected to indicis?

72. card 1859. Should be eocenicus (My notation indicates that you had corrected only the ending.) I thought I had that earlier, but can't find that card.

73 card 1864. It seems to me that the type species also has to be S. sivori ex Dutta & Sah — it can't be published in 1962 by Dasgupta, right on! as the genus was validly pub. Then!

epithet should be —us

74. Card 1867 specific epithet should be eocenicus

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75. Card 1882. 4 ll. from bottom; chagrenate?

76. card 1884, para 1, l. 5 — keyTowns (cf. 2 ll. from bottom)

77. card 1734 (=4312), para. 3, l. 5, clawi !!

78 card 1931, last l. ? --- surface.) ✓

79 card 1933. This is a general comment: in what is a taxonomic work, the author responsible should be provided for new taxa, as it I gave you is here for Peltandra virginica but not for Smilax herbacea. I'll do it in future.

80. Card 4021., It seems to me that the type species nov. comb. was not valid. pub. if N. Comb. was validly published.

81. card 1939, para. 3, l.? annulus?

82 card 1974, para. 1, l. 2 palisades

4. should spec. ep. either be corrected to meusicii?

83 card 3589 para 3, l. 3, margins. (I thought of margo.

84 card 2073, para 3, l. 3 chagrenate

85 card 2075, para 4, l. 2, annulus (also in para 3, l. 4)

86. Card 2107. This is a mare's nest. As far as I can figure out, Polypodites Bolkhovitina 1959 was val. pub. as conserved for gen. spec. descr. that ruins 2107. But, there is also the problem of Polypodites Goeppert 1836.

Not on our shelf; can't comment for now.

Is Goeppert name validly published?

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87. Card 2118, para 4, l. 4 rejuvenation

88. Card 2131. I assumed that spec. ep. that should be corrected to pramathii — not so? I wouldn't know; — ii only if it is a patronymicum.

89. Card 2152. Last line: Iversen

90. Card 2201. 3rd line from bottom: annuli

91 Card 2209. 2nd l. in for bottom: Thymelaeaceae

92 Card 2221. ~~The only objection is that~~ the use (twice) of "pollen grains" as one word is not English I suppose it comes from Pollenkern or the Swed. equivalent.

93. Card ~~2225~~ 2225 ~~Also seems [illegible] of pertaining to~~ I can. I can convince myself that V. d. H - G. M. really published a ~~nov.~~ nov. com. for the type — full & direct ref. is lacking? There is also a P. stellatocolpatus (Puri 1963, but it was don't have Puri. not val. pub. — no type designated.

94 Card 2228. Last line: Guiana (cf. Card 2229)

95 Card 2231, para 1, l. 6 Col. 6. a

~~96. Card 2233. This is also the problem of~~

96. Card 2257, a para 3, l. 4, annulus

b. I believe it is possible to argue that the genus was val. In agreement with my acceptance of Pollenites as of May 1931 (See Fredskrift), pub. by Raatz 1937, as contrast P. stellatus was valid. Will you apply for an gen.-spec. decis. because Pollenites official rejection of Pollenites? I think it was not val. pub. in 1931. to be beneficial to accept the earliest possible date for Pollenites.

97. Card 2305, para 2, l. 8: Clavus

98 Card 2316. Should not specific ep. thet be ~~variolatus~~ to henricii? No. Henricus is a given name (I think for Henry Potonié); thus not a patronymicum.

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99. Card 3755. Why "1979"? I thought 1978, app. We talked about this basis of arrival of my copy. Before. I'll write Lucknow for clarification.

100. Card 2324. Was type specier transfer strictly: no val. pub.? I thought no direct reference apparently Kotz. has all kind of direct references to descr. + type, but no basionym. Were J+H first ones to effect valid transfer?

101. Card 2345. last paragraph, head.: end last word: wrong word. Variability is meant. Translators work, not mine. (but I should have edited it).

102. Card 2351: per Art. 73.9, the specific ep. that ~~I thank~~ should be changed to aforementioned

103. Card 2352, 4th line from end: anthulus

104 Card 2376. OK re Erdtmani Reticulomap. Vimal 1952, p. 136, ¶1: "I have ---- arranged (my species) under the non-committal classification of Erdtman (1947)." ~~Vimal (1952) no specier, they weren't reg. until pre-1958.~~

105 Card 2384, para. 3, l. 2 colpi: Kremp also lists colpa, = but prefers colpus

106. Rat: palyad. ter Puri: 1963 — why no card? I don't have Puri 1963! Help! Admittedly, it's unavailable (because of the illustration). But you're the kid for worse! In fact, I'm not convinced that Art. 38 really forbids what Puri did.

107. Card 2393. Shall the specific ep. that be corrected to angelii? I suspect this to be a given name; if she's a girl, it should be: angelae; if a boy: angeli.

108. Card 2402 It seems to me you screwed up slightly here, though the end result isn't affected. Viburnum mayrlum needs to be mentioned in the explanation of your other similar cases) Sorry; it is a bit cryptic. However, the key to the solution is in the last line.

109. Card 2407. a. para. 2, l. 6 illegitimate

b. It seems to me your argument is unnecessary - it's just a plain old homonym of R. Ramanujam, period.

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110. Card 3904, para 4, l. 3 cf/ pus

111. Card 2439, next to last l. Spheroidal? American, hé?

112. Card 2468. a) As Sabalocidites was monotypic, and a very clear reference was made in P-Th-Th. to the description of S. areolatus, the sole species, in 1934, why exactly is S. val. published? Its status as a synonym of Sabalpollenites is irrelevant to this question.

Sole, but not new: Pollenites areolatus Pot. '34

b) l. 2: what has "1951" got to do with it?

113. Card 2488. We have a major disagreement here. I believe Sambucoidites was val. pub. by P-Th-Th '51 — a miniature descr. prior, an illustration. Why not? Maybe a prelim. name, in anticipation of future acceptance. Certainly no rank indicated.

and what about Viburnoidites?

114. Card 2509, 4 ll. from bottom: sinuous

115. Card 2512. I believe you should have noted that generic-specific descriptions were sp. op. or logical parts. Correctly printed in plate description.

116. Card 0516. last line: Columbia

117. Card 2527 a) sp. op.: knoxi ✓; b) l. 4 trilete is not an noun — yet. I guess I blame Saksi. I don't mind if the language keeps changing "a trilete" is unambiguous, and brief.

118. Card 2530. Here I've got big problems, and I earnestly hope you'll help:

Do you mean Cicatricosisporites? See also card 468

1) C. cicatricosisporites according to your interpretation should be P. flagellaris Pot. & K.?

Please take the time to formulate your problem specifically + precisely. I can't see any problem with my conclusions, derived from this mix-muddle.

2) Really urgent — explain why C. cicatricosisporites was not val. pub. by Potonié & Kremp 1955 [1954]?

Where did I say this? See Card #467; it sure is valid in 1933

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119. Card 2566. para 1, l. 4, usually

120 Card 2615, last line: injured damaged
quaint usage, maybe, but they are synonyms.
a pollen grain cannot be injured?

121 Card 2617, a) para. 3, l. 3 chagenate
b) specific epithet should be corrected to cechovicii?

122 Card 2618, l. 4 selected quaint, but synonyms,

123 Card 2627 a) According to any notes, S. was monotypic when pub. Why did Bot. need to designate a type? (fig. 11, 12 were different specimens)
b) para. 4, l. 4 annulus

124. Card 2657, para 5, l. 3 — my copy looks like marginal

125. Card 2663, specific epithet must have accent mark removed.

126. Card 2672 last line: "only a few ...."

127 Card 2679. a para 2, l. 2, Ganulus
b should it specific epithet be corrected to documentos? (not that it matters much)
No, noun in apposition (="proof")

128 Card 2695 last two lines:
Good, we were so young and innocent, then. not "Rules ...." but "International Code of Botanical Nomenclature"
(I'm surprised by this change)

129 Card 2700. This is a funny one. V.d.H 1951a was the generic name seems not to me as much as Naumova 1939 & 1954 (specific not required). However, a scheme, not a classification. de[illegible] description of type of 1954 must be followed, right? And this does it.

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130. Card 2703. a) Para. 3, l. 10 : annulate

b) was the spec. epithet validly transferred? Acc. my notes, there is no direct reference [no basionym!] ex Trabut? Pot. Syn. II

131. Card. 2754, para. 2, l. 4 : spec. fei apparently epithet is, also screwed up. Should be : sarstedtensis. Also, why is Knutzsch in parenthesis?

~~132 Card 2775,~~ DELETE COMMENT

133 Card 2778. What do you do about Striatosporites Puri 1963? don't have

134 Card 2781. I believe your heading at top is wrong. Should be Striatricolporites (Van der Hammen) Leidelmeyer? (Van der Hammen) Leidelmeyer? See xerox; I'll leave things as I had them. (is a subgeneric name changed in rank not a "ex" case, despite the fact that the subgen. name was illegitimate?)

135 Card 2787 a) there is a case for "ex": Van der Hammen ex ---- no: homonymous

b) para 1, l. 7 and stomosing See # 3413

136. Card 2796. Why isn't Subpilonapites mal. pub. as of Venkat? Lack of specific does not make pre 58. Monotypic?

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137. Card 2799. Para 2 is baffling. Unless used each of the type species of Longapolleniti., I don't foresee much logic. These are Sah's remarks, with which I do not agree. They give a perspective on his work.

138. Card 2800, l. 3, annuli, 3 lines from bottom: endannulus

139 Card 2818, para. 3, Columbia

140 Card 2823. I agree that the thing is not worth it, let's leave it lie, then much, but I believe it was validly published — didn't need a type? There is nothing that says a descr. can't be published in key form. There is the matter of rank; the keys are on more than one level.

141. Card 2824. a) Specific ep. should be corrected to Rokkii — not so?

b) Para 2, l. 1 microns

142 Card 2830, next to last line annulus

143. Card. 2839. It seems to me the argument can be made that Taxodiaceaepoll. was OK in 1949 as to descr. para: the species was new (not prev. published). Therefore what is provided by Pot. Kremp is OK as combined descr. Ref. is an illustration. The type species was not validly "transferred", but it didn't used to be. The problem is use of the name — Kremp clearly did not mean more "Taxodiaceaepollen. ter pollen. ter" Coal" is capitalized and connected by a dash, not a hyphen. Kremp was merely saying the O. hiatus was taxodiaceae. (Also, the last sentence in para 1 is very nearly opaque — Indeed, I cannot understand its meaning. not clear English.)

it was validly described as Pollenites hiatus Pot 32, thus no option for later combined description

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144 Card 2841. In most (all?) other cases such as you say, nomen nudum. Why not this? Also — the label "obligate junior synonym" is ridiculous — published 7 years earlier! It is just a nomen nudum. This could confuse people as to what "junior" means!

mea culpa; I must have blundered when I copied Potoini's text. You are correct

145 card 2566, l. 3 chagrenate

146. card 2873, next to last line. Reference is pres. to Elaeocarpaceae. (In a transl. I don't see any reason to perpetuate a misspelling, if it came from TFR.)

147. card 2876, para 2, l. 5, endaxylus

148. card 2899. If this were presented to the General Comm. for a vote as to whether it is effecting a homonym of Thymelipollis I'd prob. vote yes! I might, too; maybe. However, we are not free to correct a wrong connective vowel in a genus name.

149. Card 2903, para 1, last line, chagrenate

150. Card 2904, last para. Can't be "junior" — whatever else it is. (I. was pub. in 1953!) You are correct; maybe I thought "if it were still to be validated)

151. Card 2919. According to my notes, the elevation and transfer of tonus was not val. pub. in Krutz. — no direct ref. to Hassiongyna. Comment? Yes, but on p. 92 in Krutz '59

152. Card 2954. Para. 1, l. 6, annulus, endannulus

Para 2, l. 5 chagrenate

poor quality control. (N.B. Card 4875 spells annulus two ways: para. 1, l. 6 vs. last line)

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153. Card 3010. right column, l. 2: anecdotal ouch!

154. Card 3036. correct sp. epithet to riedelii

155 Card 3044, para. 2, l. 2 gleicheniod

(I believe "and" in l. 2 was previously corrected.)

last para: Palaëont. (twice)

156. Card 3062, para. 1, l. 7, dannulus (also in next to last line)

157. Card 3081, para 1, l. 5 annulus (same thing in para 2, l. 2 twice)

158. Card 3110, para. 3, l. 5, according

159. Card 3121, para. 2, l. 5, annulate

160 Card 3139, l. 5, Verrucaria. las

161 Card 3198, last line Colombia

162 Card 3259, a) diacritical mark must be removed from sp. epithet.

b) para. 3, l. 4 aanuli

163 Card 3263, para 2, l. 4 columellae

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30 October, 1990

Dr. Jan Jansonius
Esso Resources Canada Ltd.
Esso Plaza - Room 1415
237 Fourth Ave SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Response to telephone message Betty took: lists of "sanctioned names" (such as the existing one for mycology) operate pretty much as does the current list of conserved names, i.e., a Code is still needed for naming procedures and for the handling of names other than those on the sanctioned or conserved lists.

Check card 1523 for an example of JJ (uncanny) anticipating the future--Lucknowia was rejected (apparently) because the type species lacks a holotype!

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

TO AT

DATE 25.X.90 TIME 1:15

WHILE YOU WERE OUT

M. Jan Jansonius

Of

Phone

TELEPHONED	☒	PLEASE RETURN CALL	☐
CALLED TO SEE YOU	☐	WILL CALL AGAIN	☐
RETURNED YOUR CALL	☐	RUSH	☐

MESSAGE Think about this: after list of names in common usage is out, shouldn't change the Code any more, or will make hash of list? Should then throw out the Code?

e.g. he is working on proofing Fensome & Williams' Taxa of Acritarchs, and has found name Aktasia pollenites. This genus was valid until Code got hold of it and declared it invalid.

[p.s. hope I got this straight!]

Signed BT

The Standard Register Company

no urgency re returning call.

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18 September, 1990

Dr. Jan Jansonius
Esso Resources Canada Ltd.
Esso Plaza - Room 1415
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

faxed to: 403-237-2197

Dear Jan:

Your specific comments on my outline (90-IX-13), the letter of same date that goes with it, and the 90-IX-11 "chapter coördinator" letter are here. All of the items on the 13th came, in fact, twice, about eight minutes apart! But I'm a 110% convert to fax--marvellous!

I appreciate your comments and suggestions very much. They will be most helpful. I hadn't realized, for example, that you were partly thinking of a how-to approach. That's good to know. The suggestion for getting Dan N.'s help is very useful. I am surprised and disappointed that Chaloner has refused to be involved.

I gave a paper on Devonian spores at APP in Krefeld (in German) last year. For weeks I practiced saying the Latin names more or less as Latin (not English) and did pretty well, but it's hard after so many years--and not without problems (as Pinus, for example).

More later.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

Dr A. Traverse
Deike Bldg. 435
University Park, PA 16802, USA FAX (814) 865-3191

Dear Al,

90 09 12

Thank you very much for the chapter outline. We apologize for being behind schedule. Several of the chapter outlines were late themselves, and we find - probably as many of our coordinators do - that we are involved in many other things and have many other competing obligations. We will address the matter of scheduling again in Banff, and would like to hear from you. We want to keep the schedule as tight as is feasible, without, however, thereby jeopardizing the quality of each contribution.

First, there are some general remarks:

1-We will have an integrated bibliography in the back of the book. Reference to authors in the text can be made by citing author and year. We will not include references for authors that are only cited in nomenclature. Although the Bibliography is in the back, we expect each author to include his share of this list to come out of his own page allowance.

2-We will have - in smaller font - an Appendix with all the names mentioned in the text together with their author and year; these authors will not be automatically included in the Bibliography.

3-We will not have a Glossary of morphological terms. There is a good Glossary in Traverse, and Punt et al. are working on another one to be ready in 1992. However, we probably will allow a concise glossary of such terminology in the chapters dealing with Fungi and Miscellaneous Groups, as well as in Chapter 2, dealing with Codes.

4-Names used in the text can be given without author citation, as complete documentation is listed in the Appendix. Authors are responsible for preparing such an extended listing as an appendix to their typescript.

5-In many chapters there will be an unavoidable overlap, as it makes sense to mention paleogeography and ecology in the context of taxonomy, and vice versa. We encourage individual authors to be aware of such occurences, and to limit their own discourse to pertinent remarks and cross-references to the more elaborate treatment in the section dealing with these aspects in greater depth.

Whenever an author has significant data in the other field, he should contact his counterpart as early on as possible and offer such data for inclusion (over his own initials) in the other chapter, at the same time arranging for proper cross-referencing.

6-In our original book outline, the main headings in section 2 included captions on 'applications' and 'paleoprovincialism'. As suggested under 5), these should be dealt with in section 3, with appropriate cross-references in section 2.

7-We will let our UK authors write in English spelling, and our North American authors in 'American'; however, it might be useful to keep to one spelling style within each chapter.

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Now for more specific comments on your own chapter outline!

Overall it seemed to us that you want to discuss many problems that are worthwhile in their own right, but that not necessarily guide the uninitiated towards proper practice. We suggest that you run through the main requirements of the ICBN and explain the do's and dont's, possibly as exemplified by significant examples. (To mind come things as Van der Hammen's Recent types; Tasch's taxa based on pseudosculpture.)

The ICBN is of most immediate importance to us. Of course, sometimes we must use the ICZN, and the different approach there should be discussed. However, this points up to the need for a small glossary, explaining such things as: 'correct' is the equivalent of 'available'; 'effective' is not 'valid'; 'valid' may be 'illegitimate'; 'indicated' means...'directly or indirectly cited'?; the various kinds of types; etc.

We thought that your section VII may get uncomfortably close to polemics in an area still very much in flux, and we wonder if this chapter is the proper soapbox to take a stand on.

Proper formation of new names is another point where the public often appears ignorant or confused; I wonder if Dan Nicolson might want to collaborate on that. The grammatical requirements of a name (endings, connective vowels), abhorrent fashions (giving us "judithlentinae"), proper derivation from place or personal names, adjectives versus nouns, etc.

[One of my pet peeves is the pronounciation of names. If names are to be Latin, or treated as if they were Latin, we should try to pronounce them in a Latin way, and not as if they were American (Tree-lay-tes, rather than Trey-lee-tees). This would make papers more easily understood in an international audience. Of course the spate of Chinese names, based on the new Pinyin Romanisation of Chinese phonetics, makes for some pretty weird Latin pronounciations!]

In general, we were pleased with your outline and the obvious amount of thought that has gone into its formalisation. We hope that our suggestions may help you in striking the balance that makes the chapter both interesting and instructive. If you want to discuss any of the above points, or any other matter that may come up, please feel free to contact me, or any other member of the editorial board. I think that the more we keep our venues of communications open and active, the better the end product will be, and the more efficiently our efforts will be directed.

Wishing you inspiration and success with your labours,

sincerely,

Jan Jansonius

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COVER PAGE

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A. Traverse
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USA

FACSIMILE NUMBER	(814) 865-3191

FROM ➤ NAME / COMPANY / ADDRESS
JJ

PHONE NUMBER	237-2438

SPECIAL INSTRUCTIONS / CC's / ETC.

MESSAGE: CONFIRM TRANSMISSION WITH END RECIPIENT

Dear Chapter Coordinator,

90 09 11

This letter will break a period of silence that reflected the leisure time of summer. However, it was not a period of non-activity.

We received chapter outlines from nearly all of you. A few outlines have not yet been submitted, but there are reasons, such as being out of town, out of the country or even out of the continent. Basically, everything is in hand.

The chapter outlines received now have been discussed by the editorial committee, and in the next week or so, you will receive written comments and suggestions. We are most impressed with nearly all the outlines, and are strengthened in our enthousiasm for this undertaking. It is going to be an outstanding contribution that wil do all of us proud.

You'll be first in line!
JJ.

We have discussed plans with AASP to keep the production "in house". However, we also have made contact with some of the "non-profit" publishing houses such as Princeton U., Oxford U. &c. There, we have found a very ready ear, and sincere interest to provide the actual publication mechanics.

At the AASP meeting in Banff, the Editorial Committee will hold a meeting to discuss various aspects of the book and its progress, and you are sincerely invited to join in. We will meet Tuesday 9 Oct. at 1400h in the Norquai room of the Banff Springs Hotel. Later that week we will also present our progress to the AASP Executive. Of course, during the remainder of that week, we will have opportunity for less formal communication.

If you have any further questions, concerns or suggestions, please drop me a FAX, letter or give me a call. Hoping to see you in Banff,

Jan Jansonius.

975580

NUMBER OF PAGES INCLUDING COVER PAGE 1

Hunt Institute for Botanical Documentation

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12 September, 1990

Dr. Jan Jansonius
Esso Resources Canada Ltd.
Esso Plaza - Room 1415
237 Fourth Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Yours of 28 August re Acaciapollenites (II) was interesting. Betty and I have searched high and low and can't find your previous letter, though we did find my response of 20-VIII-90. So, unless you can provide a xerox of yours (there were enclosures, etc.) I can't oblige as far as sending the whole on to Chaloner is concerned. Sorry. (I must have been very creative in my filing.)

I am still hoping to get your reaction to the outline-suggestions for my nomenclature chapter.

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

28 Aug 90

Dear Al,

Thank you very much for your reaction re Acaciapollenites.

Acacia myriosporites Cookson 1954, being prior to 1958 (see 37.1) was and is validly published.

Mildenhall 1972 validly published Acaciapollenites; the generic name was valid in 1978 too, but the 1983 changed 10.1 to: "The type of a (name of a) genus is the type of a (–) species", i.e. a specimen. But 37.1 maintains the 1958 date while it maintains its back-reference to 10.

There is no other conclusion, than that Acaciapollenites is not validly published when judged by the only applicable code, i.e. the latest edition. That starting date should have been adjusted!

Mildenhall 1972 states clearly: Holotype not indicated. Then, he refers to a "possible type", but did not bite the bullet. Later (last 91) he gives coordinates for his "reference specimen", but that is his own material, not Cookson's.

Will you please copy the letter I sent you, plus your reply to me, and send the lot to Chaloner for Comment?

Your notion of a bank neo-/lecto-/topotypes is valuable. That would be a much better way to invest the money now being squandered by the likes of Boulter on list of names in Common

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usage", etc.

I hope I'll hear a second reaction also from Zijlstra, Punt, Nicolson etc., but I won't hold my breath.

Enjoy the end of Summer!

John

*PS Nicolson may be "basically" right ("it is self evident") but not "technically", i.e. according to present rules.

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27 August, 1990

Dr. Jan Jansonius
Esso Resources Canada Ltd.
Esso Plaza - Room 1415
237 Fourth Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

May I have a brief critique of my proposed nomenclatural MS outline for the book?

Hope all goes well with you.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

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20 August, 1990

Dr. Jan Jansonius
Esso Resources Canada Ltd.
Esso Plaza - Room 1415
237 Fourth Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

The letter, xeroxes, etc. etc. re Acaciapollenites helped "ruin" a Sunday afternoon here. I believe Nicolson is basically right. The generic name A. looks o.k. to me as of 1972, regardless of the previous status of A. myriosporites as a specific name. I was assured in the corridor at Sydney, because the matter troubled me, that the new stipulation that a genus would now be typified by the holotype of the type specimen of the type species would not overturn existing generic names, but "merely" would require that holotypes, lectotypes or neotypes be selected in some cases. From that point of view, A. myriosporites needed a holotype. Didn't Mildenhall select a type with his "reference specimen" and thus validate the species? I would regard Mildenhall's "ref. spec." as the type for A. and for A. myriosporites. Mildenhall mentions no other specimens (see 37.2).

Another interesting aspect of the Nicolson letter was the question of typification by illustration. As you know, I have favored that for paleopalynology for a long time. We must come to it.

Somebody should sharpen up the Code vis à vis both generic names 1958-88 based on species that were not validly published but clearly indicated (I would validate the lot of them). Also, typification by illustration for palynomorphs should either be permitted, or we should start a huge bank for neotypes and lectotypes somewhere!

Yours very truly,

Alfred Traverse

AT/et
xc: Nicolson

Hunt Institute for Botanical Documentation

90/07/17

Dr. G. Zijlstra
Dr. W. Punt
Dr. D.H. Nicolson
Dr. E.G. Voss

Dear Al: for your info + amusement

Dear Gea, Wim, Dan, and Dr. Voss:

Thank you for your replies to my questions regarding *Acaciapollenites*. In this response I'll repeat some of Gea's statements in English because there is little point in sending copies of her Dutch epistle?

All of you were right, of course, that Art. 10 was changed already in 1983. Art. 37 was forgotten in 1983, but "adjusted" in 1988.

Gea, you say that holotype, lectotype, and neotype can be used on more that one level (species, genus); if a specimen is selected from syntypes, it becomes a lectotype. Similarly, if one species is selected from several to become nomenclatural type of a genus, you also call it a "lectotype". By extension, a species originally designated as nomenclatural type of a genus, is a "holotype" (species). However, Art. 7.3 states explicitly that "a holotype is the **one specimen** ..."; Art 9.1: "the type of (a name of) a species is a single specimen"; Art. 10.1: "the type of ... a genus is the type of a species"; and Art. 37.1 requires that "Publication on or after 1 Jan 1958 of ... a new ... genus is valid only when the **holotype** of the name is indicated." Holotype, in the Code, exists only on the species level.

Gea said that e.g. a section, published without a type before 1958, can validly be raised to generic rank after 1958 without designation of a (lecto) type. But: that deals with a new rank, not a new taxon as per Art. 37. This is not relevant to the case of *Acaciapollenites.*

Mildenhall, in 1972, designated a nomenclatural type, in accord with Art. 10 (ICBN 1972): "The type of ... a genus ... is a species". This was still the guide in 1978, but in 1983, this was changed to "the type of a ... genus ... is the type of a ... species."

Dan Nicolson wrote that Art. 10.1 continues with "For purposes of designation/citation of a type, the species name alone suffices, ... i.e. it is the full equivalence of its type". But he is wrong when he continues that ("therefore") "generic names are typified indirectly." Since 1983, these rest, indeed, very directly on the type specimen which is the type of (the name of) a species; see Art. 10.1.

Art. 37.1 requires explicitly "indication" of a holotype specimen, for a (name of a) genus to be valid. Obviously, there was no holotype in 1972; ergo, *Acaciapollenites* was not validly published as of 1983 (or, as Dr. Voss claims, as of 28 August 1981, that date being the closing date of the session where all changes of the Sydney Code were adopted and "became official at that time". Really, each Art. should have a date attached, because some are changed later by the Editorial Committee! I wonder how Dr. Voss squares this "starting date" of each Code - as mentioned explicitly in the

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preface to each Code - with the last sentence of the Preamble: "This edition of the Code supersedes all previous editions").

Anyway, Gea and Dan accept *Acaciapollenites* as validly published in 1972. Dr. Voss and I think it is not; but Jansonius and Hills unwittingly validated the name in 1976, when they indicated a lectotype. Of course, it was valid when published in 1972, but the Sydney Code willfully made it invalid on 28 August 1981, viz. 1983.

Dr. Voss is adamant that the 1983 Code requirement for a holotype (specimen) for valid publication of the name of a new taxon at any rank of family or below is not new, but precisely identical to the requirement of the old Art 37.

However, nomenclaturally, the exact requirement from Paris Code 1956 (Art. 9) through Leningrad Code 1978 (Art . 10) has been that the name of an order be typified by the name of a family; that of a family by that of a genus; a genus was based on a species, and only a species on a specimen (holotype, neotype or lectotype: Art. 9.1). **Taxonomically,** even the order always would have hinged on the characters of that holotype; **nomenclaturally,** until 1983 it only determined the name of the species. Dr. Voss calls, for support to have nomenclatural type = holotype, upon Rec. 37.A which really deals only with where in a protologue the type should be indicated; the phrase "...typus (or holotypus)..." does not indicate that a nomenclatural type is a holotype, but rather that there is indeed a difference between them. Anyway, a Recommendation has no binding force.

Many conversations and correspondence with Lanjouw, Traverse, and others in the past have assured me that the **nomenclatural type** of a name of a genus was indeed the name of a species. Genera proposed since 1958 with a diagnosis and a designated type species were accepted as valid, those lacking designation of a type species (unless monotypic) were not accepted (in the Genera File of Fossil Spores).

My problem - and argument with some of you - is that, in my opinion, it is very clear that the earlier editions of the Code (1956 - 1978) required that for valid publication the designation of a name of a species was required as nomenclatural type for a name of a new genus; that of a genus for a new family, etc. For a species, a specimen was required. This was explicitly stated, and nomenclaturally adequate. In the 1956 Code, with adequate warning, this requirement for a "nomenclatural type" was announced to begin per 1958.

In contrast, in 1983 the Code maintained this 1958 date, but explicitly changed the requirement of a "nomenclatural type" (a species for a genus, etc.) to that of a specimen, for new taxa of any rank up to family. This is not just semantics; it is the manifestation of a different philosophy. This retroactive change now mandates a thorough revision of much nomenclature (at least in the field of palynology, with which I am most familiar), and thus proves damaging to nomenclatural stability; all of which goes against the intentions of the first paragraph of the Preamble.

Gea's modern "cream" of botanical nomenclaturalists may think that of course, what the Code all along intended when it stated "nomenclatural type," was really "holotype". And, what matter old editions, once the latest edition goes your way? But, that was not what the old editions clearly stated.

I am not against the notion of a holotype defining the name of a genus. I am against the way it - without fair warning - was imposed as a new requirement with a starting date of 1958, in an edition published in 1983.

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If, as Gea writes, further such refinements are contemplated in Tokyo, please give the working systematicians advance warning, so they will not in all innocence burden future nomenclaturalists even more.

Gea remarks - correctly, I think - that even if Mildenhall validly published *Acaciapollenites* in1972 according to the 1972 Code, that is of no consequence if its 1972 publication does not meet the requirements of the latest Code (1988). Thus, *Acaciapollenites* could never be considered "previously validly published" (Art. 43) in context of the 1988 Code, the only current measure for validity.

I am bemused, rather than amused, by the observation that acknowledgedly expert, or at least experienced, nomenclaturalists find such widely variant conclusions supported by the same Code. I am confirmed in my belief that often it is wisest to live with an imperfect balanced status quo, rather than strive for theoretical (dogmatic) purity. I fear already for the innovations that will find acceptance in Tokyo.

Still, my warm thanks to all of you for your detailed and elaborate answers. I hope I may call on you again when I have other difficulties.

Best regards, sincerely,

Jan Jansonius

cc: A. Traverse
R. Fensome
L.V. Hills

Hunt Institute for Botanical Documentation

Dr. Jan Jansonius
Esso Plaza West, room 1415
Calgary, T2P 0H6, CANADA

Department of Botany, NHB166
Smithsonian Institution
Washington, DC 20560, USA
20 June 1990
FAX (+01 01) 202/786-2563

Dear Jan,

Basically I don't think you have a problem. Acacia myriosporites was validly published in 1954. Acaciapollenites was validly published since Mildenhall (1972), Art. 37.1 requires the type of the name be indicated. Mildenhall INDICATED the type by naming a species (miscalled a "type species", as if a taxon itself [a variable entity] could be a type), and Art. 10.1 says "For purposes of designation or citation of a type [of a generic or infrageneric name] the species name alone suffices, i.e., it [the name] is considered as the full equivalent of its type." Art. 37.2 also applies, and is satisfied in terms of Art. 10.1's equivalency.

Generic names are typified indirectly. They rest on the type of a species name and cannot directly rest on a specimen that isn't the type of a species name (except by conservation under Art. 10.3).

Note: Art. 10.1 changed in 1983 (Sydney) Code, not 1988 (Berlin) Code.

1. The generic name [not genus] Acaciapollenites was validly published in 1972. In any case, the generic name was typified since the author (used the word "type" and did not include any other species names (the genus began life as unispecific and no choice of type must be made within the provisions of Art. 10.2.

The typification of the species name is a different question probably was first "effectively" done by Jansonius & Hills (1976). Arts. 8.2-4, the only current provisions defining "effective" lectotypifications, were satisfied. As you point out, there could be doubts. However, unless there is evidence that elements belonging to different taxa are involved, the application of the name(s) is not in doubt.

There is some question whether an illustration can itself be the type of a species name of a fossil. Art. 7.18 seems to say that the type is the "specimen whose figure ... [was] ... in the original publication. This sounds like Art. 10.1: the type is not the species (illustration) itself nor the name of the species (plate/figure name and number) but the specimen underlying the name. If so, citation of the name (plate/figure) is the equivalent of its type.

2. Under my interpretation, Acaciapollenites validly published 1972, it is hard to answer your second question. If, let us say, Mildenhall (1972) included no species name(s), and thus failed to satisfy Art. 37, then your situation would arise. However, between 1972 and 1983 [your 1988] no species names could be added unless the generic name was validated.

Another scenario is that Mildenhall could have included more than one species name without specifying a type, thus failing to meet Art. 37.1. Adding more species names would not result in validation of the generic name and none could be validly published until the generic name was typified under Art. 43 (and Art. 45.1).

3. 1 Jan 1958 was chosen as a limitation date to Art. 37 because it came into the Code, as Art. 35, in the 1956 (Paris) Code.

4. No. Your account should not be applied. The revision of Art. 10 in Sydney put enough safety in that your interpretation can be avoided.

PS. Yesterday we had a memorial service for Dick Eyde.

As ever,

Dan

Dan H. Nicolson

Hunt Institute for Botanical Documentation

UNIVERSITY OF MICHIGAN HERBARIUM
NORTH UNIVERSITY BUILDING
ANN ARBOR, MICHIGAN 48109, U. S. A.

June 26, 1990

Dr. J. Jansonius
Calgary

Dear Dr. Jansonius:

Your communication of June 19 reached me at our Biological Station where, as always, I am teaching field courses in the summer. (I left Ann Arbor May 19.) I do not have with me very much in the way of nomenclatural resources other than my memory and a copy of the latest Code. As you must know, I have been trying to retire from nomenclature. I have not been Rapporteur since 1981 and have not been Secretary of the General Committee since 1987. I hope you have sent your questions to one or both of my successors in those positions, respectively Prof. Dr. Werner Greuter of Berlin and Dr. Dan H. Nicolson of the Smithsonian Institution in Washington, D.C. Time permits only some brief remarks on your questions, but I think there are some misinterpretations of the facts that ought first to be clarified, and then you may be able to deal with the specific case at hand.

First of all, the new Code did not "come into force" in 1988. The effective date for changes in the Code--any edition of the Code--is not the date of publication but the date of the closing plenary session of the Botanical Congress that accepted those changes. (See p. x in the Preface to the Berlin Code, stating that decisions "became official at that time" [Aug. 1, 1987].)

Secondly, I would not say that "1 Jan 1958 [was] chosen as the retroactive starting point for the requirement of a holotype to be the nomenclatural type of a genus [i.e. generic name]" as you do in your question 3. The Code has long required that publication on or after 1 Jan. 1958 of the name of a new taxon at any rank of family or below is valid only "when the nomenclatural type is indicated." I assume this requirement dates from the 1954 (Paris) Congress, since the starting date is logically after that. Rec. 37A has equally long made clear that "the nomenclatural type" can be indicated as either "typus" or "holotypus." Any name associated with what we would call syntypes has not been validly published since 1 Jan. 1958 and there is nothing new about that. Use of the word "holotype" to say the same thing does date from the 1987 Code.

Consequently, unless there is some special provision for fossil plants that I have neglected to note, the generic name Acaciapollenites was not validly published under the Paris Code (publ. 1956) or any subsequent Code, until Jansonius & Hills (1976)--assuming that the latter did refer to a holotype and that pl. 1 fig. 1 of Cookson (1954) which they designated does indeed represent a holotype (a single specimen or preparation; cf. Art. 9.1 & 9.4). Rec. 7B.6 is only a recommendation and the fact that Jansonius and Hills did not simultaneously publish the figure with their validation of the name does not invalidate it. (Neither the fig. nor the 1976 publ. is before me.)

And yes, since the name was, apparently, validly published by Jansonius and Hills in 1976, combinations allegedly made with that name prior to 1976 [not 1988!] are, under Art. 43, not validly published.

Sincerely yours,

Edward G. Voss
Curator & Professor

P.S.: In essence, I believe no change has been made in the Code for the past 36 years that affects your case.

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Cookson 1954 AUSTRALIAN J. BOT., V.2

The deposits in which *Acacia* pollen has occurred are all situated in Victoria. They are a ligneous clay at Grange Burn near Hamilton, the age of which, according to Gill (1952), is Upper Pliocene; the leaf beds at Sentinel Rock, Aire coast (Singleton 1941), which Dr. O. P. Singleton has verbally stated are underlain by Balcombian clays (Lower Miocene) and overlain by Pleistocene dune limestone; dark and yellow clays from the Stony Creek basin, near Daylesford, of Middle Pliocene age (Coulson 1950), and a dark grey silty Quaternary clay rich in sea shells from the shore-line between Marine Avenue and Dickens Street, St. Kilda, near Melbourne.

IV. Descriptions of Fossil Pollen of the Acacia Type

The number of pollen grains comprising the polyads of living species of *Acacia* seems sufficiently constant for use in the separation of fossil sporomorphs of this type — on this basis two generalized sporomorphs can at present be distinguished in Victorian deposits, one with 8 pollen grains per polyad, the other with 16. The frequencies of both types have been low but particularly so in the Stony Creek occurrence.

Acacia octosporites n. spm.

Plate 1, Figs. 2-4

Polyads composed of 8 grains, circular (*c.* 21-27 μ in diameter) or oblong (*c.* 32 by 37 μ) in polar view. Single grains vary in shape according to their position, central ones quadrangular, about 8 μ in surface view. The exine of the distal wall is relatively thin and even.

Occurrence.—Ligneous clay, Grange Burn, near Hamilton, Vic. (Upper Pliocene, Gill 1952).

Acacia myriosporites n. spm.

Plate 1, Figs. 1, 5-8

Polyads comprised of more than 8 pollen grains (16 in present examples), circular (37-43 μ) or oblong (37-61 by 35-59 μ) in polar view. Grains of central group about 12 μ; distal walls thin and even or slightly indented.

Occurrence.—Ligneous clay, Grange Burn, near Hamilton, Vic. (Upper Pliocene). Sentinel Rock (A.W. 2 of Wilkinson), Aire River, Vic. (above Balcombian clays (Lower Miocene) and beneath Pleistocene dune limestone). Clays at Stony Creek, Daylesford, Vic. (Middle Miocene). Clay at St. Kilda, near Melbourne.

V. Macroscopic and Microscopic Plant Remains Associated with Pollen of the Acacia Type

(*a*) *Clays at Stony Creek, near Daylesford*

(i) *Macroscopic.*—A small collection of plant remains from the dark ligneous clays of the Stony Creek basin was examined by Patton (1928).

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V.10, no. 3 1972

SYSTEMATICS

ANTETURMA : *POLLENITES* R. Pot., 1931
TURMA : *JUGATES* ?Erdtman, 1943
SUBTURMA : *POLYADITES* Pant, 1954

GENUS: *ACACIAPOLLENITES* gen. nov. Mildenhall
1956 *Polyadites* van der Hammen p. 78 (nomen nudum).

TYPE SPECIES: *Acaciapollenites myriosporites* (Cookson, 1954) comb. nov. Mildenhall (here designated).

TYPE LOCALITY: Grange Burn near Hamilton, Victoria, Australia; Upper Pliocene.

DIAGNOSIS: Obligate polyads, shape spherical or elliptical, each polyad with 8, 12, 16, or 32 individual cells, central cells approximately square or rectangular, peripheral cells elongate; germ pores, if present, vestigial; exine thin,, sexine thicker than nexine; sculpture psilate, shallowly pitted, or baculate and scabrate, with a vestigial quadrangular furrow running parallel to the amb on the distal surface of each cell; size usually over 40 microns.

DISCUSSION: This genus differs from *Polyadopollenites* Thomson and Pflug, 1953 in possessing a vestigial functionless furrow on the distal surface of the individual cells. *Polyadites* van der Hammen, 1956 is not valid since the genus is based on recent pollen grains of *Acacia retinodes* Schlecht.

BOTANICAL AFFINITY: With the Mimosaceae.

ACACIAPOLLENITES cf. **A. MYRIOSPORITES** (Cookson, 1954) comb. nov. Mildenhall
(Figs. 1–13)

1954 *Acacia myriosporites* Cookson p. 55, pl. 1, figs. 1, 5–8
1956 *Polyadites multicompositus* van der Hammen p. 78, pl. 4 (nomen nudum)

HOLOTYPE: Not indicated. For reference as a possible type see pl. 1, fig. 1 in Cookson (1954).

TYPE LOCALITY: As listed under type species.

REFERENCE SPECIMEN: Slide L3566/3; Leitz Ortholux Microscope 540328; co-ordinates 41.9 × 124.8; England Finder Reading U43/3; reference point (lower right-hand corner of cover slip) 21.4 × 107.3.

REFERENCE LOCALITY: N144/512, grid reference 379668; south bank of Makawakawa Stream, Pohangina Valley; Castlecliffian (middle Pleistocene).

DESCRIPTION: Grains in obligate polyads of 16 individuals with 8 central cells surrounded by 8 peripheral cells; individual cells anisopolar, irregular in shape, central cells quadrangular, peripheral cells more elongate with a convex distal side, contact faces usually straight but rounded at corners providing interstices; polyad spherical to subspherical or elliptical in equatorial view, oblong or elongate-elliptical in polar view, peroblate, radiosymmetrical, bi-convex; apertures not determined; exine thin, 1.5–2.0 microns thick on distal surface, thinner along contact faces, sexine thicker than nexine, sexine two-layered, noticeable thickening of exine at corners, sculpture psilate to indistinctly and shallowly pitted giving an almost scrobiculate OL pattern, pits rather regularly distributed and surrounded by a vestigial, linear furrow in exine forming a quadrangle with sides approximately parallel to the amb of each individual cell, furrow on distal surface of grain only, exine on distal surface undulate (caused by the pits), with two deeper indentations (caused by linear furrows), exine tectate.

DIMENSIONS: Individual cells 17–24 microns (peripheral cells). Central cells are slightly smaller.

Longest equatorial diameter	45(56)69 microns	measured parallel to arms of the central double tetrad of cells.
Shortest „ „	37(48)61 „	

Overall size 45(57)72 microns (longest axis in equatorial view). Polar diameter 31 microns (one specimen only). Australian specimens have an equatorial diameter ranging from 37–61 microns (Cookson, 1954).

DISTRIBUTION: This form species is found in Australia from the Lower Miocene to Upper Pliocene, and in New Zealand from the Waitotaran (Upper Pliocene) to the Holocene. It is doubtful whether the Australian specimens can be distinguished from some of the species of *Acacia* living in Australia today. Very rare in occurrence.

BOTANICAL AFFINITY: The fossil pollen grains are indistinguishable from and almost certainly represent specimens of *Acacia* (Mimosaceae). The practice of providing a form generic name for the dispersed pollen is followed here because other genera produce pollen similar to *Acacia*. This is particularly the practice in pre-Quaternary palynology where the possibility always exists of extinct genera producing pollen indistinguishable in the fossil state from present-day species. The relationship of dispersed pollen to macroplant fossils, and to extant species, is always uncertain in Pliocene or older rocks.

REMARKS: Occasional fragmentary polyads and solitary cells of *Acacia* are found, caused by disintegration during acetolysis. They are

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Re: Validity of *Acaciapollenites*

In 1954 (p.55), Cookson described *Acacia myriosporites* "n. spm.", with illustrations (pl.1, fig 1,5-8), but without designating a holotype. The abbreviation "spm" stands for "sporomorph", and indicated that the specific epithet was assigned to fossil forms, treated as form species, in contradistinction to the Recent species of *Acacia* to which she had made reference. This is here accepted, and not a reason to consider the publication of *A. myriosporites* invalid for lack of proper procedure.

In 1972, Mildenhall proposed *Acaciapollenites* as a more suitable receptacle for fossil pollen of *Acacia* type. He gave a diagnosis, and designated as nomenclatural type *A. myriosporites* (Cookson) Mildenhall. According to the ICBN then in force, this genus was validly published. Mildenhall noted the lack of a holotype, but did not select a lectotype from the 1954 isotypes; rather, he stated that "for reference as possible type see pl.1, fig. 1 in Cookson (1954)."

Jansonius & Hills (1976), rather uncritically, wrote that the holotype of *A. myriosporites* was pl.1, fig.1 of Cookson (19554), as the "lectotype designated by Mildenhall(1972)."

In 1988, the ICBN, Art 10.1 newly states that "the type of a name of a genus is the type of a name of a species". Further, Art. 37.1 requires that "publication on or after 1 Jan. 1958 of the name of a new genus is valid only when the holotype of the name is indicated".

In 1972, no lectotype had yet been designated for *A. myriosporites*, and hence for the genus *Acaciapollenites*; consequently, under terms of the present Code this generic name cannot be considered as validly published in 1972. A lectotype was identified in Jansonius & Hills (1976), but erroneously credited to Mildenhall (1972).

Questions now to be answered:

1- Is the genus *Acaciapollenites*, under the present Code still not validly published? Is it to be considered as validly published as of 1976, when a lectotype was indicated by Jansonius & Hills (even if unwittingly, because with an incorrect bibliographic reference)? In the latter case, is authorship of the genus to be assigned not to Mildenhall, but to Jansonius & Hills?

2.-If any species were assigned to *Acaciapollenites* between 1972 and 1988 when the new Code came into force, and the name *Acaciapollenites* became invalid), these must be considered as validly published in any case under Art 43, because they were "assigned to a genus that was validly published previously". Can we thus have validly published species under a genus itself not valid?

3.-Why was 1 Jan 1958* chosen as the retroactive starting point for the requirement of a holotype to be the nomenclatural type of a genus?

4.-Is the above accounting indicative for correct procedure now to be applied to all of (paleo)botanical nomenclature ?

* Would "1988" not be more prudent, and as satisfactory?

Jan Jansonius
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Hunt Institute for Botanical Documentation

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23 July, 1990

Dr. Jan Jansonius
Esso Resources Canada Limited
Esso Plaza, Room 1415
237 Fourth Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Bill Chaloner was here last week, and we were discussing Murospora. Re card 1730--Murospora: Playford's ref. to Simozonotriletes as "ex Pot. & Kr. 1954" disagrees with card 2600 "...Naumova 1939 ex Ischenko 1952." Maybe in some future revision of 2600 there needs to be an explanation of whether S. was really validated by Isch. '52 or still needed validation by P.& K. in 1954.

All the best.

Yours very truly,

Alfred Traverse

AT/et

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FACSIMILE NUMBER	814 - 865 - 3191	PHONE NUMBER	2438

SPECIAL INSTRUCTIONS / CC's / ETC.

MESSAGE:

CONFIRM TRANSMISSION WITH END RECIPIENT.

Dear Al,

Thank you for fine copy of Shaw's paper, which I was working on this weekend. Much appreciated!

Jim

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11 June, 1990

Dr. Jan Jansonius
Esso Resources Canada Limited
Esso Plaza--Room 1415
237 Fourth Ave. SW
Calgary, Alberta, Canada T2P 0H6

faxed to: 403-237-2197

Dear Jan:

Xerox of the Cheng-long Shaw paper (used a new copier with a "photographic" setting!) went off to you a while back. Let me know if it doesn't arrive soon.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

Dr. A. Traverse
435 Deike Bldg
University Park, PA 16802, USA

FAX (814) 865 3191

Dear Al, 90 05 30

Thank you for the two faxed letters. I'll briefly respond, after I found nobody home at your telephone.

As Chapter coordinator, you are entirely free to write the whole thing yourself (a "playing coach" in hockey?). But you are also free to parcel things out . One such a coopted side-show might be a contribution from Dan Nicolson on the role of Latin, and how to become sufficient in that area with the least amount of tears. However, you have complete freedom to arrange your affairs as you see fit. Of course, as editor, I may want to make suggestions at any stage where I think that may be useful. I think your ideas were on the right track.

Regarding the two chapters I did for you, it took some time, but I think such nasty knitpicking is useful at this early stage. I may have been wrong, or understood wrongly, or was seduced into a wrong conclusion by sloppy phrasing. The authors then can decide why I had cause to react the way I did. If they think it is me, rather than they that is wrong, then that is fine by me. They might be right. I also have virtually no information on the book itself, its background, scope, or intended market, and therefore could not say "that's good enough for this project". Anyway, I hope it has been of some use to you.

I was sorry to hear about the upheaval in your family, and feel sorry for your mother who now has to suffer through that dejecting last stretch of the downslope of the curve of all our lives. The trauma cuts the deeper , the happier the memories are with which this is compared. We can only be thankful for the happier times, and to those who made them so.

Et ceterum, I would like to ask if you could loan me, **soonest**, the full paper by Cheng-long Shaw 1984 (J. Taiwan Mus., **37**); suddenly, and at last, we make headway on the next Supplement of the Genera File, and I would like to have all his new stuff in there.

My best to both of you,

Esso Plaza West, 1415
Calgary, Canada, FAX (403) 237 2197 Jan Jansonius

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Deike 435
University Park, PA, USA

FACSIMILE NUMBER	814 865 3191

FROM ➤ NAME / COMPANY / ADDRESS
J. Jansonius
EPW 1415

PHONE NUMBER	237 2438

SPECIAL INSTRUCTIONS / CC's / ETC.

MESSAGE:

CONFIRM TRANSMISSION WITH END RECIPIENT.

Please send more complete copy of Cheng-long Shaw 1984, with ill., pl. description. I'd like to make cards of new genera, emend, etc., but do not have this paper, or an address.

Thanx,

Jan

Joe

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29 May, 1990

Dr. Jan Jansonius
Esso Resources Canada Ltd.
Esso Plaza-Room 1415
237 Fourth Ave. SW
Calgary, Alberta, Canada
T2P 0H6

faxed to: 403-237-2197

Dear Jan:

As you know from the "book letter," I have been pretty much "took up" lately. However, I haven't forgotten about other obligations"

I just re-read yours of 12 April about the book for which I agreed to write a chapter on nomenclature. I suddenly realize that what you may have had in mind was my getting up a joint chapter with other authors. I had intended to write by myself and then get critiques--yourself, Chaloner, perhaps others--which could result in co-authorship.

Could you counsel?

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

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29 May, 1990

Dr. Jan Jansonius
Esso Resources Canada Ltd.
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237 Fourth Ave. SW
Calgary, Alberta, Canada
T2P 0H6

faxed to: 403-237-2197

Dear Jan:

It is embarrassing to be so tardy in acknowledging receipt of your reviews of the Habib et al. and Brugman & Van Bergen manuscripts.

About the same time as these arrived, my already very confusing Spring Semester (three full courses, plus project courses, etc.) went completely awry, as I had to drop everything and go to Michigan to get my mother into a nursing home. Very difficult assignment. In a couple of weeks my sister and I assemble out there to close up the family's home, and terminate a reality that I have always irrationally regarded as permanent.

It will be interesting to compare your reviews with the others of the two chapters--that I have yet to do, but will soon. Maybe I'll wish I'd never thought of this project. Of course, you're a very harsh, approaching draconian, reviewer, but that's what I wanted!

Many thanks to you for this very considerable piece of work.

Yours very truly,

Al

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

Dear Al, 90 05 01

I finished reading the two MS; I was not too impressed!

Habib + van Pelt have a knack of writing contorted English. Their internal logic and phrasing are at times less than impressive.

Their thesis (transgression = many dino Spp.) is teasingly simple; I was wondering who determines what is a species. Some of their graphs are persuasive, but I know that in other parts of the world things are not so simple.

I was disappointed in their illustrations, which give not nearly enough info; the attempts to alleviate this by reams of descriptive text did not make things much clearer for me. I suggested quite a few changes, many in the margins (of text and illustrations).

Brugman + van Bergen wrote up a decent M Sc project, but not in a sophisticated manner. Again, internal inconsistencies, and typos in the References, indicate to me a sloppy rushed job. I couldn't see this as a valued chapter in any book; before it is that, it must be rewritten, have a more integrated format. However, having no idea what "book" this must go into, I may be too critical. (But I was thinking: is IT worth the tree?)

I wish you good luck with all this!

Best regards to both of you,

Ps: I have no objection to being identified as one of the reviewers.

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
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sent Fed. Express

2 April, 1990

Dr. Jan Jansonius
Esso Resources Canada Limited
Esso Plaza, Room 1415
237 4th Ave. SW
Calgary, Alberta, Canada T2P OH6

Dear Jan:

Thanks for your fax--only problem with it was it was "dated" 6/18/1986!

Here are the two MSS. As to deadline--I'd like to say the usual "as soon as possible", because if you do better than average, it will help me with the laggards. Let's say 1 June for a date after which I'll be hurting badly if I haven't received them back.

Best wishes.

Yours very truly,

Alfred Traverse

AT/et
encl: Brugman & Van Bergen MS; Habib et al. MS

Hunt Institute for Botanical Documentation

Nearly I 29-III-?

06/18/1986 02:31 10702024 P.01

?

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FACSIMILE NUMBER	814 - 865 3191	PHONE NUMBER	2438

SPECIAL INSTRUCTIONS / CC's / ETC.

MESSAGE:

CONFIRM TRANSMISSION WITH END RECIPIENT.

Dear Al,

How can I refuse such an offer as to review 2 chapters. I don't know how qualified I am in either subject, but will give it my best shot: ok! Please let me know your real deadline.

Best regards,

Jan.

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970690

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22 March, 1990

Dr. Jan Jansonius
Esso Resources Canada Ltd.
Esso Plaza, Room 1415 faxed to 403-237-2197
237 Fourth Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

There seems to be much truth to the old adage that if you need something done urgently, ask a busy person.

I have two (not especially long) chapters for the book I'm editing that I would like you to review for me. One is by Dan Habib ("Palynology of Sedimentary Cycles"), the other by Pim Brugman ("A Quantitative Palynological Approach to the Study of the Triassic of the Germanic Basin...."). If you're willing, I'll ship xeroxes to you, on which you red-pencil directly. I would like a review of the same sort you'd do for a journal.

Hoping to get a yes, and with best wishes,

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
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University Park, PA 16802
phone: 814-863-3419; fax: 814-865-3191

14 March, 1990

Dr. Jan Jansonius
Esso Resources Canada Limited
Esso Plaza, Room 1415
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

As IFPS archivist, I have been asked by people at Hunt Library to identify the two photos enclosed. Can you help?:

1. Who is the bearded man?--signature on reverse is illegible (Manten?).

2. What meeting is the group?--I guess it's the Tucson 1st IPC.

3. Do you know where I could obtain a key to the group photo?

4. Is that you next to Frank S. in the group? Ye gods.

All the best.

Yours very truly,

Alfred Traverse

AT/et
encl: xeroxes of 2 photos

Hunt Institute for Botanical Documentation

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13 March, 1990

Dr. Jan Jansonius
Esso Resources Canada Limited
Esso Plaza, Room 1415 faxed to 403-237-2197
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Your response came in an hour or two--marvellous!

"Aciphylla" indeed came from Boulter's list. He had sent it for me to check--I guess to you too.

Grace does plan to publish a replacement name. (I recommended Taxon.)

The critical matter re "rosalindae" seems to me, after reading what you sent and checking ICBN, is 73c.2, where a similar case is treated--Rosalind can be latinized to Rosalinda, indeed should be. So, you're right, unquestionably. However, in the rest of 73c, I don't find support for your contention that family names and given names are to be treated differently. Maybe you can help on that.

Incidentally, it seems to me that Traverse does not end with a vowel, as the 'e' is silent. I believe it should be dropped to produce traversii, though if an author did it otherwise, he must be followed (it is not a correctable error). There is one modern plant named for me by a real Latin scholar, and it's Erigeron traversii ("Traverse's fleabane"--ouch!).

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

DR.A.TRAVERSE
DEPT. GEOSCIENCES, PENNSYLVANIA STATE UNIVERSITY
435 DEIKE BUILDING
UNIVERSITY PARK, PA 16802 U.S.A. FAX 814-865-3191

90 03 12

Dear Al,

Thank you for the remarks received this morning. I'll quickly respond, as briefly as possible.
Sorry about the "Catalogue", a sloppy mistake that well may occur in a few other spots. However, I'll not start hunting for them. If you find another one, please let me know; I'll keep my eyes open as well.

I never heard of J. Hector, or of "*Aciphylla*". I'll let it go by for now; it probably will turn up on Boulter's list of Names in Common Use, and I'll take it from there.

Two reasons for "*rosalindae*". Latinization of patronymica, or given names, involves the insertion of "*i*", before the suffix "*-us*" (v.l. "*-a*"), like in Jansonius, or Julius (the most famous scion of that family was a Caesar). On the other hand, Augustus is a given name, not a patronymicum. Hence, a spore named for the Caesar would have the epithet "*julii*", one for the emperor Augustus the epithet "*augusti*". Yet, there are some irregularities; names on -er are latinized by a simple *-us*: a name in honour of Couper would give the epithet "*couperi*". Also, names ending in a vowel receive the straight suffix "*-us*"(Traverse > "*traversei*"), and names ending in "-a" receive no suffix at all, but are treated as Latin as they stand (Dijkstra > "*dijkstrae*"), much as the word *rosa.* (or Agricola). Rosalind is reasonably Latinized as Rosalinda, and in the genitive gives "*rosalindae*"; but in any case, as it is a given name and not a surname, there should not be an "*i*" (indicating that it would be a family name). See also Rec. 73C.1(a).

Re *Aletes* Somers: is Grace going to do anything about it, or leave well enough alone? If needs be, she could propose a new name that could be published in the next Supplement.
You are entirely correct re "*hohlii*". That name was given to honour Prof Hohl; I perpetuated the spelling as given by Krutzsch. I'll include a correction in the next Supplement.

Thanks, and best regards for both of you.

Most sincerely yours,

Jan Jansonius
Esso Plaza West, rm 1415
Calgary (fax 403 237 2197)

Hunt Institute for Botanical Documentation

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12 March, 1990

Dr. Jan Jansonius
Esso Resources Canada Limited
Esso Plaza, Room 1415 faxed to 403-237-2197
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Just noticed that Catalog of Fossil Spores and Pollen is misspelled in your card #11 (6 lines from the bottom). I wonder if it's o.k. elsewhere? I really think "Catalog etc." should be italicized or put in quotes, or some such.

In a list I am looking at, there's an alleged fossil pollen genus, "Aciphylla," supposedly published by one J. Hector in "Detailed Cat. Geol. Exhib. Indian Colon. Exhibs. London" Do you recognize? (no other data). I suspect it's not a palynomorph.

On card 57 you change "rosalindii" to "rosalindae." Why not (per 73c(b)) to "rosalindiae"?

I note (card 58) that you were aware that Aletes Somers is a homonym--but Grace treated it as news when I wrote her very recently!

On card 284, should hohli be changed to hohlii? I seem to have convinced myself at some point that it was a name.

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

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12 February, 1990

Dr. Jan Jansonius
Esso Resources Canada Ltd.
Esso Plaza, Room 1415
237 Fourth Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Many thanks for the boo-boo list from Paleopalynology. If I should live long enough, and if sales (despite, ahem, certain unfriendly reviews) justify it, your packet will be very helpful in putting together a second edition. You did a hell of a lot of work on the deal!

Re Boulter: I have had some feelings akin to yours. But I know Mike well enough to believe that he would have no ulterior motives. Of course, he is trying to keep his head above very turbulent waters in the UK academic scene, and one can't blame him for doing what he can to avoid being exiled to Aberystwyth, or canned altogether. Nevertheless, at the moment I have the feeling that IAPT.CFP is on the verge of being blindsided into obscurity. However, we have some trump cards--nothing can happen permanently or officially to nomenclature without our being involved. In the meantime, I suppose that Mike's efforts to get up a database etc. is, at worst irrelevant, at best helpful. It's too bad that one or both of us can't go to the Kräusel Symposium in May--that's where the big decisions on NCU, etc., are to be made.

Your move sounds horrific! We've made appropriate changes to your card here.

All the best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

Dear Al,

90 02 18

I never have been the best organized person, and the increasing lack of secretarial assistance does not help. Thus it is that I had a pile of correspondence on my desk with various requests that took more than just casual effort, as well as input from our technician in the lab. That pile became a thorn in my side, so early this year I started to systematically clean it up.

So I came across an old request from you (± 2 years old?) for some material containing scolecodonts. Some of my best material was from core, of which there is none (or little) left. Enclosed please find the residues of two cored intervals from "Bear Biltmore #1", location 7-11-87-17-W5 ; the footages are on the vials (the 3-676/677 refers to entry in our processing record book).

township range meridian

The age of these samples is Frasnian. There should be lots of material to play with. I hope it still is of interest to you; if it had taken less of a concerted effort and delving into stored records, you would have had it sooner.

I tried to obtain a copy of Puri 1963; I got the reference from you, but it was too long for you to copy. Our librarian can find no listing for it in the computer. Would you consider loaning it to me (by registered mail) so I can copy it? I promise to return it in a week from receipt.

Best regards to Betty + yourself,

Chris.

C. J. Jansonius

Hunt Institute for Botanical Documentation

90 01 31

Dear Al,

For what it is worth, here is a list of my scribblings I made in the margin of your book as I read through it. Obviously, I skipped some bits here and there, and at other times my remarks are very nitpicking. As you realize, they are not meant to express criticism; rather, I compiled them to give you an opportunity to respond to them where you think you want to. It would not surprise me to see your book go to a second printing, and maybe you can make adjustments.

On a different topic: I just got IOPB Newsletters 39 + 40 and I have

Hunt Institute for Botanical Documentation

the feeling that Boulter is hijacking our IOP in order to advance his own schemes.

What do you think of all this?

I may, if I get a bit of time, write to IOP to raise a dissenting voice. But we also will move (inside the Bldg.; my new room no. will be Esso Plaza West 1415 as of 12 Febr), I just packed 150 boxes with books/reprints, and will have to unpack them in 2 weeks. And I have some microscope work coming up, plus choir and other hobbies.

We'll see!

Best wishes for Betty + yourself,

Hunt Institute for Botanical Documentation

I called back — 3:50 p.m. on 8-XII-89 = 1:50 in Calgary. he wasn't in

TO _____ AT _____

DATE 7.XII.89 TIME 3:30

WHILE YOU WERE OUT

M. Jan Jansonius

of [224-19-1001]

Phone 403-237-2438 Fax 403-237-2197 403-237-2183

TELEPHONED	X	PLEASE RETURN CALL	X
CALLED TO SEE YOU	☐	WILL CALL AGAIN (Friday)	☐
RETURNED YOUR CALL	☐	RUSH	☐

MESSAGE Grame Williams and Rob Fensome editing new book, Palynology and Stratigraphy, ~~to be put out under AASP auspices in 1993.~~ Will have multiple authors and cover all ~~specialties in paly.~~, e.g. dinos, chitinozoans p&s, etc., etc. Designed to supplement your textbook as a reference volume for advanced students and professionals. Could you write chap. on nomenclature and systematics? (Originally, Fensome planned to write it, →

Signed B.T.

The Standard Register Company

Jan J was willing to do it

Hunt Institute for Botanical Documentation

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16 November, 1989

Dr. Jan Jansonius
Esso Resources Canada Ltd.
Esso Plaza West, Rm. 1859
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Probably I should wait 'til I hear from one letter before I load another on you. However, this is a "leaf on the water" type of request.

You will recall that when you wrote me to tell me what a lousy book my text for a college course is, you stated that you had a list of errors from it and would be willing to share it with me. I responded that I'd like to have it. Naturally I would acknowledge use of it in producing a corrected edition--an event that may actually happen one of these days. So, please do send me your list. Needless to say, I have others, especially from two years of students in Geosciences/Biology 423!

Having mentioned the course leads into subject #2: could you let me have a couple of slides of scolecodonts for the students to look at, or (better) a couple of pieces of rock we could process for same? Some of our local rock produces them, but very poorly preserved and very sparse.

Thanks.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
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435 Deike Building
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Fax to:
403-237-2197

3 October, 1989

Dr. Jan Jansonius
Esso Plaza West, Rm. 1859
237 4th Ave. SW
Calgary T2P 0H6, Alberta, Canada

Dear Learned Colleague:

This a.m. I have been working with Piérart's report for the C.I.M.P. Megaspore Group in Palinologia num. extraord. 1, 1978.

There are four new generic names. I worked up the names for my generic list. Then I turned to your cards to see what you did with them. We differ a bit, and I'd like you at your convenience to tell me where I went wrong.

1. In my opinion the author is not Piérart but "C.I.M.P. Megaspore Group in Piérart."

2. All of the new combinations for the type species are not validly published, for lack of required complete reference to basionyms. For Crassilagenicula (card 3545) you propose a "lectotype," but you don't say why it was necessary to do more than validate the transfer. For Auritolagenicula (card 3531) you propose a lectotype and seem to explain why (last para.) you needed to do that instead of just validating the transfer. In any case, these two combinations should be designated "...(Zerndt) C.I.M.P. Megaspore Group in Piérart 1978 ex Jansonius & Hills 1979," but up to and including the ex may be omitted.

3. For Zonolagenicula (card 3626) you note that the n. comb. was not validly published but for some reason let the matter drop instead of proceding as with the two genera just mentioned. Why?

4. For Sublagenicula (card 3617) you don't say that the type species transfer was not validly published, in effect therefore saying it was. The matter is still hanging, not so? Or is there something I'm missing?

Hang in there.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

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Fax to:
403 - [redacted]
237 - 2197

22 August, 1989

Dr. Jan Jansonius
Esso Plaza West, Rm. 1859
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

As I write I am on USAir flight 421, Chicago-Pittsburgh, returning from one of the endless visits to Michigan re my mother. I am reading yours of 89-06-01. I opened the envelope when I returned from Europe in June but didn't realize there were personal parts, so it went into the pile with reprints, etc.

Read about Sinsosporites, gender of -pollis, etc. I look forward to hearing about the genera you will "amend" (4th line from bottom, p. 1). I'm not sure I understand what you're getting at.

The letter to me of 89-03-26, in the same wrapper, would have pleased Jim Schopf. I guess we should all watch carefully how the fungal folks' list of "sanctioned names" stands the test--for some years. After many sessions at various IBC meetings watching the Code being fine-tuned ever again, I really do sometimes wonder if we wouldn't be better off to have a commission à la zoology, which would rule on troublesome cases, as referred to them. We could swallow hard on certain ones that are dear to us and go on to more important business. That is only obliquely related to the "lists" question. The list of conserved names already in the ICBN dominates that book and will soon have to be a separate volume, which will eventually become several volumes....

Anyway, I'm sure you know that I have little or no influence in IAPT. I was just reporting to the Committee on what seems to be happening, and suggesting that we'd best consider the matter carefully and perhaps take some action. You will be interested to know that your negative response was the only response of any kind to the IAPT.CFP letter. WGC and I have interpreted that to mean lack of interest or lack of funding to do anything. Boulter in London is far along with a proposal for big bucks to get up a computer-based list of NCU and other related matters. It might go.

Hunt Institute for Botanical Documentation

Furthermore, don't worry--even if there were a strong tide running in the direction of universal lists of sanctioned names, it will never happen in our lifetimes!

Best wishes.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

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31 July, 1989

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Thanks for yours of 20 June, just read. Re Lorente's paper--it is too late to get her into the book as an author, but the work should be mentioned where appropriate, and I might well have forgotten if you hadn't reminded me.

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

Dear Al, 89 06 20

Just a quick note: I talked to Stan Pocock the other day (through Bell) and he mentioned to be working on a contribution for a book you work on, on Particulate Organic Matter and its interpretation.

Did you attend, in Brisbane, the talk by Maria Lorente? She devised a system based on automatic pattern recognition, where a camera/computer combination can define and recognize different environments from a deltaic sequence in a very convincing manner.
I sent her the samples from DSDP, on which Summerhayes + Masran published Atlantic environments in the Cretaceous, and she was going to plug those into her system.

I thought you might want to contact her. I do not know precisely what the aim of your book is, but Lorente's efforts might fall within the circumscribed area of interest.

Best regards,

Jan

Hunt Institute for Botanical Documentation

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20 February, 1989

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Well, I knew I made mistakes, but you really caught a barrel-full of them. Yes, I would like a complete listing for the 2nd edition (if I live long enough).

It seems that you may have come pretty close to "damnation with faint praise." I guess I wish the obviously overall appreciation of the book had not been so overwhelmed by the long list of boo-boos.

By the way, I wish I had thought of asking you to be one of the editorial board (along with Chaloner, Scott and Barnosky). You would even have made a few $.

The nomenclatural question you raised a while back had a good secondary effect for me--I ordered a copy of the Berlin Code, which I had forgotten to do.

Best.

Yours very truly,

Alfred Traverse

P.S. After John Birk's review of Paleopalynology in Nature, it had to be downhill.

AT/et

Hunt Institute for Botanical Documentation

21-4-89

Dear Jan:

I slept on your review, and I would like to add to the 20-4 letter that I feel your review is somewhat unfair because it does not evaluate the book in the light of its announced purpose — college instruction at the undergraduate level. I know the book is no better than a 7.5 on a scale of 1–10, but is your review graded for fairness better than a 5⁻? Its factual errors are regrettable, and I regret that I would like your complete list for a possible revision. The book does have some (mostly pedagogical) strong points (as I proceed from teaching the semester with it), and a balanced review would have to mention them. The book is not a reference book.

As ever,

Al

Hunt Institute for Botanical Documentation

Dear Al, 88 02 09

Thank you for your answer to my two earlier letters; I got a fairly good response, and hardly could hope for 100%. The variety, even divergence, in opinions is remarkable. I will, eventually, tabulate the results of my little survey and let you all know what came out of it.

Your lapis + caulis were well taken as masculine; my abbreviated grammar gives another ½ dozen; all others on —is are feminine. So, it would make sense if new formations like —"sporis", "-pollis" would follow the rule, rather than being an exception of late date. And there should be one rule for each suffix, rather than go with either ♂ or ♀, depending what the original author fancied.

For your amusement and information I enclose a copy of my review of your book. If I am wrong in any particular (like pricing) please let me know. I read through the book with good attention, and if you want a list, sometime, of all the nits I picked, just say so and I'll send them. (Maybe before a new printing/edition?). I think you, and Betty, earned the right to be proud of it, its a big job and you did it well (even if it is not perfect; but who am I to find fault with that?).

Jansonius

Hunt Institute for Botanical Documentation

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February 13, 1989

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Just got a copy of McNeill's letter to you of 1-II-89. It puzzles me, as I don't see how a person can designate as type for a new genus a (type of a) species that he explicitly chose not to designate. That sort of thing could result in accidental typification clean contrary to an author's intent. Better no such valid publication.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

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12 February, 1988

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

In working over the recent batch of most welcome addenda etc., I came accross the 601 Corniculatasporites item. If I understand what's going on, you should have said: "(Bolkh.) Kuv." becomes "Bolkh. ex Kuv.", or am I missing something?

Re 1374: My card was already written on: "Not 1939: nominally 1938, but preprints were distributed in 1937". Did I get that from you in a previous go-around?

Re 4009 et al.: Why do you feel that the Chinese should get their whole given names, not just surnames? Khlonova gets just that, not Khlonova, Anna Fedorovna (or whatever). Xiang-jun is the same deal as Anna Federovna. He or she should get just Sun, or at the most Sun, X.-j. or better, Sun, X.

Re 4136: Can a nomen nudum have a "synonym"? I wouldn't have thought so.

Re 3127: You really had trouble with this name, huh? It is not repeated on card 4362 as it was given on 3127--ha! (I believe this one should be just between JJ & AT.)

Keep up the good work.

All the best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

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12 February, 1988

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

In working over the recent batch of most welcome addenda etc., I came accross the 601 Corniculatásporites item. If I understand what's going on, you should have said: "(Bolkh.) Kuv." becomes "Bolkh. ex Kuv.", or am I missing something? *

indeed you're correct.

**/ Re 1374: My card was already written on: "Not 1939: nominally 1938, but preprints were distributed in 1937". Did I get that from you in a previous go-around?

Re 4009 et al.: Why do you feel that the Chinese should get their whole given names, not just surnames? Khlonova gets just that, not Khlonova, Anna Fedorovna (or whatever). Xiang-jun is the same deal as Anna Federovna. He or she should get just Sun, or at the most Sun, X.-j. or better, Sun, X.

Re 4136: Can a nomen nudum have a "synonym"? I wouldn't have thought so. "Valid Senior name" might have been better. The Senior name has a jr. invalid Synonym, which was published earlier.

Re 3127: You really had trouble with this name, huh? It is not repeated on card 4362 as it was given on 3127--ha! (I believe this one should be just between JJ & AT.)

Keep up the good work.

Thanks!

All the best.

Yours very truly,

Alfred Traverse

AT/et

**/ Nominal date was 1937, but from info on p. 48, it seems likely that H. 183 was effectively published in (early 1938). It certainly was not 1939!
Potonié (Synopsis III, p. 99) indicates that Raatz in preprint (Separatum) was published in 1937.

* fig. in right upper corner is fig. 19, ∴ type of Corniculatisporites. fig. in right lower corner shows organization of genus after Kuvaeva

Best regards to both of you

Jan

Hunt Institute for Botanical Documentation

I give the full name for Chinese, because their "family" name is in effect a clan name, shared by a large group of people. Although there are some minor, exclusive clans, the bulk of the 1×10^9 Chinese carry one of the standard 100! clan names.*

These clans are often powerful organisations, with lodges in many places. In Calgary, I often walk by the "Mah Society" clubhouse. Thus, you will find Chinese authors clustered around Li (= Lee), Zhang, Sun, etc. These names give little useful info, and in time such references will mean little.

For instance, I now have on file publications by

Zhang Chun-bing
Zhang Lu-Chin
Zhang T-G
Zhang Yi-yong
Zhang Yu-Lan
Zhang Zhong-ying

Chang may be the same clan, but transliterated (romanized) using a different convention.

Zheng Fen
Zheng Shao-Lin
Zheng Ta-hui
] idem dito ?

* These names are usually included in a separate listing in Chinese dictionarys

Hunt Institute for Botanical Documentation

1194

DEC 14 1938

Dec. 14 1938
acquisition date stamped on
by Geol. Surv. Canada Library

Abhandlungen der Preußischen Geologischen Landesanstalt
Neue Folge, Heft 183

Mikrobotanisch-stratigraphische Untersuchung der Braunkohle des Muskauer Bogens

Von
Georg Viktor Raatz
in Berlin

Hierzu 1 Tafel und 5 Abbildungen

Herausgegeben von der
Preußischen Geologischen Landesanstalt

BERLIN
Im Vertrieb bei der Preußischen Geologischen Landesanstalt
Berlin N 4, Invalidenstraße 44
(1937) 1938
sep. p. 48

nominal publication date : 1937
effective publication : (early?) 1938
effective publication separatum Raatz : 1937 (fide Potnié, Synopsis III, p. 99)

Hunt Institute for Botanical Documentation

VIII. Schriftenverzeichnis

Die im Text erwähnte Literatur findet sich, soweit nicht im folgenden angeführt, in den Arb. Inst. Paläobot. 4, S. 315, zusammengestellt.

1a. Erdtman, G.: Neue pollenanalytische Untersuchungsmethoden. — Veröff. bot. Inst. Rübel, Zürich. 1935.

1b. —: Pollen Statistics. 1935.

2. Gothan, W.: Paläontologische Braunkohlenstudien. — Braunkohlenarchiv, H. 45, S. 3ff. 1936

3. —: „Nochmals die ‚Graskohle': Nadeln der Schirmtanne (*Sciadopitys*)." — Z. Braunkohle H. 40, S. 736—738. 1936.

4. —, Menzel & Sapper, J.: Neues zur Tertiärflora der Niederlausitz. — Arb. Inst. Paläobot. 3, H. 1. 1933.

5. Issel, Fr.: Abschnitt „Das Forster Revier", 50-Jahres-Festschrift des deutschen Braunkohlenindustrievereins, S. 545ff. 1935.

6. —: Eine neue Abbaumethode mit Bandförderung. — Z. Braunkohle, S. 261ff. 1932.

7. Keilhack: in Klein: Handbuch Braunkohle. 3. Aufl., S. 221.

8. Kirchheimer, F.: Neue Ergebnisse und Probleme paläontologischer Braunkohlenforschungen. Z. Braunkohle, H. 45/46. 1934.

9. —: Die Erhaltung der Sporen und Pollenkörner in den Kohlen sowie ihre Veränderungen durch die Aufbereitung. — Bot. Archiv, 35, H. 1/2. 1933.

10. Kräusel: Die Pflanzen des schlesischen Tertiärs. — Jb. preuß geol. L.-A., 38, Teil II, H. 1/2. 1917.

11. Linstow, von: Über das Alter der Samländischen Braunkohlenformation. — Z. Braunkohle, H. 16, S. 340. 1926.

12. Menzel, P.: Über die Flora der Senftenberger Braunkohlenablagerungen. — Abh. kgl. preuß. geol. L.-A. u. Bergakademie, N. F. 36. 1906.

13. Pietzsch: Die Braunkohlen Deutschlands. — Handbuch der Geologie und Bodenschätze Deutschlands. Berlin 1925.

14. Potonié, R.: Zur Mikrobotanik der Kohlen und ihrer Verwandten. — Arb. Inst. Paläobot., 4. 1934.

15. —: Über den Muskauer Faltenbogen, seine Oberflächenformen und deren Abhängigkeit von der Beschaffenheit der Braunkohle. — Jb. preuß. geol. L.-A., 51, S. 393. 1930.

16. —: & Venitz, H.: Zur Mikrobotanik des miozänen Humodils der niederrheinischen Bucht. — Arb. Inst. Paläobot., 5. 1934.

16a. Schenk: Paläophytologie, S. 643. 1890.

17. Wolff, H.: Mikrofossilien des pliozänen Humodils der Grube Freigericht bei Dettingen (Main) und Vergleich mit älteren Schichten des Tertiärs sowie posttertiären Ablagerungen. — Arb. Inst. Paläobot., 5. 1934.

18. Teumer, Th.: Probleme der Braunkohlengeologie und des Braunkohlenbergbaues und das Braunkohlenmuseum des Niederlausitzer Bergbauvereins zu Senftenberg (Lausitz). — Abhandlungen der naturforschenden Gesellschaft zu Görlitz, 30, H. 3. 1929.

19. Erläuterungen zur geologischen Karte von Preußen, Lieferung 266, Blatt Muskau, Nr. 2549. Preuß. Geol. L.-A. Berlin 1928.

Urschrift eingegangen am 12. März 1937
Druckauftrag erteilt am 25. November 1937 ←

Märkische Druckanstalt W. Hentschel K.-G., Berlin N 65

Hunt Institute for Botanical Documentation

Ilexpollenites Thiergart 1937 (Separatum) und Thiergart in Raatz 1937, S. 25
Taf. 6, Fig. 112

Typus: *Ilexpollenites* (al. *Pollenites) iliacus* (R. Pot. 1931, 4. Mitt., H. 27, S. 556, Abb. 3; 37 μ; Präp. IV 46a, gut erhalten in Krefeld) Thiergart 1937, S. 321.

Locus typicus: Rheinische Braunkohle, Ville, Beisselsgrube bei Ichendorf; Miozän.

Gestalt ovoid bis kugelig, Äquator trilobat (dreipaßförmig) bis kreisförmg, Poren ± erkennbar, Äquatorialrugae vorhanden (tricolporat). Die Exine zeigt dicht, aber frei stehende Pilae oder Clavae, nicht ganz gleicher Größe, die keulen-, pilz- oder birnenförmig, manchmal auch warzig oder fast stabförmig sein können. Diese Elemente sind polwärts z. T. größer. Exolamelle fehlt.

Der Name des Taxons „Ilexpollenites" ist Thiergart 1937, S. 321, zuzuschreiben. Raatz 1937, S. 25, benutzte ihn auf Anraten von Thiergart (s. die Bemerkung von Raatz 1937, S. 7, oben).

Morphographische Beziehung: Aquifoliaceae.

Ilexpollenites (al. *Pollenites) iliacus* (R. Pot. 1934, 4, S. 72, Taf. 3, besonders Fig. 22, 23). — Geiseltal bei Merseburg, Grube Cecilie; Eozän.

Ilexpollenites (al. *Pollenites) iliacus* (R. Pot. & Ven. 1934, 5, S. 34, Taf. 3, Fig. 89). — Ville, Beisselsgrube bei Ichendorf; Miozän.

Ilexpollenites (al. *Ilicoipollenites) iliacus* (R. Pot. 1951, Stuttgart, Taf. 21, Fig. 109 bis 112; 1951 Wien, Taf. 1, Fg. 31, 32). — Ville und Geiseltal.

Ilexpollenites (al. *Tricolporopollenites) iliacus* (al. *iliacus major* Thoms. & Pfl. 1953, S. 106, Taf. 14, Fig. 44). — Helmstedt, Oberflözgruppe; Mitteleozän.

Ilexpollenites iliacus (R. Pot.) Thiergart 1937, S. 321, Taf. 25, Fig. 30; 49,5 μ. — Niederlausitz, Senftenberg, Grube Marga; Miozän.

Ilexpollenites (al. *Pollenites) margaritatus* (R. Pot. 1934, 4, S. 73, Taf. 3, Fig. 25; 40 μ) Raatz 1937, S. 25. — Geiseltal; Eozän.

Lectoholotypus von *I. margaritatus*. Die Form steht in der Größe zwischen *I. iliacus* und *I. propinquus*.

Der Name *margaritatus* erscheint zuerst bei R. Pot. 1931, 1. Mitt. H. 16, S. 332, Taf. 1, Fig. 32, 33. Er bezieht sich dort auf zwei voneinander abweichende Figuren aus dem Geiseltal. Es ist nicht angebracht, hier den Holotypus zu wählen; die Bilder sind schlecht, und die Präparate fehlen.

Die nächste Stelle, wo *margaritatus* erwähnt wird, ist R. Pot. 1934, 4, S. 73, dort wird nur ein Exemplar (Taf. 3, Fig. 25, Taf. 6, Fig. 10; Präp. wohl in Berlin) als Photo und Zeichnung abgebildet. (Dieses darf nicht, wie später bei R. Pot. 1951, Stuttgart, S. 146, Taf. 21, Fig. 115; Wien S. 227, Taf. 1, Fig. 33, zu *propinquus* gestellt werden. Das Original von *propinquus* weicht ab. Es hat sich jetzt wiedergefunden und befindet sich in Krefeld). Als Lectoholotypus von *margaritatus* ist also die Fig. bei R. Pot. 1934, 4, Taf. 3, Fig. 25, zu bezeichnen.

Nach I. C., 1956, S. 295, Anhang IV, Ziffer 6, Bestimmung der Typen, ist als Lectotypus wenn möglich ein Exemplar zu wählen, das bei der ersten gültigen Veröffentlichung abgebildet wurde. Im vorliegenden Falle ist das nicht möglich. — Daher darf ein in der späteren Hauptarbeit veröffentlichtes Exemplar gewählt werden, das zudem von den meisten Autoren stillschweigend schon als Gattungstyp angesehen wurde. Hier gilt Anhang 1, Ziffer 4 des I. C., „die automatische Auswahl der ersten zitierten Art oder des ersten zitierten Exemplares" sollten vermieden werden. — Der oben gewählte Typus stammt, wie erwünscht, aus dem gleichen Material vom gleichen Fundpunkt.

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEPARTMENT OF GEOSCIENCES
PALYNOLOGICAL LABORATORIES
435 Deike Building
University Park, PA 16802
(814)863-3419 or (814)865-6393

4 February, 1988

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Congrats on latest ('87) batch of updates!

A hint you could pass on to whomever: it should be much more obvious what one owes. The present method of "billing" is like an honor-system! There should be a no-nonsense invoice that pertains precisely to what one received, included with the package. The present long sheet says, in effect, "If you think from this that you owe something, you may send in a check."

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
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21 August, 1987

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

This will be inadequate thanks for your help in Calgary, but I did want you and Bette to know that it was all deeply appreciated.

The visit to Drumheller was one of the high points of my life. It is absolutely 'mind-boggling'. We also viewed hoo-doos (not as good or numerous as in Cappadocia, but interesting) and Horseshoe Canyon, etc. Andy and J.-P. had never seen badland topography before.

Once again, thanks very much.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

8046

Al,

87 08 17

I spoke to Dennis Braman. He will drive to Calgary Tuesday night ± 1730, will be at his folks at ca 1900 hrs. (18 St + 28 Ave NW)

He can be reached, then, at 289-5421, and is willing to come down to U of C or downtown to meet you (for he'd like to meet with you).

However, he is short of time, because Wednesday morning he has to attend a reception and a press-conference, and at 1100 they will be airborn for China. He will be, as a member of the Alberta-China dinosaur research agreement, working in the Gobi desert til mid October.

He still urges you to drive out and see the Museum. If you could visit Drumheller on Tuesday afternoon he should have some time to meet with you and show you a few things. You can reach the Drumheller Museum during office hours by dialing 294-1992 (no long distance charge!).

It looks like I will be busy on Wednesday, and should stay in Calgary.

Hope your talk went alright.

Best wishes,

Jan Jansonius

Hunt Institute for Botanical Documentation

8046

Dear Al, 9 June '87

I just got your letter, and hope that this answer will beat the mail strike that is pending.

Do contact me when you arrive in Calgary on 15 August. I should like to meet with you and discuss Berlin. And I will keep the trip to Drumheller in mind as a possibility. However, your visit coincides with a visit from my oldest son Johannes (and his Lyne), and a program for that occasion has not yet jelled. Since we have not seen him for a year, you'll understand that I will give them priority if there appear to be conflicts. On the other hand, there should be enough lead time to prevent such conflicts. So, let's wait and see what develops.

I wish you a pleasant sojourn in die jetzt Stadt Berlin, and best regards,

Jan Janssonius

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
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27 May, 1987

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

As soon as I get back from the IBC in August I leave immediately for Calgary, for the Devonian Symposium. I will arrive in your city about noon on Sat., 15 August. As the Symposium doesn't really do anything until late Sunday, 16 August, it has occurred to me that maybe I could meet with you, talk a little nomenclature, or whatever, on Saturday afternoon-evening or Sunday a.m.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

P.S. If Drumheller isn't too far, I might like to visit it (rent car, perhaps) on Sunday afternoon? Perhaps you'd join me.

Hunt Institute for Botanical Documentation

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30 June, 1987

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Re card #200, l. 6: should it not be corrected to "laevigate"?

See you in August--I understand about family priorities, per your letter.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
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24 April, 1987

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Shouldn't card 2057 carry the date of Potonié, Thomson & Thiergart? One needs the date to evaluate the situation. Mostly (always?) you seem to provide such data.

As ever,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

file Jansonius-Hills

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13 March, 1987

Genevieve Johnston
Department of Geology & Geophysics
University of Calgary
2500 University Dr., N.W.
Calgary, Alberta, Canada T2N 1N4

Dear Ms. Johnston:

Thanks for yours of 9 February. Glad to have the 1983 cards. Check for $27.00 enclosed. I am sorry I somehow sent the wrong amount last time.

Yours very truly,,

Alfred Traverse
Professor of Palynology

AT/et
encl: check

Hunt Institute for Botanical Documentation

THE UNIVERSITY OF CALGARY

2500 University Drive N.W., Calgary, Alberta, Canada T2N 1N4

Faculty of SCIENCE
Department of GEOLOGY and GEOPHYSICS

Telephone (403) 220-5841

February 9, 1987

Alfred Traverse
Pennsylvania State University
Dept of Geosciences
Palynological Lab
435 Deike Building
University Park, PA 16802

Dear Dr. Traverse:

Please find enclosed a copy of the 1983 Genera Card file of Fossil Spores. The cost of the supplement is $19.50 (Canadian), and with an outstanding balance of $7.50 on your 1985 supplement, which was $27.50 instead of $20.00. The total comes to $27.00.

Sincerely,

G. Johnston

Genevieve Johnston

file :

Jasmine - Hill

Hunt Institute for Botanical Documentation

ALPHABETIC LISTING OF 1983 SUPPLEMENT

4059 Afropollis
4060 Alabroidaepollenites
4061 Ainusalispollenites
4062 Aneurospora
4063 Anizonosaccites
4064 Archaegloeocapsa
4065 Attritasporites
4066 Avalropollenites
4067 Avictuspirulina
4068 Bicingulispora
4069 Biswasiaspora
4070 Boehlensipollis
4071 Boichovitinaepollenites
4072 Boitenhagenipollenites
4073 Breconisporites
4074 Brevitrilletes
4075 Canthospora
4076 Caratinipollenites
4077 Carbaneuletes
4078 Cavagnettoaepollenites
4079 Centaurespollenites
4080 Cerceausepollenites
4081 Coleogleba
4082 Contrahofilum
4083 Crickmayispora
4084 Cyathidites
4085 Cyclophorusisporites
4086 Daquingopollis
4087 Devoniana
4088 Didecitrilletes
4089 Dissizonotrilletes
4090 Doushantuonema
4091 Elosporites
4092 Eomycetopsis
4093 Eupunctisporites
4094 Fastisporites
4095 Florinites Card 2
4096 Foveotetracolporites
4097 Geminospora
4098 Geminospora Card 2
4099 Gloeodiniopsis
4100 Gondlastrilatites
4101 Granulamembranispora
4102 Gyalosphaera
4103 Hsuisporites
4104 Hungaropollis Card 2
4105 Imparitrilletes
4106 Jarzenipollenites
4107 Jugasporites Card 2
4108 Julianaceaepollenites
4109 Kuprianipollis
4110 Labrateroidaepollenites
4111 Lacinitrilletes
4112 Laingipollenites
4113 Liliaceaepollenites
4114 Lomentunella
4115 Lonicerapollis
4116 Magnaperiporites
4117 Mediterraneipollenites
4118 Medusipollenites
4119 Microfoveolatispora
4120 Minutiafilum
4121 Namlingspora
4122 Namlingpollis
4123 Neonostoc
4124 Ollivieraepollenites
4125 Pakria
4126 Palaeocalothrix
4127 Palaeonostoc
4128 Palaeopleurocapsa
4129 Palaeovaucheria
4130 Paleonostochopsis
4131 Paragranatisporites
4132 Pentabrevicolporites
4133 Perforonuxpollenites
4134 Pilasphaeridium
4135 Pilaspora
4136 Polaroporis
4137 Polycolpites
4138 Polycolporites
4139 Portaepollenites
4140 Prenudopollis
4141 Prezaipollenites
4142 Properisispora
4143 Pseudoquadrisaccus
4144 Pseudoromeinipollenites
4145 Pteridaceoisporis
4146 Pterisispora
4147 Pterisispora Card 2
4148 Quasililinites
4149 Retistephanocolpites
4150 Retiverrucisporites
4151 [illegible]
4152 Russellipollenites
4153 Rutaceoipollenites
4154 Rutaceoipollis
4155 Samoilovichaepollenites
4156 Sandiegopollis
4157 Sarjeantia
4158 Schizosporis
4159 Scissilisphaera
4160 Scolocyamus
4161 Sculptisporis
4162 Setispora Card 2
4163 Shaanxipollis
4164 Sofrepites
4165 Sphaerocongregus
4166 Stanleypollenites
4167 Sterelgranisporis
4168 Streelispora
4169 Strophomorpha
4170 Tongchuanispora
4171 Torridonlphycus
4172 Trevisanaepollenites
4173 Triangulisaccus
4174 Triangupapillaesporites
4175 Triatriopollenites
4176 Tricristatispora
4177 Tripunctisporis
4178 Tschudypollenites
4179 Velumousspora
4180 Verrucingulatisporites
4181 Verruoculopollis
4182 Verrutetratrilletes
4183 Vilaflorpollenites
4184 Virgatasporites
4185 Vologdovia
4186 Xilinisporites
4187 Xujiahesporites
4188 Zinjispora

This was never received until '87

J. Jansonius & L.V. Hills — 1983
GENERA FILE OF FOSSILS SPORES - SUPPLEMENT
Special Publication - Dept. Geology
University of Calgary -- Canada

Dear Al,

Here's the list for 1983

There was no Supplement in 1984

The next Supplement, on which I'm working, likely will not see the light before 1987.

Good luck!

In haste,

Jan J.

Hunt Institute for Botanical Documentation

7 January, 1987

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada
T2P 0H6

Dear Jan:

Editorial notes from all over:

Card 2541 has a typo in line 5 up: Whittleseya.

Card 869, l. 4 up: accommodate

Card 552, last line: Romania (other spellings in English are possible, though this is preferred. The one given is just wrong, however.)

Card 3531, description, line 9: receptacle

Bestest wishes for 1987 succeeded in completing my set, despite letters to headquarters (I lack '83 supplement). Can you help? An encyclopedia lacking J is in trouble.

Best to you for 1987.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

19 September, 1986

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Thanks for yours of 86-09-09. I do not have the '85 supplement. See enclosed copy of letter ~~to~~ Len H.

Now the plot thickens. The last supplement of which I have a record is 1982. I always 3-hole punch them (as I do the "cards"), and put them in the volume labelled "Supplements," as a record thereof. Was there a 1983 or 1984? Could I have a list thereof, as per 1985, to check whether I have 'em? Sorry to bother you, but I am clearly one of the people who most needs a complete set!

Thanks. Betty joins in greetings.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY

DEIKE BUILDING
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College of Earth and Mineral Sciences
Department of Geosciences
Palynological Laboratories

Area Code 814
863-3419

3 September, 1986

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

PTO

Dear Jan:

I just noticed in the PSBSA bibliography an announcement of the 1985 supplement to the cards. I have no recollection of receiving same. I do have a standing subscription, not so? Or does one have to order each set from scratch?

Could you let me have one representative name and number (mine are filed alphabetically) from the '85 supplement, so I could check for its presence? I really worry about not having the complete set.

Has there been an '86 supplement?

I don't think so

This never received

4190

ALPHABETIC LISTING OF 1985 SUPPLEMENT

4191 ANACOLOSIDITES
4192 ANGUSTISACCUS
4193 ANIMIKIEA
4194 ANNULISPORA
4195 ANULUSACCUSPOLLENITES
4196 ARASPORITES
4197 ARTHROSIPHON
4198 ARTIOPOLLIS
4199 ASCETOSPORA
4200 ASCLEPIADOIPOLLIS
4201 BACUBREVITRICOLPITES
4202 BALMEELLA
4203 BARAKARELLA
4204 BLUFFOPOLLIS
4205 BOCCIASPORA
4206 BOHEMISPORITES
4207 CAPPASPORITES
4208 CARBANEULETES
4209 CARPOTHECA
4210 CELYPHUS
4211 CEPHALONYX
4212 CLAVAPERIPORITES
4213 CLAVASYNCOLPITES
4214 COLLOSPERMUMPOLLIS
4215 CONEOPOLLIS
4216 CORAMANDALIPOLLIS
4217 CORDYLOSPORITES
4218 COSTATHECA
4219 COSTATIPOLLENITES
4220 COUPERIPOLLIS
4221 CROOKSHANKITES
4222 CROTONOIDAEPOLLENITES
4223 CROTOTRICOLPITES
4224 CTENOLOPHONIDITES
4225 CYCLOBACULISPORITES
4226 DENSIVERRUPOLLENITES
4227 DETTMANNITES
4228 DIADEMASPORA
4229 DIADOCUPRESSACITES
4230 DICOLPOPOLLIS
4231 DICRASSIPOLLIS
4232 DICTYOTHYLAKOS
4233 DIPTEROCARPACEARUMPOLLENITES
4234 DIPTEROCARPACEARUMPOLLENITES
4235 DIPTYCHA
4236 DISCOIDELLA
4237 DISTANCORISPORIS
4238 DUOSPORITES
4239 ECHIMONOCOLPITES
4240 ELATERITES
4241 ELATEROCOLPITES
4242 ENDOSPORITES
4243 ENDOSPORITES
4244 EXPRESSIPOLLIS
4245 FLORITHECA
4246 FLORSCHUETZIA
4247 FOVEOTETRADITES
4248 FUPINGOPOLLENITES
4249 GHOSHIA
4250 GLENCOPOLLIS
4251 GNETACEAEPOLLENITES
4252 GNETACEAEPOLLENITES
4253 GORDONISPORA
4254 GRAMINIDITES
4255 GRAPNELISPORA
4256 GREWIPOLLENITES
4257 GUINETIA
4258 GUZMANIPOLLENITES
4259 HARRISIPOLLENITES
4260 HEERISPORITES
4261 HEKOUSPORITES
4262 HIMALAYISPHAERIDIUM
4263 HIPPOCRATEACEAEDITES
4264 IBEROESPORA
4265 INCROTONIPOLLIS
4266 INDOSPORA
4267 IUGOPOLLIS
4268 JUGASPORITES
4269 KERHARTISPORITES
4270 KIELMEYERAPOLLENITES
4271 KYRTOMISPORIS
4272 LACRIMAPOLLIS
4273 LAIPHOSPORA
4274 LAXIAPOLLIS
4275 LATEROPORA
4276 LATUSIPOLLENITES
4277 LEIOSACCUS
4278 LEIOZONOMOLETES
4279 LIBOPOLLIS
4280 LILIACIDITES
4281 LIMITISPORITES
4282 LONGAPERTITES
4283 LORANTHIPITES
4284 LUMINIDITES
4285 LYTHRAITES
4286 MACULOPORITES
4287 MAGNOMONOPORITES
4288 MALAYAEASPORA
4289 MALVACEARUMPOLLIS
4290 MARGINIPOLLIS
4291 MARGOCOLPORITES
4292 MARSUPIPOLLENITES
4293 MARSUPIPOLLENITES
4294 MELIAPOLLIS
4295 MEYERIPOLLIS
4296 MICROCYSTIOPSIS
4297 MONOCOLPOPOLLENITES
4298 MONOGEMMITES
4299 NAVALESPORITES
4300 NEOPHORMIDIUM
4301 NONIASPORITES
4302 NOVEMPROJECTUS
4303 OCULOSA
4304 ORNATETRADITES
4305 ORNATRIPORITES
4306 PADAPPAKKARAPOLLIS
4307 PALAEOCHROOCOCCUS
4308 PALAEOHYDROCOCCUS
4309 PALAEOLYNGBYA
4310 PALAEONOSTOC
4311 PALAEOWESTIELLOPSIS
4312 PARAVURIPOLLIS
4313 PECHOROSPORITES
4314 PELLICIEROIPOLLIS
4315 PICEAEPOLLENITES
4316 PILASPORITES
4317 PINJORIAPOLLIS
4318 POLYBREVICOLPORITES
4319 POLYCOLPITES
4320 POLYGALACIDITES
4321 PRIMITIVIPOLLIS
4322 PROXAPERTITES
4323 PSEUDONOTHOFAGIDITES
4324 PSILODIPORITES
4325 PSILOSPORA
4326 QUADRANGULOSACCITES
4327 QUALISASPORA
4328 QUANTONENPOLLENITES
4329 QUILONIPOLLENITES
4330 RAJMAHALISPORA
4331 RETIDIPORITES
4332 RETISTEPHANOCOLPITES
4333 RETUSOTRILETES
4334 RHABDOSPORITES
4335 SACCIFERA
4336 SAHNITES
4337 SELLASPORA
4338 SENECTOTETRADITES
4339 SHANGHUPOLLIS
4340 SIWALIKIATHYRITES
4341 SPERMATITES
4342 SPINOSISPORITES
4343 STEREISPORITES
4344 STRIACOLPORITES
4345 STRIATISACCUS
4346 STYX
4347 TAKATISPORITES
4348 TETRACOLPORITES
4349 TRILATIPORITES
4350 TRILOBOZONOTRILETES
4351 TUBISTEPHANOCOLPITES
4352 TUMIDULIPOLLIS
4353 UMBELLIFERAEPOLLENITES
4354 VALANJINELLA
4355 VERMIFOVEOPOLLENITES
4356 VESICOPHYCUS
4357 VESTIGISPORITES
4358 VIALOVIPOLLIS
4359 VINDHYACAPSIOPSIS
4360 WARKALLIPOLLENITES

Hunt Institute for Botanical Documentation

10 February, 1986

3 September, 1986

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

I just noticed in the PSBSA bibliography announcement of the 1985 supplement to the cards. I have no recollection of receiving same. I do have a standing subscription, not so? Or does one have to order each set from scratch?

Could you let me have one representative name and number (mine are filed alphabetically) from the '85 supplement, so I could check for its presence? I really worry about not having the complete set.

Has there been an '86 supplement?

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

86 09 09

Dear Al:

You probably have a standing order. Len did not send out the 1985 Suppl. to people in arrears; however, I have heard from quite a few people who have not rec'd 1985. There is no doubt that the administration of the file — which is Len's responsibility — has been poorly run in past years.

If you have a claim, you should write to him directly, but I don't mind having a copy so I can nag. It may well be that you are not in arrears, and yet were not sent 1985.

I'm still working on 1986; have some 120 entries worked up, but around 40-50 Russian entries need yet to be translated, and I have to make drawings for about 50-60 items. I hope we'll make it before year-end, but have some doubts.

Best regards, both of you!

Hunt Institute for Botanical Documentation

10 February, 1986

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Thanks for yours of 86-01-13.

I spoke, worked and voted against the change in generic typification at Sydney. After the vote I was depreseed, and various people comforted me, assuring me "it would make no difference."

Of course, the fact that a genus could be published pre-Sydney with a not validly published species as type was a mistake too--a loophole, as it were.

But I'm afraid you're wrong about the intentions at Sydney. The people who wanted this wanted exactly this--there now has to be a specimen, not a concept.

If the generic name was published post i-I-1958, apparently there has to have been a holotype for the type species. (One reason I'm for allowing photos or other illustrations!) This should be repealed, but won't be. Perhaps a phrase could be inserted "post i-I-83"?

The Scholtz case is too bad. His department was just barred from an archeological convention in UK (later moved because of this), so he'll (she'll?) be in a foul mood for sure now!

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

P.S. Our committee was represented in Berlin during the editorial drafting of the 1983 green book, by W. Chaloner. Perhaps you should write Bill?

Hunt Institute for Botanical Documentation

Dear Al, 86 01 13

We're still short of typists, so here we go once more in longhand. Thank you for your good wishes we received at Xmas time; I hope you and yours will fare well in 1986.

I enclose a copy of a leter to Schaltz; I shan't recapitulate it here.

I am bothered by the lack of a starting date for the novel concept introduced in the current version of Art. 10.1. In the past, Stafleu, you and I have agreed that a genus could be validly published if a nomenclatural type (= type species) had been designated/indicated, even if that species itself was not validly published. Sometimes that invalidity hinged on the lack of a holotype. Now, are all those no more validly published? Aside from the sheer labour involved, I am loath to seek out and reevaluate all these cases now contained in the General File as "validly published", because at the next congress we might see a reversal or modification of Art. 10.1. This type of change is distinctly frivolous, and not amenable to nomenclatural stability.

I am thoroughly annoyed by this. Why is the final MS of such changes not circulated among members of the various nomenclatural subcommittees? I can't conceive that this was the intention of the Sydney Congress.

What do you think? Best regards,

Hunt Institute for Botanical Documentation

Dr. A. Scholtz
Dept. Archaeology
University of Stellenbosch
Stellenbosch 7600 SouthAfrica

86 01 11

Dear Dr Scholtz,

This weekend I wanted to prepare the MS for an entry on Spinitricolpites Scholtz 1985, to be included in the next Supplement to the Genera File of Fossil Spores. However, I was stymied by a technicality: you did not designate a holotype for the type species. Thus, that species has not been validly published, because it does not now satisfy all requirements of the Intern. Code of Botanical Nomenclature.

In the past, a genus could be validly published if it had a proper name, an adequate diagnosis, description, and a designated type species [even when the latter was not (yet) validly published for some technicality]. However, in the latest edition of the ICBN, the type of a genus is no longer the type species, but the nomenclatural type of that type species, in other words: a specimen! This situation has not been encountered earlier, but I am inclined to think that therefore your genus also must be considered as not validly published in 1985 (Ann. S.A. Mus., v. 95).

I notice that you did not designate any holotypes in your paper; yet, on p. 13 you state that the types are deposited and catalogued at the Stellenbosch University.

Hunt Institute for Botanical Documentation

In one case, only one grain was illustrated in the description of a new species; such a grain is automatically the holotype. If it is not the specimen listed in your records as the holotype, your records would have to be adjusted: Camarazonosporites bankiensis Scholtz 1985 is the only species validly published in your paper. However, more than one specimen are illustrated for: Reticulatasporites grandis; Podocarpites kamiesbergensis, P. riembreekensis; Triorites operculatus, T. sphericus; Triporopollenites namaquensis; Propylipollis meyerorum [note the -orum ending; this is an automatic and obligatory correction, because more than one person die honored; see ICBN, Art. 73.10; Recomm. 73C.1 (b)]; Spinitricolpites jennerclarkei; Tricolporopollenites grandis, T. arnotiensis, T. brinkiae [if Brink is a family name, the name is correctly derived; if it is a given first name, it must be given as "brinkae"], T. coetzeeae; Grootipollis reyningii; Triporotetradites sphericus.

I suggest that you write a brief note to Taxon, or to the Review of Palaeobotany and Palynology, or Grana, in order to at last validate the publication of these species, and the genus. You will have to designate/identify the holotypes in print, and provide the full name, plus the bibliographic references to page and figure number on which the original descriptions and photographs were published ("effectively", but not "validly" so). The priority date of publication will then be that of your nomenclatural note.

At the same time, you might want to validly publish the new combination Spinitricolpites latispinosus (Holotype), which you

Hunt Institute for Botanical Documentation

used on p. 69. A new combination, in order to be validly published, must be used in that combination (which you do); but also must, at its first introduction, be accompanied by the basionym under which it originally was published, complete with page and figure numbers, and a complete bibliographic reference (which may be relegated to the REFERENCES) of the original publication. Although all these elements can be found in your text, they do not occur in an order that "clearly indicates" that you were publishing, or intended to publish, a "n. comb." (see Art. 33.2).

I am sorry to have to inform you of this blemish on your publication, which otherwise is quite attractive in many respects. However, these things will happen, and will happen again (but not to you!).

With best regards,

Yours Sincerely,

J. JANSONIUS - EPW 1859
ESSO RESOURCES CANADA LTD.
237 - 4 AVENUE SW
CALGARY, ALBERTA
CANADA T2P 0H6

Hunt Institute for Botanical Documentation

15 November, 1985

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

This one is further to the question of ICBN Art. 63, ex. 11. I guess we've got past Cuviera, which I discussed in mine of 16 August. I mistakenly thought that was your primary concern.

Re Hordelymus, 63.3 with ex. 11 clearly admits that except for this statement (63.3), it would be illegitimate. Because it is based on the stem of Hordeum, 63.3 makes of it an exception. The fact that 6.4 seemst to contradict this, is the reason why 63.3 is in the Code. Maybe this is an undesirable provision?

The reason why I would lean toward typification by illustration in some cases, is that we will be faced with long series of neotypes and lectotypes eventually.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

15 November, 1985

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

This is, much too late, a response to yours of 19 Sept. 85.

I believe you're onto something re typification of a generic name. Art. 10 should say "...of a validly published species...." Perhaps it could be urged that "species" in Art. 10 clearly means "validly published species", but I don't think so, as one of the requirements for valid publication could be lacking and yet a holotype perfectly clearly designated. I believe a proposal to change the wording as suggested above would be a very good idea, to remove what really is perceived as an ambiguity.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

11 October, 1985

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

re card 3784--is it really Scheuring "1980"--not "1978"--just asking.

Best wishes as ever.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

85 09 19

Dear Al :

In your letter of 85 08 16 you answered some nomenclatural questions I put to you.
The 3rd one, where you disagreed with me, you did not really answer. I enclose it once more.

In Ex 63.11, we deal not only with Curviera, of which the jr. homonym was conserved. We also deal with Hordelymus. By my reading of Art. 63.1, the latter name was illegitimate. According to Art. 6.4 an illegitimate name can only be restored by conservation. Hordelymus was not conserved. Ergo .... ?

You also asked me if I would prefer an illustration over a specimen, in typification of fossil plants. Never! Once our eyes are opened, we can always see more relevant detail in a specimen. Thus, by new descriptions and critical study, we can save and stabilize nomenclature that otherwise would become a bothersome burden.

Bye for now,

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEIKE BUILDING
UNIVERSITY PARK, PENNSYLVANIA 16802

College of Earth and Mineral Sciences
Department of Geosciences
Palynological Laboratories

Area Code 814
863-3419

16 August, 1985

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Well, here we are for a few days at our mountain retreat--first time in two years for me. I have brought yours of 85-05-07 along. I have it tucked in a copy of ICBN-Sydney, but I have no other literature. I hope you have a copy of the letter! (It always bothers me that your hand-written letters perhaps lack copies.)

Case 1: as far as I can tell from your presentation, if all is o.k. with Senegalinium Jain & Mill. 1973, it was the first generic name for what Lentin & Will. called Alterbia in 1975. A. recticornis should therefore have been transferred as S. recticornis (Vozzh. 1967) L.&W. 1975 instead of erecting a new genus. According to their then understanding, Andalusiella Riegel should also have been submerged in Senegalinium, as superfluous, and the species transferred.

If in 1977 L.& W. wished to maintain Senegalinium (type S. bicavata) and Andalusiella (type A. mauthei), as well as Alterbia, at that point whatever was to constitute Alterbia still needed to be published. Ex 63.11, which you cite, has to do partly with conservation: Cuviera DC in the Rubiaceae was conserved against Cuviera in the Gramineae, making Hordelymus available. Alterbia could only become correct if something like that happened to Senegalinium, and if it is "based on the stem....". Alterbia could be conserved, but I would vote NO! I certainly am what XXX had in mind!

Case 2: Liu could have, it seems to me, created a genus, Fupingopollenites, bigger than, and including, Dipterocarpacearumpollenites, providing that he gave the new genus its own type-species. He could not select the same type as already existed for another genus. I would describe this situation just as you have.

Case 3: Here we have to deal with what is "superfluous", etc., in respect to conservation. Cuviera Koeler is on the list of nomina rejicienda (see p. 270), a special status. It has been banished, like a shunned Amishman--an "un-person". It not only no longer exists, but like Soviet history-books, we "play" that it never existed. (Conservation only began about 1930, as I recall, by the way.) Conservation is a funny game—the easy way out (like the decisions of the Zoological Commission). By conserving Cuviera DC, one says that Cuviera Koeler never existed.

Therefore, I fallibly decide that your conclusion is wrong in this case.

Wow, I'm glad most things are easier!

Yours very truly,

AT/et

Alfred Traverse, Professor of Palynology

Hunt Institute for Botanical Documentation

Ex. 63.11 : 1802 : Cuviera Koeler
type sp. Elymus europaeus

1863 : Hordeum subgen. Hordelymus Jessen
type sp. Elymus europaeus L.

1885 : Hordelymus (Jessen) Jessen
type sp. Elymus europaeus L.

1807 : Cuviera D.C.
type sp. : C. acutiflora D.C.

In my opinion, Cuviera D.C. was illegitimate, and must be given a new name, or be conserved. It was conserved.

The conservation makes Cuviera Koeler an illegitimate (if sr.) homonym, which must be replaced. It was not nomenclatural superfluous when published, and thus seems to be excluded in the wording of Art. 63.3.
The list of nomina conservanda does not give the date of the acceptance in this list.
Yet:
If the conservation of Cuviera D.C. postdates 1885, then Hordelymus (Jessen) Jessen is illegitimate when proposed, because its choice of type species mandates that this taxonomic group should have been named Cuviera K. (if Jessen had been fully informed). Per Art. 6.4, Hordelymus in the rank of genus, becomes and for ever remains illegitimate. A name only has priority in its own rank; the legitimacy of Hordeum subgen. Hordelymus cannot influence its status in generic rank.
CONCLUSION : Ex. 63.11 is contadictory to Art. 6.4, and should be deleted.

Hunt Institute for Botanical Documentation

85 09 19

Dear Al,

Just a quick note regarding a marginal scribble I made in the margin of my ICBN.
It used to be that a genus could be validly published as long as a type species (= nomenclatural type) had been "indicated", even if that species itself was not validly published.
But now that the type of a genus is a specimen: how does that affect the procedure that was acceptable in the previous paragraph?

I forwarded your letter to Pocock. His address is:
Dr. S.A.J. P.
R.R. 1
Creston, B.C. V0B 1G0, Canada

Ph.: (604) 428-7963

Best regards,

Judi Lentin is in Novosibirsk for 6 weeks, complete w microscope, film, etc., to photograph + redescribe all Vozzhennikova's types.

Hunt Institute for Botanical Documentation

16 August, 1985

Dr. Jan-Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Well, here we are for a few days at our mountain retreat--first time in two years for me. I have brought yours of 85-05-07 along. I have it tucked in a copy of ICBN-Sydney, but I have no other literature. I hope you have a copy of the letter! (It always bothers me that your hand-written letters perhaps lack copies.)

Case 1: as far as I can tell from your presentation, if all is o.k. with Senegalinium Jain & Mill. 1970, it was the first generic name for what Lentin & Will. called Alterbia in 1975. A. recticornis should therefore have been transferred as S. recticornis (Vozzh. 1967) L.&W. 1975 instead of erecting a new genus. According to their then understanding, Andalusiella Riegel should also have been submerged in Senegalinium, as superfluous, and the species t transferred.

If in 1977 L.& W. wished to maintain Senegalinium (type S. bicavata) and Andalusiella (type A. maithei), as well as Alterbia, at that point whatever was to constitute Alterbia still needed to be published. (Ex 63.11, which you cite, has to do partly with conservation: Cuviera DC in the Rubiaceae was conserved against Cuviera in the Gramineae, making Hordelymus available. Alterbia could only become correct if something like that happened to Senegalinium, and if it is "based on the stem....". Alterbia could be conserved, but I would vote NO!

Case 2: Liu could have, it seems to me, created a genus, Fupingopollenites, bigger than, and including, Dipterocarpacearumpollenites providing that he gave the new genus its own type-species. He could not select the same type as already existed for another genus. I would describe this situation just as you have.

Case 3: Here we have to deal with what is "superfluous", etc., in respect to conservation. Cuviera Koeler is on the list of nomina rejicienda (see p. 270), a special status. It has been banished, like a shunned Amishmanp-an "un-person". It not only no longer exists, but like Soviet history-books, we "play" that it never existed. (Conservation only began about 1930, as I recall, by the way.) Conservation is a funny game—the easy way out (like the decisions of the Zoological Commission). By conserving Cuviera DC, one says that Cuviera Koeler never existed.

Therefore, I fallibly decide that your conclusion is wrong in this case.

Wow, I'm glad most things are easier!

Yours very truly,

AT/et

Alfred Traverse, Professor of Palynology

Hunt Institute for Botanical Documentation

85 05 07

Dear Al,

Your letter (au verso) arrived next morning after our telephone conversation. There is no problem in using my line drawings. (Of course, I have cribbed freely from what was available in the literature, always without asking; never had any problem). Any credit will be welcome publicity.

If you want, I can xerox from the full-size originals, or lend you some originals. Batten et al. got full-size xeroxes of originals from the Norma-poles, which they used in their paper.

I enclose the Summary of 3 puzzles: did I arrive at the correct conclusions? I wonder if Case 3 (= Ex. 63.11) should be sent to Taxon.

Best regards,

Jan

Jansonius

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEIKE BUILDING
UNIVERSITY PARK, PENNSYLVANIA 16802

College of Earth and Mineral Sciences
Department of Geosciences
Palynological Laboratories

Area Code 814
863-3419

1 May, 1985

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Regarding my pesky textbook project, I wondered if there are any problems about my using some (10 or so) of your little spore-caricatures from the famous cards as an illustration, with abundant credit, of course. I there a copyright problem?

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

1837 Albertia Schimper

1967 Albertia Vozzhennikova : jr. homonym ∴ illegitimate

1975 Alterbia Lentin & Williams : nom. nov. pro Albertia Vozzh.
Type species : A. recticornis (Vozzh. 1967) L+W 1975

included species : A. bicavata (Jain + Millep. 1973) L+W 1975
= type species of Senegalinium Jain + Millep. 1973
(Senegalinium treated as superfluous by L+W, 1975)

A. polymorpha (Malloy 1972) L+W 1975
in which is included as jr. syn. by L+W. 1975:
Andalusiella mauthei Riegel 1974, the type species of Andalusiella.

In my opinion, this makes Alterbia illegitimate per Art. 63.1; L+W 1975 should have adopted Senegalinium as the oldest legitimate name for this taxonomic group.

In 1977, L.+W revised their opinions, and maintained Senegalinium as a discrete genus, as well as maintained A. mauthei and A. polymorpha as discrete species within Andalusiella.

In my opinion, Art. 6.4 necessitates a new name for Alterbia. Although there are disturbing similarities with Ex. 63.11, I consider that Alterbia either must be conserved, or must be replaced.

Hunt Institute for Botanical Documentation

1969 : Dacrydiumites guillauminii Nagy

1969 : Dipterocarpaceaepollenites hidasensis Nagy
= type species of Dipterocarpaceaepollenites

1980 : Tricolporopollenites wackersdorfensis Thiele-Pfeiffer
included in synonymy : ? Dacrydiumites guillauminii Nagy
? Dipterocarpaceaepollenites hidasensis N.
(in Remarks, Th.-Pf. explains that original descriptions and illustrations preclude a definite identification of conspecificity)

1985 : Fupingopollenites Liu Gengwu
type species : F. wackersdorfensis (Th.-Pf.) Liu
of which are given as synonyms (without indicated doubt):
Dacrydiumites guillauminii Nagy
Dipterocarpaceaepollenites hidasensis Nagy.

Liu, by his inclusion of the types of the 2 Nagy species, was obliged to use one of these older epithets as taxonomic synonym in preference to wackersdorfensis (which became superfluous, but not illegitimate, by this treatment).
Fupingopollenites, however, was made illegitimate by this same treatment, as Liu included in the synonymy the nomenclatural type of Dipterocarpaceaepollenites; he should have adopted this later name.
Even if later workers question his synonymy, and choose not to accept it, per Art. 6.4 the name Fupingopollenites cannot be resurrected other than by conservation.

Hunt Institute for Botanical Documentation

Ex. 63.11 : - 1802 : Cuviera Koeler
type sp. Elymus europaeus

1863 : Hordeum subgen. Hordelymus Jessen
type sp. Elymus europaeus L.

1885 : Hordelymus (Jessen) Jessen
type sp. Elymus europaeus L.

1807 : Cuviera D.C.
type sp. : C. acutiflora D.C.

In my opinion, Cuviera D.C. was illegitimate, and must be given a new name, or be conserved. It was conserved.

The conservation makes Cuviera Koeler an illegitimate (if sr.) homonym, which must be replaced. It was not nomenclatural superfluous when published, and thus seems to be excluded in the wording of Art. 63.3.
The list of nomina conservanda does not give the date of the acceptance in this list.
Yet:
If the conservation of Cuviera D.C. postdates 1885, then Hordelymus (Jessen) Jessen is illegitimate when proposed, because its choice of type species mandates that this taxonomic group should have been named Cuviera K. (if Jessen had been fully informed). Per Art. 6.4, Hordelymus in the rank of genus, becomes and for ever remains illegitimate. A name only has priority in its own rank; the legitimacy of Hordeum subgen. Hordelymus cannot influence its status in generic rank.
CONCLUSION : Ex. 63.11 is contradictory to Art. 6.4, and should be deleted.

Hunt Institute for Botanical Documentation

435 Deike Building May 22, 1985 863-3419

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P OH6

Dear Jan:

This is not a real answer to yours of 7 May - I'll work on that a little later. Betty and I are having some severe problems re her parents, and I must join her in Indianapolis in a few days. I'll take your "puzzles" along and work on them some evening.

Thanks very much for permission to use your "sketches." I may take you up later on the kind offer of full-size xeroxes.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

/jb

P.S. What do you think of the specific prohibition in the Code against use of illustrations for types for fossil plants? It seems to me that for sporomorphae illustrations might be preferable as types??

Hunt Institute for Botanical Documentation

Thank you for your good wishes that we like to return to you: a happy 1985 we wish for you.
We honored Schütz at X-mas, and are now off on JS Bach's B-Mass for Good Friday: joyful sounds to the Lord!
We thought you'd appreciate this bit of Dutch folkart, even if it's a bit past 25 Dec.

Knipwerk W. TI. Lever

With our best regards
Jan J.

CALGARY
2 1
JUN

Uitgave: Ned. museum van knipkunst, Westerbork (Dr.)

The A. Traverse family
RD2 Box 390
Huntingdon PA 16652
USA

Hunt Institute for Botanical Documentation

15 January, 1985

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Thanks for your Christmas greeting.

Just wanted you to know that we too are laboring mightily on behalf of our friends Bach, Handel and Schütz. In our fall Choral Society concert, we offered "Ein feste Burg", a solo piece by Handel extolling the glories of music, and a marvellous work none of us had ever sung before: "Der musikalischen Exequien" by Schütz, a three-part requiem based on favorite biblical passages in the same way as Brahm's Requiem. Now we are preparing (as are many other choruses all over) "Messiah" for performance the end of February. Had originally intended it for late March or early April, but couldn't get the hall, so must rush along and have extra rehearsals. Of course, many of us have sung parts of it before, some many times, but few of us have sung the entire work at one time, so we have a lot to do. Rehearsal tonight.

Happy new year.

Yours,

Elizabeth I. Traverse
Research Assistant in Palynology

Hunt Institute for Botanical Documentation

30 November, 1984

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Could we just forget the $-surplus question? I notice you called it in your letter the "ICP Surplus". Interesting slip of the pen. I was asked for an opinion and stated it, without nearly enough regard for how my opinion would affect my old, dear friend, Jan. It seemed clear to me that the local committee could not simply decide that the money was theirs to keep, though they could of course have "arranged" the books to show no profit. It is hard to imagine hypothetical circumstances, but I believe I would have rendered the opinion if asked, that, yes, IFPS should attempt to raise $ to bail out a local committee if it lost money. I never advised "sue them", but "see a lawyer" to get a more meaningful reading as to what is legal. I believe I advised you similarly. I hold no office in IFPS, and my opinions are just personal opinions. Forgive me if I need forgiveness, and forget it.

Many thanks for the corrections of scolecodont figures. If you ever see Len Hills, please beg him for me to help me re megaspores. He knows what I need.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Dear Al,

Rt 11 21

Sorry for the delay with the enclosed stuff: it reached me just before the 6. IPC, and then got buried on my desk. I just unearthed it during a general clean-up.

I was disappointed in your hard, "American" sue-them stand regarding any ICP surplus. Had you really thought that through? What if we in Calgary had miscalculated and gone in the hole for $10,000? Of course, it would be us to hold the can. The IFPS should never become involved with the IPC budget. (Compare IOC: Montreal's loss, Los Angeles surplus; but: the stadium etc. is Montreal's to keep).

If you think that a change is needed, the relationships between IFPS, IPC etc should be changed before anyone has become involved. And what about the local sponsors that support the budget (corporate, organizational, government, University grants): should they not be welcome? If so, should they not be taken into account?

I'd say: things seem to run pretty well as is. Leave well enough alone!

In haste, best wishes.

Hunt Institute for Botanical Documentation

16 November, 1984

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Thanks for looking at "Fig. 62". I think your advice is good. I'll send it to Henk V. as a start.

The final, ultimate end of fall-out effects of 6 IPC have gone now, with the departure of Jiang Dexin. She's back in China after a month here. I asked her 2-3 times, 2-3 different ways why, if she could come to North America to visit us, she couldn't have come a couple of weeks earlier and given the paper she submitted to Calgary? The inevitable inscrutable answer was, "I only have rank of associate professor". She gave the paper (as a poster) at the AASP meeting (where it was not in the program--that has symmetry) in DC. Yet, her visit here was clearly, somehow, a product of 6 IPC.

Thanks again.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

29 October, 1984

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. S.W.
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Am working with my visitor from PRC and using your cards and my generic file--I find no J-H card for Podocarpidites Cookson ex Couper 1953. Have I lost it? Could you (urgent!) send me a xerox copy thereof?

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

TO AT

DATE 25 Oct. '84 TIME 4:30

WHILE YOU WERE OUT

M. Jan Jansonius

of ~~403~~

Phone 237-2438

TELEPHONED	☒	PLEASE RETURN CALL	☒
CALLED TO SEE YOU	☐	WILL CALL AGAIN	☐
RETURNED YOUR CALL	☐	RUSH	☐

MESSAGE call today (is earlier there)

(p.s. I didn't identify myself, as wasn't named certain who it was - had forgotten his thick accent!)

Signed BT (don't call him.)

The Standard Register Company

apparently wished to ~~see~~ meet me re 'wiping' what was one of my business' re $18,000 profit of 6 yrs ago

I need more money

Hunt Institute for Botanical Documentation

18 September, 1984

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Congratulations to you and all members of the Calgary committee. I'm getting to the point where I caa do a lot of comparing, and the Calgary meeting has few if any peers.

I'm still not over the sore-throat and cough business, but am much better. de Jersey left yesterday, and now I need to get ready for the next visitor (Jiang Dexin) and the next convention (AASP)!

All the best.

Yours very truly,

Alfred Traverse
Professof of Palynology

AT/et

Hunt Institute for Botanical Documentation

14 May, 1984

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

I'm sorry that pressure of PSU semester-end work has still further delayed an answer to your interesting letter of 22-III-84 re Galea and all that. Here goes for an answer (I have not checked original sources andjjust go by what you say):

a. Tautonyms are not validly published--see Article 32.1.b. They do not "...have a form which complies with...Art. 16-27....". (However, why couldn't 23.4 come clean and say so?)

b. The above means that Galea first has status as of Sarjeant 1964 as "B. galea Sarjeant" ("ex Maier" could be added, but in my opinion is better avoided, as it confuses people and is not required). The other two spp. of Galea were validly published in 1959 (as far as can be told from what you give me) and therefore would be B. mespilanas (Maier) Sarj. 1964 and B. levis (Maier) Sarj. 1964.

c. Lentin and Williams seem to have acted in accordance with the rules in transferring G. mespilana to Cheiropteridium as C. mespilanum (Meier) L.& W. 1973.

d. Sarjeant was wrong in transferring G. galea in the manner you describe. It was really to be treated as Baltisphaeridium galeum (Sarjeant 1964) Sarjeant 1983.

G. levis Maier 1959, G. mespilanus Maier 1959 and Cheiropteridium dispersum Gocht 1960 would not be affected by what Sarjeant did (just listing them in a synonomy without change of name).

e. If Sarjeant used Baltisphaeridium galeum in 1964, with full and complete reference to the original publication, it is clear to me that "galea" is from that point 100% validly published and legitimate. It doesn't matter that the original publication was not validly published.

f. If galea, levis, mespilanus and dispersum are all taken to be synonyms, someone must choose between mespilana and levis for the name of the species so considered.

Hunt Institute for Botanical Documentation

g. The situation in Chiropteridium in particular: assume we are going along with the idea that galea, mespilana, levis, and dispersum are synonyms. Galea was validly published in 1964; mespilana and levis were validly published in 1959, dispersum only in 1960. I therefore agree with almost all of what you say, except that you seem to feel that mespilana has some sort of priority over levis. But you don't tell me that anybody has yet made the choice. Or did L.& W. 1973 in effect make such a choice?

h. My advice: if you're in a position to do so, go ahead and make the choice.

Wow!

Yours as ever,

Alfred Traverse
Professor of Palynology

AT/et

P.S. Don't (please) forget my urgent need re the textbook project.

Hunt Institute for Botanical Documentation

14 May, 1984

Alfred Traverse, Professor of Palynology, 435 Deike

Claire Usher, Assistant Director, Graduate Admissions, 209 Kern

Read with interest the material in the Grad. School Bull. 33:5:2.

Big problem you all should and must address: many of our competitors spend real $ on recruitment. Several of my current undergrad advisees were invited by various universities to visit. Some paid ½ of travel costs, some paid all of costs, some paid all costs after student arrival at subject university. And I don't recall that "Sleeping space... on the floor" was part of the deal. If we want to get the top candidates, $ have to be found for recruiting costs. If they can't be found, we must be content with what we get.

AT/et

Hunt Institute for Botanical Documentation

9 April, 1984

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

The enclosed copy of letter to Chaloner is probably self-explanatory. I hope you will be willing to make a brief statement in the first go-round of the forum.

I think it's proper that from here on, all communications about this should go to Nichols (USGS, P&S Branch, Federal Center, Box 25046, MS 919, Denver, CO 80225).

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: D.J. Nichols
encl: copy of letter to Chaloner

P.S. I'm working on the Galea etc. question. Don't forget my textbook-related queries and requests.

Hunt Institute for Botanical Documentation

84 03 22

Dear Al,

In haste, and hence by hand: please release me from my bafflement.

Sarjeant 1983 concludes that he considers as conspecific: { Galea galea (jr. homonym; tautonym: Art. 23.4) / Galea mespilana / Galea levis } all Maier 1959

and that these names thus are synonyms.

Sarjeant 1964 had transferred these 3 spp. to Battisphaeridium

Gocht 1960 proposed Chiropteridium dispersum

Lentin & Williams 1973 transferred G. mespilana to Chiropteridium mespilanum (Maier 1959)

Sarjeant 1983 transfers G. galea to Chiropteridium, as Ch. galea (Maier 1959)

and includes in his synonymy G. levis Maier 1959
G. mespilanus Maier 1959
Ch. dispersum Gocht 1960

---

galea is a tautonym (Art. 23.4), and Galea is a jr. homonym. However, Art. 23.4 does not indicate if an effective tautonym is invalid or illegitimate: is it to be replaced immediately, and forgotten; or does it have standing, with priority of its first date, in a new combination? However, thru the index I find Art. 72.2 instructing me to replace it by the oldest legitimate name or epithet.

Such cross reference should be incorporated in the text of the ICBN!

I conclude that galea is doomed to oblivion. But: must I choose between mespilanum and levis for a replacement? In the genus Chiropteridium, mespilanum has priority over levis. Yet, Chiropteridium dispersum, which is another name for the same taxon, was there already in 1960. Is Ch. dispersum the legitimate name by correct interpretation of the ICBN? Or, ~~must some author publish~~ must it be considered a jr. syn. of Ch. mespilanum? Or, must some author as yet publish his preference for mespilanus over levis in order to make an official, effective and valid, choice to replace galea (on Sarjeant's authority that all 3 are conspecific).

My gut feeling is to maintain Ch. mespilanum as the oldest legitimate name in this genus.

What would be your opinion/advice?

Best regards,

Hunt Institute for Botanical Documentation

22 March, 1984

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Many thanks re deere. My two French dictionaries contained no info on suffixes. Very good--but the draftaman will hate me!

Good news re 6IPC. But Colin's word that USA is dragging its feet is disheartening.

Look forward to promised scolecodont photos. The delay hasn't mattered--I just work on other things. But, eventually, I'm counting' on you!

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEIKE BUILDING
UNIVERSITY PARK, PENNSYLVANIA 16802

College of Earth and Mineral Sciences
Department of Geosciences
Palynological Laboratories

Area Code 814
863-3419

15 February, 1984

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Miscellaneous inquiry #606: In Paris, 1981, monograph on chitinozoans there are some neat morphological diagrams I'm going to use in my textbook. I translated everything into English without difficulty except (Fig. 59) periderre, ectoderre, and endoderre, which I have provisionally left untranslated. What do you think? Is there an English exact equivalent for periderre, etc.?

As ever.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Dear Al,

You make me think of my wife and her "derrierre".

The French suffix "-derre" is derived from Grk derma, and thus:

Fr.	Eng.
périderre	= periderm
ectoderre	= ectoderm
endoderre	= endoderm

Have not had time to find you some scolecodont pictures, but hope to get to that in the near future. Please excuse this sloppy way of answering your letter? In haste, best regards,

PS: IPC has 380 abstracts (incl. 45 posters); promises to be a good meeting!

Hunt Institute for Botanical Documentation

Ja. Jansonius: call aboard

Scalecodes [will send]

→ Figs. 39 etc.

Chilinoson

recommends Florida Fair

take to photo

also re genera cards

will make full-size tracings if

Sato

15 February, 1984

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Miscellaneous inquiry #606: In Paris, 1981, monograph on chitinozoans there are some neat morphological diagrams I'm going to use in my textbook. I translated everything into English without difficulty except (Fig. 59) periderre, ectoderre, and endoderre, which I have provisionally left untranslated. What do you think? Is there an English exact equivalent for periderre, etc.?

As ever.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

84.01.18

Dear Al,

Rats! You + I have the same dictionary, so I can't use the excuse of my reference being insufficient.

If πάλη and pollen both mean "finest flour", it is not originally restricted to what now is referred to by "pollen" in English.

But still, it seems to me that the word palynology is derived from πάλλω; if we were to use πάλη as root, the study of it would come out as "palology".

Thank you for enlightening me on the Greek word for pollen.

Judi Lentin only has 1 or 2 abstracts in on nomenclature: do you know of any more to come? If so, she should have them soonest - I'm happy to see that you'll be in Calgary.

Jan.

Hunt Institute for Botanical Documentation

Dear Al,

I just read your apology in the ~~AASP~~ Newsletter* on sporomorphs and palynomorphs. Obviously, "pollen" and "palyno-" have a shared root; maybe the Greek word for pollen is something like "palynon". However, the dictionary for classical Greek gives only the verb παλυνω (palynoo): I strew, scatter. Thus, all the dust-size particles that have come to us in fossilized form are quite properly included in "palynomorphs". Personally, as you do, I would exclude those that are not acid-resistant, but others have included coccoliths in their understanding of that term.

In haste, best regards,

You overlooked

.. παλη, see attachment

"finest meal or flour" – cf. Blütenstaub

Pollen ≙ παλη

83.12.07

* or Palynology?

Hunt Institute for Botanical Documentation

6 January, 1984

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Gotcha note re Art. 133, but I don't like the current version, though I seem to agree with you more or less completely in philosophy. (I note that Wilson Stewart's new text has a long discussion of whatsa fossil and includes in his definition, "pre-historic". Baloney.)

My problem with your current proposal is with "conspecific living...." I don't see what that's got to do with it, though I understand you're trying to exclude the leaf imprints in our concrete walks here. (The leaves at the base of a tree you mention would not be "in a sedimentary matrix"?) I'll buy the concrete impressions as fossils, but I doubt that it matters. How about: Any non-living specimen of a plant of part of a plant, including non-prganic traces, occurring in a sedimentary matrix, is considered to be fossil. This also avoids "collected" which implies it can't be a fossil unless picked up.

My view is that any definition that tries to restrict the meaning on the basis of age, made of occurence, relationship, gets into trouble.

You confuse me with "13.12.83". You had convinced me that 83.12.13 is better!

All the best for '84.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Dear Al :

13.12.83

Cleaning up my desk, I came across a note made after you sent me a copy of your letter to Graham Williams.

At the time I wrote down the following definition for Art. 13.3 :

~~Any specimen collected as non-living material from a sedimentary matrix, at a locality~~

Any non-living specimen, collected from a sedimentary matrix at a locality where conspecific living material cannot be ascertained, is considered to be fossil.

I dropped a clause "permanently embedded in matrix", because how long is "permanent" : some fossils rework. A 1000 year old lotus seed which is still viable, I would not consider a fossil (although it may be surrounded by organic fossil fragments). On the other hand, I'd like to exclude last years leafs from an oak or pine tree, accumulating at the foot of its trunk. Yet, if one of those leafs was torn off by a tornado and dumped into the deep Atlantic, I would call that a young fossil.

Ciao,

John

Hunt Institute for Botanical Documentation

Dear Al,

I just read your apology in the ~~AASP~~ Newsletter* on spheromorphs and palynomorphs. Obviously, "pollen" and "palyno-" have a shared root; maybe the Greek word for pollen is something like "palynon". However, the dictionary for classical Greek gives only the verb παλυνω (palynoo): I strew, scatter. Thus, all the dust-size particles that have come to us in fossilized form are quite properly included in "palynomorphs". Personally, as you do, I would exclude those that are not acid-resistant, but others have included coccoliths in their understanding of that term.

In haste, best regards,

Vaughn

83.12.07

* or Palynology?

Hunt Institute for Botanical Documentation

Jansonius

VI IPC Calgary 1984

August 26 - September 1

Please direct all correspondence to:
Sixth International Palynological Conference
Lois Kokoski Faculty of Continuing Education
The University of Calgary
Calgary, Alberta, Canada, T2N 1N4
(403)284-5051

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Lois Kokoski
Conference Co-ordinator

83 11 23

Dr. A. Traverse
Palynology Labs
College Earth Sciences
Deike Building
University Park, Pennsylvania 16802
U.S.A.

Dear Al:

I received your letter with the copy of "form 2A", and passed same on to our program chairman, Judi.

I am sorry that the date is causing problems for you; nothing can be done about it other than accommodating you in your request that you'll be "on" later in the week. I was happy to see that your accompanying member signed X on the dotted line. We look forward to seeing both of you. Too bad you shan't bring your retinue of grad students.

The Duck Choi paper looks interesting; he kindly sent me a few of the plates of his thesis to better interpret the paper in Science.

Best regards,

Dr. Jan Jansonius
Co-Chairman,
Sixth IPC

JJ/mes
A-79

Hunt Institute for Botanical Documentation

14 November, 1983

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

It has suddenly occurred to me that I may quite likely never have sent in my "Form 2a" for 6 IPC. I have now done so, but to be on the safe side I'm sending you a copy too, so that you could intercede for me if the missed deadline really matters.

I have a serious problem about the Conference: our new semester calendar is now in effect, and the Fall Semester 1984 begins on 20 August, first classes on 24August. I have discussed this with the dean, who says if I get the classes started, I can then fly out to Calgary for a couple of days, but it's going to be a mess. My plans to bring a coupld of graduate students are clearly down the tube. However, I'll submit abstracts timely (i.e., next month) and just hope that the papers, if accepted, fall on the days I can be in Calgary! If the taxonomy session flies, I of course hope that'll be held on a day in the middle or toward the end of the Conference, as it'll be the first couple of days that I'll probably have to miss.

All the best, as usual.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: copy of "2a"

Hunt Institute for Botanical Documentation

7 October, 1983

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada
T1P 0H6

Dear Jan:

Re yours of 83-09-16 (arrived 6 Oct.), for which thanks:

Your errudition is beginning to astonish me. I probably would have "corrected" to dodgestownii without thinking about it.

I would guess that the reason one doesn't have to correct Spinner's error is that 1) a choice exists, 2)either choice would be a considerable change. In general, as a non-classical-scholar, I favor things about as they are--correction of only gross errors (gender, etc.) in the Latin, and keeping to the original author's spelling, mistakes and all, as much as possible. If you had heard the endless debates about names with -er at 5 or 6 congresses, you would be leery that big orthographic business will almost surely fail.

I guess that covers this from my point of view.

By the way, I believe I forgot to comment on your "Russian typewriter" dig in a previous letter. As you surely know, in 1983 it is only a question of a "cyrillic ball" about the size of a walnut, inserted in the same old typewriter. Of course, one must have a convernion chart for the keys. It's really not a big problem.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

ESSO RESOURCES CANADA LIMITED

Exploration Department
ESSO PLAZA 237 FOURTH AVENUE SOUTHWEST
CALGARY, CANADA T2P 0H6

R. A. F. WILKINSON
Vice-President and General Manager

W. D. EVANS
Assistant General Manager

83 09 29

Dr. A. Traverse
Department of Geosciences
Pennsylvania State University
435 Deike Building
UNIVERSITY PARK, PA, U.S.A. 16802

Dear Al:

Thank you very much for your comments on my nomenclatural note. Alreaday I had removed some of the poetics, and your suggestions further helped me in giving it a polite appearance. (I feared you would want me to type the Greek in alphas and betas).

I will not support an amendation of the Code disallowing specified languages for scientific expression. If you want to revert to Latin diagnoses as a requirement, you better first demonstrate, in print, how this can be done. We would need a vocabulary and idiom first for all the microscopic detail and procedure. Any good vernacular description can be adequately translated. Our problems are due to insufficient observation, incomplete description, and imprecise communication, whenever an ignorant worker feels an urge to make a contribution to our understanding (without regard to his own). More important than legislation is education and example. Also, "approved" languages is a very political thing: is English "approved" in Quebec? Written Chinese is the most understood written language (or possibly, now, second after English), but would you act on it? And if you read some of the Chinglish or Franglais, or even English by their fellow Saxons, the Germans: at times you have not the foggiest notion what they are about. Far better to have a solid description in simple terms, in the best language the author can muster. And some photographs of American quality (or Japanese ?).

Thank you, also, for the comments on Piceaepollenites; I will do nothing rash, and maybe nothing more.

Best regards to you and your faithful E.T.,

Dr. Jan Jansonius
Expl. Res. & Applic.
(Biostratigraphy)

JJ/ljh

P.S. "ogival" is an architectural term (my first career choice).

Hunt Institute for Botanical Documentation

23 September, 1983

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Did I ever ask you about the work "ogival" in line 10 of card 748 on Deltoidospora ? Where did it come from? Very erudite.

Different subject: would you support me in amending the Code to eliminate the present provision that new fossil taxa names may be written in any language? I am not sure about going to required Latin (on the other hand, why not?), but how about requiring as of 1 Jan., 1985, a diagnosis in one of a list of approved languages? Esperanto? What brings it to mind is Yuke's "Humanispora", 1981, the description of which might as well be in Martian.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

23 September, 1983

Dr. Jan Jansonius
Esso Resources Canada Ltdx
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Thanks for yours of "83-09-02" which just (!) came.

The complete bibliography you propose would be nice.

I maintain still that transliteration of Cyrillic is an awfukl practice--translation is better. Best is to use hhe Cyrillic. If you will dook at any Soviet palynological contribution, you will note that they usually extend the reciprocal courtesy. Latin and Cyrillic are the major alphabets, as Atatürk decided in 1921. Hangul, etc., are another problem. As I believe I said before, however, as far as valid publication is concerned, I believe we ought to consider repealing the paleobotanical any-language'll-do provision and go Latin.

"Skeptical about", "skeptical of" are good usage. "Different from" is better than "different than", but the latter is gaining.

Re Piceaepollenites: as I probably told you, I keep my own generic list, begun when I was helping with ING. I always have made entries independently even after your cards came out, so as to give an unbiased view. I carry P. as you ddo as of the Sitzungsberichte. It seems to me you muddy the water by getting into the Mitt. I-IV bit--it's all irrelevant. I'd feel no compunction to get into it. You're right, of coursed, P. was not a homonym in Sitzungsberichte, but why bring that up? If you do insist on the so-called "clarification", however, at least tell us directly which Mitt. is which, instead of just referring us to Taxon v. 28! The present text of your MS. does not precisely state which is which, and would confuse me if I didn't know the answer. Better: avoid the whole irrelevant issue--no genus is published in Jahrb., period.

I'm not going to AASP either--no $ or time. Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

23 September, 1983

Dr. Jan Jansonius
Esso Resources Canada Ltd.
237 4th Ave. SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Just got yours of 83-09-08, urging prompt response. But, honest-injun, it just came to my desk on 83-09-21!

I think your points are all very well taken. However, I wonder if you do not confuse someeneophytes by saying this or that generic name should have been transliterated this or that way--that's a good point but not really compelling. Also, why make a morse enemy of Wicander than necessary--let him down gently. You should concentrate your fire on the out-and-out errors, of which there appear to be plenty, and tone down the polemics a bit. (Your analysis of W. confirms that "a little knowledge is a dangerous thing". He'd have been better off to make up arbitrary names!)

Incidentally, I admire your command of Greek and Latin. I've forgotten mine, almost utterly.

I enclose your MS with some penciled comments perhaps illustrating my points.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl

Hunt Institute for Botanical Documentation

ESSO RESOURCES CANADA LIMITED

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ESSO PLAZA 237 FOURTH AVENUE SOUTHWEST
CALGARY, CANADA T2P 0H6

R. A. F. WILKINSON
Vice-President and General Manager

W. D. EVANS
Assistant General Manager

83 09 19

Dr. A. Traverse
Department of Geosciences
Pennsylvania State University
435 Deike Building
UNIVERSITY PARK, PA, U.S.A. 16802

Dear Al:

When Spinner (Pollen Spores, v. 25, p. 124-126) names a spore after the town of Dodgesontown, as G. dodgesontowni, I knew at least that I must not double the final "i". Rec. 73D recommends that the epithet should have been latinized as "dodgesontownianus" or "dodgesontownensis". However, Art. 73.10 mandates that errors against Rec. 73C.1 must be corrected, but its silence about Rec. 73D, or for that matter Rec. 73C.4, means that subsequent workers are not to correct orthographies at odds with those Recommendations? (In the past, I think I made all Mc's into Mac's!). Should the Code be amended to include the latter two Recommendations?

In haste,

Dr. Jan Jansonius
Expl. Res. & Applic.
(Biostratigraphy)

JJ/ljh

Hunt Institute for Botanical Documentation

ESSO RESOURCES CANADA LIMITED

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R. A. F. WILKINSON
Vice-President and General Manager

W. D. EVANS
Assistant General Manager

83 09 08

Dr. A. Traverse
Department of Geosciences
Pennsylvania State University
435 Deike Building
UNIVERSITY PARK, PA, U.S.A. 16802

Dear Al:

Enclosed is the MS of a raillery against (if that is the proper proposition) the fad of using Greek in botanical nomenclature, as introduced mainly by Loeblich and Sarjeant. Will you please give this your immediate attention, and return it this week with your comments.

I first thought to send it to the AASP Newsletter, but now wonder if Palynology may be a better forum. Taxon does not reach the audience that may benefit from reading this.

With abated breath and best regards,

Dr. Jan Jansonius
Expl. Res. & Applic.
(Biostratigraphy)

JJ/ljh
Enclosure

Hunt Institute for Botanical Documentation

ESSO RESOURCES CANADA LIMITED

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83 09 02

Dr. A. Traverse
Deike Building
(Palynology)
UNIVERSITY PARK, PA, U.S.A. 16802

Dear Al:

I didn't write you for so long, if feels as awkward to address you again as it felt not to be in contact.

Presently, I am wrapping up the 1983 Supplement to the Genera File; it got delayed partly out of a notion that I could reduce the crush by having two updates in three years, and wedge the Sixth International Palynological Conference in between. Also, I got off on a dinoflagellate kick this winter/spring, and had much fun doing some honest research. But that got shoved aside by a slow-moving mass of service work (since Staplin elected retirement, I'm back in the Beaufort). Anyway, as the back-log of undigested genera mounted up, I got uncomfortable with it and lost my peace of mind. If I'd've left it much longer, I might have lost it all . . . not just a piece.

I got rid of "Polaroporis": the French will reprocess the material (the original preparations were lost) and (re)describe the type-species designate; I hope they'll also publish the genus correctly with it. And I don't mind if they keep putting it off.

I am now reading yours of 82 12 03 with many small corrections and/or questions.

Re: the Precambrian stuff. I would be very happy if I had a clear-cut boundary. However, just as with the fungi (where I have carded spores, hyphae, and fructification-genera), I don't know when a plant spore is not a "spore" anymore. Many of the acritarchs (in spite of what that name implies) are rather clear-cut, and I have avoided those. But how do we recognize that a Precambrian oligoseptate hypha is <u>not</u> a fungus; or a tetrad of round bodies is <u>not</u> a group of spores; or a pluricellate trichome is <u>not</u> a functional reproductive body? For that matter, there are several younger "palynomorphs" <u>incertae sedis</u>, which I carded. I hope to be fairly complete (as far as I want to go) on these things (believe me, I know of more pleasant things to do with my time), but it is better to err on the side of being too comprehensive, than to have omissions in places where users may be looking for information. Basta.

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#2372 "Subrelated" must be my own invention (maybe I thought of "poor relations"?); what I probably had in mind was "subjugated".

#2921 "Divisive" should be "-divided"; a Germanism.

-pollis is taken by me as having fem. gender (Recom. 75A.1).

#3940 You may be right with "Tournaisian"; the Russian has "Turneiskii yarus". I just followed our translator.

This brings up the point of bibliographic references. Do you think we should try to prepare, as celebration of our first decennium, a complete list of articles used and/or quoted, in the Genera File?

#3956 "Laesure" was copied from the original; is this qualitatively different from "colpus-colp", "ambitus-amb", etc.?

Your thought is reasonable, to cross-reference using name and J-H# of previous cards. Also, I did not omit on purpose "(CFSP16:14)" following CARPINITES: I simply was not aware. However, since there are so many cards where we did not do this, we would introduce a measure of lobsidedness. Further, all entries are alphabetically ordered, but with each following Supplement, they are numerically more in confusion. But this also is an argument pro your position: the more cards are re-issued, the more they need the number as identifier. I'll make a point of adding the J-H# in future.

This matter of internal consistency also is the reason for still using the simple "μ", rather than "μm". Am I becoming too inflexible?

Another see-saw is "Beijing" vs "Peking"; even the Chinese will use both in one letter. I'll just go with the tide, it is hardly worth too much attention.

Your insistence that we use Cyrillic type for Russian bibliographic information makes me think that this is an oblique way of letting me know that you got a Cyrillic typewriter. Congratulations, especially because you learned the language. But would you insist next on Chinese and Japanese script? (Thank God there are no Greek palynologists; and the Arabs all use English - I hope.)

My reasoning for according the Chinese palynologists both family and given names is that, among the billion-or-so of them, most carry only one of ca 100 "family" names. Actually, these are more like clan names, and there is a "Mah-society", or "Huang-society", in most major cities with a significant Chinese population. How many "Traverses" or "Wigginses" in palynology? But you check your Lis, Lus, Zhangs, etc. and you are soon confused. For that matter, I did recognize S.K. Srivastava as such; and if ever the Sikhs get into palynology, you'll have to be more specific than Singh. Also vide 46.1 Examples: R. Keller; 46A.4. In cases of multiple authors, I have not been consistent in this ("Sun & Zhang"), and I better give them full value as well.

I must have chosen wrongly with "skeptical against"; would it be "re" or "about", or "towards"? I have a long-term gripe against English dictionaries, because they do not give the correct prepositional combinations. Is it:

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different than, different from, or different to (dissimilar to)? Etc. This is very difficult to pick up.

in that order

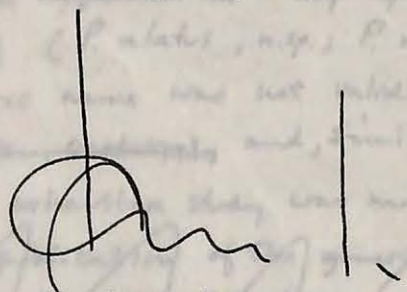
The Dutch custom on names with "van", etc., is not very good. In the phone book you'll find: Berg, J. v.d.; Berg, K. ter; Berg, L. de; Berg, M. v.d. I don't think you'd ever have to capitalize the v of van, although some might when it starts a new sentence. There seems to be a move to adopt the English handling of it, but I'm a bit out of touch.

Curiously, just now I received your letter (83 08 13) with your report on Van der Eem's "proefschrift", and its handling of the prefixed vans and vanders. Like the devil is playing with it! There was a bit on this curious problem in a Dutch-language Canadian monthly out of Vancouver. However, from its discussion I only wound up more confused. This thesis seems to confirm that things are changing.

Thank you very much, Al, for your comments and various corrections. They are always useful to me.

I don't fully expect to be in San Francisco for the AASP meeting, although the day of the dinoflagellates looms large and should be an important meeting. But at least I hope to see you next year in Calgary.

Best regards,



Jan Jansonius
Expl. Res. & Applic.
(Biostratigraphy)

JJ/ljh

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PICEAEPOLLENITES Potonié 1931 (15 July)

Synopsis II, p. 64

Sitzungsberichte

use previous illustration, but upgrade distal saccus base from 15 July 1931, pl.2

Type species: ~~Piceaepollenites alatus Pot. 1958, ibid.~~

→ Piceaepollenites alatus Pot. 1931 (May), ~~Jahrb. Preussischen Geol. Landesanstalt, Bd. 52, p. 5, fig. 31 (dornung); 1931 (15 July)~~, Sitz.-ber. Gesellsch. Naturf. Freunde Berlin, pl.2, fig. V-70a (photograph); not validly published in 1931 (May, Jahrb. Preuss. Geol. L.-A., Bd.52, p.5)

Diagnosis (1958): "Equatorial outline more or less oval, sacci in this view half-circular or smaller; crest (i.e. optical section of the proximal cap) more strongly developed than in Abiespollenites; in lateral view outline more or less bean-shaped, with the distal germinal region located in a depression shallower than that in Abiespollenites and Pinuspollenites; the outline of the sacci joining that of the central body without a distinct jag; the sacci attached proximally of the equator, but not as far proximal as in Pinuspollenites

Remarks: Frederiksen & Ames 1979 (Taxon, v.28, p. 557-561), in a review of the genus Pollenites definitely give the exact publication dates for the four 'communications' published by Potonié between 18 April and 15 July 1931. It appears that his 'second communication' actually appeared last in this series. It is preceeded by the third communication, which appeared in May 1931, and in which two species are assigned to Piceaepollenites (P. alatus, n.sp.; P. microalatus n.sp.); there is no generic diagnosis, and therefore the generic name was not valid through a combined description ~~as earlier held by Timonius & Hills, cand 1993~~ and, similarly, both species names also were not validly published.

No exhaustive study was made of the literature, but, to the best of our knowledge, the first publication of the generic name Piceaepollenites in which all requirements for valid publication were satisfied is that of Potonié 1958. If this proves true, Piceaepollenites would be an obligate junior synonym of Piceapollenites (q.v.)

~~Further~~ On 15 July 1931, Piceaepollenites alatus n.sp. was published again, and in this publication ~~the~~ all requirements for valid publication of a monotypic genus based on a new species ~~were~~ through a combined description are satisfied.

Al, Please look this over for me; this has been on my mind for a year or so already. I think the flaw in my reasoning here is the "again" in third line up. Is the second time it got published a junior homonym, or not? If I read Art. 64 properly, it is not; for the first time round the name was not validly published and thus is not [illegible] Still I am not

29 August, 1983

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

I got the enclosed dues notice from Bernard. But I was sure I paid for 1982. In any event, check for $6.00 enclosed. If I do owe also for 1982, please let me know! If not, please inform Bernard in appropriate fashion. I don't want to be dropped.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: dues notice, check for $6

Hunt Institute for Botanical Documentation

19 August, 1983

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

I just got a "Proefschrift" from van der Eem, sponsored by Henk Visscher. Throughout--it's especially notable in the bibliography--all "vans", "van ders" and "der" are capitalized and alphabetized accordingly. Thus, Van der Eem, alphabetized under 'V'. van der Hammen isn't in the thing, but would be Van der Hammen, under 'V'. de Jersey is De Jersey (though the man himself hasn't changed the spelling as far as I know) under 'D'. Is this a new Dutch policy? If so, should it be followed? Etc.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

3 December, 1982

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Well, here we go again. I have enjoyed your last letter (82-11-06) and addendum very much.

The Palaroporis business is perplexing. As I recall, he wanted you to "take care of it"? Line drawing is a perfectly good "illustration", so you could validate the whole deal. "Indication" in my mind does mean (at least) a printed indication. Per Sydney Code, I guess (I wait with bated breath) our problem may permutate to a genus validly published because type specimen is properly indicated, though the species is not validly published. I do hope Chaloner et al. rode herd carefully on that!

I haven't got AASP Newsletter yet so can't comment really. Our position should be to encourage use of para-nomenclatures (e.g. turmae) while insisting that binomial nomenclature is ultimately normative.

I got your point on pissarti & pissardi, but it should be pissardii, in any case.

I guess you may have one re 45.1, though I am not convinced. Clearly that article has to do with a situation in which a name remains not validly published until all requirements are met, meaning, for example that the author who finally designates a type is the author to be credited. If "printed" means at various times, obviously the times have to be dated, which is certainly difficult. I guess an errata slip separately distributed would have the publication date of the slip, not of the original publication, but the whole matter seems wrong to me. "Printed" refers to one printing and especially to one printing made available at one time (Art. 30). Art. 45.1 doesn't seem to me too pertinent to this issue.

Here are my comments on the recent supplements. I do wish you would limit the file to sporomorphs. None of the Precambrian stuff would qualify.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: comments on recent supplements

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3933; l. 5: read user for "use"

3934, #2372: problem: card 2372 doesn't say "Accussporis" but Assus sporites". Explanation? While we're at it, l. 2 up: what language is "subrelated"?

3934, re 2921: but "divisive" shouldn't be used this way anyhow. It's not proper English usage.

3934, co. 2, l. 10: solicit is misspelled.

3936, l. 4: ellipsoidal
l. 10: segregated

3938; do you happen to know why it's "barghoorniana"? I have been unable to find "-pollis" in my lexicon to check the gender. (-pollenites is masculine and most people seem to regard -pollis as such.)

3940: Biblio. ref. should (must, if it's true) contain at end: "(in Russian)"; last line: any chance it said "Tournais (=Tournaisian)"? (You know how I feel about this sort of biblio. stuff--it should be in Cyrillic, or else in translation, not i n transliteration! The reason is obvious--in a library, you usually have to seek for the publication in Cyrillic or translation.

3956, l. 5 up: shouldn't it be "aesura"?

3959, col. 1, l. 13 up: accommodate

3962: Just a thought: would it be helpful at least in cases such as these to give the J-H # of the other genera cited? Also: how about Carpinites? (CFSP 16:14). I mean, shouldn't it be dealt with?

3965, l. 3: Just a thought: in future shouldn't you go with "Beijing"? (cf.#3979)

3986: Isn't it your usual policy to put the author's name on the title-line? Is certainly a good idea.

3987: Again, in my opinion, this should say "(in Russian)" after biblio. ref., unless it was originally in English. You could, as I have said, use Cyrillic (following the Russian practice for refs. in our alphabet) and avoid this problem.

4006: Remarks: ...shows up as(?)....

4010: Caption: "Chlonova": Annie herself prefers "Chlonova", but given the transliteration scheme you have adopted, it's Khlonova, and besides you should be internally consistent!

4018: (any many other cards) What is your reasoning for according Chinese palynologists both family and given name, western palynologists only family name? Here, for example, why not just "Yu", for consistency?

4021, l. up. 11: Aquilapollenites

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4025: Biblio line: If "H.2" why not "S.79"?

4031: Remarkd: don't change it, but watch it: "...skeptical against" is not proper English

4038: Well, here's oofie in which the Chinese do get only surnames. Better come up with a policy on this and follow it.'

4041, l. 1: van der Hammen (you, of all people) you Ofinedunserip home). Of course, it should also be corrected in card 2783. Whoops, I checked elsewhere, and I guess it's a policy (dumb policy?). But if it is a policy, why do you abbreviate as "v.d.H."?

4043, l. 3 up: The

4051, l. 1: Here Sung & Zheng are o.k. with family names only when first mentioned, but require given names when second mentioned?!

4052, l. 3: Check comments on 3938. If -pollis is feminine this should be corrected to magnispinosa. If not, barghoorniana is wrong on 3938?

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82 11 16

Dr. A. Traverse
Deike Building 425
Palynology Department
UNIVERSITY PARK, PA, USA 16802

Dear Al:

I received your letter of 2/XI; the form 82 11 02 is that newly prescribed in the SI when that was introduced in Canada; if it is not itself a standard measurement of historic time, at least this reasonable standard was introduced on the rational coat-tails of the SI.

Re Daphniphyllumpollenites: I accept it as validly published not from the date of the original publication, but as of that of the printed rectification; I consider Art. 30.2 (in a perverse sense: this is not an advance, but a delayed publication) and especially Art. 45.1 pertinent to this case. Your interpretation that "printed" clearly means "printed at one time" is at odds with Art. 45.1, and certainly not spelled out in the Code.

Re Polaroporis: I am lost in ambiguity here. I proposed to Medus to validate the genus in the Genera File if he preferred me to do so, but of course with the implied notion that the basic premise was validatable (if you follow me). Now, knowing that mariae-andrei was not validly published, makes this all rather questionable. If the species were validly published in the File (obviously illustrated only with a line drawing, even if it would be my best effort), would the genus be valid as per Medus, or only ex the File? If Médus were to renege on his earlier "indication" of type species, could he describe and name his own material and make it the nomenclatural type without creating a junior homonym?

We agonized over "indication"; the Regn. Veg. Glossary is not much help. However, in Art. 32.3 "indication" is linked to the concept of "effectively published". In our case, that does not hold for mariae-andrei. Would the same implication of effective publication be associated with "indicated" in Art. 37.1? I had received the notion, somewhere in the past, that a genus could be

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validly published even if the type species itself (as long as it was designated or "indicated") was not simultaneously validly published. That still may be true, but I may have overlooked that, by (remote) implication, it was required that it be at least effectively printed. I am afraid to muddle too much in this pailful of muck without prior advice of a neobotanist who can advise what ramifications are involved in any proposal to clean up the wording of the Code in this area.

We just got in the AASP Newsletter, in which Norm Hughes keeps up his campaign. Should we initiate a public discussion in Calgary along his proposal? As it is, we have a session topic "Taxonomy, Nomenclature and the ICBN". What exactly was his Sydney proposal? I am beginning to think that all this is a waste of effort and energy; however, can we afford to just let him burn himself out without further damage, or must he be contained more formally?

Best regards,

Jan J.

Jan Jansonius

JJ/ljh

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P.S.

Just as I had answered your letter of 2 Nov., another one came in under the date of 5 November.
Apparently you suffer from the same difficulties as I: I, also, do not always clearly remember the question that you are answering. Maybe both of us should try to be more explicit.
In the funny department: I blush at the realisation that I wrote St. James where I meant King James. Still, it is an awkward problem: how to form plurals of Latin technical terms. I don't believe you object to the use of the plural "genera", or "algae" in an English language article. Therefore, your objection to "varietates" strikes me as irrational. It might help to italicize the word with an underline?

Apparently you missed the point re pissarti (Rev. Hort. 1880) ↔ pissardi (Rev. Hort. 1881); from the Code (please q.v.: Art. 73.10) the name was first published with a "t", but later corrected to "d" presumably because named for a Mr. Pissard. The doubling of the "i" to "ii" is a different matter altogether.

Thank you for your advice regarding bulls (= bellus?, bullatus?). I shall correspond with the coining and validating authors and straighten it out properly.

In deference to our air mail system, I should take this opportunity to wish you and all yours a good X-mas, and a happy and prosperous new year!

John L.

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author ("Sung & Lee ex" can be omitted--the short, perfectly correct form is C. bullis Zhang))

Lastly, I am always amused by your "airmail" envelopes and stickers and rubber stamp. Yours was postmarked 20 October, arrived here on 3 November. "Airmail", my foot. In North America, "airmail" doesn't mean a thing, as the USPO recognized some years ago by abolishing domestic (North America) airmail stamps and rates. Of course, for 35¢ (=28¢)

5 November, 1982

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Your letters often pose exegetic problems, because I really have to have the previous letter in hand in order to understand, and this is often impossible; (we file letters about once every 2-3 months and are behind at the moment). I haven't the slightest idea what you mean by "St. James" in yours of 19 October, but as you use "vulgate" in the same sentence, you must mean the King James version of scripture? (I have for several years now read scripture only in German, however.) In any event, "varietates" has no business in an English language article.

I don't recall saying that Art. 46 says "a name is invalid or illegitimate without citation". Nevertheless, it is certainly good practice to give the name of the author. The L.W. III, as I recall, had the business badly mixed up as regards autonyms--they misunderstood the question of author citation in that connection completely.

Re "correction" of originally published names. I have always understood that one critical matter is apparent intention. Thus, Linnaeus' "pensylvanica" can't be corrected to "pennsylvanica", even though the L. version is wrong--L. intended to do it that way. Further, generic names are to be treated with much greater reserve than specific. Changing Lespedeza would be a big problem, whereas Renghas doesn't bother. Top of your second page--look at 73.10 again: pissardii is correct, not pissardi, as you say. This is a question of incorrect Latin, which should always be corrected.

Now, as to ...bulls. Obviously something must be done. A generic name can be any combination of letters, but a specific name must be handled as a Latin adjective--should be bullsii, or whatever, but not bulls. If the plate description has bellus, o.k. Failing that, I guess we have to accept the word of somebody in China, preferably Zhang. (Had it been a generic name, it is my opinion that Bulls would be there for the ages.) If there were no word from Zhang, Ouyang, or anybody, I suppose somebody would have to tidy it up as bullsii, or whatever. I believe I would write Zhang, if I were you. Is bellus in the paper somewhere? If not, what did Zhang intend?

Also on your page 2, "... maybe Sung & Lee intended the Printer....", but the style "Sung & Lee ex Zhang" means that Zhang was the publishing

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author ("Sung & Lee ex" can be omitted--the short, perfectly correct form is C. bulls Zhang))

Lastly, I am always amused by your "airmail" envelopes and stickers and rubber stamp. Yours was postmarked 20 October, arrived here on 3 November. "Airmail", my foot. In North America, "airmail" doesn't mean a thing, as the USPO recognized some years ago by abolishing domestic (within North America) airmail stamps and rates. Of course, for 35¢ (=28¢) you should get airmail!

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

2 November, 1982

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Ye gods! The new batch of J&H supplements is in. Time really does fugit.

Did you ever pick up a card #1530, Desc. 1. 9, "Kerper" for Keuper?

Thanks for yours of "82-09-28"—that's a pretty good notation. I use 28-IX-82, but in any event, the 9-28-82 scheme is idiotic. Your feeling is that your notation allows 9-28 while still having logical sequence, I guess?

Re Daphniphyllum pollenites: it seems to me that "printed" clearly means "printed at one time". I don't believe pasted-in slips can possibly count unless perhaps there is evidence that the paste-in was part of the original publication. And even that is an interesting point--what is the status for an example of an errata slip intended by an author to supersede the printed text? I can't recall an instance where it matters, but it could.

Re Médus and all that, I enclose a xerox of a recent letter from him. I do regard Polaroporis as not validly published in Médus, 1981, because there is no persse really identifiable type species. You are right that although (post-1958) a genus has to include at least one species (the type), that type species need not be validly published, as long as it is "indicated". However, "indication" seems to me to imply more than just dropping a name, which is what Médus has done. To "indicate" is to point at something in particular. (However, it is clear that you and/or I should submit an amendment for Berlin, specifying what the requirement for "indication" is!). I don't think you can validate Polaroporis without at least sharpening up P. mariae-andrei!

Sydney made a specimen the generic type, not the way you print it on p. 2 of my letter (though one may continue to cite the species). As far as I recall, valid publication of the types is still not clarified.

Best to you and yours.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

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Assistant General Manager

82 10 19

Mr. A. Traverse
Department of Geosciences
Pennsylvania State University
435 Deike Building
UNIVERSITY PARK, PA, USA 16802

Dear Al:

Thank you for sending me copies of your letter to Graham Williams, and Zhang Lu-jin's letter to you.

I had read the bulk of L.W.III, and made notes in the margins explaining some of my reasonings. However, I was never sent the introductory parts, for which I was sorry (as you can imagine).

The word varietates is plural of varietas; "varietate" is no word (although, of course, it is an ablative form of varietas). I take it that you prefer St. James' over the vulgate.

Regarding author citation: Article 46 does not say that a name is invalid or illegitimate without citation. Rather, "for the indication of the name . . . to be accurate and complete", the authors who first validly published the name should be appended. The Articles 46-50 then set out standard and approved procedure. Thus, in the running text of a discussion, it is hardly needed to give the authors in full.

Now that I have your attention, when may the spelling of a name be corrected from its original published form? I always have suffered under the impression (Article 73) that Ginkgo was misspellled in its original publication (the author having intended to write "Gingko"); however, as it was in print in its wrong form, we had to learn to live with it.

The Code confuses me with its examples: Gluta benghas L. should be corrected to Gluta renghas L. (73.1 - the vernacular name used as an epithet is "Renghas", not "Benghas"); yet Lespedeza is not to be altered, even when we know that it commemorates mister de Cespedes (73.3).

. . ./2

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But pissarti is corrected to pissardi (73.10)! I am asking because I just received a copy of a few pages out of a Chinese book which was published in few numbers and now out-of-print. In it, the new genus and species Cyclophorusisporites bulls Sung & Lee ex Zhang is published. It was sent to me by Ouyang & Li, who in the text corrected "bulls" to "bellus", and who in their letter state that "obviously, the type species name was misspelled". I did not receive a copy of the plate description (where maybe "bellus" was used?). I can sympathize with our Chinese colleagues and their difficulty with Latin gremlins, but does that sympathy give me leave to change bulls to bellus? Quant-à-ça: the spore is ornamented with large globular spherules à la Pistillipollenites, and maybe Sung & Lee intended the printer to set "bullatus"? ("Bull" etymologically is related to "balls").

Looking forward to your comments on this, and once again with my thanks for your consideration in sending the copies.

With best regards,

Jan Jansonius

JJ/ljh

Hunt Institute for Botanical Documentation

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82 09 28

Dr. A. Traverse
The Pennsylvania State University
Palynological Laboratories
435 Deike Building
UNIVERSITY PARK, Pennsylvania
U. S. A. 16802

Dear Al:

Thank you for picking up the misspelling of Alnusalispollenites. I must have slipped in copying the name when preparing the MS.

Re Daphniphyllumpollenites: I accepted this pasted-in slip, because it had been printed. The same "new edition" of the reprint (effectively published later than the original article) contained additional written corrections of orthography in Huang's hand; all those I ignored. Since I warned him of his mistakes early-on, I presume many corrected reprints were distributed.

Pterisporis is valid through Huang in Jansonius & Hills 1980. His other own nomenclatural note does not give a full and complete reference to Huang 1978; it would be hard to decide the priority if it came down to date of effective publication. In either case, Huang is the author.

Just now your letter (copy) to Médus dropped in; curious, because I was going to mention "Polaroporis" next. In my view, the genus is validly published, because a nomenclatural type was indicated. The ICBN does not require that this type species itself be validly published. However, you are correct in your surmise that the Latron thesis was not published; in that thesis, Latron names a new monotypic genus "Sphaeropollis mariae-andrei". Médus did not take over the generic name because he considered it poorly diagnosed, but (desiring to honor Latron's parents, André and Marie?) does refer to the specific name. Why do educated people do such things?

Do you consider Polaroporis not to be validly published? Of course, without a proper nomenclatural type, it is difficult to know what the content of the genus is. I asked several people in Dublin if I should

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actually validate the species "P. mariae-andrei" in the Genera File, or not. The majority said: not. All I have to go by is a xerox copy of Latron's thesis; the illustration is poor, the type may not be preserved. So, even if I feel I must accept Polaropollis as valid, it is a monument to Médus' ineptitude. I don't see how I could do a satisfactory job trying to rehabilitate "mariae-andrei". Médus, himself, shrugs the matter off, and has no intention to write a "nomenclatural note".

A propos: if indeed the consequence of Sydney will be that a nomenclatural type for a genus must be a validly published species with a properly identified type specimen (I'm all for that), then there has to be a date prior to which this requirement cannot be applied retroactively. Because, as Stafleu and you have told me before: at present it is possible to validly publish a generic name, without the genus possessing a validly published type species. However, the nomenclatural type must be "indicated" (whatever the ICBN means by that).

I'm looking forward to your reply to this.

I heard Norrie's talk in Dublin; she's clever, exciting, and a rare jewel. It must have been great fun to work with her!

Best regards to your better half (ET) and yourself,

Jan Jansonius

JJ/ljh

P.S.

American dollars will be fine; Mexican dollars not accepted.

Hunt Institute for Botanical Documentation

6 September, 1982

Dr. Jan Jansonius
Essor Resources Canada Ltd.
339 50th Ave. SE
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Before I forget—did you pick up that #3811 is misspelled? It should be Alnusalispollenites.

Re Daphniphyllumpollenites: can a pasted-in slip do the trick? How do we know when T.C. glued 'em in? Isn't a separate publication required? I mean there must be "Taiwania 25" around without the slip!

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

P.S. T.C. can't count either—abstract says there are 12 new genera—there are 14 (plus Pterisporis....which reminds one: whose validation counts, T.C.'s or yours?).

Hunt Institute for Botanical Documentation

8 August, 1982

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

We have been gone off and on, and yours of 1 June just bobbed again unexpectedly to the surface on my desk. I am not sure I ever took care of it.

Two "holotype" specimens instead of one = no type specimen, as far as I can tell.

You may be right about the Loquin-Naumova analogy. Certainly I never wanted to be listed as aggressively pro-Naumova. But pre-1958 "content of species" was not required for a new genus. That argument fails (page 2, line 5). Whether Loquin's "rehash of syllabalized...." are better than Naumova's descriptions, is seemingly what is really disputed.

Verbitzkaya 1965, or a reference to it, I do not have. Presumably you have tried some of our Soviet colleagues? S. Meyen answers fastest (faster than Traverse sometimes; that's for sure!).

Hope you are o.k. Where has the summer gone? Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

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A.S. GIBSON
Manager
Exploration Research Division

PALEONTOLOGY GROUP

82 07 05

Dr. A. Traverse
Palynology Labs, Deike Bldg.
Pennsylvania State University
UNIVERSITY PARK, PA., USA 16802

Dear Al:

Thank you for your letters; although dated a week apart, they arrived here together.

I had overlooked the "stat. nov." of Sculptisporis and Tripunctisporis; I agree with you that these do not meet the requirements. Personally, I feel like ignoring this; or mention it as a minor addendum in the next Supplement of the Genera File; it would be a waste of money to re-issue the cards for Sculptisporis and Tripunctisporis, which are still valid in their conclusion.

Pollen Spores, v. 22, p. 493, figs. 23, 24: these may well be the same specimen, but one rotated 120°. As always, with photographs cut out individually, the outlines look quite different (compare figs. 27, 28; or #29, 30, with #30 rotated 120°). All in all sloppy work, not showing the author(s) to be really dedicated to their subject.

Anyway, if these Fgen. are indeed "W. Kr. stat. nov.", they remain typified by the nomenclatural type assigned by Krutzsch; not by the figures given in this publication by Kedves & Herngreen.

I am curious how the next Code will formulate the requirement that a specimen be the type of a genus (the old Genotype of Potonié). I have always recognized that the holotype specimen of a type species de facto carried special weight in the circumscription of the genus: it is the only concrete thing veryfying man-made conclusions. But I also like the subtle elegance of having a species as nomenclatural type of the genus. I really was puzzled why the change was made so suddenly in Sydney (but, of course, in Australia everything is upside down!).

Re CIMP: I have received a yearly contribution from you from 1975 through (and including) 1981. It begins to look like a Canadian dollar does not pull its proper weight! One reason for the words "in their own currency" is, that it is so convenient without incurring (as far as I am aware) extra banking charges either here, or in USA. Basta, with best regards,

Jan Jansonius

JJ/ljh

Hunt Institute for Botanical Documentation

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A.S. GIBSON
Manager
Exploration Research Division

PALEONTOLOGY GROUP

82 06 01

Dr. A. Traverse
Palynology Lab
Deike Building
UNIVERSITY PARK, Pa 16802, USA

Dear Al:

I am cleaning up my desk, and come across your letter of 24.3.82. I want to comment briefly.

By my records you did not yet pay your CIMP 1982 contribution.

Regarding Naumova 1939, I guess there is no clear cut answer to be found. Your gas-tank is half-full, mine is half-empty. The problem with accepting as validly published a name for an ambiguous or poorly defined concept is that someone feels obliged to accept the senior synonym; yet not everyone will share his faith or follow his judgement. If Ibrahim could be precise and correct in 1933, why should we lean backward so far towards Naumova that an impartial observer would say you lacked a back-bone? You asked me to check Art.62(1): line 3 - but that article pertains to legitimate names. And I don't see her names as validly published a priori; if there was no original meaning, how can it be lost?

Did I ask you the question before on the new approach in the Code re the specimen serving as generic type? A genus can be validly proposed without a valid type species being designated. Difficult to swallow, but okay. Now, what happens with a case like ARCHEHANSENIA, with a combined description, but no holotype (or rather: two "holotypes" designated)?

Are you familiar with Locquin's classification for fungal spores? The 1981 Supplement has examples of his names:

3813	Amepiospora
3817	Asyregraamspora
3898	Psiammopomopiospora
3899	Psiamspora
3900	Psidimobipiospora

. . ./2

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All these are abbreviated "diagnostic" sentences, like Linné's early names. Locquin diagrammed, graphically, all potential fungal spore configurations he could think of, and stuck names like the above to each pigeon-hole. Some he also provided with a diagnosis (a fully stated rehash of the syllabelized abbreviated name), and a content of species. Those I have accepted as validly published. But it is a wrong way to make a classification; a form genus is not a genus for a "form", it is for a fragment based on its observed morphology. However that be: essentially, that is also what Naumova did in 1939.

A last question: Do you have a reference to a paper (or names) published in 1965 by Verbitzkaya?

I am wrapping up the 1982 Supplement, and hope your answer to the last question may reach me in a few weeks.

Best regards, best wishes for a pleasant summer to both of you,

Jan Jansonius

JJ/ljh

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ARCHEHANSENIA Locquin 1981

Trav. Lab. Micropaléontol., Univ. P.-et-M.-Curie, No. 9, p. 215

Type species: Archehansenia pachycingula Locquin, ibid., pl. I, fig. A, B*

Diagnosis (combined description):
"Body elliptical to slightly almond-shaped, asymmetrically ringed by a cingulum with a thickened ridge. Occasionally the cellules are encased in what looks like a diagenetic mucilage. The surface is smooth. Average size: 1 x 0.6 µ."

Recent analogue: hemisaturnian yeasts.

Permo-Triassic; Madagascar

*Remarks: Locquin (ibid.), under Notes importantes, states that the holotype of each species in this paper is indicated by a white triangle on the SEM photographs. For A. pachycingula, both photographs A and B have a marked specimen. Thus, until someone selects one of these two, the species itself is not validly published. (Inasmuch as the ICBN - Sydney edition - apparently will make the holotype of the type species also the nomenclatural type of the generic name, the status of Archehansenia is not comfortably secure. JJ.)

JIAOHEPOLLIS Li in Wang Cong-feng & Li Wen-ben 1981

Xerox pix of p. 533

Acta Palaeontologica Sinica, v. 20, p. 533 (536 Engl. abstr.)

Type species Jiaohepollis verrucata Li, ibid., pl. 3, fig. 6 ↔ { Verbitzkaya 1965, pl. 7, fig. 126 Podozamites macropunctatus: quoted as synonym in

if the quoted figure is the 1965 holotype, its name is a sr. syn., and should be adopted.

I have no reference to Verbitzkaya 1965: do you know any?

Diagnosis: "Monosaccate pollen grains. Saccus finely infrareticulate, raised from the distal face of the body. Aperture indistinct, a faint leptoma traceable in the centre of distal face."

The distal displacement of the saccus bases is not found in any other monosaccate pollen (although overall aspect may resemble that of Callialasporites)

Monotypic.

55-82 µ (holotype 80 µ), central body 54-73 µ (60 µ) in diameter

Description of type species: monosaccate pollen, alete; central body distinct, with circular amb, proximal face markedly arcuate, distal face flattened, no tetrad mark visible; exine solid, scabrate or infrapunctate;

↳ Saccus attached distally, ring-shaped, 12.5 - 27.5 µ wide in polar view, finely infrareticulate, frequently folded.

Lower Cretaceous; P.R. of China.

Hunt Institute for Botanical Documentation

21 June, 1982

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

More later on the substance of your recent letter--it crossed with mine of 14 June which, as you will have noticed, touches on the same specimens-as-generic-type question that you raised.

However, this letter is re CIMP dues. How much do I currently owe? Could you let me know if I ever paid 1981 dues and if so how much? (My tax records don't show it.)

Are you weakening on the "local dollar" idea? What if $Can. goes to $US 0.50?

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

14 June, 1982

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. SE
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

What do you think of Kedves & Herngreen's "Fgen. Schlptisporis... stat. nov."? (Pollen et Spores 22:492). This is obviously one of several' they intended to raise from subgenera to genera, but they haven't done enough, huh? Are figs. 23-24 supposed to be the same specimen?--don't seem so, but have same coordinates. (In the new Code, a generic name is typified by a specimen) There must be a validly published type-species. There is none here--no description, but it will be worth checking the new language from Sydney later--we might well be back to validly published genera with no species validly published--if the specimen is o.k. How about "Tripunctisporis stat. nov."?

Doesn't Pollen et Spores have a referee who knows anything about systematics? $250/yr. deserves better editing!

See you. Best as ever.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

24 March, 1982

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

I always feel bad when I make too much trouble for somebody. It wasn't worth an in-depth analysis of why Canadians pay 82¢ and USers $1.00--"The days are not long past...the Canadian dollar was US$1.10", however, does some violence to "not long", but doubtless you have observed that is a function of increasing age! Anyway, do I still owe for 1982, as the chart seems to say? I'd like to be current?

The same letter mentions "...a long epistle I sent you not too long ago discussing Naumova 1939". That letter was received here as mostly in response to my nit-picks on the cards. I guess I didn't respond about the Naumova business. You say that N. 1939 was a paper in which a system was proposed. Granted. But that does not necessarily make names proposed therein not validly published. That question must be decided otherwise. Your long historical analysis of Naumova's work and her motives is not necessarily relevant to throwing out all the names proposed in 1939, because they clearly are not always merely "incidentally mentioned" per Art. 34. Each one must be examined individually, inasmuch as N. 39 is not a seed catalog, or an autograph privately distributed, or any of those things. My personal philosophy has trended more and more in the direction of accepting nearly everything as validly published, if that interpretation is marginally possible. It does less mischief than having a twilight zone of names which some accept and others don't. If you possibly can, accept the marginal Naumova names as validly published, and then, if you prefer, avoid use of them, except to avoid homonymy. Do you follow the philosophy? (I even would like to accept Pollenites, but still can't go that far.) "Some never were adopted"--what do you mean to imply?--please read Art. 62.1, line 3. "Intention", by the way, is a very sticky wicket in both religionsand nomenclature. You must judge N. 39 names solely by the requirements of the Code. Did she marginally meet the requirements? Even if "by accident"? (All sorts of names have been validly published by accident--I wonder if "Hamipollenites Wilson in Jansonius 1961" was such a case?--haven't checked.)

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

17 March, 1982

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Re yours of 2-III-82: thanks for the sympathy--it helps. It was and is a dreadful experience for us.

Apparently you do have CFSP 28. If you do, you already have the pertinent originals of Malyavkina, 1958, as they are presented in photocopy in the 3-page treatment per 28:52. The stuff you sent in French translation is the material presented in CFSP 28:52, p. 1. You should also see the stuff on CFSP 28:52, p. 2 (ggneric description). (It was about volume 28 that I threw several temper tantrums to get Spackman to agree to stopping the idiocy of "28:52" not meaning vol. 28, p. 52. Ames never did like m y dictum of "one page = one page".)

Well, I have now re-read the passages (I also have the book itself--i.e. Mal. 1958), as I can read Russian (but not French). Malyavkina certainly did some confusing things! You may have a case re alternate names here, but let's consider another possibility . Why doesn't Malyavkina have a right to take a part of her grab-bag genus, Spirellina, that part she says resembles Cedrus, and make a new genus, Abieticedripites, from that part of Spirellina? That is certainly a possible interpretation of what she has done--it was mine. If someone, for example, wrote about Quercus and said he is now making a new genus based on Q. alba var. stricta: "Jansonipites" with type-species Jansonipites stricta, the fact that he slipped somewhere and mentioned "Quercus stricta" wouldn't invalidate, though it would confuse. Right? Please read CFSP 28:52, p. 2. She says "Spirellina (pars)".

The best thing to do with many (most?) of Malyavkina's genera is to recognize them as legit. and watch out for homonyms, but otherwise ignore them. In a few instances what she meant (Corollina?)is so clear that she must be followed.

I would have thought that the type species of A. compacta is typified by the type of A. compacta var. compacta, nbtvar. mongolica, even though var. compacta is called "f. typica".

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The new "Sydney" rule re specimens as generic types has ±no practical consequences that I foresee, because the provision says that it is acceptable to continue to cite only the species itself. Ultimately, however, the specimen (or its illustration for us?) must be available, so I would think yes, it must be somehow designated in print. Species since 1958, have required types, of course.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

ESSO RESOURCES CANADA LIMITED

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March 2, 1982

Dr. A. Traverse
Deike Building 435
University Park 16802
Pennsylvania
U. S. A.

Dear Al,

Just a quick dash to see if I can "uncross" our correspondence.

We are sorry to hear about the burglary; not only is the invasion of your privacy most offending, but missing things that have been part of the family for a long time must feel like an amputation. I don't think either that it has to do with materialism; it is the memories attached to them, why you are happy to have the responsibility of their safe-keeping. We are truly sorry - you didn't deserve that.

The "xerox copies of Malyavkina" are from a French translation that was for sale around 1965. I sent them to you because of the "alternative names" included in the text of the description of the type species (p. 92):

"Comparisons and observations: Pollen of the described variety in the artificial classification (my double underline. JJ) of the author is referred to the type Spirellina, to the sub-type Abieticedrispirellina and in structure of the pollen grain it is close to Spirellina simplicissima β - compacta Mal.

Seeing as how it is inconvenient to designate the pollen as a subvariety, the name of the pollen Spirellina simplicissima β - compacta must be modified by isolating the variety β as a separate species Spirellina compacta Mal.; the rationale for such modification is that Spirellina simplicissima f. typica Mal. is closer to the pollen of Abies and Spirellina compacta Mal. is a bit closer to the pollen of Cedrus, which is shown already in the description of the Mongolian variety."

Sorry, it is on the shelf, I after all.

We do not have the original Malyavkina 1958, so I can't easily check all things (we must borrow it somewhere, and copy it). However, to me this text reads as though Malyavkina juggled two simultaneous systems

.....2

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of classification, a "natural" system (Abieticedripites) and an "artificial" one (Spirellina). By assigning the species compacta to both genera in the same context, I am afraid that she invalidated both. It is on this aspect I would like to have your advice. Spirellina simplicissima β - compacta was published as such in Malyavkina 1949; however, "Spirellina compacta Mal." seems to appear first together with "Abieticedripites compacta".

I hope I was correct when I made A. compacta mongolica the type of the genus; I now realize that a genus can be validly published, even if its nomenclatural type is not. What is the impact of the new Sydney directive, where the type of a genus is the specimen: does that not imply that it must have been designated in print? (at least after 1958?)

Bye for now,

With best wishes,

Jan Jansonius

JJ/ap

Hunt Institute for Botanical Documentation

Dear Al:

Below is a copy of my book on paid-up-ness; I put you down for 1981 on this last payment, which avoids a hiatus. Payments can be made in all manners, so the record below is just showing my receipts in Calgary. If you can check the last few years, maybe you can show that I missed recording one of your payments. I may have done last year what I did now, i.e. put your payment down for the last unpaid year, not for the year in which it was received.

You are right that Canadians have it easy, the last few years; however, the days are not long past (if maybe gone forever) that a CAN $ was worth US $ 1.10. For that matter, the Belgians come off easier than the Dutch or Germans. Who cares, if no one has to sacrifice to support this thing. You'll realize that many of

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G Taylor				ø								
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Hunt Institute for Botanical Documentation

24 February, 1982

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Many thanks for your thorough (as usual) letter of 2 Feb. I often wonder if you keep a copy, as you do not go through a steno pool. Your hand is eminently legible, as if produced by some sort of machine. Thank God, you don't see mine, as exigencies of weariness, hunger, frustration, anger, etc., affect the quality enormously.

Romainipollenites was probably a typist's error (ahem) but as she (EIT) doesn't keep my originals, we'll have to fight about that! However, "Linnaeus' pensylvanica" (instead of pennsylvanica) was what I meant. It is the usual example given of a spelling "error" that must be retained, not corrected (example: Acer pensylvanica L.). However, you are probably right about "tusiformis"--your case would be stronger if you could cite correspondence with the author. She has a right to tusiformis if she wants it. The Lyubomirova case shows one problem with transliteration about which there was great debate at the Leningrad Congress: does an author have to be followed in his/her transliteration? Khlonova personally insists on "Chlonova" ("like Christ", as she says). The Chinese are another case in point--whole slews of them have been changed recently (Chiang Te-Chin = Jiang Dexin, etc.) by PRC fiat. Actually, as far as I am concerned, science could refuse to go along. The Chinese names didn't change, and who can dictate how non-Chinese shall transliterate the names into Latin letters? However, this is a case where I do approve some sort of transliteration, as Chinese characters are not phonetic in principal. But Cyrillic (two ls) should not, in my opinion, be transliterated. (Hebrew, Arabic...ye Gods).

You misunderstood my point about #3830, Cuneisporites Zhang 1980. My point was that there is also a Cuneisporites (abbrev. "C") Ravn, #3671, and that you usually note such homonymy.

I aggree with you re Dicolporopollis--unless someone else has already done it.

Just checked Chlonovoia. Yes, I guess I'd now go along with the reference being marginal but acceptable. I wonder if that isn't the very case I ran past Stafleu?

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the east block [redacted] from South America [redacted] etc., are simply carried by those who can freely dispose of hard valuta.

You didn't mention a long epistle I sent you not too long ago discussing Navmova 1939 — I hope you have received it in the mean time?

Best regards,

John L.

Hunt Institute for Botanical Documentation

#3870--let's see, that was...Lophozonotriletes (I need a numerical list). No, IIthink "amends" is not correctly used. It is a different English word, as I said, and it refers only to compensation for loss or injury.

Nice to be reminded of the Rijksmuseum. We were there just before the "Night-watch" was slashed.

No, I never told you (I hope) that there are no conserved names for fossil plants (see pp. 411-412 in the Leningrad Code). You are right that I tend to oppose conservation of fossil plant names. In fact, conservation is one of the chicken ways the botanists avoid having a commission to rule on problems. No, you do not need to make amends (see above)!

I've had my 4IPC Proceedings for quite a while. But God knows what the real date of publication is. Maybe the Canadian postal system is at fault for your non-delivery?

Best regards.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

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A.S. GIBSON
Manager
Exploration Research Division

Dr. A. Traverse
Penn-State Univ.

2 Febr. 1982

Dear Al,

I was happy to receive your letter of 22 Jan., discussing errors in the Genera file. It's unfair in nature, that you never notice your own mistakes, but can't help but spot those in the work of another (I trust you meant Romeinipolleniter; and Linnaeus' "sylvatica" rather than "pensylvanica").

However, since my ideal always was to eliminate as much as possible all gross and piddling errors, I am glad with the close scrutiny that you give the File and the updates. Please keep it coming!

With regard to transliteration versus translation of Cyrilic / Russian / Ukrainian text, I probably shall maintain my old practice, more to appear consistent rather than from a deep conviction. But, indeed, it should be done in a consistent manner, and thus it should be Lyubomirova rather than Lubomirova (as in the author's own usage, which I copied without checking).

I didn't give Ravn's initials, because I was (and still am) unaware by paleopalynological literature by C. Ravn.

On Dicolporopollis: I agree with your opinion on P. middendorfii (as stated also on # 3745 POLLENITES). I shall give a correction, indicating that line 3 should read: "Pot. ex Jans. & Hills 1981" for "(Pot.) n. comb.", because we effectively, if unwittingly, validated the species. Do you agree with that?

Our Palaeobotanist v. 26 is out for binding, so I can't check your point on adequacy of bibliographic citation for his n. comb. . However,

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your leniency and charity were a pleasant surprise to me. Have you also changed your mind on Chlonovaia and do you now agree with our treatment of the validity of the typespecies n. comb. in 1975 (Elsik, p.528, last 17 lines)?

I hope you agree with my use of "amends" on # 3870; it was intentional.

You are correct that Pseudochordasporites fusiformis was printed consistently as "tusiformis"; that last word was also used, mistyped, in the description of the species (Zhang, p.4, l.7). I think that in Art. 73.1, and in the Examples of typographic errors (Globba brachycarpa → Globba trachycarpa; H. alba → H. alta) I am given authority to correct this obvious typo. (Or do you think that this spore has a characteristic tusiform outline?).

On "Riksmuseum": the Swedish is indeed "Riksmuseet"; Guy-Ohlson wrote in her letter about the "Riksmuseum" (which I copied uncritically); Van Landingham (his name suggests a Dutch ancestry!) on p.173 states to have deposited his types in the "Rijksmuseum" (which is in Amsterdam, housing Rembrandt's "Nachtwacht").

In the matter of the "Nomina rejicienda", I seem to remember that I read about the rejection of a proposal to establish such a category. But for the life of me I can't remember in what context. Also, didn't you tell me that so far there is not one conserved name for fossil plants, and we want to keep it that way? I think, I'll just let this thing die, unless you want me to make amends.

Under Triporina the matter of Nawmova 1939 comes up; I do not accept that paper as a work containing a large Systematics section, but rather as one in which a system for classification is proposed. That this system is partly based on elementary knowledge (or advanced knowledge) of spores does not alter the fact that her paper sets out one possible system of grouping and naming dispersed spores. In these early tentative years of palynology, guide lines for a system of making

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the naming of spores more orderly were seen as a desirable thing (witness Ibrahim, Potonié, Erdtman, Pant, Leschik, the Lille group). However, Nawmova for instants gives Zonotriletes Valts (which I accept as validly published genus name as of Luber & Valts, 1938) subdivided in 10 "subgroups", which include Leiotriletes, Acanthotriletes, etc. These latter names subsequently have been recruited as names for formgenera, in papers by Nawmova or others, often in the sense originally intended, but others in quite a different sense. And some never were adopted. Frinstants: in 1939

Eurytzonotriletes: to me looks like a monosaccate pollen (cf. Nuskoisporites), but Ischenko validated and used the name for densospores.

Trematozonotriletes: looks like a kind of Densosporites, but was never adopted.

Chomotriletes: a trilete spore with concentric ribs is pictured (cf. Cicatricosisporites?, Contiguisporites?), but the name now is that properly given to algal spores or cysts with concentric banding and no trilete mark.

Triporina: figures as a slot for obvious angiosperm pollen (cf. Tilia), but wound up as a name for algal cysts(?) of Late Paleozoic age.

Then, in a following section of her paper, she discusses characteristic assemblages for some broad stratigraphic intervals. Here, she uses a number of binomial designations, most of them utilizing the older Luber & Valts generic names ("Zonotriletes explanatus Nub.") which earlier she used as name for a "group" in her proposed system. Elsewhere, she does give "Entylissa cuspidata Nawm.", "Monoptycha retroflexus (Lub.) Nawm.", but these are in my opinion Art. 34.1 (b) instances of provisional names and not to be construed to validate the exposition of names first announced to the world in this paper (but not yet validly published). Too bad for her that the war interrupted this work. Still, one wonders why (more than a more) no mention was made of Ibrahim's work, or a reference to Knox given. It is clear that, in (partly self-imposed) isolation, Nawmova had developed a workable nomenclature in MS, and in 1939 announced the principles on which it was based.

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Well, I guess that brings us to the end of your list; as I said earlier, Palaeobotanist v.26 is at the binders, but I will double check that date. I must, however, trust their "official" (not their "nominal") date of publication, which always is stated in that Journal. Birbal Sahni is very slow in mailing; we never yet received the Proceedings of 4. IPC., although various reprints are dated 1978! What can one do? – they probably just can't pay for postage. But is that grounds for considering the Proceedings not yet effectively published?

It was nice to have another chat with you, and I thank you once again for your close attention to our file. I am a bit discouraged that you could pick up so many things on top of a number that I noticed myself (when it was too late!). Such as on card 3917, l.3: read "selected by Jansonius & Hills 1976 (card 2877)" and Cuneisporites being a jr. homon., or the earlier handwritten correction onto card 637, which was left off card 3546. As this thing keeps growing in all directions, it takes more of an effort to control it. I shall endeavour to do better with this years update, for which I have some 40–50 new cards already.

Best regards to your charming typist and yourself,

Jan

Hunt Institute for Botanical Documentation

22 January, 1982

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Just got through the current supplements. Either my eagle eye is failing, or your editing is getting better--it was hardly any "fun" at all this time! However, I submit the following for your consideration:

[First, miscellanaous corrections not in the Supplement: #1518: para. 7, l. 3. Did I already mention misspelling of Lorisporites? #3410. How about, last line, m isspelling of tenuifolia?]

3813. Amepiospora, l. 15: priorities

3818. Bifurcatisporites Inosova. Once again, I object to transliteration, except for a few words such as "Trudy", which are better not translated. Either translate or--best--give it in Cyrillic (with modern typewriters this is no problem). Please note that the Soviets don't transliterate Latin alphabet into Cyrillic!

3830. How come the homonymy with C. Ravn isn't mentioned? I mean, you often make such a note.

3835. Dicolporopollis. Well, here's an illustration of difference of opinion. In my view P. middendorfii was not validly published in 1931, and this view would change the treatment.

3842. Echinodiporis. l. 18: occasionally

3853. Glomeratia. Names of authors are an exception to the non-transliteration policy I would advocate. However, if you do transliterate, you have to get it right. This party's name is: _____, thus Lyubomirova (or Liu.... in some schemes, but not Lu....!)

3855. Granustephanocolpites. I see your point, but I accept the transfer of the type species by Saxena because it does say in the "Remarks" that it is from Venkatachala & Kar, 1969, and the page numbers are given in a perfectly unequivocal way in the bibliography. I found the intended original in 90 seconds. Stafleu and I corresponded about such cases long ago, and he said that Art. 33 dasn't intended to catch that sort of case--if the referenee is obvious, it should be accepted. This one is: in my opinion, the bibliographic citation is clear and direct.

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3870. Lophozonotriletes, col. 1, last line: "amends" is not an abbreviation for amendments, but a different word altogether.

3895. Pseudochordasporites. How do you get "fusiformis"? It's t tusiformis, as far as I can find out. Recall Linnaeus' "error","pensylvanica"?

3908. Romeinpollenites: next to last line, "...cut out their specimens....". Surely you meant "photos"?

3915. Staplinites: last line & 1. 15. "Rijksmuseum" is a hybrid. It's "Rijksmuseet" (I believe) or Royal Museum.

3919. Tetraporina, 6th line from bottom: nomina rejicienda are listed, as against nomina conservanda, that is--this is often referred to as the "list of rejected names".

3924. Triporina. Maybe I need help again to understand why Triporina was not validly published in 1939. (Remember, you accept Pollenites.) It seems irregular but acceptable to me;(lack of species certainly doesn't matter, especially as her use of binomials in some cases makes clear that the units were at the generic level).

3925. Trisyncolpites. Are you sure of "1979"? I had a note to myself that my copy of the journal was delivered here in late September, 1980.

Yegad! If I can't nit-pick better than that, it really worries me. Congratulations on a good job!

Yours as ever,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

15 February, 1982

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Yours of 25 January is the sort of thing that makes me wonder about ESP. Probably you have now received my letter.

I am writing while still in a state of shock: our home was recently burglarized, and we sustained grievous, irreparable losses of heirlooms--it's a terrible feeling.

Ken Hsü is a professor at the Geological Institute, E.T.H., Zürich. Has been there for perhaps 20 years. When I was there he was Director (Trümpy has now succeeded him). We are very close friends from Shell days in Houston. Through him Betty and I have acquired a great love of Chinese cuisine--but otherwise we are not really "China-hands".

Your enclosure puzzles me. It's not, as you say, "xerox copies of Malyavkina". It seems to be a copy of some sort of abbreviated and rearranged translation of Mal. 1958 into a language I read poorly. It may surprise you to know that I am not an apologist or whatever for Malyavkina. I do say that we should apply the same standards to her work as to anybody else's. The infraspecific taxa (all infraspecific taxa of palynomorphs?) aren't of any value, of course. Only a (misguided?) desire for completeness.persuaded us to include them. The var. mongalica is best ignored. What does matter here is that Abieticedripites seems to have been validly published because it's monotypic. It is quite clear that the whole thing is based on the former Spirellina simplicissima β-compacta. Whether that, in turn, is any good is a taxonomic question. I doubt it. But the nomenclatural situation is quite clear, and Abieticedripites and A. compacta as names that must be considered in synonymies, etc., seem to me to be validly published.

Good to hear you're o.k. and working hard. All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

8 February, 1982

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Recent notice from B. Owens re my CIMP dues elicits enclosed response. Once again I am reminded that it's not equitable to charge some people $6.00 and others $4.00 by the clever subterfuge of "IN THEIR LOCAL CURRENCY". If the fee is x, it should be x for all However, I appreciate your good offices in the troublesome job of collecting these monies. No pleasure for you, I am sure.

By the way, do you keep track of who has and hasn't paid? My form said I was paid only through 1980, but I was pretty sure I had paid for 1981.

Best wishes as always.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: CIMP dues (1982?)

P.S. The version of "CIMP" you wanted on the checks is too long to go on the blank.

Hunt Institute for Botanical Documentation

Dear Al, 1981.01.25

It strikes me that we have not exchanged letters for a long time. Is it too late to wish Beth and you a prosperous and succesful new year?

Some time ago, I made the enclosed xerox copies of Malyavkina – would you care to comment on my annotation in the margin?

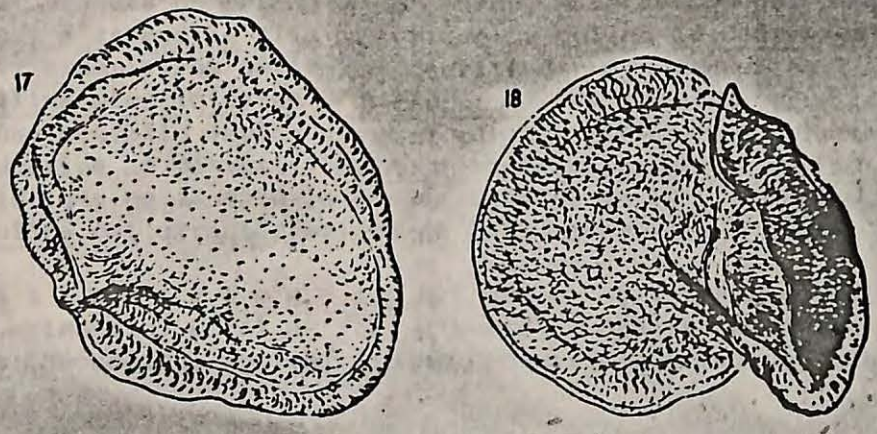
Was Hsü in Zürich on a temporary basis, or there to stay? I just finished reading John Fraser's "The Chinese. Portrait of a people". He was the Toronto Globe + Mail's Peking correspondent 1977-1979, just after the thaw following the Cultural Revolution. It is a fascinating book, informative, but also using China as a mirror for ourselves, and vice-versa.

I have been working on Mesozoic palynology, but got bogged down a bit in service work on rather poor samples. I hope to pick up steam with the promise of a new season, and do some real work that will increase my understanding of what went on in dino evolution, etc.

As there is no more "real" news, I end with best regards,

Jan

Hunt Institute for Botanical Documentation



Pl. XI, fig. 17-18 - Abieticedripites compacta Mal. var. mongolica var n.p. 103.
17. Holotype; 18. Paratype. Région de Dzunbaj.

Genre ABIETICEDRIPITES gen. n.

1949. Spirellina (pars) MALJAVKINA. Manuel de détermination des spores et pollen, p. 112, pl. 41, fig. 1-4.

D e s c r i p t i o n d u g e n r e. Dimensions : diamètre longitudinal de 50 à 150 µ, diamètre transversal de 25-35 à 75-90 µ, rapport longueur largeur en vue latérale 2:1, en vue distalo-proximale 1:1 environ. Contour du pollen en arc concave en vue latérale et arrondie ou ovale en vue distalo-proximale; corps grand fermé ou semi-fermé, convexe; sacs aérifères gros, en position distalo-proximale ils recouvrent entièrement le corps; la partie du corps comprise entre les sacs aérifères est de largeur moyenne ou étroite, les zones d'insertion sont grandes en demi-lune, pas toujours bien développées;l'exine du bord des ballonnets se transforme progressivement en bourrelets sur le côté dorsal du corps; en position latérale elle forme une crête et en position distalo-proximale un liseré, avec de petites rides radiales. La distance proximale (dorsale) est petite : de 1/5 à 1/8 du corps. La distance distale (ventrale) est un peu supérieure à la moitié du corps. Les alvéoles de la muraille sont de dimensions moyennes*.

V a r i a b i l i t é. Pollen monotype, les grains de pollen diffèrent par leurs dimensions. On connaît trois espèces et plusieurs variétés provenant des dépôts crétacés.

*Cette phrase a été traduite littéralement, le sens exact nous en échappe.(N.d.C.)

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Comparaisons et remarques. En vue latérale les grains de pollen sont proches du pollen de Cedrus, mais de dimensions plus grosses. Le pollen ayant l'aspect le plus typique du pollen de Cedrus est Spirellina compacta Mal., exemplaires petits, décrits dans le manuel de l'auteur sous le nom de Spirellina simplicissima β-compacta sp. et var. n.; ce pollen doit être classé dans une nouvelle espèce ayant plusieurs variétés, chacune caractérisée par son domaine de répartition. Abieticedripites compacta f. typica Mal. est caractéristique de la Sibérie occidentale et de la Cisouralie, A. compacta var. mongolica est caractéristique de la Mongolie.

Répartition géographique et âge. Cisouralie occidentale et orientale, Oural du Sud, Sibérie, Mongolie. Essentiellement dépôts crétacés (Néocomien, Aptien, Albien-Cénomanien) rarement Jurassique supérieur.

***

Hunt Institute for Botanical Documentation

Abieticedripites compacta Mal. var. mongolica var. n.

Pl. XI, fig. 17-18.

Holotype. Préparation n° $\frac{2275 - 1 - 4}{V}$; Paratype : préparation n° $\frac{2186 - 1 - 4}{V}$, conservées au Laboratoire de Paléobotanique de VNIGRI.

Gisement de l'holotype. Mongolie orientale, région de Dzunbaj : Crétacé inférieur (Néocomien), sub-formation gris-verdâtre de la formation de Dzunbaj.

Description de la variété. Dimensions : diamètre longitudinal de 60 à 100 µ, diamètre transversal de 40 à 65 µ ; rapport longueur largeur en position latérale 2:1, en position distalo-proximale environ 1:1. Contour du grain de pollen en vue latérale arqué-concave; en position distalo-proximale arrondi-anguleux; corps grand, fermé ou semi-fermé; en position distalo-proximale les ballonnets recouvrent presque entièrement le corps. La couleur du grain de pollen est jaune ou jaune-foncé. Les grains de pollen vus dans les préparations en position latérale sont ridés. Nombre d'exemplaires ayant servi à la description de la variété, 100.

Variabilité. Grains de pollen peu variables, essentiellement ce sont les dimensions qui varient; la couleur de l'exine aussi change un peu de jaune au jaune-foncé.

Is this an alternative name in the sense of Art. 34.4 ?

Comparaisons et observations. Le pollen de la variété décrite, dans la classification artificielle de l'auteur est rapporté au type Spirellina, au sous-type Abieticedrispirellina et par la structure du grain de pollen il est proche de Spirellina simplicissima β-compacta Mal.

Etant donné qu'il est mal commode de désigner le pollen comme une subvariété le nom du pollen Spirellina simplicissima β-compacta doit être modifié en isolant une variété β en une espèce particulière Spirellina compacta Mal.; la raison d'une telle modification est que Spirellina simplicissima f. typica Mal. est plus proche du pollen Abies et que Spirellina compacta Mal. est un peu plus proche du pollen Cedrus, comme d'ailleurs la variété mongolienne décrite.

Répartition géographique et âge. Mongolie orientale : Crétacé inférieur (Hauterivien, Barrémien, peut-être Valanginien et aussi Aptien-Albien).

* * *

No matter how you read these sentences, they do not make much sense.

For that matter, perusing CFSP 28:52 does not make me any wiser, either.

Hunt Institute for Botanical Documentation

26 August, 1981
(dictated July, '81, but held
until end of mail strike and/
or AT's return from IBC-Sydney)

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Got yours (undated--ca. 1 July?) when I got back from Europe the other day. Went back to recuperate from my two May operations--appendicitis and complications. Also got a pack of work done!

Glad you can continue on our Committee, though your absence at Sydney will be a handicap (lots of folks will also be absent!).

I agree with you re the slugs of proposed amendments--it is sickening. I spent 2 whole days working on my mail ballot. Clearly the problem is that botanists do not have a Commission, and therefore try to take care of every little problem with yet another clarification (expansion usually) of the Code. The zoologists have just a little bitty code which they change little, and do most of their business with case law, running to many volumes. It's heresy for a botanist, but I am currently convinced that it's really a better idea, by analogy, to go with statutes and case law rather than endlessly patching up a complicated constitution. The botanists are not really practicing sophistry--they just operate from a faulty assumption.

Betty does not go to Australia. However, she is at our summer home in the Adirondacks with her aged and rather infirm parents. Last Sunday she walked 20 miles, she says! (Or was it 20 km.?)*

Can't you get ol' Frank S. under control? That letter of his in RPP (I think that's where I read it) about the desperate plight of palynology from too much nomenclatural monkey-business was a crock! At worst such juggling isn't helpful, but it does no particular harm, does it? Surely you practical guys just ignore such work--blissfully? Does F.S. really understand what a type is supposed to do? (Lots of organisms can have only photos or other figures.) The formal nomenclature associated with fossil spores doesn't particularly help or harm practical palynostratigraphy, does it? Gotta dash. All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

continued--

Hunt Institute for Botanical Documentation

*(notes from the typist:

1. That particular hike was closer to 20 km. than to 20 miles--had to cut it short in order to be back in time for dinner.

2. Speaking of walks, I'm sure you remember Isolde Blersch, our very pleasant Swiss companion on our country walk the last evening in Cambridge last summer. We saw her again in Basel (Al lectured there to the local Botanical Society)--she sends her regards.

AT/et

Hunt Institute for Botanical Documentation

16 January, 1981

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Just got yours of 5 January. (From now on, you can leave off "cØo Hsü"--we--AT/et--are now famous--notorious here!)

I am glad that you receive my suggestions/etc. with more or less interest. I am very interested in the whole project, of course.

Unfortunately, I have none of my files or books here. For example, what is a "calaphon"? I can't remember the names I looked up that would be in trouble if "Vesicula" is. One that comes to mind is your dear "Pollenites" --and be careful--if you say it's not a morphological term, you're in some trouble. How about Triletes? or Lagenicula? or Monoletes? or Tricolpites? I believe the Code meant to exclude things like Radicula, that occur in the Latin (!) diagnosis. I admit it's arguable.

About the Chinese names, I'm wild--they should have made an exception of scholars, including scientists, whose names occur in bibliographies. It is crazy that my friend and colleague-to-be at PSU, Chiang Te-Chin is now Jiang Dexin--puts her in two places in a bibliography. The concept of nomenclatural stability should include human! (My host here, for example, is now supposed to be "Xu". But there should be a grandfather's clause to exclude only for scientific purposes those who have published under an older transliteration!)

It is a blow that you won't make it to Sydney--of course, Betty and I can't figure out how on earth I could pay my own way, either--and it might be necessary. The only hope of some financial help I have is $500 from Bot. Soc.'s NSF grant--a drop in the bucket. We need your help to get Art. 3.2 straightened out, and your help would have been of great assistance. It is also frustrating for me that if I do get to Sydney I surely can't go on one of the marvellous field trips.

Best wishes to you, old friend.

Yours very truly,

Alfred Traverse
Visiting Professor

AT/et
P.S. We did order another set of the cards. On of these days I'll need a third set.

Hunt Institute for Botanical Documentation

5 Jan 1981

Dear Al,

I spent the holiday season preparing the next Supplement to the Genera File. At times I feel like a modern Sysyphus continuously struggling to rok uphill the millstone that is like an albatross around my neck. (Wish I could mix in another metaphore yet !).

You sent some remarks on previous cards in a letter dated 22 August. I'll answer some of your points:

- 1726 Multimarginites is the spelling used throughout that paper, but in the heading introducing the new genus the name is given as Multimarginatus. I consider the latter to be a typo.
- You are right, I think, in wanting to cite the author-name of a genus when using the autonymic subgenus name.
- Indeed, any new combinations, new names etc. proposed in the Genera File should be attributed to J. & H.
- 3450 Bikolisporites: the date of 1977 is given in the Colophon.
- 3473 Inderites. All cards on this name pertain to the same thing. It was very difficult to get the necessary literature. If I had known in the beginning that it (likely) is a Tasmanites, I would never have included it in the File.
- Transliterations: it soon is clear whether a Russian title is given in the German, French, or English standard system of transliteration. (For an English speaker it may seem strange that in other languages the alphabet has also a phonetic meaning). 3484 Ornatisporites is not transliterated, but direct from the Polish.
- Redundant names are included in the File because in many cases it took me much time to find out that they were not validly published. That in itself is useful information.
- A list of numbers plus names of the successive updates was a necessity for me to read your letter. I shall include that list in the next update!

Hunt Institute for Botanical Documentation

- Translation of Band, Heft, Jahrgang into English often is ambiguous. Heft = number, issue, part. But part = Heft, Teil. Maybe all this looks a bit messy, but I try not to confuse librarians. Maybe I should be more careful (I am too aware that these cards usually are looked at individually, and that over the years standardisation has gotten out of practical reach); but always I try to be as informative as possible. That is why I kept on puzzling on what kind of information a book seller might want to have to fill an order of the Chinese book.
- I will use "lines up" more often in the future.
- Nowhere does the ICBN say that technical terms are those listed in Bold or Jackson. But if saccate pollen are those having vesiculae, then I consider that word a technical term. You said you knew other names in this category; if you'll send them to me, I'll look into them.
- 3553 Fushunpollis is [in Chinese], not [in Russian].
- New cards may be issued for several reasons, usually for addition of a new emendation, occasionally to correct erroneous statements. Often I utilise such an opportunity to also upgrade the earlier text and or illustration. I'll talk over with Len Hills your idea of identifying such "replacement cards" or "expanded cards"; usually a check of the date of issue will explain what is going on. It might be useful to include a reference to the old number.
- 3564 Lagenicula: I did include on that card all the explanation of the term "hologula" that Piérart cared to give.
- 3587 Pleosporites. Its structure is similar to other microthyriaceous forms included in the File. I thought this one should be included for comparison.
  Regarding Propythium (a fossil fungus found in a bryozoan = endobryozoan!): this is similar to Peronosporites and Rhizophagites, and should be compared with those. All these things are found as "Spore dispersae".

Hunt Institute for Botanical Documentation

- 2072 Polyadites. I rechecked this and still think that it was not validly published in 1954 b. What is your reasoning for thinking otherwise?
- Regarding Chinese transliterations: earlier systems (a.o. "Wade", "Yale") now should give way to the official Peking system (in which q is pronounced as "ch", x = "sh", etc.); too bad that the superscript tonal indicators are left off in our newspapers, etc. Anyway, Li is the older Lee; but at least one should maintain cross-references under Lee.
- 3612, 3502 Scabiosapollis: these two were issued in subsequent years.

—×—

I didn't go into every detail that you touched upon, but I thought I owed you at least this much. I greatly appreciate your detailed comments (wished that more people would bother!) and hope you will continue to give them.

I think I will not go to Sidney. There is no way that Esso will pay for me after Cambridge (Frank and Stanley are higher up the totempole than yours truly) and I can't see that I would get out of that meeting, or be able to contribute, enough to justify my spending that much of my own taxed dollars. A neighbour just showed his slides of Australia + Sydney, and it is a delightful place.

I hope you enjoy your stay in Germany and Switzerland, and that you had a gemütliche Weihnachten. Best regards to both of you

from "yours truly"

John.

P.S.

Hunt Institute for Botanical Documentation

did I send you earlier this replacement card which you requested? ↓

STYX Norton & Hall 1967 (card 2)

2794

Bergad 1973 (Micropaleontology, V.19, p.56) assigns Styx minor, i.e. the type species of Styx, to Balmeisporites longirimosus Kondinskaya 1966; this makes Styx a junior synonym of Balmeisporites.

Bergad (l.c, p.57) also re-investigated the type material of S. minor, and redescribes it:

> 39 - 77μ (largest body diameter), spines varying from smooth to fimbriate; perine not two -, but single layered, surface smooth to finely reticulate; the muri caused by uptolding of the perine, which may lead one to believe that it has two layers; the 'verrucae' noted on some specimens are in reality the bases of muri viewed end on.

Bergad also (l.c., p.56) gives a detailed description of the material he identified as B. longirimosus:

> 39 - 90μ (largest body diameter); exine 2 - 2.5μ, smooth, trilete rays nearly reach equator, simple, obscured by perine; perine reticulate, lumina triangular, triangles arranged into polygons (usually hexagons); a spine arising from each corner of each triangle (thus seven per hexagon), 14μ high, basal diameter 9 - 11μ, tips usually smooth; muri 2 - 2.5μ high, forming concave arcs between spines; perine surface smooth — pitted — foveolate; secondary muri may occur between primary ones; acrolamellae 15-20μ long, without muri but otherwise as rest of perine.

J. JANSONIUS & L.V. HILLS -- 1976
GENERA FILE OF FOSSIL SPORES
Special publication - Dept. Geology
University of Calgary -- Canada

Hunt Institute for Botanical Documentation

22 August, 1980

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

This is being written at Betty's family place in the Adirondacks. I have finally got around to taking care of both the 1978 and abd 1979 instfallments to the Supplement to ahh J-H cards. I am keeping my whole set in small 3-hole notebooks, which now occupy about a 4-foot shelf.

Did I do this?

Before I forget it, can you let me know what a complete additional set would cost, ouput in a file case for the graduate-student office?

Now, for the cards--I have a number of comments, which I offer for whatever help they may be. I have spent a good deal of time myself as a palynomorph catalogues, and think that perhaps my editorialish comments might be of more significance than those of the average palynologist. I do hope, however, that you will not take me to be hyper-critical. Your cards are immensely useful, and I only want to be helpful.

Unfortunately, I am not at my office, so I merely offer these comments ad seriatum, without being able to double-check the references:

#1317: please check this one. Isn't it Infernopollenites, not Infernopollis? right

#1725: Vilain? right.

#1726: check spelling Multimarginatus in heading of protologue is a typo; ites is used throughout paper

#2375: In my opinion, the author of the genus should be given on these--more accurate! yes, good point

#2794: Could you send me a replacement for this #? I discovered I am lacking it because of the correction.

do this

#3060: "Triporocolpites" should be Triporocolpotus (a correction of a correction!) See what you mean. don't get you. I have Triporocolpotus

#3438: Srivastava says "microspore", but he means just "not mega spore". For example, they could be isospores (most of his "microspores" indeed are obviously isospores!) likely so, but I only guess.

#3441: I object to "synonymized"--even if it does come from Muir and Sarjeant. It is inelegant English. yes, but it does not bother me any longer

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#3444: (Asbeckiasporites), #3446 (Asteropollis), etc. In all cases such as these, wouldnit it be nice to refer to the previous card, by #? Maybe, but it doesn't seem worth the extra bother

#3447: Is it your view that you (J&H) have made the nov. comb.? It would be mine. (I'm noting it as A. cancellata (B&F)J&H). correct.

#3450: Where are you getting "1977"? My copy shows 1975. By the way, you were smart to push up this genus! I missed it (no heading), and so did Srivastava himself in his own table of contents. I always look for a Colophon.

#861: Dryptopollenites? Correction needed. Thanks

#3473: relation to Inderites Dyupina? (not clear to me).

Perhaps this would be a good place to tell you that I dislike intensely the J&H way of handling Cyrillic: Transliteration is abominable because if you know Russian, it's hard to read, and if you don't, it's impossible anyway! Further, transliteration gets you into impossible snares: the Cyrillic b, for example, is w in German, but v in English. Cyrillic ϕ is f in English, v in German, etc., etc. The best way is to use Cyrillia. Second best is to translate to English, followed by [in Russian]. J&H use a mixture of 3rd and 2nd best--transliteration and translation. Sometimes, you do use mostly or entirely translation--cf. #3474. In #3484 the reference is translated but does not include the required "[in Swahili]" or whatever. What language was it in by the way?--and from whence? Also i n #3484 (and many others), when you give info from another card, why not give the # of that card?--the # is like a page reference, after all. Polish, which is written in Roman

#3486: Palaeoscytonema. This is carrying completeness too far! [Do you recall writing me years ago threatening to cancel your CFSP subscription because we were being too complete?--among other things. I have the same comment for #3584, #3567, and #3582 (this one because it perpetuates use of the name needlessly]. I don't agree with you here! I puzzled over and traced these names to ensure that they are not valid. This is solid and easy info now.

General Comment: how about providing your subscribers some day with a numerical list? I carelessly noted only the numbers on #3584, etc., just above, and couldn't re-check the cards now that they're filed, even if I had to! I may need to be able to recover #3584 some day! Is being done.

#3491 (Pereisporites): Good, sharp eye! This does not appear in Srivastava's own list of spores (p.2)!

#3506: Spelaeotriletes. Why not mark such cards "card B", or "replacement card"? I gather you think that subscribers will throw out the old card? No way--your own statements are only original the first time published. So, I will keep all cards. I do in one of my own sets.

#1375: What does lunctella mean?--lower case l? upper case I? (Sorry--I figured out later per #3061, but I don't like the orthography.)

#1518: If it's "H." 65, it should be "S." 63. This sort of hybridity (on many cards--e.g., 2431 and 2477) of orthography is annoying

Heft does not translate well ("number", "issue", "part": take your pick)

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hybrid indeed, but less ambiguous than other systems

and not up to your scholarly standards.

General comment: Why not "lines up" more often? "Line 29" is a nuisance to count. You did use "up" a few times. Alternatively, you could have little numbers down one margin, say every 5 lines.

good point.

#3525 (Ambiguaspora): Again, mixture of transliteration and translation--examples "Moscow" (Mockba), "p." 16 (s.).

#3518: Vesicula. This is not in Bold's glossary, not (more significantly) in Jackson's Glossary of Botanic Terms. I therefore doubt you're right that "vesicula" is a morphological term, per ICBN's prohibition. If we get so strict, I can think of other names that may be in trouble!

which ones?

General comment: You badly need to standardize your bibliographic citations and abbreviations. I recall that when I came to PSU in 1966, that was one of the first things I had to do with CFSP. I discovered that one journal was being referred to 12 (or 13?) different ways largely because of differing abbreviations. So, I set up two files, one of abbreviations, one of bibliographic citations, and after that I would allow only the standard. It was a big job (hundreds of cards), but was worth it. An example of what happens when you don't have such standard files can be seen in J&H by contrasting #3537 with #3538, etc., etc. This easily gets out of hand altogether. Cf. #3532 with #3550 (Central), etc.

#3551: You're right about petalum. Again, I protest mixtures of transliteration and translation!

#3553: Another example where the title should add (I guess) "[in Russian]".

? It's Chinese (Sinica!)

#3556 (Ghoshispora): I think I have a very valuable suggestion here: Why not put: "revised card", "corrected card", "replacement card", "expanded card", or whatever is appropriate, and always refer to previous cards by number? One reason for this suggestion is my perplexity in trying to figure out why the new card--there seems to be more than one possible reason?!

several reasons possible.

#3564: How about a glossary!--"hologula"? Also, here is a supplementary card labelled "card 2". Apparently #3565 is a "replacement card"--why not so label it?

I'm not responsible for "" quotes I added as much as Pietsch cared to give us way of glossary

#3587 (Pleosporites): Yeah, yeah, I know it says "_____sporites", but so what? The one that is an endobryozoan (?)-can't find it--also seems to me to be going very (too?) far afield--would it ever be Sporae dispersae?

This one fits right in with the microthyriaceous forms.

#2072: (Polyadites): In my opinion, this was validly published in v.d.H 1954b.

not so! I W JJ

#3598: In several places in J&H I notice "Pennsylvanian" for

not here, though

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"Stephanian B", etc., When it seems to me anomalous (not used outside of North America). Why not "Mississippian" here?

#3600 (Radiorugoisporites): o.k., which is it, "vol. 4" (here) or "Pt.4" (#3538)? Also note (per above comment) different abbreviations. I don't rightly know. I can read the chinese for "4" & this "4" is a big book.

#3601 (Radiopollis): cf. this bibliographic reference and that of Stridiporosporites (#3616) with those for Biporisporites (#3537) and Celtispollenites (#3540). This illustrates your need to establish a fixed file for abbreviations and bibliographic references (see previous comment). Is "942-13-13" really different from "942:13-16", or is this an error?

#3604 (Retitetraspora): Here, Euphorbiacites and others. Aren't "Li", Sung and Li" the same people as "Sung, Lee and Li"? Whoops, #3612 explains, and the correct procddure usually is to use the name as published (Bhardwaj 1960, Bharadwaj 1964, etc.). However, these are transliterations, not the original names, and you are at liberty to use a standard system for transliterations. (Thus, "Khlonova" is right, not "Chlonova", even though A.F. herself demands "Chlonova"!). The official one used by Peking now is "Li"

#3607 (Rotinella): As you know, I don't agree with you on this, but that could be argued forever. In my opinion, ICBN doesn't require that descriptions be in any particular format. "Accompanied" (of generic descriptions) means (I think) within the same publication. Incidentally, your statement that ICBN requires "a diagnosis" is incorrect. It says description or diagnosis. It must occasionally bother you that "description" is given first, then the afterthought (er) "or diagnosis".

#3612 (Scabiosapollis): o.k., I give up--why did you get out both #3502 and #3612 in the Supplement? different years! I can not know beforehand that errors are to be corrected.

Well, this has been fun, and I hope helpful--at least provocative!

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

10 November, 1980

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

The enclosed xerox will perhaps interest you. You will note that my "no." implies the lateness with which I was added--quite literally at the last minute (you'll recall my trip to Calgary to try to complete my list from your MS "cards"). You'll note that nobody added so late contributed anything like so many names (Ms. Farr, #110, is really a part of the ING team, and her 1,087 names were really "mop-up").

I also enclose xerox of a couple of sample pages. I wanted you to note how scary it is that mycologists throw....spora around so much!

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
enclosures

Hunt Institute for Botanical Documentation

4 November, 1980

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave SE
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

We're reaching the frantic stage in sabbatical preparation, so my reaction to yours of 27 October will be brief.

Dan Nicolson says the Storch proposal will be in February Taxon. However, it doesn't really matter, as either Feb. or May will be after the preliminary mail vote. My report on the IAPTCFP ballot will be in the May issue--but you are being sent a copy of the MS today.

Re effective publication, I don't think "intent" should have anything to do with it! I never thought I'd run into that one this side of theology. "The last refuge of a scoundrel is intent"--but I paraphrase.

Why bother to publish your 3.2? Just come to Sydney and help us get something sensible into the Code. We can do it from the floor.

Can't remember whether I sent WGC a copy of your "previous letter" (Again I am reminded of seminary, and Pauline studies) or not. Probably did, I suppose.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology
Secretary, IAPT-CFP

AT/et

Hunt Institute for Botanical Documentation

30 October, 1980

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. SE
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Am sitting by a roaring fire at home reading your "replacement letter" of 9 October--don't you keep copies, so you could run off a xerox?

The worst thing about Keystone was the altitude (9,600 ft.--ridiculous for a cerebral meeting, o.k. for mindless skiing and affiliated activities)--but a second bad feature was your absence. I had a big folder of nomenclatural stuff to discuss. Bill Evitt was your sub.

One of my graduate students and I spent two weeks in the field pre-Keystone, collecting Triassic materials.

Your explanation of lack of uniformity in the cards is helpful to my understanding of the history. However, as I told you earlier, preparation of three card files for CFSP for abbreviations, transliterations, and literature citations only took me about a month in my spare time in 1967. It is not too late for you to do it--or have a helper do it.

Re transliteration of Chinese names--I really hoped you'd comment on the heart of the matter--I think it is clear that an author has no control over transliteration. I have a couple of letters from Khlonova complaining bitterly that CFSP didn't use Chlonova, as she had plainly requested. We didn't, because the transliteration system we selected renders X as Kh (as in Khrushchev). There was some heated discussion of the matter in Leningrad, but the Soviets dontout in their attempt to have it decreed that authors control the transliteration of their own names . In other words, if Khlonova writes "Chlonova" in her own abstract, I am free nevertheless to use Khlonova. You are free, as I see it, to stick to Lee, Li or whatever--or is the circumstance different?

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Dear Al,

27 oct 1980

Just received your letter of 16 Oct., which reacts to / answers my handwritten one accompanying the ballots. May I dash down a few points?

The Storch proposal will be in Taxon in May; possibly the paper I read in Cambridge will appear in that same issue.

My proposed amendation of Art. 29.1 pertains only to 29.1; the bit about handwritten material is in 29.3.

I consider the intent of the author as a factor in deciding whether or not something is effectively published (the Code formally is guided by that concept regarding valid publication). If a poor student or university cannot afford better "indelible" technique than multilithing, I think that is ok. But a small run essentially for internal consumption – even if an odd copy leaks out into extraneous circles – is likely not "effective". On the other hand a fairly small run, say 100 – 200, with a broad distribution intended to make the text available to all, likely is effective. I do not think one can reasonably put numbers in the Code, either of tirage or receiving countries. Even if (because of war, or political repression) distribution is less broad than desirable, I would not say that that makes it "ineffective" (maybe ineffectual). But if through war the whole lot is lost prior to broad distribution, that would make it "ineffective".

I think it is not too late to publish my proposed 3.2 in Taxon; however, I am not against the device of insistence, i.e. that the Palaeobot. Comm. lean hard on the Edit. Comm. so the latter will amend its earlier action (which definitely was not authorized). I notice you send a copy to Bill Challoner; did you send a copy of my letter along with it? I hope so, otherwise he'll not know what things are about.

Best regards to both of you!

Hunt Institute for Botanical Documentation

9 October 1980

Dear Al,

I just received your xerox of an earlier letter. I had so received the original, but presume that my reaction to it — and I did write an answer because of your enquiry about the cost of another set — that it got lost in the mails.

Boy, you are lucky, now also attending the Keystone meeting. I am just staying put here, at least for some time. It would be nice, though, to now meet the bulk of Americans that were absent in England.

Ter zake! (If you remember that one). I enclose a copy of the 5 ICP special order form, as well as the new one that we just made up. We will honor the Cambridge form till the end of the year.

I will not go into the minutiae of each and every one of your comments. Many are well taken and will be acted on at the occasion of a next Supplement.

Re the transliteration from Russian, and uniformity in the abbreviation of journals: in the original introduction we apologized for this lack of standardization. However, it just would have been impossible to do all those things. If I had started the file with the purpose of publishing it, I would have set out a schedule; as it was, the thing grew like Topsy. Eventually, it seemed worthwhile to share the file, as it was, warts and all, with the fraternity of palynologists. Yes, I too would have loved a stricter quality control.

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Now, in another 2-3-4 years we will have sold the existing stock. At that time we may
1/ Sigh with relief, sit back and do nothing.
2/ print more copies (the negatives are saved, as are the printers "plates", but I don't know how many more clear copies they will print)
3/ prepare a new edition (i.e. do not reissue replaced and updated cards, and do some judicious upgrading of the old typescripts).
Probably the cost factor will steer us toward the 2nd option, but even in that case I'll want to rephotograph all those cards (of which the enclosed #2794 is one) where the printer reduced the text more (much more) than necessary. At least that much.

Right now, I have no list of the names that go with the various numbers of the Supplements; I have "lost" those cards by filing them alphabetically. Such a list must be included in the next Supplement!

With regard to a number of odd ball entries: Pleosporites was included because we do have a number of similar fruiting bodies (mostly Microthyriaceous) in the File. Also, with a name like that one wants to forestall a jr. homonym. The Precambrian things worry me a bit, especially the "catch-up" in the last Supplement. Yet, it is difficult to know where to cut things off: many of them are organically preserved microfossils, and if the majority should be considered to be acritarchs, a number of them look like fungi, and some could be spores. Especially because I — maybe unwisely — clove these early orphans to my breast, I felt obliged to have them all, because any File like this must strive for completeness.

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Polyadites v.d.Hammen 1955 (per date printed on cover of periodical, even if nominally "1954 b"). I do not accept it as validly published; there are no species assigned, and the "tipos" are only there for illustrative, explanatory purposes, but not for nomenclatural reasons. If you accept Polyadites as validly published in 1954, there is no reason to not accept all the names in Paut 1954 as validly published as well.

As for "Part IV" or "Vol. 4" in Radiorugoisporites — here again I would not know, as it is given to me by various Chinese translators. It is apparently a textbook in several parts, each volume given to a particular group of fossils.
At Randiapollis (#3601) I gave all clues I could find on my document so as to make it easier for a librarian to fill a request for it. You are right, though, to say that the same goes for Stridiporites and the others. And your hawk-eye spotted an error: in Randiapollis, l.3, read "g42.13-16" for "g42.13-13".

Regarding "Lee" versus "Li": I gave it as it was transliterated by the author. Chinese has been transliterated in various "systems" by successive generations of jesuits, ministers, and scholars. The latest ("Li") appears in keeping with the directives of the PR China government, in the same system as in which "x" stands for "sh" (as in "she"), "q" for "ch" (as in "cheese"), while "sh" stands for a peculiar Chinese consonant, and is pronounced as you would think, but requires the tongue to be curled back while doing it. (Dear me, don't look at my grammar in that sentence).

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You are right in your implication that I do not feel like budging on my opinion of Malyavkina*. The remarks by Yaroshenko, again, indicate that Malyavkina was (or became) a charlatan, a travesty of a taxonomist, who herself did not know which way was up, and which down. Her "taxa" mostly make no sense at all. And the rest we have argued enough.

As for "description or diagnosis", the order don't bother me not one little bit. If there is only one species in a genus, all you can do is describe; if there are more species, you have to delineate conditions to be met, or features to be absent or present, in short you make a diagnosis. But did I not go into this in my other letter, too? (Or in the one that got lost). Enough said; it was nice talking to you, and I do appreciate your careful comments very much.

P.S. Scabiosa sapoltis was issued in 1978 (3502) and 1979 (3612), the second time because the "indicated" type species had been validly published. I thought in 1978 that the genus had been validly published, but now in 1980 I am of a different opinion. The cases of Ginkgoinites (# 3692) and Alnipollenites (# 3633) now make me think that a genus can only be "valid" if a type species is indicated that itself was validly published. Since a genus, and its nomenclatural type, are artificial man-made things, they must exist (in the sense of the Code) before they can be recognized.

With best wishes,

* ad: Rotinela

Hunt Institute for Botanical Documentation

30 September, 1980

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Just got yours of 23 September re my "review" of the most recent JH supplement. However, Iseent you earlier (22-VIII-80) a much longer review of the previous two supplements. Did you never get that letter? I submit a xerox, as it would appear you did not get it. At least, you did not respond to it, but to the later one.

Hope to see you at Keystone.

Youts very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl

Hunt Institute for Botanical Documentation

Dear Al, 23 Sept.

Just after I sent off my ballot, your letter with comments on the last Supplement of the Genera File came in. I'll quickly run through your letter and respond.

3633 Alnipollenites – I sent a xerox of my MS on this card to you and Chaloner, and nobody objected. The point is that I agreed (on this card) with Fred Ames that the status of Pollenites does matter and is relevant to Alnipollenites. Please read this card again and count to ten before you continue after the name Ames. Do you agree with the first paragraph of the Remarks? Alnipollenites as on our card can only be validly published if the species is new; something that is effectively but not validly published is not published as far as the Code is concerned, it does not exists in this technical sense. This is my current best interpretation of the Rules, and because it differs from my earlier opinions, it is a precedent. Therefore I refer to it on card 3692 Ginkgoinites. Earlier, like in previous Supplements in regards of some Chinese genera, I had accepted some genera as validly published with a reference to a type species that itself had not yet been validly published. Now I think that I goofed there, for a reference to something that does not exist (technically) is a reference to zilch, and you cannot base a genus on zilch.

3652 I got the date from the heading of the article; although the volume was apparently published in 1980 (we got ours about in May), the individual papers have "1978" in the first part of the volume, "1979" in the second part. I gave them the benefit of my doubt, especially because there does not seem to be a priority conflict by maintaining the 1978 date. However, Stanley will maken enquiries in India. (3652 = Cannorosporonites)

3659 Clavapalmaedites. Our library received V.25 in August 1979

3678 Doubringerispora. Both Stanley and I got our reprints around April (-May) 1980; I have always been in close contact with Scheuring, and expect him to have

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sent me a copy as soon as possible. If you had your copy in 1979, it must have been late 1979? In any case, here again I did not see obvious clashes of priority (his paper carries a nominal date of 1978), so I decided to stick 1980 on the cards. Actually, immediately after I finished those cards, the job went to the printer: no time for checking.

861 I am at a loss what "861 Drypto...?" means Not Drip (Wonder what Frenchy have to say to that one)

3692 Ginkgoinites. I already held forth on this under Alnipollenites. A type of a genus is not a specimen (as Potonié believed), but a species; nomenclaturally, a species exists only after valid publication, although the specimen on which the species is based existed much earlier. Effective (but invalid) publication of a species is not good enough to make it exist (a "dream" is "zilch"). My use of valid, in some cases, is elliptical; it is so tiresome to use "validly published" too often in one paragraph. Even if the type species is published but not validly so for lacking in one requirement, another author may publish another name for that species in the mean time, and validly.
Of course, I do not use "my" File to publish nomenclatural opinions just for the fun of it. But I have to make decisions and interpretations, and doing that, I put all my cards on the table and say Why I made a particular conclusion.

3696 Yes, I often say "monotypic", but this one must have escaped me (Goerbeepall) sorry!

3708 Interradispora versus is correct; the author states that the epithet is substantival (versus = row, line, ploughed furrow).

3745 Pollenites. There is nothing worse than a cleric turning cynic: where is your compassion, man? Of course, we are grasping for straws; but, you will recognize a "pollen" when you see one? Potonié could, and so could others, and so many species were assigned to Pollenites when many authors were satisfied with such an assignation. But the Code requires formalities retroactively, and so we do the best we can. As diagnosis, "pollen" is not really all that bad a requirement by which to measure whether or not a grain belongs. As description, "pollen" is not so hot.
Of course I know what the Code says: diagnosis or description.

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If you now read Art. 31.1, it says that a taxon must have, a.o., a description or diagnosis. In my estimation, a concrete thing can be described (a holotype specimen, a population of specimens). Assigning the basic species, or a number of them, to a genus is an artificial thing; a genus is a concept, and a circumscription of its conceived limits can be given, or a diagnosis of the conditions to be satisfied ("that which in the opinion of the author distinguishes the taxon from others"). Of course, many generic diagnoses list shared characters and only imply thru the act of omission characters that differentiate, and as such appear descriptive. Anyway, I donot think that my innocent (if rather headstrong) convention to indicate all generic circumscriptions on the Genera File as "diagnosis" is against the Code. Basta.

3748 Procoronaspora. Column 2, last ¶, is not a full quote, but still derived from Smith & Butterworth, and much in their own words. Since we did not initial the ¶ (with JJ or LVH), it should be clear that it is not we that speak. Dash the usage of the English authors, anyway, to call themselves in their own text "the author(s)". Sullivan apparently was in contact with them.
I agree with your opinion that updated cards are not really supplanting the earlier: in one file I keep all the old ones, too. But in my morphological file, I only keep and work with "the latest".

3750 Pseudo illinites. Maybe I should have said "(sic. JJ)", because it is my sic. The (further) absence of initials indicate that the comparisons are those of Rawn.
As for the homonymy, I refer you to Art. 64.2, and Art. 73, Recomm. 73 G.1.(a).(2). The connective "o" in the Greek "pseudo-o" should be dropped before the vowel "i", resulting in the jr. homonym Pseudillinites. Would you want to disagree?

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3754 Pterisporis. I wrote to Huang, informing him of his invalid publication. I also asked, at that time, if he agreed that Bikolisporites already referred to the concept of Pterisporis, so that he could just let things stand where they stood. He said no (for reasons in the Remarks), and would I please validate the genus in my File. So, that's why it is Huang in Jansonius & Hills. I am sure, though, that in the File this validation is more noticed than a note in Taxon, or even the original paper in Taiwania.

3767 Spelaeotriletes. Your point about adding the name of the author is well taken, and I hope to remember and apply it in the future. Still, author citations are not part of a name in the Code. If there were confusion (homonyms) I'd sure have done it.

3784 Triadispora. Card 2 always (I hope) indicates a continuation, a spill-over situation. It never identifies an upgraded or updated card.

2969 I will have to disavow! The detective work on Tricalporites is that of Partridge.

2873 Frantz: thanx!

Thank you very much for your careful perusal and letting me know!

Best wishes,

John.

Hunt Institute for Botanical Documentation

16 Septmmber, 1980

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Now I've gone through the 1980 supplement and submit my comments thereon. Of course, I am trying not to repeat myself too much--some of the general things I said re 1978 and 1979 still hold!

Once again I hasten to add that none of this should obscure the fact that the cards are a very welcome addition to m y bag of tricks here!

Comments, ad seriatum:

3633 What has the question of Fred. & Ames re valid publication of Pollenites got to do with it? It is nice that you agree with them, but it isn't relevant to Alnipollenites interpretation, is it? What counts is your (& my) Pollenites was not validly published before Alnipollenites. (I am still burned by the F.-A. paper because it acknowledges me but does not say that I disagreed 100%.) opinion that

3639 I have said this before--and will (maybe) try to stop belaboring it--"trichomes"--really, now...!

3652 Where do you get the date "1978"? I suspect 1980.

3659 Where are you getting "1979"?

3660, 1. 24 Accommodate (shame!)

3678 What is reason for "1980" (I would have sworn I had it in 1979).

861 Drypto...?

3692 cf. col. 2, 1. 2 up: "valid" is being used too loosely in cards--"validly published" is what you mean. "Valid" is not correct usage.

col. 2, general. What's the Frederiksen & Ames bit again? A non sequitor! You are right that the type species for a genus must "exist", but I find nowhere any requirement that it be "validly published", as you assert. (Perhaps we should revise ICBN in that direction!--not a bad idea.) For example, suppose the species designated as type was not validly published because

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3692, con.: the species designated as type was not validly published because it lacked just one required element--for example, a Latin diagnosis for a species of extant plant? Surely that would not make the new generic name not validly published. You would be on stronger ground if you said it must "exist",--e.g., by effective though not necessarily valid publication. However, I really wonder if you ought to stop publishing nomenclatural opinions, gratuitous endorsements of tangential opinions of others, etc., on the cards? Should there perhaps be an GFFS Newsletter for such matters?

3696 Don't you usually say "monotypic" (when published)?

3708 Specific epithet must be corrected to versa per ICBN.

3745 The "diagnosis" is a JOKE! "Pollen" is a functional stem.

"Remarks": The ICBN still does not require a diagnosis, but a description, or (er...) a diagnosis. Please read Art. 32.2 and realize that a "diagnosis" is much more limited in meaning than you mean by your use of "diagnosis". You mean description.

3748, col. 2, para. 2, l. 4: "The authors"--who? It is ambiguous, it seems to me. What do you call JJ-LH?--"The editors" or "The authors"? To whom is Sullivan's communication directed?--to JJ-LH, or to B&W? This paragraph's source is not obvious. It's not, apparently, a quote.

Also, I have the same comment here as in my last batch, that a card like this should be labelled "card 2", or "updated card", or something. I do not approve of the idea that such a card would replace the previous card--the previous card is a piece of the literature. For example, the statement about Potonié 1960 from the older card seems to be omitted in the new card, and yet the fact you said it can't be wiped out!

3750, col. 1, l. 27: why "(sic)"? If that is yours, it should be "[sic]" or something like that. If not yours, whose (sic) is it?

col. 1, para. last: You are wrong here about homonymy, in my opinion! This is not merely a one-letter difference, it is a one syllable difference and probably quite acceptable, though perhaps an unwise choice! I have voted on dozens of homonymy decisions of the General Committee (the one area in which we have a "commission") and I think you'd lose. I believe this is a case of your going too far! The case should be referred to the General Committee for a judgment (see Art. 75.1, footnote), but I doubt JJ-LH can make this pronouncement so authoritatively!

cf. bottom line & 5th from bottom for illustration of problems

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3750, con.: I have previously mentioned of your particular machine's capital "I".

3754 I am not happy about this "new generic name"--I haven't seen the original paper, and must accept your evaluation of it. How do you decide which nomina nuda to validate? Are the cards the right place--as opposed to a note in Taxon, for example?

Description: as you were actually re-publishing, why couldn't you clean up "...circumfluent field proximal...."?

"Remarks", 1. 3: I object still to split infinitives and wish you'd seek to avoid them.

last line: "...valid genus." is not a correcg expression, as I've repeatedly mentioned. You mean "validly published". (It is awkward not to have a single word for this--how about "valpub"?)

3756 Boo! Also, Hiss!

3765, 1. 8: "validly published"!

3767 The title line in a formal taxonomic work such as JJ-LH should always be generic name plus author!

3784 Same comment as for 3767. However, I like the fact that the card is labelled "card 2". How does it differ from other supplementary cards not so labelled? It is clear to me that you need to think through this question! Some of what I regard as "card 3" are labelled (as a second) "card 2".

3785, 1. 14 up: -ites is (per ICBN 75A.4) masculine. (i.e., not an opinion of yours)

3786 "1947A" bothers me, though I see the problem. Your "A" and others "a" may not be the same? This is a real problem. People shouldn't publish more than a paper per year! You seem to have done some good detective work here! However, incidentally, did you ever disavow the statement the2969?!

3795 I regard this as really card 3! But I repeat myself.

3797 I believe I got you on this date supra? I find no indication of pre-1980.

2873 Have you already picked up misspelling of Frantz?

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

22 August, 1980

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

This is being written at Betty's family place in the Adirondacks. I have finally got around to taking care of both the 1978 and abd 1979 instfallments to the Supplement to ahh J-H cards. I am keeping my whole set in small 3-hole notebooks, which now occupy about a 4-foot shelf.

Before I forget it, can you let me know what a complete additional set would cost, ouput in a file case for the graduate-student office?

Now, for the cards--I have a number of comments, which I offer for whatever help they may be. I have spent a good deal of time myself as a palynomorph catalogues, and think that perhaps my editorialish comments might be of more significance than those of the average palynologist. I do hope, however, that you will not take me to be hyper-critical. Your cards are immensely useful, and I only want to be helpful.

Unfortunately, I am not at my office, so I merely offer these comments ad seriatum, without being able to double-check the references:

#1317: please check this one. Isn't it Infernopollenites, not Infernopollis?

#1725: Vilain?

#1726: check spelling

#2375: In my opinion, the author of the genus should be given on these--more accurate!

#2794: Could you send me a replacement for this #? I discovered I am lacking it because of the correction.

#3060: "Triporocolpites" should be Triporocolpotus (a correction of a correction!)

#3438: Srivastava says "microspore", but he means just "not mega spore". For example, they could be isospores (most of his "microspores" indeed are obviously isospores!)

#3441: I object to "synonymized"--even if it does come from Muir and Sarjeant. It is inelegant English.

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#3444: (Asbeckiasporites), #3446 (Asteropollis), etc. In all cases such as these, wouldnit it be nice to refer to the previous card, by #?

#3447: Is it your view that you (J&H) have made the nov. comb.? It would be mine. (I'm noting it as A. cancellata (B&F)J&H).

#3450: Where are you getting "1977"? My copy shows 1975. By the way, you were smart to push up this genus! I missed it (no heading), and so did Srivastava himself in his own table of contents.

#861: Dryptopollenites?

#3473: relation to Inderites Dyupina? (not clear to me).

Perhaps this would be a good place to tell you that I dislike intensely the J&H way of handling Cyrillic: Transliteration is abominable because if you know Russian, it's hard to read, and if you don't, it's impossible anyway! Further, transliteration gets you into impossible snares: the Cyrillic b, for example, is w in German, but v innEnglish. Cyrillic ϕ is f in English, v in German, etc., etc. The best way is to use Cyrillia. Second best is to translate to English, followed by [in Russian]. J&H use a mixture of 3rd and 2nd best--transliteration and translation. Sometimes, you do use mostly or entirely translation--cf. #3474. In #3484 the reference is translated but does not include the required "[in Swahili]" or whatever. What language was it in by the way?--and from whence? Also i n #3484 (and many others), when you give info from another card, why not give the # of that card?--the # is like a page reference, after all.

#3486: Palaeoscytonema. This is carrying completeness too far! [Do you recall writing me years ago threatening to cancel your CFSP subscription because we were being too complete?--among other things. I have the same comment for #3584, #3567, and #3582 (this one because it perpetuates use of the name needlessly].

General Comment: how about providing your subscribers some day with a numerical list? I carelessly noted only the numbers on #3584, etc., just above, and couldn't re-check the cards now that they're filed, even if I had to! I may need to be able to recover #3584 some day!

#3491 (Pereisporites): Good, sharp eye! This does not appear in Srivastava's own list of spores (p.2)!

#3506: Spelaeotriletes. Why not mark such cards "card B", or "replacement card"? I gather you think that subscribers will throw out the old card? No way--your own statements are only original the first time published. So, IIwill keep all cards.

#1375: What does lunctella mean?--lower case l? upper case I? (Sorry--I figured out later per #3061, but I don't like the orthography.)

#1518: If it's "H." 65, it should be "S." 63. This sort of hybridity (on many cards--e.g., 2431 and 2477) of orthography is annoying

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and not up to your scholarly standards.

General comment: Why not "lines up" more often? "Line 29" is a nuisance to count. You did use "up" a few times. Alternatively, you could have little numbers down one margin, say every 5 lines.

#3525 (Ambiguaspora): Again, mixture of transliteration and translation--examples "Moscow" (Mockba), "p." 16 (s.).

#3518: Vesicula. This is not in Bold's glossary, not (more significantly) in Jackson's Glossary of Botanic Terms. I therefore doubt you're right that "vesicula" is a morphological term, per ICBN's prohibition. If we get so strict, I can think of other names that may be in trouble!

General comment: You badly need to standardize your bibliographic citations and abbreviations. I recall that when I came to PSU in 1966, that was one of the first things I had to do with CFSP. I discovered that one journal was being referred to 12 (or 13?) different ways largely because of differing abbreviations. So, I set up two files, one of abbreviations, one of bibliographic citations, and after that I would allow only the standard. It was a big job (hundreds of cards), but was worth it. An example of what happens when you don't have such standard files can be seen in J&H by contrasting #3537 with #3538, etc., etc. This easily gets out of hand altogether. Cf. #3532 with #3550 (Central), etc.

#3551: You're right about petalum. Again, I protest mixtures of transliteration and translation!

#3553: Another example where the title should add (I guess) "[in Russian]".

#3556 (Ghoshispora): I think I have a very valuable suggestion here: Why not put: "revised card", "corrected card", "replacement card", "expanded card", or whatever is appropriate, and always refer to previous cards by number? One reason for this suggestion is my perplexity in trying to figure out why the new card--there seems to be more than one possible reason?!

#3564: How about a glossary!--"hologula"? Also, here is a supplementary card labelled "card 2". Apparently #3565 is a "replacement card"--why not so label it?

#3587 (Pleosporites): Yeah, yeah, I know it says "_____sporites", but so what? The one that is an endobryozoan (?)--can't find it--also seems to me to be going very (too?) far afield--would it ever be sporae dispersae?

#2072: (Polyadites): In my opinion, this was validly published in v.d.H 1954b.

#3598: In several places in J&H I notice "Pennsylvanian" for

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"Stephanian B", etc., When it seems to me anomalous (not used outside of North America). Why not "Mississippian" here?

#3600 (Radiorugoisporites): o.k., which is it, "vol. 4" (here) or "Pt.4" (#3538)? Also note (per above comment) different abbreviations.

#3601 (Radiopollis): cf. this bibliographic reference and that of Stridiporosporites (#3616) with those for Biporisporites (#3537) and Celtispollenites (#3540). This illustrates your need to establish a fixed file for abbreviations and bibliographic references (see previous comment). Is "942-13-13" really different from "942:13-16", or is this an error?

#3604 (Retitetraspora): Here, Euphorbiacites and others. Aren't "Li", Sung and Li" the same people as "Sung, Lee and Li"? Whoops, #3612 explains, and the correct procedure usually is to use the name as published (Bhardwaj 1960, Bharadwaj 1964, etc.). However, these are transliterations, not the original names, and you are at liberty to use a standard system for transliterations. (Thus, "Khlonova" is right, not "Chlonova", even though A.F. herself demands "Chlonova"!).

#3607 (Rotinella): As you know, I don't agree with you on this, but that could be argued forever. In my opinion, ICBN doesn't require that descriptions be in any particular format. "Accompanied" (of generic descriptions) means (I think) within the same publication. Incidentally, your statement that ICBN requires "a diagnosis" is incorrect. It says description or diagnosis. It must occasionally bother you that "description" is given first, then the afterthought (er) "or diagnosis".

#3612 (Scabiosapollis): o.k., I give up--why did you get out both #3502 and #3612 in the Supplement?

Well, this has been fun, and I hope helpful--at least provocative!

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

8 August, 1980

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Don't forget to get the official opinion on NFH proposal here as soon as possible. Doesn't need to be exhaustive.

Yours very truly,

Alfred Traverse
Professor of Palynology
Sec'y., IAPTCFP

AT/et
cc: Chaloner

Hunt Institute for Botanical Documentation

25 July, 1980

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Just a line to tell you what a marvellous job I thought you did in presenting the invitation for 6-IPC. The circumstances were not too easy for you, but I know that at the closing session everybody's appetite was whetted, so that we're already thinking Calgary, 1984 (a fateful year?).

Best regards, old friend.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

29 May, 1980

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

I have the 1980 dues notice for CIMP for $6.00, US or Canadian. I enclose a check on the Bank of Montreal for $6.00 for my dues.

I am aware that the notice says:"...in THEIR LOCAL CURRENCY". This is clearly discriminatory and probably illegal. If Canadian dollars are acceptable, they are acceptable, period. It would be ethical to ask for US funds, or Canadian funds of everybody, but it is clearly unethical to have a double standard. If you say you'll accept Canadian funds, you, being Canada-based, are stuck with it! Of course, the logical, ethical way around it would be to state that the dues are $6.00 US, or $7.00 Canadian, payer's choice. Facts are facts.

Best personal regards. How are you coming with your review for IAPTCFP?

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: check for $6.00 Canadian

Hunt Institute for Botanical Documentation

21 March, 1980

Dr. Jan Jansonius
EssorResources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Yours of 7 March just arrived. Am glad you found my comments on your proposed MS re Alnipollenites helpful.

I guess we are going to continue to debate the diagnosis vs. description for a long time. The definition of diagnosis you got from "Webster's" is one I could buy. It is a "concise technical description" (usually in Latin). Very few of the things which authors publish as descriptions of their new taxa could be considered diagnoses. You use the term diagnosis as the general term. What I have been urging is that the general term for such pieces of information is description.

Regarding Pollenites--I was a little disappointed that you would think my opinions on a technical matter would be colored by my personal feelings. No way! Besides, I like Fredricksen very much. As I said earlier, and intend to go on repeating whenever the subject is brought up, the difficulty with Pollenites in my opinion is that it is a concept which cannot be described for fossil material. Potonie's description of it as being for "pollen" demonstrates that the "genus" was based on a meaningless concept. It apparently requires more botanical background to understand this than most palynologists have. Sporites can be a meaningful concept, even though a tremendous variety of structures are involved. I suspect that as soon as prepollen was discovered, Potonie knew that his Pollenites was in bad trouble. I have been trying to think of at least an approximate analogy that would demonstrate my point, but it seems to be very difficult. The point is that whether a thing is a pollen grain or not is a functional question. I am still listening, and perhaps one of you can convince me that Pollenites is a validly published genus. At the moment, I feel very strongly that it is not and never was. I don't think that nomenclatural stability will be hurt a bit if we throw it out. It will mean that some genera previously regarded as not validly published were validly published by combined generic-specific description, because of monotypy at the

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time of publication, but it's not a very long list.

Have been looking at the xerox you sent me regarding Coronospora rodriguez. Unfortunately in the meantime I have written Cramer about it, not knowing about the loose-leaf addendum. Your remark on Nodospora seems o.k. to me, though it may turn out yet that Nodospora is tetrahedral. Paul and I occasionally argue with each other about it.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Dear Al,

Thank you for your letter of 21 Febr. ; today is 7 March 1980.

Your comments on Almipollenites were very welcome ; I felt uncertain about "new" species that had been effectively published but not validly so. I think it is correct to consider a species that does not have a valid (i.e. validly published, sorry) name as a non-species, and thus it cannot be a new species yet. I am not sure that I have had that understanding at all times that I worked on the Genera file ; however, I'm not going to flip through it just to check that one out.

Re: diagnosis/description — I am not really stubborn about it, just dogmatic. I'm kidding ; it is a convenient shorthand, and I try to keep my format consistent. But you will notice (on Almipollenites, for instance) that I do allow a "combined description" to function as "diagnosis". Webster's have: "diagnosis 2: a concise technical description of a taxon". Philosophically I still feel that a species is a natural entity, but that a genus is an artificial concept, of which the boundaries can be described, the distinguishing features can be pointed out, but as it is not a "thing", it defies pictoring ; to me a description is a word picture. (In satellite photographs a similar distinction is made between "picture" and "image").

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Coming back to Pollenites. It has been considered by the early authors as a genus, and used in binomial nomenclature. Either we conserve the name (do you want me to propose that?) or we find a statement of the criteria that diagnose it, that indicate when a species can be assigned and when not. Since obviously the concept is very wide, its boundaries are wide as well. By tacit agreement, modern authors do not use the taxon for new species. But nomenclaturally, the thing either stands, or not. For purposes of nomenclatural stability, the earlier the date of priority, the better. I am not going to fight Frederiksen + Ames on this matter.

I can understand your antagonistic feelings re Tate; in fact in the last few years I have begun to understand why you had difficulty in working with him. I have developed similar feelings, because there is no intelligent communication.

Still, what do you consider is the status of Pollenites, and all species assigned to it? It does matter with regard to Phi-pollenites, which is not validly proposed if the Genus P. was valid earlier.

I have nearly proofed the m.s. for the next update of the General File (± 124 new cards); after that I will concentrate on the Hughes proposals. But give me a 6-8 wks for that, please.

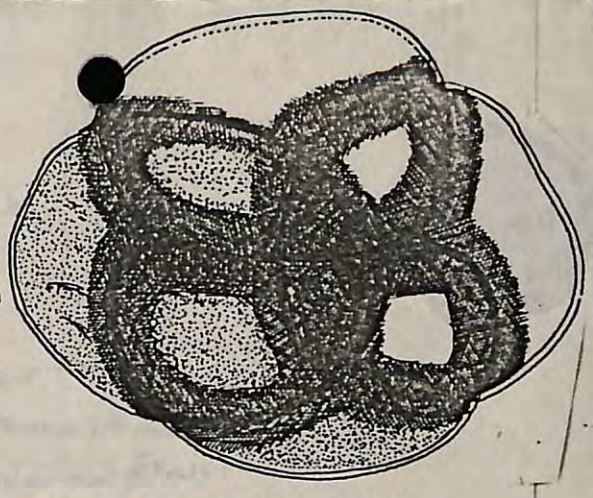
Thanks again + best regards,

Hunt Institute for Botanical Documentation

NODOSPORA Strother & Traverse 1979

Palynology, v. 3, p. 10

Type species: Nodospora burnhamensis Stroth. & Trav., ibid., pl. 1, fig. 11



Diagnosis: "Tetrads of inaperturate spores or spore-like palynomorphs, spherical to subspherical in outline, which are arranged in a cross configuration; a continuous band of thickened wall follows the line of contact of the individual spore margins; tetrad may be enclosed in a membranous sac.; tetrad diameter ranges from 38 to 82 μ; walls may be smooth or ornamented."

Description of type species: 55-80 μ (mean 65 μ) diameter of tetrad; wall surface smooth. ¶ Llandoverian (-Wenlockian); Pennsylvania, USA.

Remarks: the characteristic thickened loops, in the shape of two crossing figures 8, or crossed pretzels, seem to indicate that the tetrads are not tetrahedral, but rather planar, even if the individual monads are well inflated. JJ.

Would you agree?

CORONASPORA Rodriguez 1979

3543

* → 1979 Rev. Micropaleontol., v. 21, no. 3, loose-leaf "Addendum"
1978 Rev. Micropaleontol., v. 20, p. 218

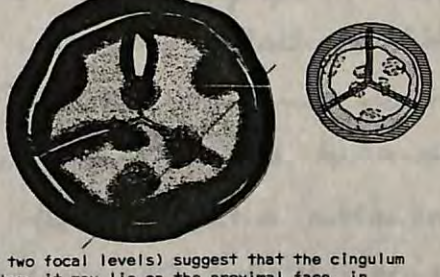
Type species: Coronaspora mariae Rodriguez 1979, l.c.
Coronaspora mariae Rodriguez 1978, l.c., pl.1, fig. 1,2

Diagnosis: "Amb subcircular. This genus features a cingulum set off by a groove on the proximal face. The spore wall increases in thickness on the proximal side of this groove, its surface remaining smooth, then gradually thins toward the center of the spore. The sutures are smooth, and bordered by a more delicate exine, extending to the outer part of the cingulum. Distally the wall shows numerous warts and muri, which may align themselves parallel, in a circle concentric with the proximal pole, or may occur anywhere within the cingulum. Some specimens show a widening of the spore wall in the interradial parts, near the cingulum, which is most pronounced and widest towards the proximal pole."

Amocosporites (sic) has a cingulum of complex structure in a subequatorial position, whereas Coronaspora has a circular proximal thickening.

Description of type species: "Outline subcircular. The spore possesses a considerable cingulum, separated from the proximal face by a groove with straight sides. From this groove towards the proximal pole the spore wall thickens, the surface remaining smooth, and then gradually becomes thinner towards the center of the spore. The sutures are straight and separated by a thinner exine which extends to the outer part of the cingulum. The thickening is narrow and not raised and surrounds the suture in a uniform manner. On the distal face a variety of verrucae are developed, three of which are disposed in such a manner that their projections above the proximal face coincide with the centres of the interradial surfaces (one per each surface), whilst the others may be located anywhere. On the proximal face three expansions occur near the inner side of the cingulum; they are most noticeable and widest towards the proximal pole.
Equatorial diameter 20-30 μ; width of cingulum 2 μ; width of proximal face 2.5 μ; width of expansion of wall 5 μ; diameter of verrucae 6 μ, height of verrucae 4 μ."

Siluro-Devonian, San Pedro Fm; Spain.



Remarks: The photographs (at two focal levels) suggest that the cingulum extends to the equator; it may lie on the proximal face, in which case the cingulum is sharply delineated towards the proximal pole; or on the distal face, separated by a sharp, narrow groove from a second circular thickening, which does not have a sharp inner delineation but rather thins gradually if rapidly to the normal exine thickness. On the distal face occur a number of flat verrucae, the delineation of which mostly is similarly indefinite; three of these tend to merge with the inner circular thickening, alternating with the trilete rays. The trilete rays extend to the equator, or nearly so, not appearing to cross the cingulum, but definitely crossing the inner circular thickening; the labra, about half as wide as the cingulum, have indefinite outer margins and narrow near the proximal pole.
Rodriguez appears to be in error when he described the inner circular thickening (and the cingulum?) as being proximal; the apparent crossing over of the sutures argues against this interpretation. Of his two photographs, he called "distal face" that one focussed sharply on the trilete sutures, and "proximal face" the one focussed on the verrucae.

(If my morphological interpretation is correct, there would not seem to be much structural difference between Coronaspora and Amicosporites. J.J.)

Nomenclature note: A second species, Coronaspora cromatica (Rodriguez, 1978, Rev. Micropaleontol., v.20, p.218, pl.1, fig.4) here is formally assigned to the genus by us; it was not valid in 1978, because not assigned to a valid genus.

J. JANSONIUS & L.V. HILLS -- 1979
GENERA FILE OF FOSSIL SPORES - SUPPLEMENT
Special publication - Dept. Geology
University of Calgary -- Canada

* This "Addendum" was in response to my advice to Rodriguez that his generic name was not validly published in 1978. He never did answer my question on the relationship with Amicosporites, and the exact position of the various morphologic features.

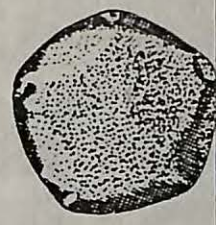
Hunt Institute for Botanical Documentation

ALNIPOLLENITES Potonié 1931 (May)

Jahrb. Preussischen Geol. Landesanstalt, Bd. 52, p. 4

Type species: Alnipollenites verus (Pot.) ex Pot., ibid.

Pollenites verus Pot. 1931 (April), Z. Braunkohle, Jahrg. 30, H. 16, p. 332, pl. 2, fig. 40



Diagnosis (Combined description, May 1931, in short-hand formula):

"Outline polygonal, lenticular in side view, surface ± smooth?, 4-5 protruding germinals, size 17-21 μ" Pl. Miocene; Germany.

Potonié 1960 (Synopsis III, p. 129) gives the generic diagnosis as follows:

"Lenticular shape, 4-7 (rarely 3) pores with labra on the corners of the polygonal to subcircular amb; a small vestibulum can be discerned; pores interconnected by arched folds of the exine (arcus); arcus running like garlands from pore to pore, on both proximal and distal faces; pores somewhat elongate meridionally (rimulae); exine with weak infratexture, surface ± smooth."

Remarks: Frederiksen & Ames 1979 (Taxon, v. 28, p. 557-561) argue convincingly that Pollenites was validly published in May 1931 (Potonié, cited above); in Potonié's earlier publication (April 1931, cited above) this generic name now is considered as not having been published in a valid manner. Consequently, the species assigned to Pollenites in this earlier paper also are. → not validly published. Among the latter is "Pollenites verus", which species is validated in the May 1931 paper, where it is assigned to Alnipollenites. Because this species was not earlier validly published, Alnipollenites is now to be considered as validly published with a combined description.

¶ In the May 1931 paper, Potonié refers back to the April paper (the species in which he considered at the time as validly published) and cites pl. 2, fig. 40, the first effective publication of this taxon. In April 1931, a photograph is given of a rather rounded specimen with indistinct arcus; in May 1931, the taxon is illustrated further by a line drawing showing very distinct arcus; in 1934 (Arb. Inst. Palaeobot. Petrogr. Brennsteine, Bd. 4, p. 58, pl. 2, fig. 13, 17, 18; pl. 6, fig. 28) Potonié illustrates more specimens, some with, others without distinct arcus. In 1960 (l.c.), the Potonié designated ~~selected~~ as nomenclatural lectotype of these pl. 2, fig. 17 of the species verus. We do not accept Potonié's procedure, which seems to run counter to the recommendations of the ICBN. Rather, we accept the first published figure, to which Potonié keeps referring in his subsequent papers as a part of his taxon verus; it is the single one element included in the first effective description of the species, to which Potonié refers back in the first valid publication thereof.

Polyvestibulopollenites thus becomes an obligate junior synonym of Alnipollenites, because it has the same type species but was published in Thomson & Pflug only in 1953.

This is my revised MS., incorporating the various suggestions that you wrote.

Hunt Institute for Botanical Documentation

3 Feb. 80
Sunday PM

Dear Al,

Here is the cheque from CIMP for ICP; I trust the account number has not changed since last year.

I am very busy with my Tertiary pollen, and also just finished 100 new genera cards (even of a large %-age is of Precambrian stuff). Now I have to get into the Hughes problem, which also is the subject of my talk in Cambridge (on strength of which I got permission to go).

Please read through the attached MS of *Alnipollenites*, and see if you can 1./ understand; 2./ agree with my reasoning in it. I'd appreciate if you could give it some priority.

Best regards for both of you,

Jan. Jansonius

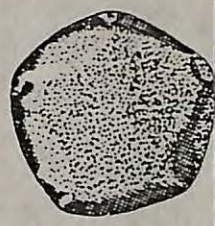
Hunt Institute for Botanical Documentation

ALNIPOLLENITES Potonié 1931 (May)

Jahrb. Preussischen Geol. Landesanstalt, Bd. 52, p. 4

Type species: Alnipollenites verus (Pot.) ex Pot., ibid.

Pollenites verus Pot. 1931 (April), Z. Braunkohle, Jahrg. 30, H. 16, p. 332, pl. 2, fig. 40



Diagnosis (Combined description, May 1931, in short-hand formula):

"Outline polygonal, lenticular in side view, surface ± smooth?, 4-5 protruding germinals, size 17-21 µ". Pl. Miocene; Germany.

Potonié 1960 (Synopsis III, p. 129) gives the generic diagnosis as follows:

"Lenticular shape, 4-7 (rarely 3) pores with labra on the corners of the polygonal to subcircular amb; a small vestibulum can be discerned; pores interconnected by arched folds of the exine (arcus); arcus running like garlands from pore to pore, on both proximal and distal faces; pores somewhat elongate meridionally (rimulae); exine with weak infratexture, surface ± smooth."

Remarks: Frederiksen & Ames 1979 (Taxon, v. 28, p. 557-561) argue convincingly that Pollenites was validly published in May 1931 (Potonié, cited above); in Potonié's earlier publication (April 1931, cited above) this generic name now is considered as not having been published in a valid manner. Consequently, the species assigned to Pollenites in this earlier paper also are to be considered as not validly published. Among the latter is "Pollenites verus", which species is validated in the May 1931 paper, when it is assigned to Alnipollenites. Because the species was not earlier validly published, Alnipollenites must be considered validly proposed with a combined description.

¶ In the May 1931 paper, Potonié refers back to the April paper (the species in which he considered at the time as validly published) and cites pl. 2, fig. 40, the first effective publication of this taxon. In April 1931, a photograph is given of a rather rounded specimen with indistinct arcus; In May 1931, the taxon is illustrated further by a line drawing showing very distinct arcus. In 1934 (Arb. Inst. Palaeobot. Petrogr. Brennsteine, Bd. 4, p. 58, pl. 2, fig. 13, 17, 18; pl. 6, fig. 28) Potonié illustrates more specimens, some with, others without distinct arcus. In 1960 (l.c.), he selects of these pl. 2, fig. 17 as nomenclatural type of the species verus. We do not here accept Potonié's procedure, which seems to run counter to the recommendations of the ICBN. Rather, we accept the first published figure, to which Potonié keeps referring in his subsequent papers as a part of his taxon verus; it is the only one element included in the first 'effective' description of the species, to which Potonié refers back in the first 'valid' description thereof.

Polyvestibulopollenites thus becomes an obligate junior synonym of Alnipollenites.

~~Neotypification~~

Hunt Institute for Botanical Documentation

4 January, 1980

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

We have researched the question you raised re IPC, and submit the following:

1-IPC (Tucson, 1962): Too long ago to be relevant.

2-IPC (Utrecht, 1966): 301 participants plus 37 "accompanying members"; 5 simultaneous sessions (plus, as always, one large plenary session auditorium).

3-IPC (Novosibirsk, 1971): Irrelevant because of special circumstances--very large attendance, mostly Soviet.

4-IPC (Lucknow, 1976-7): 280 participants; 6 simultaneous sessions (see xerox).

5-IPC (Cambridge,1980): 325 registrants as of this writing--expecting 500. At least 6 simultaneous sessions.

I guess that is enough. Perhaps you should get in direct contact with Norman Hughes if you aren't already, because 5-IPC would be more pertinent to 6-IPC than anything else would be.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology
President, ICP

AT/et
encl: xerox

Hunt Institute for Botanical Documentation

Nov. 79

Dear Al,

Thank you for the last several letters which I did receive. I've been very busy with a pressing report that now nears completion.

Your letter on the ICP mini-meeting in Dallas was received too late to allow for any action. However, I understand that John Utting is looking into the possibilities of Banff as a meeting place, and also Calgary. He'll be in contact again, but since he is the local ICP representative, he is now carrying the ball. I'll also phone some people, and in the next few weeks we should have a meeting on just this matter.

I also got your request on the Hughes rebuttal, and am willing to prepare that — although it is difficult to say anything more than Jim Schopf did. An interesting wrinkle is, that Hughes has struck some responsive chords in Frank Staplin's heart. It must be the heart; a critical brain could never accept his notions as progress. When should this rebuttal be ready?

Just read Frederiksen + Ames; I cannot but accept their findings; they checked into all the details and deserve credit for it.

I'll quit for now, but expect to write again in a couple of weeks. So long!

Jan Duncan

Hunt Institute for Botanical Documentation

XXXXXX

6 July, 1979

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

I enclose my current (1979) CIMP dues. According to Bernard Owens I am paid through 1978. Hope all is going well with you. Perhaps I'll have the privilege of seeing you in Dallas in the fall.

Best wishes to you and your family.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: check for $6.

Hunt Institute for Botanical Documentation

Sept 79

Dear Al,

I'm in the office just for a minute*, and want to ~~answer this way now~~, rather than postpone.

The Carboniferous slide set is sent out by Bernard Owens, as a revenue maker for CIMP. Details are in the CIMP Newsletters, which you should have; the set goes with a publication by

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G. Clayton + al. (1977).
(Mededel. Rijks Geol. Dienst, v. 29)
which is (was) also available
thru CIMP, with Maurice
Streel.

I should think both would
be still available. Good
luck, + best wishes

*/ I am on vacation, nominally.

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Eskimo
Fidelity Onion Skin
50% COTTON

xxxxx

5 September, 1979

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada
T2G 2B3

Dear Jan:

Bill Chaloner and I agree that you would be well suited to render our official opinion of the recent Hughes proposals (Taxon 27(5/6):497-504, November, 1978) for use in putting together our report for 1981. As you know, I am sure, Schopf before he died wrote his opinion (Taxon 27(5/6):505-512), but we need a second opinion, or more accurately, an opinion of an extant committee member.

You may write your opinion in any form you like. Presumably, it must deal with the JMS comments too. I will quote your (anonymously) opinion, and give the overall opinion when, later, we call for a committee vote. The anonymity will be strictly preserved, so you need not be concerned for NFH's reaction to what you write.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology
Secretary, IAPT Comm. for
Fossil Plants

AT/et
cc: Chaloner

Hunt Institute for Botanical Documentation

ESSO

IMPERIAL OIL LIMITED

500 – 6th Avenue S.W., Calgary, Alberta, Canada
T2P 0S1

EXPLORATION DEPARTMENT
C.R. EVANS
Vice President and General Manager

23 April 1979

Dear Al,

Thanks. you very much for your recent letters, mostly dealing with Cercosporites. I'm happy my conclusions were in line with your thinking.

What a marvelous surprise in the latest Taxon finding Jim's rebuttal of Hughes' proposals. That was an excelent critique; any further comment would be a waste of words.

I trust you received the information on the fungus Seminar? I did, although I shant be there.

A propos the Dallas meeting: don't spoil a "perfect record" for the reasons you gave: I'm sure that your presence will please many individuals, and vice versa. And why should you worry IF a few people in a sideroom will discuss the CFSP. On the other hand, I can understand if you start to tire a bit of these yearly

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AASP meetings. Each one of them certainly has some merit, but not every one is a special occasion any more, and the routine is becoming a bit boring. [Especially if, when you get to our age, it seems that no sooner is one done with than the next one is ready to go.]

I would have liked to attend last year's meeting, but am not particularly enticed by the idea of Dallas. Stan + Frank will go there for sure, to talk organic matter, and I don't know that it will be easy for me to get authorised for this one as well. We'll see what'll happen.

With best wishes for Betty and yourself,

Jan

Hunt Institute for Botanical Documentation

Dear Al,

A few days ago I sent you a m.s. genus card for Cercosporites. I asked your opinion as to how sound my reasoning was.

Just now I receive Taxon; on p. 488 Jim Schopf seems to object to the procedure I followed: "if there are no species, there can be no genus". But, the neotaxonomists say: not necessarily so. I, too, think that in this particular case, Cercosporites is valid as per 1903. Wodehouse 1933 has a similar case: a generic description, plus an unnamed species illustrated. I think Jim should have specified, whether he accepts the notion of an unnamed species in the context of his thesis. Too bad I can't ask him anymore! But the more I'd appreciate your thoughts on it.

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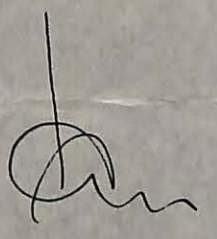
I received a bunch of information on the fungal spore seminar: 9-11 May, at Kent State. I trust Allan Graham sent you a similar sheaf.

All the best,



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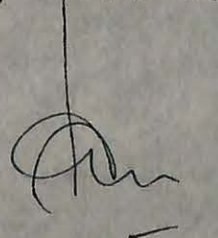
PS I just found the original for Cercosporites; I opened the envelope + enclose it.



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5780

1903
J. Bot., v. 41, p. 127-130

CERCOSPORITES SP., A NEW FOSSIL FUNGUS.

By Ernest S. Salmon, F.L.S.

In a recent paper entitled "Microflora e Microfauna nel disodile di Melilli in Sicilia" (Rendiconti della R. Accad. dei Lincei, vol. xi. 2 sem. 250, 251 (1902)) Dr. Pampaloni has described two organisms occurring in these "disodile" beds—a tertiary deposit belonging in all probability to the Middle Miocene—as fungi belonging to the *Erysiphaceæ*. Two genera are described, *Uncinulites*, with the single species *U. Baccarini*, and *Erysiphites*, with the species *E. Melilli*. As I have for some years studied the *Erysiphaceæ*, I was anxious to see, if possible, the fossil remains which have been referred to this family. I have to thank Dr. Pampaloni for his courtesy in allowing me to examine the two slides of the preparations of these organisms, and also for permission to publish the following observations on another fungus found in one of the preparations.

Erysiphites Melilli is described as follows:—"Perithecia sparsa, superficialia, sphæroidea, undique clausa, atra, coraceo-membranacea, 170 μ, mycelio arachnoideo, persistente, appendicibus ramosis, flexuosis, sordide luteis, numerosis." The type-slide sent to me contains one example of the organism described above. This single example consists of a globose body, the diameter of which measures 180 μ. From the periphery of this body, which is of a dark brownish colour, numerous pale brown septate threads radiate. These threads are 2–3 μ in diameter, and have all the appearance of mycelial hyphæ. But there are, I think, reasons to doubt if the central body is a perithecium. In examining the specimen with a high objective, a slight pressure on the cover-glass caused the globular body to begin to separate along several lines. Neither after this occurrence—as well as before it—could any cellular structure be observed in the "perithecium," and in my opinion the central globular body consists of but a single cell. Further, the bounding membrane of this cell seems to me to resemble that found in animal rather than in vegetable structures. In the same slide, it may be noted, three other globular bodies occur, of the same colour and of about the same size as that described above. These three bodies are all quite naked, being without any trace of fungal hyphæ. One of them, except in this absence of the peripheral hyphæ, resembles so closely the individual first described above that it must, I think, be regarded as being of the same origin. The two other bodies show a more transparent and apparently chitinous membrane, and resemble closely the eggs of a mite, or some such animal. It is quite possible that the single example first described above may be of similar origin, and that the investing fungal hyphæ are extraneous. Whatever be the correct interpretation of these bodies, *i. e.* whether they are of animal or vegetable origin, they cannot, I consider, be regarded as perithecia, and have therefore nothing to do with the *Erysiphaceæ*.

Uncinulites Baccarini is described as follows:—"Perithecia subglobosa, tenui membranacea, nigra, astoma, 80–85 μ, appendicibus

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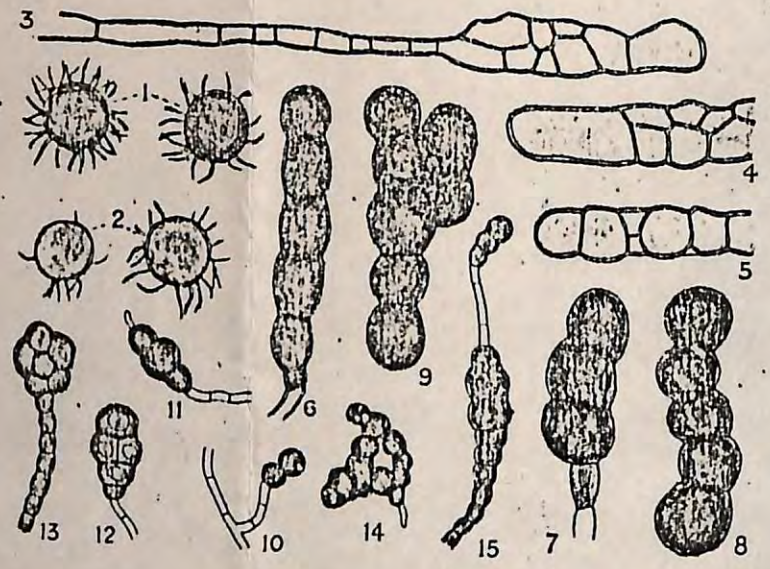
18–25 simplicibus apice uncinatis perithecium fere æquantibus, indivisis, ad apicem fuscis ad basim atris."*

The type-slide of the above is of extreme interest, since it contains a number of well-preserved fungus spores and fragments of



Explanation of Figures. — Fig. 1. Two examples of *Uncinulites Baccarini* Pampaloni, × 400. Fig. 2. Two younger examples of the same, more transparent, and showing that each individual consists of a single cell, × 400. Fig. 3. Mycelial hypha giving rise to a plate-like expansion × 400. Figs. 4, 5. Portions of similar expansions × 400. Figs. 6–9. *Circosporites* sp. 6, 7. Mycelial hyphæ giving rise to chains of enlarged subglobose cells, which probably function as sclerotia × 400; 8, 9, loose clusters of similar cells × 400. Figs. 10–15. *Cercospora acerina* Hartig; groups of "Dauermycel" × 220 (from Hartig, *l.c.* Taf. iv, fig. 6).

mycelium. First, as regards the objects called "*Uncinulites*." These are small globose bodies measuring 20–22 μ in diam., covered, when ripe, on the outside with straight or curved filiform or spine-like projections—the so-called "appendages"—which attain sometimes to half the diameter of the globose body (figs. 1, 2). Both the central globose body and the projections are brownish in colour. These globose bodies, which are termed "perithecia" by Pampaloni in the diagnosis given above, are in the majority of instances so darkly coloured as to be rendered opaque (fig. 1); in a few cases, however, individuals—evidently in

* This is an emended description sent to me by Dr. Pampaloni; in the original the words, through an error, run "appendicibus simplicibus, 18–25 om. longis."

a younger stage of development, as is shown by the almost complete absence of projections—occur which are more transparent, and in these, under a high power with a strong illumination, it can be seen that each globose body is not a compound mass of cells, but a *single cell* (fig. 2). It seems clear, therefore, that each is to be regarded, not as a perithecium with appendages, but as a single spore with a spinous epispore. On the precise determination of these spores I am not able at present to throw any light. It may be pointed out further that neither in the small size of the supposed "perithecium" nor in the nature of the "appendages" do the present objects bear the slightest resemblance to any of the species of the genus *Uncinula* of the *Erysiphaceæ* (cfr. Salmon, Monogr. of the *Erysiphaceæ* (Mem. Torr. Bot. Club, ix. 79–120 (1900)); and "Supplementary Notes, &c." (Bull. Torr. Club, xxix. 96–103 (1902)).

In the same mount with the "*Uncinulites*" fragments of mycelium and chains of globose spore-like bodies occur. These mycelial hyphæ are 5–8 μ in diam., and rather closely septate. They can be seen clearly giving rise to rows of enlarged more or less globose cells, which are at first somewhat hyaline (so that the wall of each cell is apparent), but which become finally more or less opaque. The enlarged cells measure from 15 to 23 μ in diam., and sometimes occur clustered, as shown at fig. 9. In the same slide similar hyphæ are found which give rise to expansions two cells wide in places (figs. 3, 4, 5). It is possible that these are the early stages in the formation of the clustered enlarged cells (cfr. figs. 3, 4, 5, and 9).

These hyphæ agree in the production of enlarged opaque spore-like cells so closely with those of *Cercospora acerina*, described by Hartig (Untersuch. aus dem forstbot. Institut zu München, 1, 60 (1880)) that the present fungus may safely, I think, be referred to this genus. Hartig (l.c.) thus describes this stage of development in *C. acerina*:—"Nachdem die Conidienbildung beendet ist, sieht man das Mycel hier und da anschwellen und eine zunächst bräunliche Färbung annehmen. Die Umwandlung des Mycels zu einem fädigen Dauermycel, zu der einfachsten Form der Sclerotien, schreitet fort, bis die ganze Blattsubstanz von zahlreichen Ketten und Gruppen desselben durchzogen ist. Die Zellen des Mycels schwellen bei dieser Umwandlung zunächst an. Es finden weitere Zelltheilungen theils parallel den ursprünglichen Scheidewänden, theils rechtwinklig zu diesen statt, so dass zuweilen complicirte Gewebekörper entstehen. Im Allgemeinen bleiben aber diese Dauerzustände einreihig und es sind immer nur kleinere oder grössere Gruppen von Zellen, welche die Umwandlung erleiden, während die sie verbindenden an der Umwandlung nicht theilnehmen, später absterben und aufgelöst werden." The figures given by Hartig (*l.c.* Taf. iv.) of the enlarged cells, which function as sclerotia of the mycelial hyphæ of *Cercospora acerina*, agree closely with those drawn from the fossil examples occurring in the slide sent by Dr. Pampaloni, as may be seen by comparing figs. 6–9 with figs. 10–15.

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I wish to express my obligation to Mr. George Massee, F.L.S., for having directed my attention to Hartig's figures.

The following diagnosis may be given of this fossil fungus:—CERCOSPORITES sp. Hyphæ myceliares filamentosæ singulatim repentes dilute brunneæ septatæ 5–8 μ diam. hinc inde in cellulas magnas 15–23 μ diam. maturitate opacas atro-brunneas plus minus globosas 3–6-catenulatas vel raro biseriatim aggregatas probabiliter pro sclerotiis habendas subito inflatæ.

KENT RUBI.

By E. F. LINTON, M.A.

THE notes which follow record the Brambles observed by Dr. D. T. Playfair and myself, including some detected by Dr. Playfair alone, in July, 1901. All have been submitted to the Rev. W. M. Rogers, F.L.S.

The localities are mostly additional to the *Flora of Kent*; they are all within about three miles of Bromley, and therefore in District 1, to which some (marked *) are apparently new. None are new to the county, unless one excepts *R. Gelertii*, of which, however, the variety *criniger* Linton has been recorded by Mr. Rogers for District 8 (and see p. 93).

Rubus Rogersii Linton. Farnborough Common; and also, I think, Hayes Common, the only locality in the county previously recorded, but we failed to take specimens hence.

R. carpinifolius W. & N. Bromley Hill Woods.

R. pulcherrimus Neuman. Farnborough and Hayes Commons.

**R. Selmeri* Lindeb. Keston Common and Hayes West Hill.

**R. argentatus* P. J. Muell. forma. Barnet Wood.

R. Sprengelii Weihe. Barnet Wood and Farnborough Common.

**R. micans* Gren. & Godr. Keston Common.

**R. Gelertii* Friderichsen. Farnborough Common.

**R. infestus* Weihe. A very strong form. Hayes Common. Farnborough Common.

R. Babingtonii Bell Salter. Bromley Hill Wood and Farnborough Common.

**R. scaber* W. & N. Hayes Common and wooded roadside on the way there from Bromley.

R. pallidus W. & N. var. **leptopetalus* Rogers. On a bank in private grounds near Bromley Hill Wood ("apparently *leptopetalus*," W. M. R.). Farnborough Common. The former looked to me like ordinary *R. pallidus* W. & N., and I recognized it at once. The latter plant, with neater and smaller habit, narrower leaves and petals, seemed a new form to me, and I owe the name to Mr. Rogers, who recognized it as exactly his variety; not given in the *Flora of Kent*, but noticed by the Rev. W. M. Rogers, pp. 94, 95.

R. foliosus W. & N. Westmorland Road, Bromley, and Keston Common.

R. rosaceus W. & N. var. *infecundus* Rogers. Barnet Wood. Bromley Hill Wood. Keston Common.

Hunt Institute for Botanical Documentation

2 April, 1979

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada
T2G 2B3

Dear Jan:

I just got your additional supplementary info on Cercosporites. There is no question in my mind that this is a perfectly validly published description of a new genus and, providing that it has priority, and so forth, it is a name to be reckoned with. Jim Schopf, rest his marvelous spirit, was good at nomenclature and taxonomy but not perfect--who is? Since 1958 and the requirement of a type, a generic name is not validly published unless it has at least one validly species within it. That is quite clear. However, before 1958 it was quite possible to publish a generic name validly without any included species. If this were not the case a great many of the best known names in neobotany would be in danger. Still, what Jim says per your quote (I don't have the paper with me here) is correct as far as it goes: "if there are no species, there can be no genus". It is true that a genus described without any species can be presumed to have species! However, it is not required that any of these have been validly published until 1958. I once made a special trip to the Smithsonian to fight this out with various nomenclaturalists down there. I went down convinced that it was an impossibility to have a genus with no species in it validly published, but came away realizing I was wrong or, at least, my rigorous point of view is not practical.

Best wishes as ever.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

23 March, 1979

Dr. Jan Jansonius
Esso Resources Canada Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada
T2G 2B3

Dear Jan:

My efficiency in getting around to your letter of 12 march astonishes me. It arrived on my desk only yesterday!

I have read your MS on Cercosporites Salmon 1903, and I concur with your interpretation of the facts. It is quite clear that it was not necessary to include named species in a newly described genus in 1903. Hence, there is certainly no reason to doubt the valid publication of Salmon's name. Of course, until somebody does designate at least one species, there won't be much action in Cercospofites!

I am probably out of line in making such a suggestion, but if I were doing it I think I would omit your last paragraph beginning with "Both Salmon and Stopes....".

Your information about CFSP is interesting. Perhaps another reason for me not to attend the Dallas meeting. I am not very happy with AASP at the moment for a number of reasons and had been contemplating absenting myself for the first time since I helped organize the association in Tulsa in 1967;(I believe I may be the only person who has never missed a formal meeting of AASP). I am unhappy that AASP has been giving the ICP Council such a hard time for no real reason except harassment. I am also unhappy that the board of directors and officers were so busy awarding each other plaques at the formal luncheon in Phoenix that they were unable to recognise the presence in the room of the President and Secretary-Treasurer of the ICP, both former presidents of AASP and hence not exactly outsiders. It was really insulting.

The story on CFSP must be clear to anybody who can analyse obvious data. The key person in CFSP was Traverse. Spackman did not at any time see it that way and preferred to go with Ames, apparently believing that I had been kidding him all those years when I told him that I was carrying the thing on my back. Spackman had his choice of Traverse or Ames and he picked Ames. I have plenty of other things to do and actually enjoy not having Mr. Ames hanging around my office and lab all the time taking up the time of my graduate students and assistants.

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Besides, I had heard most of his jokes three times.

All the best to you.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

12 March 1979

Dear Al,

Thank you very much for your letter of 23 febr. — I now understand better your situation with regard to the CFSP. It is a pity that such situations develop.

A day after your leter I finally heard from Tate, after I had succeeded in needling him. Apparently, a meeting is to be organized for the Dallas ASP Meeting, and the Editorial Committee will discuss the problems that now involve the CFSP. We shall see how these things will turn out — I have a feeling that the CFSP is moribund; for all practical purposes already it has ceased to exist.

Talking about something completely different: I enclose a copy of my m.s. on Cercosporites. I had wanted to enclose as well a copy of Salmon 1903 (so you could check my translation of his Latin diagnosis), but must have mislaid it, for I cannot find it anymore. From the information on this genus card, do you consider that I judged correctly on the validity of the genus?

I trust you received the CMP money order.

Best regards to both of you,

Jan. Jansonius

Hunt Institute for Botanical Documentation

CERCOSPORITES Salmon 1903

J. Bot., v. 41, p. 130, fig. 6-9
Type species: none designated so far
Diagnosis (descriptio generico-specifica?):
"Mycelium consisting of pale brown, septate, filamentous hyphae that grow individually and have a diameter of 5-8 μ; at more or less irregular intervals the hyphae may suddenly inflate into larger, more or less globose cells of 15-23 μ diameter which - when mature - are opaque dark brown and aligned into chains of 3 to 6 cells or rarely into biseriate aggregates, and which probably function as sclerotia."

Fig. 6

Remarks: Salmon provided a Latin description ("diagnosis") of his fossil fungus "Cercosporites sp.", but did not give it a specific epithet. He also illustrated the fossils, together with an illustrated discussion of Cercospora acerina Hartig, with which the fossils are compared.
It is here considered that the generic name is validly proposed in 1903, complying with Arts. 41 and 42 (before 1908, an illustration with analysis is accepted as equivalent to a generic description), and in a way also satisfying the requirements of a descriptio generico-specifica, even if the sole new species described was not named at the same time. The illustrations are clear; the material illustrated was observed on slides of Pampaloni, who were sent on loan to Salmon. (Because we do not know if the type slide still exists, we have refrained from naming the fossil species described by Salmon. JJ)

In 1913, M.C. Stopes [Catalogue Mesozoic Plants, Brit. Mus. (Nat. hist.), V.5 Cretaceous Flora. I; p. 276] used the name "Cercosporites, nom. nov.", to which she assigned the megascopic Cretaceous fungal remains Cercospora coriococcum Bayer 1899, which consist of concentric infestations of several mm diameter on leaflets of Dewalquea coriacea. Her diagnosis is inadequate ("Presumably with the characters of the living genus Cercospora, but incompletely known.") and makes it doubtful that she validly published that name. If so, it would be illegitimate, as a junior homonym of Cercosporites Salmon 1903.

Both Salmon and Stopes followed ~~uncritically~~ the fashion of their period to coin new names by using the suffix -ites added to the generic name of modern counterparts to which they assumed that their fossil material was related. Rather unintentionally, but not any less effectively, in this manner a number of names were created that must be considered as validly published in the sense of the I.C.B.N.

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From Stopes 1913

OTHER PHENOMENA DESCRIBED AS DISCOMYCETES :—

Genus **HYSTERITES,** Meschinelli.

[In Saccardo, Sylloge Fungorum, x, p. 774.]

Hysterites protogæus (Heer), Meschinelli.

1882. *Hysterium protogæum*, Heer, Flora foss. Grönlands, p. 20, pl. xxiv, figs. 9, 9 *a*.
1892. *Hysterites protogæus*, Meschinelli, Syll. Fung. foss., p. 774.
1902. *Hysterites protogæus*, Meschinelli, Fungor. foss. omnium Iconog., p. 24, pl. xv, figs. 6, 6 *a*.

HORIZON.—Upper Cretaceous (Atane Beds).

LOCALITY.—Greenland.

TYPE.—Heer's specimen in University Geological Museum, Copenhagen.

The twig on which the so-called fungus was detected by Heer (1882) appears to be that of a dicotyledon, which though it is not described by Heer is very common in the Greenland beds. The one specimen with the "fungus" which Heer mentions, he supposes to be a gymnosperm; but even superficially it appears much more like a dicotyledonous twig. In Stockholm, in Prof. Nathorst's rich collections of Arctic plant-fossils there are a number of similar stems which all show the small markings Heer took to be a fungus, but which bear the strongest likeness to lenticels.

Hysterites protogæus, in short, seems to be nothing but normal lenticels on woody twigs.

Order HYPHOMYCETES.

Genus **CERCOSPORITES,** nom. nov.

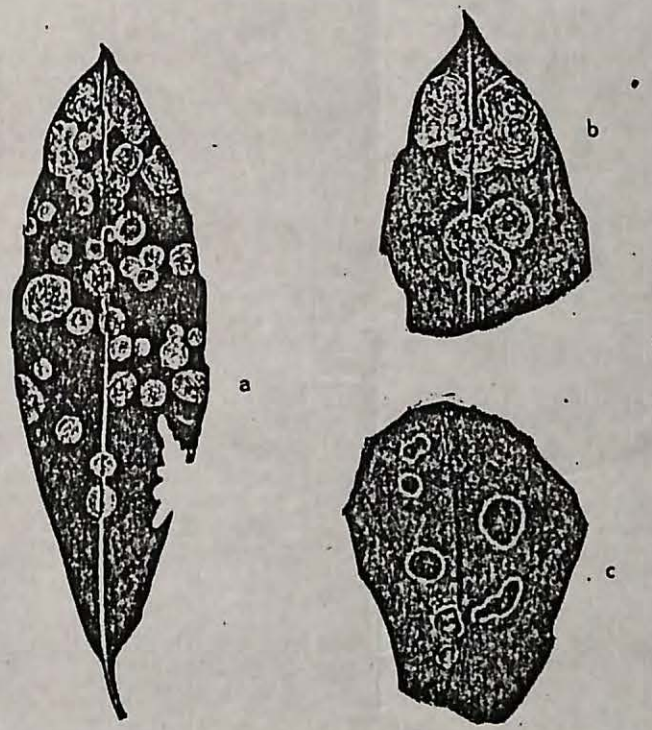
Presumably with the characters of the living genus *Cercospora*, but incompletely known.

Cercosporites coriococcus (Bayer), nom. nov.

1899. *Cercospora coriococcum*, Bayer, Sitzungsber. k. böhm. Ges. Wiss. Prag. p. 4, text-figs. 1, 1 *a*, 2.

1901. *Cercospora coriococcum*, Bayer, in Fritsch & Bayer, Studien böhm. Kreidepfl., p. 66, text-fig. 2.

The fungus forms roundish, sharply delineated patches, generally distinct from each other but sometimes merging.



Text-fig. 24.—*Cercosporites coriococcus* (Bayer). *a*. Leaflet of *Dewalquea coriacea* covered with the fungus, natural size. *b*. A small portion enlarged twice to show the surface character of the fungal patches. *c*. Portion of leaf of *Hedera primordialis* showing the same fungus. After Bayer.

These are 2–5 mm. in diameter, with a surface marked by series of concentric striations; in some cases the centre is slightly depressed. These patches lie both on the leaf-lamina and over the central nerve.

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March 2, 1979

Dr. A. Traverse
529 Deike Building
University Park
Pennsylvania 16802
U. S. A.

Dear Al,

I decided to send the enclosed money order for U.S. $100, as dues from CIMP to CIP, directly to you, rather than to the bank. It seems to me that there is less confusion about this type of deal if one is there in person.

With best regards, and looking forward to the voting results,

Jan Jansonius

JJ/ap

Hunt Institute for Botanical Documentation

23 February, 1979

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada
T2G 2B3

Dear Jan:

I am writing this on a piece of paper at the breakfast table, having just perused the neat "key" and penned in the corrections (actually, addenda)--I noted you had already done one of them! I also greatly enjoyed your enclosed letter.

Speaking of which---Long, long ago a famous member of our profession from another country was offered the job here that I eventually got. He turned it down because, as he told me, he could not, among other things, face several decades of working with Tate Ames. When I came here Gerhard Kremp said the key to making the CFSP go was to get rid of Ames. I served one decade in the position of HTA's rectifier, and that was quite enough, thank you. I offered to stay with CFSP, nay, expand its operation, if HTA could be sent out to work on his bowling alley and investment interests full time. Over a period of several years I told Spackman this was an eventuality non-negotiable demand. After I had served a reasonable sentence of ten years in that impossible situation--responsibility for the operation but no authority, I told Spackman I could tolerate it no longer and I got out. I have almost no regrets. I have had the key to the palynological laboratory changed so that Coal Research (i.e. HTA) has no automatic entrance to my lab or office and have severed all connection with the Coal Research Section. It is liberating! I know damned well CFSP depended entirely on my direction for every correct decision it ever made (including its foundation in the first place). Most of the incorrect decisions were despite my best efforts to stop them. If others didn't appreciate these matters, perhaps they now will.

I am trying to get info on the Kent State seminar, but so far nothing if forthcoming. I would like to go.

Best to B.

Yours very truly,

AT/et

Alfred Traverse
Professor of Palynology

Hunt Institute for Botanical Documentation

9 Febr 79

Dear Al,

It was a pleasure to receive a letter from your hand again — or better: from your mouth and Betty's hand.

It may be a pity that the Soviet nominations did not get in in time: I puzzled a bit over the pro-"western" skew of the list. Appearances sometimes are judged, too.

I almost did nominate Bill Schopf, but am not personally convinced that he has much interest (as yet) for the not always exciting drudgery of nomenclatural puzzles.

Re the CFSP — didn't I tell you that Vol. 41 was rather inconsistently put together; in fact, after I had spent a few evenings with it, it was clear that it was not simply a host of typos, but a

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a lot of rethinking of nomenclatural matters; on the latter score I had differing opinions, but as well could point out internal inconsistencies where mut. mut. identical cases were treated variously by Tate. I never did get a thank-you for my efforts. But that is what an editorial member must do, ain't it? Too bad you want to remain out of the thing. If it was worth doing in the first place, it should be worth keeping up. And I think the project certainly could stand your contributions, especially where your position almost implies involvement. Noblesse oblige, eh, monsieur Traverse?

I think the fungal spore seminar (that's a word you should like better!) is worth your attending at any price. I learned as much in Houston, as I did at Stanford on dmos. Because I had the thing already, "in house". Fat chance that ill be at Kent State.

Best regards, Jan Janssonius

Hunt Institute for Botanical Documentation

1 February, 1979

Dr. Jan Jansonius:
Imperial Oil Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada
T2G 2B3

Dear Jan:

Your letter of 10 January arrived in the nick of time to get your nominees on the IAPT ballot! (Some Soviet nominations just came in too late--hmmm.) I agree with your nominations--very good! I wonder how the vote will go. I doubt Bill C. is right that one ballot will do it!

Re Jim Schopf's resource library: I put in a word for the nomenclatural parts of it when I was in Columbus for the funeral. I have again brought it up in letters to Esther (his widow) and to Bill. I believe I had better leave the matter there for the present!

Re CFSP: I know nothing at all since I resigned, about two years ago. I do know (and am somewhat amused) that Vol. 41 is still not out;(nothing has happened since I quit?). Sometime I'll discuss the history of the situation with you. I am trying to keep out of it entirely.

I hope to attend the fungal "workshop" (how I hate that word), if it doesn't cost an arm and a leg. Perhaps you'll be there !?

Best regards to you and your family for 1979!

Yours very truly,

Alfred Traverse
Professor of Palynology
Secretary, IAPT Committee
for Fossil Plants

AT/et

Hunt Institute for Botanical Documentation

26 Sept '78

Dear Al,

It was with such pleasurable anticipation that I started to open your latest letter — I had not heard from you for some time — and thus the shock was so much sharper to read that Jim has left us. I had much anticipated to meet him in the near future and then discuss things, and felt always quite close to him — even if we met only a few times. But then, he was such a generous writer.

Well, his end is a memento mori that I could have done without. Even if we'll all pull a bit harder, it shall not be easy to compensate for his loss. I hope Jim's collection of old editions of the Code and related texts will be (made) accessible somehow?

write Esther

Thank you, Al; best regards,

Jan Tanrosius

Hunt Institute for Botanical Documentation

12 June, 1978

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada
T2G 2B3

Dear Jan:

Thank you so much for yours of 29 May. Bebout's Ph.D. thesis was done here under my direction, but the halcyon days of yore when I had numerous copies of theses and could distribute them for years after the fact have long since come to an end. I have only my own shelf copy, for which there is a pretty regular need for student use. The correct way for you to get a copy of this is to have your librarian make a request for inter-library loan of same from the Earth & Mineral Sciences Library, Deike Bldg., etc. (same zip, etc. as my address)--you've been there! If you have any trouble at all getting it, let me know and I can smooth the way, but I think it will be routine and will cost nothing.

Glad to hear about the further updates for the generic file. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

7 June, 1978

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

No need to clutter up your files with still more stuff about the names we've already discussed. I did want to make the point though regarding your statements on Trisaccus vs. Tetraporina that you don't seem to comment on the major point I was trying to make. I applied the test of whether the names were used in binomials at all. I did not accept Trisaccus because Sah did not make any effort to use the names in binomials whereas Tetraporina was so used, if I recall correctly. I am dictating this hurriedly while away from the office, but that's the way I recall the situation. I didn't think that whether the author referred to them as "sub-groups" was critical providing that the author obviously intended to use the things as generic names. What do you mean by "Art. 34(b)"? I admit that you have a fairly good point in urging that Naumova's names were merely provisionally proposed. However, if you will examine the examples given under Article 34 I believe it is quite clear that as brief as what Naumova presented is, it is a great deal more than the "incidental mention" to ~~what~~ which Article 34 refers.

That brings us to van der Hammen, 1956. You asked me to comment on page 69, paragraph 4. The problem is that van der Hammen didn't really know what he was doing. He says that his "spore system" is just a proposal. That would seem to be somewhat different from saying that the names are themselves merely proposed. Indeed the information on 69 all has to do with what he thinks the system should have in it.

The truth is, I'm afraid, that Article 34 is a pretty slippery slide. I believe the long and the short of it, based on the procedures of neobotanists at least, is that incidental mention must be just that in order to invalidate the publication of a name. Another way that invalidation can be assumed under Article 34 is when the author specifically states that he does not accept the names in question. I am firmly of the impression at this moment that van der Hammen's statement on page 69 of his 1956 paper does not constitute a rejection of the names by the author himself nor state that they are merely

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providional. It is the system which he regarded as provisional, or at least that is my best interpretation of what he says. Truth is that he was somewhat confused about the relationship between a nomenclatural system and names, but one must avoid being too rigorous. My philosophy has always been to accept names as validly published wherever possible. All the best to you.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

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EXPLORATION RESEARCH DIVISION
EXPLORATION DEPARTMENT

May 29, 1978

Dr. A. Traverse
529 Deike Building
University Park
Pennsylvania 16802
U. S. A.

Dear Al,

We read a reference to a thesis by J.W. Bebout (Golden Valley Fm., N.D.), and should like to have a look at the plates and geological setting. Do you think we could borrow a copy from you or Penn State? You can charge any costs, of course.

The second supplement (88 cards) to the Genera File is at the printers; it should be ready for mailing in 4 weeks. And already we have some 15 new genera (mostly Chinese) for a third update!

With best regards for Betty and yourself,

Jan Jansonius

JJ/ap

Hunt Institute for Botanical Documentation

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EXPLORATION RESEARCH DIVISION
EXPLORATION DEPARTMENT

May 15, 1978

Dr. A. Traverse
Palynology Lab./Deike Bldg.
University Park, Penn. 16802
U. S. A.

Dear Al,

Thank you for your letter and comments, which came in the nick of time - I just managed to incorporate some of your better points in the next Supplement.

However, your letter shows that we are near the limit of resolution of our problems:

#3426 Trisaccus you reject because proposed as a "sub-group". Sah, in his abstract states that he is following "Naumova 1937".

#3416 Tetraporina you accept as validly published in 1939 by "Naumova 1937", who proposed all names of generic rank as "sub-groups".

I said for Trisaccus to doubt that the description following the sub-group name pertains to it; it rather details an unnamed specimen. If so, the name lacks a diagnosis and cannot be valid. However, I think it is sophistry to deny the name only because it was labeled "sub-group", in imitation of Naumova.

On the other hand, I do not accept Naumova's names as validly published in 1939, because it is clearly a preliminary paper in the sense of Art. 34(b). Several of her names are "illustrated" by specimens that are at variance with the eventual generic content. I think it a great pity that the war intervened and denied her the proper follow-up of her work.

.....2

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#3413 Striatriletes. I cannot now find evidence about V.d. Hammen's month of publication. Potonié (Synopsis 4, p. 57) states that the publisher could not provide that month. I have written the author for information. (I think I was misled by a statement in Van der Hammen & Wijmstra 1964, p. 233, l. 20-21.)

Anyway, if you have Van der Hammen 1956, please read p. 69, paragraph 4. From it, it seems that the author gives his names "only as a proposal", and that they may not be valid as of 1956 per Art. 34. Please read that and give your comments.

#3408 Sporangites. The species papillata is only implied as being the type, because under this name Dawson sets out its description with the words " I propose the provisional generic name" etc. etc., see card. Here I have taken that (in 1863) "provisional" means artificial, and does not refer to Art. 34 in later Codes.

#3373 Nevesisporites. Yes, I knew as soon as I went to file the update cards. I should have checked.

#3355 Lophozonotriletes. The several n. spp. assigned by Naumova 1953 are not together in the text, but occur on several widely separated pages.

#3321. Collarisporites. Yes! Sorry.

#3295. Thank you for catching "Kreger" - that makes you even for "Dieke".

#2312 Quadrisporites. You are entirely correct; my text must have been written years ago and, once buried in the mass of separate cards, it was not found before you caught it. So, there's no argument, I agree fully. The year nominally is 1958, as also given in later publications (by Balme, e.g.), but on our reprint copy it is indeed given as "1958(1959)". I'll follow your 1959, although for priority reasons it makes no difference.

Thank you for your interest, and please keep sending in this type of comment!

Best regards,

Jan Jansonius

JJ/ap

Hunt Institute for Botanical Documentation

1 May, 1978

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Random comments on "recently" arrived supplemental cards, and a few others:

#3426: Trisaccus Sah. Contrary to your statement, and in keeping with previous correspondence between us, the validity of publication would not depend on there being a described species, and the description is probably also o.k. per Art. 42. However, I would not accept this name for the reason you dismissed--it is not at all clear that he meant this as a generic name--none of his "sub-groups", new of old, is associated with a specific epithet, and I would require that at least one be so combined to assure he meant to use the name, as if parts of (potential?) binomials. In fact, it seems pretty clear from careful reading of Sah that he was purposely avoiding "proper" generic names.

#3416: I treat Tetraporina as validly published by Naumova in 1939. Why not? The lack of species is irrelevant. She did use some species. Per previous correspondence between us, hundreds of familiar genera of plants were published the same way--description only, no species. This, of course, changes the type species business too--I would accept T. antiqua Naumova per Potonie's designation. But I readily admit this is a difficult decision!

#3413: How did you ascertain that van der Hammen "1956" was later than Potonie 1956? You give the month for Potonie but not for van der Hammen. Can you provide a month for him?

#3208: I wrote quite a manuscript on this once but can't find it now--my conclusions were similar to yours, except that I seem to recall that I decided Sporites was duly typified and cannot be separated from its type. If Sporangites' type is agreed to be also S. papillata, then Sporites has the same type and has got to be a synonym, whether Sporangites is "rejected" or not. On the other hand, if the concept typified by S. papillata is a good one, its generic name can only be Sporangites.

#3373: Has somebody already told you of a homonym: Nevesisporites de Jersey & Paten?

#3355: Do you mean to imply that Potonie designated a lectotype for the genus? That is certainly what your asterisks imply:

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" * designated by ....". My recollection is that the genus was monotypic. Genera are not (not, never) typified by specimens.

#3321: I suppose you have discovered that this is a homonym of C. Deak 1964?

#3295: Krieger is misspelled on this page.

#2312: Your statement that Hennelly did not validly publish because of failure to designate a type is not right--the type is a species--and that was designated. It makes no difference that that species itself was not validly published for lack of a clearly designated type. That's not relevant--the type of the genus is the species. I do not agree that strict application of the rules means the generic name was not validly published in 1958. The type must be clearly designated but not necessarily validly published. Also, I have the date as 1959. Do you have a reason for 1958? (I am not writing where my files are.)

Hang in there! All the best to Stan, etc.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

25 March, 1978

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. SE
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

Sorry it's taken me so long to answer yours of 6 Feb. I would have thought we had long ago discussed the question of the effect of a not validly published type species on the valid publication of the generic name involved. It is very clear that although a type must be designated, and the type for a generic name is, of course, a species, there is no necessity that the species be validly published. It's irregular but still perfectly valid. As was pointed out to me by Stafleu some years ago, if this were not the case, some hundreds of the most important names in neobotany by DeCandolle, Hooker, and others would not be validly published.

Regarding the statement in Cornet and Traverse, 1975, about Zauer's specimens, I'm afraid you are stirring up a small can of worms on which I thought I had the cover securely tightened. That statement in the paper came from my interpretation of correspondence with the Russians. Would you please see Geoscience and Man, vol. 15 (1976), page 141. That will answer your question.

Glad to hear the news about the Corrugatisporites business. I'm sorry that it was Srivastiva if you mean Smith, who got the credit for fixing it up! I have laterally been going around with him about his loose use of the word "microspore" for homospores, pollen grains, as well as presumably genuine microspores. I find him slightly painful in the posterior as he no doubt does me.

I am doing o.k. but not getting much palynology accomplished. What I am attempting to do here is of course a juggling act, and there is a great deal of question as to how long one can keep that sort of thing up. I sometimes feel very weary. Perhaps you would be interested in the enclosed clipping.

Regarding the new Code, I know nothing about when it

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will be out, excepting that it is too far along in the printing process to make any changes in it. My knowledge of this came about through correspondence with Ed Voss regarding the change I thought we had made in eliminating organ-genus. That sneaky Jim Schopf at the editorial conference managed to get the business about family relationships stuck back in, and it seems to me to be a pretty bad deal. How it will all come out in the end I don't know. I believe at the moment I would opt for eliminating any reference to fossil forms being different from any others and just have genera, period! You will undoubtedly be hearing more about this in the future. Thank you for your kind regards and best wishes to you for a happy Easter season.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

encl: clipping

Hunt Institute for Botanical Documentation

6 febr '78

Dear Al,

Haven't written for ages; too busy, I guess. Lately, I have begun to go through the Genera file (I made a key to the angiosperm genera) and came across some minor points.

I enclose a Xerox copy of some new Chinese entries. How invalid should a type species be, before its status affects the validity of the generic name? Do you agree with my interpretation of Zonorapollis? (There are a few more like it).

Cornet & Traverse 1975 refer to some specimens of Corollina which Zaver reputedly gave to Klaus who selected "Hilfstypus" from them. However, I can only find that Klaus referred to a specimen illustrated by Balme as "Hilfstypus". Any comment? I'm afraid I missed out on something that should be mentioned in the File.

Pacltová 1969 published a genus "Krutzchiperipollis", obviously named after Krutzsch. Should one leave such an orthographic mishap, or should it be corrected? The Code really instructs not to change intentional latinisations of names; by implication it appears that

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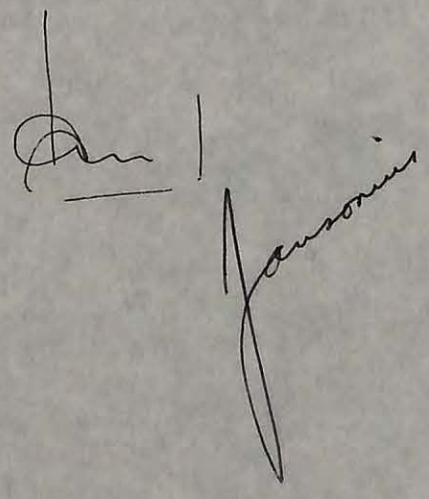
changes in orthography that are not latinisations, must be changed. Would you agree with the latter interpretation, and consequently to a change of Krutzchiperipollis into Krutzschiperipollis? after I had sealed the envelope I double checked the name, it turns out I got it wrong in the genera file. So forget about this one!

Our old m.s. dealing with Corrugatisporites - Trilites - Lygodioisporites has become obsolete, following Srivastava's raising to generic rank (1977, Paléobiol. Contin., v.6, no.2, p.23) of some subgenera proposed by Juhász 1972 (Acta biologica, n.s., v. 18, p.48). A good thing we did not get that through, it would have been quite a muddle sorting everything out. So, when Elsevier asked some months ago what to do with our m.s., I told them to toss it.

And finally, how are you doing? Does the world of palynology keep you very busy? Do you know when the new Code will be out?

I'll sign off for now; there's many small things that still need to be tended to.

Best regards, to Beth, also,



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EUPHORBIACITES Sung & Lee in Sung, Lee & Li 1976
In: Mesozoic Fossils From Yunnan; fasc. 1, p. 50 (Nanking Inst. Geol. Palaeontol., Academia Sinica; Science Press, Peking).

Type species: Euphorbiacites wallensenensis (Pflug) n. comb. ex hoc loco
Tricolporopollenites wallensenensis Pflug (in Thomson & Pflug) 1953, Palaeontographica, Abt. B, Bd. 94, p. 103, pl. 13, fig. 1
(Sung & Lee did not validly propose the new combination)

Diagnosis: 属征 花粉粒椭球形。具三孔沟，沟长达两极，孔大而圆、明显。外壁较坚固，棒至网状纹饰，总是显网状图形。
时代 新生代。

Description of type species: 35-50μ; figura (shape) broadly elliptical; exopore extends beyond caverna; cavernea run all the way to the poles; endopore very small, circular; sculpture consists of closely spaced bacula of ca. 2μ high and ca. 1μ wide.

Age:

Zonorapollis Sung & Lee in Sung, Lee & Li 1976
In: Mesozoic Fossils From Yunnan; fasc. 1, p. 50 (Nanking Inst. Geol. Palaeontol., Academia Sinica; Science Press, Peking).

Type species: Zonorapollis brevicolpatus Sung & Lee (m.s.!, unpublished report), textfig. 4

Diagnosis: 属征 花粉粒具三孔沟，孔常破裂而沟通，呈环孔状。
时代 第三纪。

Remarks: One specimen of 33×25μ is described as Zonorapollis sp. The type species designate (illustrated in textfig. 4) is described only in a (so far) unpublished report and thus has not been published in a valid manner. However, the genus appears to have been published in accord with the technical requirements of the ICBN and is here accepted as valid.

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1) 本文关于湖北江汉平原的资料系引自宋之琛、李曼英，1965；湖北江汉平原白垩纪—第三纪孢粉组合(手稿)。

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