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About the Institute

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.

Dear All,

92 11 02

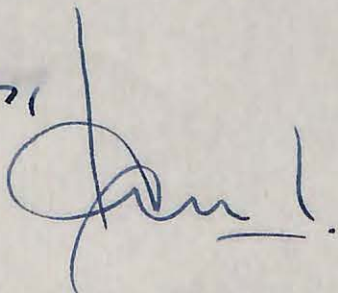
In great haste (I'll have to move floors again in ca 2 wks).

I think Portucathipollis may be an effectively published name; but it was not validly published.

- 1/ Still, publication "is not effected" (Art. 29.1) by communication at a public meeting.
- 2/ Art. 34.1.b applies, I think: the name was merely proposed in anticipation of future acceptance. This will be confirmed by any future "official" publication of the name.
- 3/ Art. 37.4 requires that the type be explicitly identified as such. 37.5: specify where it is conserved.

It was close, but - thank heavens - no cigar! I do not think that the authors avoided valid publication by design!

Best wishes,



JAN JANSONIUS

PS: 92 11 03 Just now your xerox of Grennel came in - thank you for sending it. (I later had realised my geographic gaffe - I now have what I need thanks again).

THE PENNSYLVANIA STATE UNIVERSITY
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26 October, 1992

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

Dear Jan:

Your expert opinion is desired! cf. p. 147 of abstract volume of 1992-IPC. Is Portucallipollis validly published? Sure gets close--maybe this should be inserted in appropriate place in my chapter as horror story example?

Yours very truly,



Alfred Traverse

AT/et

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29 October, 1992

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

Dear Jan:

Re yours of 14 Oct.: you should use your review copy of my book! The Guennel paper is referenced in text, bibliography and index (I admit a slip of the pen: I messed up his "macrospore"). I needn't repeat here, except to say that the paper was published by Indiana, not Pennsylvania. I enclose xerox copies of the only two pp. you need for this purpose. You got the date (1952) right!

The xerox from Playford & Dettmann (I hear the relationship is socially now dissolved) was interesting. I gather you aren't suggesting a response of some sort?

All the best.

Yours very truly,

Alfred Traverse

AT/et

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AT/et

Dr A. Traverse
Penn State

Classified & Dotted / 24

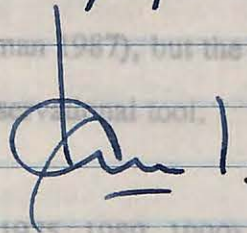
Dear Al,

92 10 14

I notice that we never had a copy of
Guennel 1952, in which the term "microspore"
was defined.

Do you know if it is still available from the
Penn State Geol Surv.? If so, could you forward
this letter to them, constituting a formal request
for a copy. They can invoice me personally at
the address below. Thank you for your trouble.

Yours as always,



JAN JANSONIUS
% ESSO PLAZA WEST 1555
237-4 AVE SW
CALGARY CANADA T2P 0H6

affinity or phylogeny) and hence about the seemingly arbitrary/artificial precepts of spore nomenclature and taxonomy. This, Schopf claimed, was much more geared to the needs of stratigraphic correlation than to plant-evolutionary considerations. Most paleopalynologists would concede such to be the case, as a virtually inevitable consequence of the morphologically diverse array of the microfossils at hand, and the relative paucity of reliable evidence as to their botanical affinities (although the latter are normally indicated where possible and used as a basis for a range of paleobotanical inferences; e.g., Dettmann 1986).

Advances in the optical quality of conventional microscopes and, more dramatically, the increasing application of electron microscopy (both SEM and TEM), have added new detail and precision in morphological and taxonomic documentation of palynomorphs. Ideally, these instruments are complementary to each other for spores as well as pollen grains (Batten 1986a, p. 255-256; 1986b, p. 2; 1987; cf. Chapman 1987), but the high-quality binocular microscope still retains its role as the primary observational tool.

In a series of publications, Hughes (e.g., 1975, 1989, 1990) has advocated fairly drastic and far-reaching changes in systematic procedures for paleopalynology (and for paleontology in general: see Hughes 1989). These involve abandonment of traditional Linnean binomial nomenclature in favor of more flexible and prospectively more precise approaches (use of, *inter alia*, biorecords and graded comparisons based on large collections or "populations" from given samples). Hughes claimed that these innovations would or should greatly enhance biostratigraphic resolution. Many of Hughes's precepts and arguments must assuredly be regarded as cogent (Smith 1986), but the nomenclatural and taxonomic solutions he advances have yet to receive widespread acceptance and, indeed, have caused some consternation [e.g., Traverse's (1975) commentary on the inaugural "Hughesian" proposals].

An extended and useful discussion of the current ICBN has been provided by Fensome *et al.* (1990, p. 5-18). Though referring specifically to acritarchs and prasinophyte phycmata, the points raised in their discussion are equally applicable to form-taxa of dispersed spores (genera, species, and infraspecific taxa).

When naming dispersed fossil spores, paleopalynologists for the most part adhere to the provisions of the International Code of Botanical Nomenclature [ICBN: Greuter et al. (1988) being the latest version, as adopted by the 14th International Botanical Congress, Berlin, July-August 1987]. The ICBN takes due cognizance of the fragmental complexities of the paleobotanical record by formally recognizing, in addition to normal whole-plant taxa (modern "eutaxa"), form-taxa ("parataxa": i.e., form-genera and form-species) which are applicable to a whole range of disjunct fossil-plant entities (viz., dispersed spores and pollen grains, together with dissociated leaves, roots, fructifications, etc.). By ICBN stipulation (its Article 3.3), form-genera are those that lack sufficient diagnostic characters to enable their assignment at family level: this is the case with the large majority of pre-Quaternary spores (some of which may, however, be attributable to suprafamilial taxa). Thus, as discussed by Chaloner (1986), there is some paleobotanical/palynological linkage or interdependence between nomenclature and taxonomy, which are traditionally regarded as more or less mutually exclusive and usually are so in the case of extant plants. (See also Ch. 2)

Al: two pages of Ch. 8 A for your info. JJ

The essential raw materials of spore-paleopalynology are disjunct microfossils of diverse ages and diverse morphology. The latter obviously provides the ~~principal and only~~ ^{taxonomic} ~~really~~ objective / basis for ~~nomenclatural~~ discrimination; consequently, form-genera (and their constituent form-species) are indeed aptly termed. Accordingly, genera of spora dispersae are circumscribed on the grounds of such characters as nature of germinal aperture (haptotypic criteria), equatorial outline (amb), wall stratification, wall sculpturing, and any structural modifications of the spore wall (e.g., development of cingulum, zona, or patina). Genus names often reflect the characteristic morphology of the forms to which they are applied; e.g., Cyclogranisporites R. Potonié and Kremp 1954, with circular amb and granulate sporoderm. Or, especially in the case of some earlier established names, they may connote a suggestion or likelihood of particular botanical alliance; e.g., Polypodiidites Ross 1949. In such instances, however, the genus is applicable strictly as a form-taxon (Polypodiidites embracing morphologically distinctive monolete spores with verrucate sculpture). Some authors (e.g., Schopf 1969) have expressed strong reservations about this approach (i.e., reliance on exclusively morphological criteria, without consideration of probable or possible botanical

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16 August, 1992

Dr. Jan Jansonius
Esso Resources Canada Ltd.
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237 4th Ave. SW
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Dear Jan:

The MS went off several days ago.

Here are several other items:

1. The MS you "just sent off" came today. I see editorial things I could have caught for you: e.g., the second sentence has very strange "Wortstellung." It should read (if almost the same words are kept): "Also, these changes were not indicated in the Preface in a clear and systematic manner, because the number of an article with changed text was not always cited in full."

Para. 3, l. 9--must say International Botanical Congress. Furthermore, they occur (about) every 6 years (1992, 1987, 1981, 1975, 1969,....). In para. 3 you continually talk of 4 years.

What is the "Nomenclatural Committee" in para. 5?
I believe the reporting body would have been "the Bureau of Nomenclature."

Of course, I had too much on my plate to do this for you at this time.
(Incidentally, I met Below in Germany in April. He said, "I'm the most cited scientist in the world: "see Below....")

2. Years ago you very generously arranged for me a xerox copy of your original, handwritten MS for the first batch of J&H cards. I was working on the ING list) and visited you in Calgary--a long time ago!)

I still have those slips mounted on 8 1/2 x 11" sheets, arranged in 3-hole binders. The whole thing takes up almost 2 meters of shelf space! Can you think of any reason to keep them? They're old friends, but I could sure use the space, and I now have my beautifully organized set of the final "cards" (= slips). (Maybe I should save them and organize them morphographically?)

Best wishes.

Yours very truly,

Alfred Traverse

AT/et

International Association for Plant Taxonomy

International Bureau for
Plant Taxonomy and Nomenclature

→ AT
IAPT - BERLIN

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Dr. J. Jansonius
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Calgary, T2P 0H6
Canada

13.8.1992

Dear Dr. Jansonius,

let me break the bad news first: the deadline for submitting proposals has long passed (it was the 30th of June), and even if the nomenclature editor of *Taxon* would wish to be lenient he could not since the synopsis of proposals with the Rapporteurs' comments is already fully written and ready for the printer. This, I may add, does no harm since most if not all the points that made you draft the proposals rest on misunderstandings of the present rules. Let me address them one by one (and incidentally answer your urgent question).

Your letter and proposals, in a general way, demonstrate the difficulty one has in communicating when one has not the same vocabulary and information background. Most of my present points I had already made in my previous letter, but I obviously failed in making them understood. Sorry. I shall try to do better this time.

Your general point (and last proposal) is procedural. If the Section is in the right mood and if there is time left at the end, topics of that kind may again engender lively debates in Tokyo, as they did in Berlin. If you have a moment to spare, read the corresponding portion of the Berlin Proceedings (Englera 9: 198-205). In spite of good intent, your proposal would have presented unsurmountable practical difficulties, one of them being that each Nomenclature Section is autonomous as to procedure and can bind only itself, not the following ones. Also, while there may occasionally be proposals for which a quarantine might well be appropriate, there are other, more numerous ones for which it would be undesirable. Personally, I would find worth while considering a quarantine option (to be confirmed by an extra vote) for proposals which received 50% yes votes but not the required qualified majority of 60%.

To your qualms about Art. 37.1 I had already responded when I stated that, for the purposes of the Code, a name always has a type (and is considered to have it from the moment of its valid publication) irrespective of the fact that the type may have been designated later, or remain to be designated in the future. This answers your question on *Acaciapollenites*. The (holo)type of that name is the type of *Acacia myriosporites*, even though it was not yet designated when the generic name was validly published in 1972. Just read Art. 10.1: "the species name ... is considered as the full equivalent of its type." [It does not say: "previously designated type"!]. Further you may note, in Art. 37.1, that the word "indicated" is (purposely!) used in preference to "designated". One of the main implications is that by use of a binomial you may "indicate" the type (ultimately the specimen) of a generic name without explicitly designating it. It may seem awkward at a first glance that the holotype of a name of a genus may be (and often is) a lecto- or neotype (of the species name), but this is what is intended, and as should be.

Your proposal on Art. 37.1 would, presumably, carry not even your own vote once you realized its implications. The provision that for valid publication of a name at any (!) rank the type (meaning the holotype) must be indicated was first introduced, non-retroactively, by the Paris Congress (Art. 33 of the 1956 Paris Code). It was later (editorially) restricted to the ranks of genus and below, since in suprageneric ranks the name itself (being formed from a legitimate name of an included genus) indicates (!) the type. All names that, since 1 Jan 1958, have been published without indication of the type have been universally rejected as being invalid. Your second proposal would make them retroactively valid, and throw nomenclature into chaos and utter disrepute.

On Art. 37.2, which you interpret correctly, all I can add is that of all botanists I got to know Al Traverse was the first, and you the second, who interpret the pre-Sydney Code the way you do. I gave you the *Rhodymenia* example which you failed to understand (lack of communication between persons speaking different languages?!), perhaps because past and present Code editors thought their conventions to be so self-explanatory that they failed to explain them. In type indication for conserved names, the (optional) addition of a name in square brackets, preceded by the equal sign, gives the name (and sometimes its basionym) that is believed to be correct for the species to which the actual type of the generic name belongs, when it is different from the "type name" (or one of its homotypic synonyms, as given in round parentheses). In other words, a hypothetical entry for *Compositosphaeridium* would have, for a type entry: "T.: *C. costatum* (Davey & Williams) Dodekova (*Hystrichosphaeridium costatum* Davey & Williams) [= *C. polonicum* (Gorka) Erkmen & Sarjeant, *Hystrichosphaeridium polonicum* Gorka]." Of course, the implied taxonomic synonymy, as all taxonomic synonymies, is subject to change with taxonomic opinion, but the type of the generic name will not change (let me add: as should be!).

Incidentally, I have some sympathy with your complaint about cumbersome wording. The zoologists have found a clear, concise and elegant solution, for which I envy them, when they created the term a "nominal taxon", to denote an entity without a circumscription and consisting only of the name and its (actual or potential) type. Their Code says: "the type of a nominal genus is a nominal species", which is exactly what the present Art. 10 states in many more words. It is also what all botanists (except palaeopalynologists) appear to have understood by the phrase "the type of a genus is a species", ever since the introduction of the type method into botanical nomenclature. To quote your above example, for a zoologist *Compositosphaeridium costatum* and *C. polonicum* are two distinct, independent nominal species, irrespective of whether or not one considers them to belong to one and the same taxonomic species.

Now to Art. 37.3: I grant you that the present wording is not a model of elegance, and perhaps not even of clarity. But your proposed change would make matters worse. At present, all the third and final sentence of the paragraph is there for is to explain the meaning of the second sentence. If you leave the second sentence but remove the third one and make a Recommendation out of it, you not only disrupt this association but introduce the logical inference that the two statements are not congruent. Since, as you say, you agree with the statement in the second sentence, perhaps no more than an editorial change is needed to make clear that your points are all covered by the present rule. Either: replace the full stop after "holotype" by a colon, add "at least" before "the collector's name", and replace the three "and/or" by "or". Or, alternatively, make the present third sentence into an independent Note, and replace the final "is required" by: "is acceptable as a concrete reference to a specimen".

Art. 37.5 you have also misunderstood, and continue to misunderstand in spite of my explanations. Perhaps things become clearer when you look at the Code before Berlin and compare it with what happened at Berlin. Before, we had Rec. 7A advising to deposit original material in a "permanent, responsible institution", and Rec. 37B to encourage authors to make it explicit where their types were to be found. At Berlin, there were two relevant sets of proposals (forgetting a third one that deserves that fate). The first, to make Rec. 7A into a rule (under Art. 37), was wisely rejected. It would have made deposit of the holotype in a "permanent, responsible institution" obligatory as from 1990, the penalty for non-compliance being invalidity. This is what you (rightly) object to, but as I said, it was rejected. The second was to make Rec. 37B into a rule, as from the same date, also to bear upon validity of future names. This is what was accepted. The intent of the rule is not to make deposition of types in a public herbarium mandatory, but to force authors to make the whereabouts of their types publicly known. Rec. 7A still stands, and rightly so, since it has not become redundant. Art. 37.5 has "herbarium", not "public herbarium" (nor, unfortunately, "collection" which is what is meant); and it has "conserved" not "deposited"! If Below included a statement: "type in my private working collection, presently at the XY institute at Utrecht" he fully complied with Art. 37.5.

Incidentally, there will be a new proposal at Tokyo (now in press), to enforce deposition of types in a public collection as a condition of valid publication, as from a future date. I will vote against it (although it makes special provision for Below-like cases), and so probably would you. But, more importantly, this very proposal makes it abundantly clear that the present rule is not what you have thought it to be, because in that case the new submission would have been unnecessary.

As to the *Spinitricolpites* case, I now wonder whether this is not a pseudoproblem. Unfortunately Scholtz's publication is not available to me, but from what you now write I understand that he designated a single type (presumably a preparation or microscope slide, under Art. 9.1), and that he did not illustrate three different specimens but three portions of what he (justifiedly under the Code) considered to be a single specimen. So his generic name is valid. If the type is now missing, and until such a time when it may turn up again, a neotype may be designated - but this in no way affects the validity of the name.

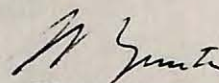
I will not comment on the purely palaeontological nomenclatural question. I realize that there are problems there, both in the objects you study and in the rules as they stand (and perhaps in the people working with fossils as well). My feeling is that any proposal for an improvement that (a) affects the nomenclature of fossils only and (b) has the clear support of the Committee for Fossil Plants (better known under the designation "fossil committee") will likely be approved by the Nomenclature Section. However, before trying to introduce special provisions for "palynomorphs" into the Botanical Code, you might better make it clear that lepidopteran scales and other not really botanical objects are not to be covered by that term.

One last but perhaps important point. As I said, you missed the boat for Tokyo proposals (and, I may add, the next opening for submitting proposals will not occur before the Tokyo Code has been published for at least 6 months). But there will be a call for editorial suggestions (including new examples), by anyone, for the benefit of the Editorial Committee, to be published in the May 93 issue of Taxon. The deadline (not to be missed!) will be some time in October 93. If you are satisfied with any of the editorial suggestions I made above, I claim no copyright on them: feel free to submit them as your own. In cases where it is doubtful whether the change is really purely editorial, it might be wiser to bring the matter up from the floor for consideration at the Tokyo Nomenclature meeting.

I am sending copy of this to Dan Nicolson for information. Please feel free to make further copies for anyone interested (including Al Traverse).

With kind regards

Yours sincerely


Werner Greuter

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12 August, 1992

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

Dear Jan:

Well, here it is!

After we talked the other day I recalled my colleague, K.C. Kim, head of the big entomological institute here, and the world's authority on sucking lice of aquatic mammals (!). He teaches a course on zoological nomenclature, and I occasionally give a guest lecture on plant nomenclature. He says that Greuter is right--the type of a generic name is a specimen: holotype, lectotype or neotype. ("Syntype series" is appended to allow for older papers in which the author designated a slew of specimens. A lectotype is supposed to be selected by anybody who then re-does the material.)

I believe for cost reasons, I will not send the whole red-pencilled printout from your diskette (there are two such versions, because I revised "your" MS twice)--you can figure it out from your file. However, I would like to add these notes (in no particular order!):

1. in editing the book on palynomorph sedimentation, the Dutch authors insisted that in present-day Netherlands, all Van names are being written with capital V and alphabetized under V, regardless of what the owners of such names used to do--and Van der Hammen was so handled.
2. I regard the present version of the MS as very fine-tuned--so don't change even an apparently harmless word without consultation (this stuff is like a legal brief!). You could fax me.
3. Many of the rather technical sounding phrases have their origin with Greuter and therefore especially need to be left pretty much alone. He said specifically in response to a faxed question that I don't need to quote him--he urged that I not do so.

Jansonius, 12.VIII.92, pg. 2

4. In the material beginning on p. 32 (of present MS), I experimented with other techniques of listing, such as your "space-saving" suggestion. Unfortunately, none is so readable. Please leave it as is. Also, on Greuter's recommendation, new headings were added: "validity" vs. "availability" and "description."

5. I think Fensome's hypothetical example "Blottispora" is very useful to understanding and have taken it over. I have it in writing from RF that I can use such material that he provided. He is referenced.

6. I do hope you'll allow Table 1 to survive!

7. I am not in sympathy with McGregor's (implied and stated) notion that the chapters for your book should be put into a food processor to assure that they come out the other end with the same consistency and particle size. There is no reason why the writing style and mode of expression should be uniform in a multi-authored book.

Have fun! Best wishes.

Yours very truly,

Alfred Traverse

AT/et

encl: MS; diskette with 2 files: (1) new version of MS and (2) table

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3 August, 1990

Dr. Jan Jansonius
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faxed to: 403-237-2197

Dear Jan:

Here's my first go at a outline for the nomenclature deal. I'd be interested in your comments and suggestions.

Yours as ever,

AT
Alfred Traverse

AT/et
encl: outline

→ P.S. Please substitute
this corrected
Title page of outline!
(Title, of course, was
wrong.)

Outline for AT chapter-JJ et al. palynology book

- I. Introduction
 - A. The purpose of nomenclature generally and in paleopalynology specifically
 - B. Nomenclature with relation to taxonomy and systematics
- II. Classificatory schemes and their links to nomenclature
 - A. "Artificial" schemes vs. "natural" schemes and related nomenclatural problems
 - B. The turmal system and other classifications-cum-nomenclature: Potonié-Ibrahim, etc., Elsik, etc.
 - C. The Biorecord system of Hughes and its potential links to nomenclature
- III. Informal vs. formal systems of nomenclature
 - A. Raistrick's alphanumeric system and other, later, similar schemes (Shell system, etc.)
 - B. The rationale for application of Linnaean-style nomenclature to fossil palynomorphs--advantages and disadvantages
- IV. The basic mechanics of the use of Linnean nomenclature for palynomorphs, with comments on the fact that palynomorphs occur in 4 of the 5 kingdoms of organisms, and the problem that palynomorphs are often parts of a life cycle or parts of an organism that includes other named entities
- V. Differences between the formal codes of nomenclature as these impinge on palynomorph systematics, with special emphasis on



the botanical and zoological codes

- A. The special peculiarities of the rules as they apply to fungal spores, dinoflagellate cysts, chitinozoans and scolecodonts, with additional comments on special situations such as lepidopteran scales and foraminiferal linings
- B. Conflicts between the codes, e.g., as to illustration and specimen-conservation requirements

VI. Special nomenclatural problems for palynomorphs

- A. Typification, lectotypification and neotypification
- B. What constitutes an "illustration," a "description," a "diagnosis," "adequate conservation of type specimens," etc.
- C. Form-genera and "organ-genera" vs. "genera."
- D. Higher taxa--are they needed for palynomorphs? How about sub-genera, sub-species, varieties, etc.

VII. "Registration" of names and journals, "sanctioned names" and other matters possibly on the horizon

- A. Would it better serve palynology to have a commission decide what names and what types are "correct," making a "bible" of sanctioned names?, or
- B. Should palynology depend on the existing Codes, the endless modification of them, and occasional conservation of names?

VIII. Summary.



The Pennsylvania State University
 Department of Geosciences
 503 Deike Building
 (814) 863-7823 [FAX]

Date: 29-VII-92

Marked here if urgent reply is requested.

Fax Transmission to:

Name: Dr. Jan Janssonius
 Company/Institution: Esso Plaza W. 1555
 Department: Calgary, Canada
 Fax Number: 403-237-2197
 Phone Number: (403-237-2438)

Transmission from:

Name: A. Traverse
 Phone Number: 814-863-3419

Total Number of Pages (Including Cover Page): 1

Message:

Dear Jan -
 Just to keep you posted: after I figured out where things were coming from, I concluded that your revision was very good, and I based my work on it. I have now finished working on the JCN, and RAF critiques also. Greater yet to do. So, you'll get the new MS soon.

As ever,
 AL

*If any portion of this transmission is unclear, please call _____.

PENNSTATE



The Pennsylvania State University
Department of Geosciences
503 Deike Building
(814) 863-7823 [FAX]

Date: 10-11-92

*This is new and
only number*

Marked here if urgent reply is requested.

Fax Transmission to:

Name: Dr. Jan Jansonius
Company/Institution: Esso Plaza W. 1555
Department: Calgary, Canada
Fax Number: 403-237-2197
Phone Number: _____

Transmission from:

Name: A. Traverse
Phone Number: 814-863-3419

Total Number of Pages (Including Cover Page): 1

Message:

Dear Jan: Your fax helped a lot!
I guess phone call now not necessary.
I'll go back to trying to produce a new
acceptable version, tho' I may argue
about some things!

Please note that you used the old fax
number - it now comes in in a
different "institute" (Earth Systems Science
Center).

Best,

AL

*If any portion of this transmission is unclear, please call _____.

PENNSTATE

Dr. A Traverse -- Penn State University
 Deike Bldg 435, University Park, PA 16802, USA
 FAX 61 814 865-3191 / 863-7823

Pennsylvania State University
 Department of Geobotany
 503 Deike Building
 University Park, PA 16802

Dear Al,

92 07 10

I just got your fax sent on 92 07 09; I hasten to answer. Please do not feel that I wanted to subvert or hijack your chapter. What I did was try out the new program on my computer, which purportedly would give me the capability to take an electronic file produced under another software program, work with it, and put it back into its original format. From the fax you sent a day earlier, I understand that that aspect of this exercise was not an unqualified success. Your MS was the first I worked with in this fashion; it was an experiment in many aspects. But please, do not feel that this is what I want to change your MS into -- rather, it shows what I thought might be possibilities for you to consider. But, I also thought that, with your hectic schedule of travelling, it might be helpful for you to have a 'modified' and shortened text to work from; however, if that frustrates you, please treat my version as an extended marginal note - I did incorporate all my notes in this version - and work from your own first draft. There was a definite feeling among several of the reviewers, however, that your text should be made more compact, with fewer tangential asides; you would probably have weeded much after having been away from it for some time. I did juggle some sections of the text in a manner that to me seemed to provide a better flow; but here again, please do not feel bound by that, rather appreciate that as one person's response to your prose (as such it may show you that it elicited a reaction different from what you had in mind). In a few instances, I also added one or two sentences, adding information that I thought might be useful to a reader. If you do not agree, please cross them off. I admit, I did change your bit on "diagnosis" and "description", particularly where you gave an incorrect interpretation involving the genera file. Maybe all that can be deleted. I did not introduce in your text any of the 'major' things, marked by me in yellow pencil, where you must make a decision. That included some of the things that Greuter wrote, but also some remarks by the other reviewers.

You ask for suggestions: please read once quickly through "my" revised version that I copied to disk, then go back to your own original text, and start working with that, while cross referencing it with the copies marked up by the other reviewers. Please cross out what you want to cross, and add/modify what you want to add. No two persons will ever precisely agree on what is clearest or most instructive (e.g.: I thought your motto was quite a propos, and embellished upon it - while Colin thought it was not a propos; use your own judgement).

Best wishes and regards for now,

Jan Jansonius.



The Pennsylvania State University
 Department of Geosciences
 503 Deike Building
 (814) 863-7823 [FAX]

Date: 9-VII-92

Marked here if urgent reply is requested.

Fax Transmission to:

Name: J. Jan Jansonvic
 Company/Institution: Esso Plaza W. 1555
 Department: Calgary, Canada
 Fax Number: 403-237-2197
 Phone Number: _____

Transmission from:

Name: A. Travers
 Phone Number: 514-863-3419

Total Number of Pages (Including Cover Page): 1

Message:

I've worked all day on pp 1-5, and
I'm very discouraged. It seems to me that
my essay has been gutted. It's one thing to
ask me to omit offensive paragraphs and for me
to consolidate here and there - standard procedure.
But you've produced a subtly different essay
which says things I don't want to say, and
ruins my "train of thought." # Suggestions? I
won't have my name on stuff I didn't write.

*If any portion of this transmission is unclear, please call _____.

THE PENNSYLVANIA STATE UNIVERSITY
DEPARTMENT OF GEOSCIENCES
PALYNOLOGICAL LABORATORIES
435 DEIKE BUILDING
UNIVERSITY PARK, PA 16802
Phone: (814)863-3419; Fax: 814-863-7823

faxed to 403-237-2197

8 July, 1992

Dear Al,
I want to thank you for your informative text on nomenclature and taxon my... and is a sound guide to good practice. Having said that, of course there are a number of things considered, modified or stated more clearly.
I am philosophically not in agreement with your argumentation on...
I am finding the review copies, and letters with comments by the reviewers. I seem to have misplaced my own marked up copy, and therefore will refer to copy some of my major (yellow line) comments.

Dr. Jan Jansonius
Esso Plaza W. 1555
Calgary, Canada

Dear Jan:

Just to report that I'm back in the office. Your Worldwide Express pkg. of 29 June was waiting for me. I'm working on your, your reviewers' and Greuter's comments, etc., using your "chap2rev.doc" file as reference. Betty, who has produced a hard copy from it for me, says there were many "imbedded errors" and "superfluous and contradictory commands" that made it hard to print in correct format, but, as I write, she has finally cleaned those up. Now she must restore the underlining.

She leaves tomorrow for a week's trip to visit her mother and attendant matters. By the time she returns, I should be able to respond with a "version 3." I think Table 1 is very helpful for the uninitiated. I hope you don't insist on expunging it.

Best.

Yours very truly,

Al

Alfred Traverse

AT/et

STRATIGRAPHY AND PALYNOLOGY

Editors: J. Jansonius
D.C. McGregor

Dear Al,

92 06 25

I want to thank you for a comprehensive and informative text on nomenclature and taxonomy; it has much food for thought, and is a sound guide to good practice. Having said that, of course there still are a number of things that need to be reconsidered, modified or stated more clearly.

Enclosed please find the review copies, and letters with comments by the reviewers. I seem to have misplaced my own marked up copy, and therefore will write this letter to convey some of my major ("yellow line") comments. *However, you may want to check all other comments as well.*

I am philosophically not in agreement with your argumentation on Nyssa pollen, and your apology for using modern generic names as receptacle of fossil pollen species. The other reviewers all seemed to have no quarrel with it, and it seems innocuous enough, but when you say that Nyssa (=tupelo) "eocenicus" is not really a species of "tupelo", then you are contradicting yourself, because you have named it as such. I also feel that this usage is in conflict with your position that pollen found in a fructification, even if it resembles "Florinites" pollen, may not be designated as Florinites pollen, but only as "Cordaianthus pollen". Your argument with Nyssa was that if it resembles Nyssa closely enough, use Nyssa! Then why may we not use Florinites, if Cordaianthus pollen has the same morphology? Or, should we all from now on use Cordaianthus instead of Florinites, if the pollen are closely similar? The argument that the same strata yielding the "Nyssa eocenicus" pollen, also yield Nyssa type fruits, does not prove that the pollen and fruit were produced by the same species, or even genus. I personally would be happier to have Nyssapollenites or Nyssoidites (two different genera for dispersed pollen of presumed Nyssa affinity, as their names imply) as receptacle for the fossil pollen, with the 'Botanical affinities' remark that they may represent Nyssa; that would agree with your position that Cordaianthus pollen may be assigned to Florinites if found dispersed.

Another concern I have is with the Zoological code, and its typification policy. If I understand Greuter correctly, the zoological "nominal species group" is an abstract concept for "the name plus the holotype only"; the remaining assigned specimens all fall into the concept "taxonomic species group". Please check again the ICZN on this, also its glossary and definitions; for long this was not clear to me, and it would be useful to elucidate it in your chapter.

I think the Table 1 does not add much; the subject matter is not very amenable to this format, and the textual treatment explains all these things more clearly already.



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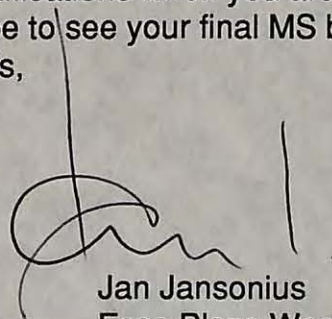
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Hunt Institute for Medical Documentation

I typed the many small corrections into the electronic file, and printed the latter out in my "Word for Windows" format. I then saved the file onto your diskette as a WordPerfect 5.1 file. I hope that that conversion was succesful, and that you can use it if you want to. I did all this, because it made a good test for my new WinWord 2 program. Also, it allows you to read through the altered text and see how you like it. I did not alter the style in any real sense, and where I made some additions, I tried to emulate your speech habits. Some repetition was eliminated by combining statements on different pages, and in a few cases, I switched a paragraph to another page, where it seemed to be more immediately pertinent. Yes, some of your personal flavour was lost in the inevitable need to reduce the bulk of your contribution: but better to lose some of the flavour, than not have room for the real meat and potatoes. Also, the three Canadian reviewers -- independently -- agreed that some of your statements were too combative and personal, and out of character with the rest of the book. I trust that you can see what we intended to do, and that you will modify your MS in that spirit.

You sure lead a busy life; your plate in Pennsylvania is full to the brim! Our book is now making good progress, but not everything is in yet, or written, and you will have time to do the MS modifications when you are again settled and ready to tackle that. Nevertheless, I hope to see your final MS back in the near future. In the mean time my best regards,

sincerely,



Jan Jansonius
Esso Plaza West, room 1555
237 4 Ave SW
Calgary, Canada T2P 0H6

EMPFÄNGER : 403 237 2197
SENDER : SNG KUHWARDSTRASSE
DATUM : JUN '92 14:19
DAUER : 00'55
MODUS : STAND
SEIT. : 01
ERGEBNIS : OK

Faxed to 001-403-237-2197

from : A. Traverse

Seuck. Mus., Seuck Anlage 25

6000 Frankfurt a. M.

Germany (Fax 069-794-004-50)

Dr. Jan Jansz
Esso Plaza W. 15-55
Calgary, Canada

Dear Jan,

which doesn't actually
say you received any fax
of 3-11 - but you must
have.

Yours of "92-06-3" just in. I fly
back to USA two weeks from today,
but the last week in Germany will be
spent in Stuttgart area, and I begin packing
up next Monday - so no good to send
anything here. I'll be in PA from
2 July (but very preoccupied for a while,
obviously - e.g., income-tax forms for USA,
AA, and local govts were postponed to July
(from 15 Apr.) for 1991 taxes). That's a solid week
of work, at least.

I know from your review of my book that you don't
like my style, but if you make it sound too
un-Traverse I will draw the whole thing!

Best,
Al

FORSCHUNGSINSTITUT UND NATURMUSEUM SENCKENBERG
PALAEOBOTANISCHE SEKTION
SENCKENBERGANLAGE 25
6000 FRANKFURT A.M. 1, DEUTSCHLAND (Germany)
fax: 069-794-004-37
phone: 069-794-004-50

faxed to 001-403-237-2197

3 June, 1992

Dr. Jan Jansonius
Esso Plaza West 1555
Calgary, Alberta, Canada

*interesting! All
other countries seem to
have their own "Vorwahl"
— only Canada shows one w.*

Dear Jan:

Yours of 11 May just in--wow! Fax is way to go! But I'm not sure this is still your number.

I gave copies of my MS to Greuter and Chaloner to critique. Greuter has already returned it with lots of comments. Some are about questions of fact. I'll check them and make corrections, if necessary. Other comments are questions of opinion, and I probably won't go along with him on much of that. But he might convince me to change a paragraph here and there.

Obviously I need to add literature references for your work--I was handicapped here by lack of library. Thanks for sending the papers.

What do you think I should do?--hold all these corrections and additions until I've also heard from Fensome, Nicolson and Jansonius, and then do a grand revision all at once? Or?

In another vein, WG is now after IAPT.CFP to get "at least the fossil algae NCU list ready" (to be shoved through in Japan). He has given up on the total fossil plant list as such. I told him that there is no way the PFR list of Boulter can be turned into a complete list of paleobotanical generic names without man-years of work, and I pointed out homonyms, superfluous synonyms, otherwise illegitimate names, etc. How do I know the list for, say, bryophytes, is better? There's an unseemly haste about the whole thing.

Best.

Yours very truly,

Al

Alfred Traverse

EMPFANGER : 403 237 2197
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AT/et

International Association for Plant Taxonomy

International Bureau for
Plant Taxonomy and Nomenclature

IAPT - BERLIN

The Secretary-Treasurer: Prof. Dr. Werner Greuter

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Dr. J. Jansonius
Esso Plaza West, room 1555
237 - 4 Ave SW
Calgary, T2P 0H6
Canada

25.5.1992

Dear Dr. Jansonius,

I will be as direct as your own style of writing allows me to be: the text you have sent me for comment (which I understand has been or is being published) is inaccurate in detail, uninformed in general, and unnecessarily polemic. I would have reason to feel outraged personally, since much of what you write accuses the work of the Rapporteur and Editorial Committee. But I will just point out that one who has not been involved in the Nomenclature Sessions of the last three Congresses (if in any one at all) might be expected to be more careful in his judgement.

Nomenclatural work is much more careful than you appear to think. Examples of decisions taken hastily then regretted, and reversed at a later Congress, are exceedingly rare (none comes to my mind in connection with the last three Congresses). Cases in which not all consequences of a decision had been foreseen, or adequately pointed out, do occur, but rarely; some have led to subsequent corrections in the rules, but in most cases it was later found that the unexpected consequences were such that one could well live with them. The Rapporteur (my role in Berlin) has the duty to point out hidden implications of a proposal; it is rare that the Section accepts a proposal against his warning.

Let me get through the "horrors" you mention, one by one.

Art. 37 was modified at the request of indexers (your peers) who, having to decide in their daily work on the validity or otherwise of newly published names, found the former Art. 37 much too vague. Art. 37.3, while accepting that (from 1958 to 1989) inclusion of a single element in a newly named infrageneric taxon is acceptable as "indication of holotype", rules that it must be clear from the printed context that the single element included is indeed an "element" in the sense of the Code. Thus, the statement "Type: Mt. Kenya" is not acceptable as indication of type; but "Collected by me in 1982 on Mt. Kenya" is sufficient since "me" stands for the name of the author and 1982 is a date. Your diatribe against the last sentence is difficult to understand. Syntax will tell you that you can replace all the "and/or" by simple "or" if you add the words "at least", without changing the meaning. (Perhaps, if you feel this to be clearer, you might suggest it as an editorial amendment.) The answer to your parenthetical question (bottom p. 1) is also obvious. Art.

37.3 is retroactive without limitation (Prin. vi) but has bearing on valid publication only from 1958 onward, in connection with Art. 37.1 (not retroactive when originally introduced); Art. 37.5 (ditto) is an additional requirement from 1990 onward.

Art. 37.1 has remained unchanged in Berlin except for the expression "nomenclatural type" being replaced by "holotype of the name". For names of new taxa these expressions are coextensive (type can mean holotype or lectotype or neotype, but you cannot designate a lecto- or neotype in the protologue of a name of a new taxon). Your (imaginary) problem does not stem from a change effected in Berlin, but from the new rules on generic typification (Art. 10) introduced at Sydney. The reason why the problem is imaginary is found in Art. 10.1, second sentence (which also explains how the new Art. 37.2 works): under the Code, names are assumed to have types from the moment of valid publication, although they may be actually designated much later (perhaps at a future date). Your second proposal, to change the date in Art. 37.1, is what I would call a "criminal" proposal, which you would have to strongly oppose yourself if your warning against tampering with the rules and introducing instability post festum is meant seriously: it would confer validity to hundreds of post-1958 names that were treated as not validly published under previous Codes, and would be one of the most destabilizing proposals submitted during my term of office as Rapporteur.

Art. 37.2 is also not to be blamed for your second difficulty, exemplified by the case of *Compositosphaeridium*. The Sydney rewording of Art. 10 may have something to do with it, although – as I tried to explain to Al Traverse – most botanists did not interpret pre-Sydney Art. 10 as you and he do! Let me outline what happened. Originally the Code indeed stated that "the type of a genus is a species". This was later realized to be sloppy wording, since not genera but their names have types, so it became "the type of the name of a genus is a species". I have always strongly argued against taxa serving as types, since this can only mean either of two things: (1) that the type is not stable but a changing concept with a variable circumscription, or (2) that the original taxon concept of the author of the protologue counts (which is stable but cannot usually be confidently reconstructed). In Leningrad I had proposed the rule to read "The type of a name of a genus is a name of a species", which was defeated mainly on the grounds that a name was not a constituent element of a taxon. In Sydney, the wording "... is the type of a name of a species" was then approved. It has withstood successfully the test of time, and has been considered by most as a definite progress and clarification. In essence, it brings botany into line with zoology where the generitypes are "nominal species", meaning the name plus its type but without a circumscription. Now what you and Al had in mind obviously was the pre-Sydney rule with option (1), i.e., the variably circumscribed taxon as generitype. It strikes me as highly unsatisfactory that a name should be permanently attached to a variable, and even before Sydney this is certainly not what was generally accepted. You can find proof of this e.g. in Appendix III of the Leningrad Code, where generitypes are always designated by the names (if any) used in the protologue, with synonyms added parenthetically. The correct name, when not based on the same type as the name designating the type, was sometimes added in brackets and preceded by the words "syn. tax.", as a supplementary, optional item of information (see, e.g., the entries for *Rhodymenia* and *Cymatopleura*). In this respect, what happened to Art. 10 in Sydney was a rewording in conformity with general usage (but not, as it seems, with "fossil" usage).

Art. 37.5: The new requirement for valid publication, for which there were excellent reasons as you well know, was not designed to place an undue burden on validating authors, let alone to make validation impossible. First, there is indeed no obligation other than a moral one to tell the truth. After all, there is no legal obligation for a validating description to match the type (or "reality"). Not that I would commend to publish deliberately false statements, of course; but if this has been done, I cannot see that it would render the corresponding name invalid (this is my personal interpretation of the Code; others might disagree). Second, there is no intent to make deposit in a (public) institution mandatory (otherwise, Rec. 7A would have lost its reason of being after Berlin). Perhaps it is unfortunate that the term "herbarium" in this paragraph (which covers both public and private herbaria) is too specific and may be held not to include such things as slide collections. Why not, then, propose adding ", collection" after "herbarium", or to substitute that term for "herbarium"? This would be less of a change than your fourth proposal, and certainly more acceptable.

Art. 7.18 and 38: Neither was changed by a iota in Berlin. Cross references (unless expressing conditionality) are editorial devices, and if mention of Art. 17.8 under Art. 38 is helpful it can be editorially added, but this will not affect the present effects of the rules. Art. 38 tells you that an illustration is necessary for valid publication (incidentally, I agree that the requirement of "essential characters" is problematic); Art. 37 rules that (and how) a holotype must be indicated; and Art. 7.18 specifies which elements can be types. In your *Spinitricolpites* example, no valid publication occurred in 1985; whether or not you validated the name in 1990 depends, not on whether and where the type existed at that time, but on your compliance or otherwise with Art. 45.1 (note that, technically, the type indicated by you would be the holotype, not a lectotype). *Spinaepollis* was validly published in 1961, just as *Acacia-pollenites* was in 1972 (see my foregoing comments). Incidentally, there are indeed some problems in the application of these provisions, but not those you mention. It is not at all clear how 7.18 interacts with 7.10: logically (but not as worded) 7.18 does not apply to neotypes; also in good logics, one would expect a requirement for fossil neotypes having to correspond to a published illustration. As the Code stands, some might argue that names of fossils cannot be neotypified at all. Another problem, that may become more pronounced if some of the proposed Tokyo amendments should pass, is the present lack of definition of the term "specimen". For practical purposes, a whole slide is acceptable as "type specimen" for non-fossil microorganisms (Art. 9.1), whereas Art. 7.18 uses the term "specimen" for a single "fossil piece". There may be matter for thought by the "fossil committee", there.

The proposals you have drafted are, all four of them, destabilizing to a greater or lesser degree, since they would effect that names that are not validly published under the present rules become valid retroactively. If I may take you by your own words, this is not what I would expect you to endorse, and it is certainly not a decision to be taken lightly. Your first proposal affects names of fossils only, so that, if you convince the CFP to advise favourably on it after careful consideration, it has good chances to pass. The other three concern all groups of plants, and if you decide to go ahead and submit them formally, I will have to warn the Section of the consequences of their being approved.

Incidentally, may I draw your attention to the stringent deadline for submission of proposals (Taxon 41: 151).

With kind regards

Yours sincerely


Werner Greuter

PALYNOLOGY AND STRATIGRAPHY

Editors: J.Jansonius
D.C. McGregor

Dr. A. Traverse
Beethovenstrasse 36, No. 25
6000 Frankfurt a. Main 1
Germany

Dear Al,

92 05 11

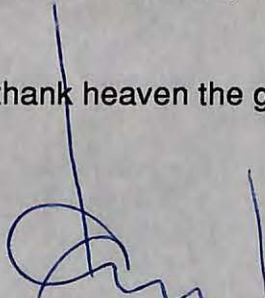
Thank you very much for the MS of Chapter 4, which arrived here in good order just before the weekend. I have made a few copies, and will send those out to Rob Fensome - who will return in a few days from U.K. - and Dan Nicolson. I also have read through it myself, and will come back to that in the next week or so. There are some things I would like to comment on, but not yet at this time. I will try to expedite the review of your chapter, so that revisions may be done before you are leaving Frankfurt.

I also want to thank you for the various letters I received in the last weeks, all dealing with nomenclature. I was particularly piqued by the comments of Greuter, that the date of requirement of a specimen as type for a genus, is 1981, and not 1958 as it is written in the Code. He reminds me of a pope: he seems to know God's intentions better than the holy spirit himself.

I shall leave this now, as there are many things on my plate: just this morning the Playfords faxed the imminent arrival of their chapter. Things are really starting to move now, and I reasonably expect all MSs to be in by fall, and thus a publication date in 1993 is most probable.

But before then, I must finalize my note on the Code, and send that off!

With best regards to you both, (and thank heaven the garbage is taken out again!),



Jan Jansonius
Esso Plaza West, room 1555
237 - 4 Ave SW
Calgary, Canada T2P 0H6

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5 May, 1992

Dr. Jan Jansonius
Esso Plaza West, room 1415
237 - 4th Avenue SW
Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Here's my chapter (2 hard copies plus disk--see also below*). I know you will not agree with me on all points, but unless they are factually wrong, I guess I have a right to my opinions?! I have toned down my own hobby horses as much as I think possible.

I do want to argue vehemently about one point: in my opinion, each chapter must have its own references-section, for the following reasons:

1. So that the chapters can be free-standing (xerox copies of chapters, for example, will be made!)

2. To avoid confusion:

a. in cases where the same paper is cited in several chapters, e.g.: "Evitt 1963a and 1963b" from your communication of 91-04-05 is treated by me as one paper, viz. "1963." For all I know, it could be "1963b" and "1963c" in still another chapter;

b. in cases of different transliterations of names: Malyavkina vs. Maljawkina, or (worse) Chlonova vs. Khlonova. The same publication could thus appear several places in the "community" bibliography.

All the best.

Yours very truly,

Alfred Traverse

*There are 3 files on the disk (all WordPerfect 5.1):
ATMSS\NOMENCLA.MS is the manuscript plus biblio., TBL is Table 1, HDS is the heading summary you requested.

Xerox
quick
for Fri

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18 April, 1992

Dr. Jan Jansonius
Esso Plaza West, room 1555
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Calgary, Alberta, Canada T2P 0H6

Dear Jan:

Just had a phone call from Schaarschmidt from a hospital bed-- he's had an emergency operation for, as it turns out, cancer. As recently as day before yesterday he was fine, and we were planning to spend Easter with them.

So, I'm in a state of shock and staying home for the rest of the day. Gives me an opportunity to send you another short note. I have my chapter almost done. Hope you'll like it.

Also, I wanted to report again on my two afternoons with Greuter. He has really played his cards close to his vest, but now there is no pretense that anything else is going on except getting the NCU lists ready to be whisked through in Tokyo. You may be interested to know, however, that fossil plants may be excepted, only because he doesn't like "our" list. He pressed a copy on me, with the request that FS and I drop everything and work on the list. FS has other things on his mind just now.

One item from the Greuter/Traverse confab should interest you. I outlined for him the theoretical (I didn't have your letter with me) problem of a generic name published in 1959 and thus requiring typification, by a species, as was then the case. The species, however, didn't have a holotype, but was nevertheless validly published--in 1956. According ~~to~~ ^{to} Greuter, the generic name is perfectly o. k. A lectotype should be designated by somebody, but that doesn't change anything at all, except that one fact. (The first person to do it must be followed.)

I guess I'll have to go to London for the Chaloner-Boulter-Holmes-Galtier-Traverse summit.

All the best.

Yours very truly,

Jansonius 24.11.92
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24 March, 1992

Dr. Jan Jansonius
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Dear Jan:

The damned book for Cambridge Univ. Press was finally shipped off last week, and I was trying to concentrate fully on completing another writing project for a certain JJ, when, woe, groan, I got this very difficult letter from you....

You are right that incessant tinkering with the Code has been bad--but without a Commission to adjudicate thorny problems, generations of frustrated folks have tried to make a perfect Code their job. If we had had a Commission à la zoology, we wouldn't have had to depend on (1) revision of the Code, and (2) conserved names ("lists of sanctioned names" are just this solution in a new package). I've been saying this for decades. It was always clear to me that either there would be a commission, or there would eventually be wholesale conservation.

I have two other things to say that I felt strongly about:

(1) Typification of genera by holotype specimens was a big mistake. I remember in Sydney being in a state of near shock about that one--it should be repealed.

(2) Typification of fossil palynomorphs by illustration should be approved and introduced! A permanent commission (the IAPT.CFP?) could be designated to adjudicate the appropriate ^{ness} of controversial illustrations--why not? The sort of charade you describe vis à vis Potonié's type-bearing slides, Krutzsch's manipulation, etc., and much more, could be avoided (it will get worse with the years) if we permitted typification by illustration.

Now to your little "MS":

p.1, para 3. In my opinion, Art. 37.3 is poorly worded, but the meaning is clear: concrete reference to a specimen is required. This concrete reference can be name of collector, or the coll. no., or the date of collection, or reference to some other detail of the specimen that permits its identity to be established. You are off here in your "2 out of 3", etc., etc., interpretation. The English is clearly a list of the possibilities, only one being required.

p.2, para. 2. You've got a date mistake in here somewhere--I gather that the "1979" in l. 7 is wrong--should be 1972? The problem you outline is a result of the specimen-typification of generic names. Therefore, you should be

Herbarium Institute for Botanical Documentation

campaigning to change that (not the date of its application)!

p. 2, para. 3. What do you mean, "Another effect of...37.2"? You haven't discussed 37.2 before. The case you outline is a really knotty one, but I don't think I can agree with your conclusions, and I would summarize as follows (I used only the data you give):

When Dodekova described and named Compositosphaeridium in 1974, she is "supposed" to have known about Beju 1971; that she didn't is not a relevant fact. If Beju was right about H. costatum D&W vs. H. polonicum Gorka [I can't check--and he may have been wrong!], Dodekova should have called her type species "C. polonicum (Gorka) Dodek."

Erkmen & Sarjeant's action as you describe it was wrong and irrelevant as far as typification of the generic name was concerned. It is not clear from your info whether E&S made the transfer of polonicum to Compositosphaeridium. If they did, the reference is now C. polonicum (Gorka) E&S. In any case this would be the type of Compositosphaeridium, and the holotype of that name would be the holotype of C. polonicum (G) E&S--if all of the info, including synonym, is correct as you give it. I mean the Code assumes that Dod. should have known that she should have used C. polonicum. However, if Beju and others were wrong--and that's possible, I suppose--the type would still be C. costatum D&W and its type would typify C.

In the absence of a commission to adjudicate, this remains a question of opinion just like Corollina vs. Classopollis. There is no way to determine objectively whether Dodekova's specimens justified separation from Gorka's polonicum or not. That she didn't know about the latter is irrelevant, of course--she could still have been right (or wrong). So, in summary, the name Compositosphaeridium has two candidates for type species and (ugh) type specimen: (a) H. costatum D&W (and its type specimen), and (b) H. polonicum Gorka (and its type specimen). In neither case is the result that the type specimen and type species are in conflict; that is, in my view, impossible. You only achieved that result by setting up an incorrect scheme, reminiscent of proving that "you are your own grandpa."

p. 3. Your problem with Art. 37.5 as illustrated by Below, etc., would be eliminated if typification by illustration for palynomorphs were the rule!

p. 4, last l. of only full para. Don't you mean "ambiguous and unclear" ?!

2nd l. of last para. "after" (sp.); l. 8 Should say "...on the other hand requiring the type to be a specimen one of the illustrations of which accompanies the description...."; l. 11. "designation" is not mentioned in Art. 38.

p. 5, l. 2. "...or whether it requires that the type specimen be clearly identified as such." I believe this is obvious, and that Art. 7.18 is sufficiently explicit. It is true that Art. 38 should cross ref. 7.18, as already exists in the other direction.

As I've said above, the whole problem you describe here would be solved by the simple expedient of using illustrations for typification of fossil palynomorphs.

para. 2. Nothing is said in 37.5 about "official"--but the problem could be solved by stating that "the collection in which the material occurs" must be stated. However, again, if illustrations were accepted, there would be no problem!

p. 6, proposal re Art. 7.18. I would add "...and is designated in" after "accompanies" in l. 3. Also, I would leave the rest of the present 7.18 in--seems o.k. to me.

proposal re Art. 37.1. No! The problem here was not the date but the change to holotype specimens for generic names! If we change the date for required typifications from 1958, it will affect all sorts (thousands? hundreds?) of previous decisions we've all made, based on the 1958 starting point for typification--just the sort of change you're supposed to be against! Like trying to fix a house by setting fire to it!

proposal re Art. 37.3. You would also need to rewrite what's left, as sentence 2 assumes sentence 3.

proposal re Art. 37.5. As I've said, I would leave this, but add the words "or collection."

References: Are the Below publications actually cited?

Need references to Dodekova, 1974; Gorke, 1965.

Potonié ref: I assume the word is Paläobotanik?

My basic criticism is that you are not dealing with the one really big "mindless change" that should be reversed--the change to specimens typifying generic names!

Now back to working on the bigger "JJ problem"

Best regards.

Yours very truly,



Alfred Traverse

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FAX 611-49-69 794 004 37

Dear Al,

92 06 03

You are right, there is nothing like fax for rapid communication at a reasonable price.

I'll give you a quick answer to some of your questions and concerns. I have read through your MS (Ch. 2), and also managed to convert your WP5.1 document into my WinWord2.0, so I could mess with the text on my computer, and print it; I also 'saved' this file on your diskette in WP5.1. I have, in that manner, rearranged your text, if you will, in a form that I thought showed better flow. I also shaved three pages worth of words off the total MS. There are several points where I do not agree with your interpretation of things, and those areas I have not changed for you, but marked in the margin of a copy of your MS. I also sent copies to Dan Nicolson, Colin McGregor, and Rob Fensome. Colin sent back his comments yesterday, but I have not yet opened his envelope; Rob promised me to send his comments in a few days. I did not yet send you anything back, because I may want to further modify 'my' version of your MS, and also it would be more efficient if you can go over the whole kit and caboodle in one go. From all this attention you can understand that we consider this an important chapter. I asked all the reviewers to hasten in dealing with it, so you will have a chance to modify the MS before you have to leave Frankfurt.

I just got an answer from Greuter, to my letter and a copy (in its early stage) of my MS for proposals to turn back the code.

He was not amused, either by my forward tone or by my arguments. I will send you a copy, I was struck that he writes that the 1958 date is the correct and intended date for requiring a holotype for a genus -- did he not tell you that that should be interpreted as 1981?

You'll hear from us shortly. Best regards, Jan J.

*Sorry, I was frustrated again
by the FAX machines, now
send this off by mail*

Vansoni's
Coverp.

Al, for your info (I just sent this off)

PROPOSAL FOR SOME CONSERVATIVE CHANGES

In the last editions of the ICBN, a number of changes have been introduced some of which appear to not have improved the stability of botanical nomenclature. These changes, also, were not indicated in the Preface in a clear and systematic manner, because not always was the number of the article with a changed text cited in full.

Justification for changes is becoming increasingly ephemeral, with the result that some changes have consequences that may not have been intended by the authors of these proposals, or were not foreseen by the delegates of the International Botanical Congress voting on them.

If we subscribe to the principle that the ICBN is meant, before all, to provide stability of botanical nomenclature, we should collectively become more conservative in proposing alterations in the Code. One mechanism to attain that goal might be a change in the process by which changes are approved: rather than have a one-stage vote for approving the inclusion of proposed changes into the new edition of the Code, we could vote out all proposals found to lack sufficient merit, then vote to accept the meritorious proposals for a four year evaluation period, after which their incorporation into the Code will be reaffirmed by vote in the following International Botanical Conference. This would delay final acceptance by four years, but would ensure that accepted changes, and their wording, have been fully evaluated by all groups that have adopted the Botanical Code for regulating their nomenclatural requirements.

Conditions in the Articles of the Code must be clear and unequivocal; their application must be simple to understand, and the text must be applied literally, without latitude of personal interpretation. Now, we have **Art. 37.3**, in which (as a new **condition** of valid publication of a species) is stated that "Citation of the collector's name **and/or** collecting number **and/or** date of collection **and/or** reference to any other detail of the type specimen or illustration is required." This requirement has retroactive force, and appears to apply to all holotypes of species (and subspecific taxa). It will be nearly impossible to comb the existing literature for this type of information in order to verify whether or not the species were indeed validly published.

Art. 37.1 now states that the "Publication on or after 1 Jan. 1958 of the name of a new taxon of the rank of genus or species or below is valid only when the holotype of the name is indicated (see Arts. 7-10; but see Art. H. 9, Note 1 for the names of certain hybrids)." This Article was approved in Berlin after expressed assurance from the Nomenclatural Committee that it would not affect accustomed normal practice, or the status of validity of names previously proposed. Yet, this requirement is at variance with previous practice. Where, previous to the Sydney edition, the type species (a subjective group, allowing

some flexibility in its interpretation, although that species was anchored by its type) was the nomenclatural type of a genus, now the type of a genus is a specimen (the type of a species). And, this requirement has been made retroactive to 1958! It is unusual to make the starting point of such a radically different approach in nomenclatural practice a date 25 years before the date of its first introduction. The practical consequence in paleopalynology is that many genera, validly published in that active 25 year period, and typified by validly published species, retroactively have been made invalid, if their type species had not been typified by a single holo-/lectotype.

One example, out of many: Mildenhall (1972) validly published the generic name Acaciapollenites, with Acacia myriosporites Cookson 1954 as nomenclatural type. Cookson had not designated a holotype, but the present Code still recognizes this species as validly published. Mildenhall was aware of its lack of a single nomenclatural type, and tentatively suggested a lectotype, without formally designating it. Although Mildenhall had published the generic name Acaciapollenites in a valid manner in 1972, and although it remained a valid name through 1982, the changes in Art.10.1 in the 1983 ICBN made it invalid. Jansonius & Hills (1990, card 4588) observed that they had, in the 1976 Supplement, formally designated as lectotype the specimen suggested by Mildenhall. By accident, therefore, they were the first to satisfy the requirement of the 1988 Code for a single type specimen for this generic name; consequently, they must be considered as the valid authors of the name Acaciapollenites, of which the date of publication -- as things stand now -- is 1976. Any species assigned to the genus before 1976 were not validly published. The ICBN has not indicated how we should deal with the long ingrained concept "type species"; under the present Code it is no longer supported, or even recognized, and no longer even is included in the Index.

The new **Art. 37.2** causes that we now can have a name of a genus based on a type specimen that is not the same as the type specimen of the name of the (type) species to which it must be assigned. An example: Compositosphaeridium Dodekova 1974 was validly published as a monotypic genus for dinoflagellate cysts, with as nomenclatural type the species Hystrichosphaeridium costatum Davey & Williams 1966. Beju 1971 had considered this latter name to be a junior synonym of H. polonicum Gorka 1965; Dodekova 1974 did not include Beju 1971 in her bibliography. [Erkmen & Sarjeant 1980 supported this synonymy, but proposed the new name C. bulgaricum for the specimens described and illustrated by Dodekova 1974. They also included C. polonicum (= C. costatum) in the genus, but proposed that C. bulgaricum be considered as type species of that genus, because it was on the basis of this material that Dodekova instituted the genus Compositosphaeridium. This, however, is contrary to ICBN Art. 7.2, which stipulates that "A nomenclatural type is that element to which a name of a taxon is permanently attached, whether as correct name or as a synonym".] Thus, the holotype of (the type species of) Compositosphaeridium, C. polonicum, is the specimen designated by Gorka 1965 (l.c., pl. 3, fig. 5), while the holotype of the

1# not on disk

generic name Compositosphaeridium remains the holotype of (the incorrect) name H. costatum Davey & Williams 1966 (l.c., pl 10, fig. 4). So much for the notion of a hierarchy of taxonomic groupings based on a single type specimen.

Art. 37.5 contains another new requirement for valid publication: "For the name of a new species or infraspecific taxon published on or after 1 Jan. 1990 whose type is a specimen or unpublished illustration, the herbarium or institution in which the type is conserved must be specified. Note 1. Specification of the herbarium or institution may be made in an abbreviated form, e.g. as given in the Index Herbariorum."

In previous Codes, this practice was included as Recommendation 37B - which duplicated the text of Recom 7A; nobody could have objections to such recommendations. The rationale for the change from non-binding Recommendation to Article, binding on an author as a condition for valid publication, had much to do with a squabble between a few taxonomists who had developed such a degree of possessiveness of "their" types that they did not allow the other access to them. I can understand their frustration. However, this new condition for valid publication creates other problems. In palynology, types are often contained on a glass slide on which hundreds of other potential types are still waiting to be described. If an author has unfinished business on a slide from which some type specimens have been published already, it is not reasonable to force him to cease work on those specimens merely to satisfy the curation requirement of Art. 37.5.

An example is provided in the publications by R. Below, a leading researcher in the field of fossil dinoflagellate cysts, who has published the first three parts of a series of epoch-making taxonomic reviews of Mesozoic dinoflagellate cysts, including the description of many new genera and species. Unfortunately, in our present world, it is difficult for a young researcher to find a permanent base, and consequently Below has led a wandering existence. His first papers were produced in Bonn, now he is working out of Utrecht. He may be based somewhere else in five years. Because his preparations contain more than one specimen he cannot, and did not, deposit his types in an institution; rather, he needs them for continuing observation, on which the following parts of his publication are based. He could state that his types are in an institution, when that would not be true --- yet, his new taxonomy and nomenclature would be accepted by the Code if he resorted to such a subterfuge? However, (and maybe for that reason) many institutions now require actual possession of the specimens, before numbers are assigned to them. Once they are curated, specimens are rarely sent out on loans, certainly not on long-term, open-ended loans. So, must we consider the new names proposed by Below as invalidly published? Dinoflagellate workers will not accept such an inane proposition, and are forced to ignore the Code.

What about carelessness? To illustrate this point, I refer to the generic pollen name "Spinitricolpites Scholtz". Scholtz (1985, p. 13) wrote: "The full series of slides with holotypes and paratypes ringed and documented is deposited in the South African Museum. A register for these holotypes and

paratypes is given in the species list (e.g. SAM-K6155)." Scholtz illustrated some of his new species with more than one specimen, and did not indicate which of these illustrations represented the holotype. The new generic name Spinitricolpites, with nomenclatural type S. jennerclarkei, is illustrated by three specimens, one as SEM photograph, one in polar, and one in lateral view under transmitted light. The holotype is documented as No. SAM-K6182 (l.c., p. 16), but is not further identified in the text or plate legend. Jansonius & Hills (1990, card 4774) initially overlooked the list of type specimens, and inferred that no type had been designated. They wrote to the South African Museum for further information; when they did not receive an answer, they selected fig. 15J as lectotype, and thus thought to have validated the publication of the species and the genus. [Art. 7.18 specifies that: "The type of the name of a taxon of fossil plants of the rank of species or below is the specimen whose figure accompanies or is cited in the valid publication of the name (see Art. 38). If figures of more than one specimen were given or cited when the name was validly published, one of those specimens must be chosen as the type."] However, after renewed correspondence in 1991, I received information from the South African Museum (Fax from R.V. Dingle, 26 April 1991) that Scholtz's material is not to be found in the Museum, and probably never was submitted there; Scholtz told Dingle that he did not remember where the material had gone, after he finished his study of it at Stellenbosch University. The concept defined under the name Spinitricolpites is necessary and used in the literature; under the current circumstances, it is difficult to decide whether the name is validly published -- if so, by whom and when; if not, how it can be salvaged using the material so far involved (which per se is unambiguous and clear); the attempt to validate the name by Jansonius & Hills (1990) may not be sufficient because they did not indicate in which institution the type has been stored.

Expanding a bit further on the curation requirement of **Art. 37.5**: who determines which institution is sufficiently official to qualify as a permanent receptacle for type specimens, so that the names based on those types will be accepted as validly published? How indirect may a reference to an institution/herbarium be before we should consider that Art. 37.5 has not been satisfied? If no mention is made in the text as to where the types are stored, is it enough to assume that sample numbers like "FGS 333" indicate that they are stored at the Finland Geological Survey, if the authors of the paper happen to be employed by that Survey? Even if that appears probable, I think it is not sufficient. In many regions paleobotanists or palynologists have been employed by coal mines or oil companies with research institutes; publications have gone out, and continue to be published, in which storage at their institute is advertised. Yet, in the present economy, many of these institutes are folding, and it is not always clear where the types have gone. Does that retroactively invalidate the publication of a name earlier accepted as validly published? And, is storage "in the authors private collection" really less trustworthy if measures have been put in place that his types will be sent to a larger institution at the end of his working career?

Art. 38 reads, as in previous Editions: "In order to be validly published, a name of a new taxon of fossil plants of specific or lower rank published on or after 1 Jan. 1912 must be accompanied by an illustration or figure showing the essential characters, in addition to the description or diagnosis, or by a reference to a previously and effectively published illustration or figure." **Art. 38** does not refer back to **Art. 7.18** (quoted in above paragraphs), and we thus have an ambiguous situation, where valid publication requires on the one hand an illustration showing the essential characters, on the other hand requiring (one of) the illustration(s) accompanying the description to be of the type. If the single illustration of the holotype does not show "essential characteristics", the publication of the name presumably would not be valid?

But **Art. 7.18** is also ambiguously phrased: does it require a designated specimen (whose figure accompanies or is cited in the description); or: does it require a specimen whose figure accompanies (or is cited in) the description? At present, it is not clear whether the Code accepts as validly published, the name of a fossil species provided it has a designated type specimen and illustrations showing the essential characters (whether or not the figure of the that type specimen has been identified, as long as it is included in the figures), or whether it requires that the type specimen be clearly identified as such. This quandary is illustrated by the genus Spinaepollis Krutzsch 1961, with type species Pollenites spinosus Potonié 1934. Potonié had not designated a holotype from among his three illustrated specimens; Jansonius & Hills 1976 (card 2645) had designated Potonié's pl. 5, fig. 18 as lectotype. However, Krutzsch 1961 had stated that the original slide containing Potonié's specimens needed to be restored, and that after his restoration the three original syntypes could no longer be identified as such; for that reason, he designated another (or possibly one of the same?) specimen(s) on that slide as neotype (slide I89, coordinates 2.9 x 122.5), and provided new photographs of specimens assigned to the species, without specifying whether or not the neotype was included among them. Is the generic name Spinaepollis now validly published as of 1961 (based on a single neotype for the genus by Krutzsch), or as of 1976 (designation of lectotype from among published figures), or still invalid, because we now happen to know that the lectotype cannot longer be identified on the slide, and the neotype may, or may not, have been illustrated? However, Art. 7.18 does apply only to (the name of) a species, not to a genus; still, Art. 37.1 now requires a single type since 1958. For sake of nomenclatural stability, I consider the name validly published as of 1961.

PROPOSALS:

Art. 7.18: First sentence (cited above) to read "The type of the name of a taxon of fossil plants of the rank of species or below is the designated specimen

whose figure accompanies, or the designated holotype/lectotype cited in, the valid publication of the name (see Art. 38)."
Add: "For fossil palynomorphs, the illustration of the holotype may serve as nomenclatural type if it shows the essential characters."

the illustration
(not on disk!)

[If a holotype has been explicitly designated and stored in an Institution, but has not been expressly identified among the illustration(s) accompanying the description, the publication of the name is considered to be valid if the illustrations satisfy Art. 38.1.]

[Fossil palynomorphs are inherently impermanent, and commonly mounted in media that do not ensure its longterm survival.]

Art. 37.1: Change "1958" to "1983" (or a later date).

Art. 37.3: Change last sentence of Article to status of Recommendation, or specify that the last sentence does not apply if the condition expressed in the first sentence of Art. 37.3 has been satisfied.

[As presently worded, citation of the name of the collector and/or various other details appear to be a condition for valid publication for the names of all species.]

Art. 37.5: Change Article 37.5 back to Recommendation 37A.2.

[In practice, most authors have responded to the old recommendation, which is still in the Code as Recommendation 7A.1, that type material must be preserved, and have indicated where such material has been deposited. However, some authors have unwittingly not satisfied the literal requirements of Art. 37.5, others have not been able to do so, because they cannot afford to deposit their strew slides containing many undescribed specimens without crippling their future research; this is particularly true for paleopalynologists, who are one group currently describing large numbers of fossil species. As it is, there is an obvious conflict between Recom. 7A.1 and Art. 37.5, the one urging to do something, the other making this same thing a condition for valid publication. I strongly suggest that few undeserving names, now not validly published under Art. 37.5, would be retroactively valid by the change here proposed. However, on the other hand, numerous very deserving names (e.g. Below, l.c.) would be acceptable as valid as of the date of their effective publication.]

Division III.4.(c): Add: "Proposals for changing the Code, accepted by the vote of the Plenary session, will be published in the form decided upon by the Editorial Committee, in a special section of the Code, as "Proposed future changes of the Code"; objections, demonstrating deleterious

3 not on disk

consequences of these changes, or their wording, may be submitted for discussion before final approval is voted at the next International Botanical Conference."

References:

Below, R., 1987. Evolution und Systematik von Dinoflagellaten-zysten aus der Ordnung Peridinales. I. Allgemeine Grundlagen und Subfamilie Rhaetogonyaulacoideae (Familie Peridinaceae). *Palaeontographica*, Abt. B, Bd 205, p. 1- 164 ; pl. 1- 26.

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Tansoni

(1)

All, would you please read through this and give me any criticism or other comments you can think of? Thanks!

Can you
write

PROPOSAL TO STOP MEDDLING WITH THE ICBN, AND TO TURN BACK SOME RECENT CHANGES

J. S. Bur

In the last editions of the ICBN, a number of changes have been introduced that have improved neither the Code, nor the stability of botanical nomenclature. The Code has been a standard against which validity of publication of names could be objectively measured. However, with the mechanism of opening the wording of the Code to revision at each International Botanical Congress, the temptation to remake the Code in their image has proved irresistible to too many taxonomists. The justification for changes has become increasingly ephemeral, and the implication of many changes was insufficiently thought through, with the result that these changes have consequences never intended - or even imagined - by the authors of the proposals. If we subscribe to the principle that the ICBN is meant before all to provide stability of botanical nomenclature, we should collectively become much more conservative in proposing alterations in the Code.

The more the Code is "upgraded" and "improved", the louder become the voices of those who want to abolish the Code and its Rules for validity and priority, and replace these Rules with a list of Names in Common Usage (NCU), which is based on hasty research by a small number of overworked people. This NCU list may be submitted in Tokyo for approval; with a bit of lobbying and maneuvering, it might well be accepted by the vote of the floor; and we would exchange an admittedly imperfect document, the ICBN, of which we more or less know the principles and shortcomings, for a new product that will turn out to have as many flaws as what went before, but will lack the safeguards and objectivity of the Code.

Let me point out a few of the horrors that have been accepted by the last two Congresses, or by the Editorial Committees responsible for the last editions.

Validity of publication always had to satisfy the demands of clearly spelled-out conditions. Now, we have **Art. 37.3**, in which (**as condition of valid publication**) is stated that "Citation of the collector's name **and/or** collecting number **and/or** date of collection **and/or** reference to any other detail of the type specimen or illustration is required." As they say in California: **GET REAL!** Does this Article imply that anyone can come along and refuse to recognize the validity of a name because he considers that not enough detail was provided on the time of the day, or week, when the specimen was collected? Or on where it grew, in what type of soil, or whether the fossil derived from a sandstone or shale? Or, on who drew the illustration? Or, because the author did not provide all the initials of the name of the collector? And what, if only two out of these three and/or more conditions were met? And at what date become these requirements operative? (In Art. 37, there are two dates, 1958 and 1990, listed in two other subarticles; the Code itself was published in 1988).

its poorly worded, but to use it means that anything is acceptable

number of collectors
a date
or some other detail
that permits identification
to be established

2

Art. 37.1 now states that the "Publication on or after 1 Jan. 1958 of the name of a new taxon of the rank of genus or species or below is valid only when the holotype of the name is indicated (see Arts. 7-10; but see Art. H. 9, Note 1 for the names of certain hybrids)." This Article was approved in Berlin with the expressed assurance from the Nomenclatural Committee that it would not affect accustomed normal practice, or the status of validity of names previously proposed. Yet, this requirement is at wide variance with previous practice. Where previously the type species (a group, allowing some flexibility in its interpretation) was the type of a genus (a subjective group, not necessarily a naturally cohesive unit), now the nomenclatural type of a genus is a specimen. And, this requirement has been made retroactive to 1958! It is highly unusual to make the starting point of such a radically different approach in nomenclatural practice a date 25 years before the date of its first introduction. The practical consequence in paleopalynology is that many genera, validly published in that active 25 year period, and typified by validly published species, retroactively have been made invalid, if their type species had not been typified by a single holo-/lectotype.

One example, out of many: Mildenhall (1972) validly published the generic name Acaciapollenites, with Acacia myriosporites Cookson 1954 as nomenclatural type. Cookson had not designated a holotype, but the present Code still recognizes this species as validly published. Mildenhall was aware of its lack of a single nomenclatural type, and tentatively suggested a lectotype, without formally designating it. Although Mildenhall had published the generic name Acaciapollenites in a valid manner in 1979, and although it remained a valid name through 1987, the changes in the 1988 ICBN made it invalid. Jansonius & Hills (1990, card 4588) observed that they had, in the 1976 Supplement, formally designated as lectotype the specimen suggested by Mildenhall. By accident, therefore, they were the first to satisfy the requirement of the 1988 Code for a single type specimen for this generic name; consequently, they must be considered as the valid authors of the name Acaciapollenites, of which the date of publication -- as things stand now -- is 1976. Any species assigned to the genus before 1976 were not validly published.

should go back to species, not holotype

Another effect of the new **Art. 37.2** is that we now can have a genus based on a type specimen that is not the same as the type specimen of its type species. An example: Compositosphaeridium Dodekova 1974, validly published as a monotypic genus, with as nomenclatural type the species Hystichosphaeridium costatum Davey & Williams 1966. Beju 1971 had considered this latter name to be a junior synonym of H. polonicum Gorka 1965; Dodekova 1974 did not include Beju 1971 in her bibliography. Erkmen & Sarjeant 1980 supported this synonymy, but proposed the new name C. bulgaricum for the specimens described and illustrated by Dodekova 1974. They also included C. polonicum (= C. costatum) in the genus, but proposed that C. bulgaricum be considered as type species of that genus, because it was on

what do you mean "effect" you haven't mentioned any sp. in this part.

was Beju's? r.s. at?

Seems wrong to should have been called C. polonicum by Dodekova as the type of C. pol. (Gorka's spec.)

the basis of this material that Dodekova instituted the genus Compositosphaeridium. This, however, is contrary to ICBN Art. 7.2, which stipulates that "A nomenclatural type is that element to which a name of a taxon is permanently attached, whether as correct name or as a synonym". Thus, the holotype of the type species of Compositosphaeridium, C. polonicum, is the specimen designated by Gorka 1965 (l.c., pl. 3, fig. 5), while the holotype of the generic name Compositosphaeridium remains the holotype of H. costatum, as designated by Davey & Williams 1966 (l.c., pl 10, fig. 4). So much for the notion of a hierarchy of taxonomic groupings based on a single type specimen.

Art. 37.5 contains another new requirement for valid publication: "For the name of a new species or infraspecific taxon published on or after 1 Jan. 1990 whose type is a specimen or unpublished illustration, the herbarium or institution in which the type is conserved must be specified. Note 1. Specification of the herbarium or institution may be made in an abbreviated form, e.g. as given in the Index Herbariorum."

In previous Codes, this practice was included as a Recommendation; nobody could have objections to such a recommendation. The rationale for the change from non-binding Recommendation to Article, binding on an author as a condition for valid publication, had much to do with a few researchers being stymied by colleagues who had developed such a degree of possessiveness of "their" types that they did not allow any other taxonomist access to them. I can understand this frustration. However, this new condition for valid publication creates other problems. In palynology, types are often contained on a glass slide on which hundreds of other potential types are still waiting to be described. If an author has unfinished business on a slide from which some type specimens have been published already, it is not reasonable to expect him to cease work on those specimens merely to satisfy the curation requirement of Art. 37.5.

An example is provided in the publications by R. Below, a leading researcher in the field of fossil dinoflagellate cysts, who has published the first three parts of a series of epoch-making taxonomic reviews of Mesozoic dinoflagellate cysts, including the description of new genera and species. Unfortunately, in our present world, it is difficult for a young researcher to find a permanent base, and consequently Below has led a wandering existence. His first papers were produced in Bonn, but now he is working out of Utrecht. He may be based somewhere else in five years. Because his preparations contain more than one specimen, he cannot deposit his types in an institution; rather, he needs them for continuing observation, on which the following parts of his publication are based. He could state that his types are in an institution, when that would not be true --- yet, his new taxonomy and nomenclature would be accepted by the Code? However, (and maybe for that reason) many institutions now require actual possession of the specimens, before numbers are assigned to them. Once they are curated, specimens are rarely sent out on loans, certainly not on long-term open-ended loans. So, should we consider the new names proposed by Below as invalid? Dinoflagellate workers will not accept

newly
illustrations

such an inane proposition, and will ignore the Code. And I think, they are correct.

What about fraud or carelessness? To illustrate this point, I refer to the generic name "Spinitricolpites Scholtz". Scholtz (1985, p. 13) wrote: "The full series of slides with holotypes and paratypes ringed and documented is deposited in the South African Museum. A register for these holotypes and paratypes is given in the species list (e.g. SAM-K6155)." In his publication Scholtz illustrated some of his species with more than one specimen, and did not indicate which of these illustrations represented the holotype. The new generic name Spinitricolpites, with nomenclatural type S. jennerclarkei, is illustrated by three specimens, one as SEM photograph, one in polar, and one in lateral view under transmitted light. The holotype is documented as No. SAM-K6182 (l.c., p. 16), but is not further identified in the text or plate legend. Jansonius & Hills (1990, card 4774) had overlooked this last statement and inferred that no type had been designated. They wrote to the South African Museum for further information; when they did not receive an answer, they selected fig. 15J as lectotype and thus thought to have validated the publication of the species and the genus. [Art. 7.18 specifies that: "The type of the name of a taxon of fossil plants of the rank of species or below is the specimen whose figure accompanies or is cited in the valid publication of the name (see Art. 38). If figures of more than one specimen were given or cited when the name was validly published, one of those specimens must be chosen as the type."] However, after renewed correspondence in 1991, I received information from the South African Museum (Fax from R.V. Dingle, 26 April 1991) that Scholtz's material is not to be found in the Museum, and probably never was submitted there; Scholtz told Dingle not to remember where the material had gone, after he finished his study of it at Stellenbosch University. The name Spinitricolpites was validated, because the genus is necessary and used in the literature; under the current circumstances, it is difficult to decide whether or not it was validly published -- if so, by whom and when; if not, how it can be salvaged using the material so far involved (which per se is unambiguous and clear).

ambiguous & unclear?

Art. 38 reads: "In order to be validly published, a name of a new taxon of fossil plants of specific or lower rank published on or after 1 Jan. 1912 must be accompanied by an illustration or figure showing the essential characters, in addition to the description or diagnosis, or by a reference to a previously and effectively published illustration or figure." **Art. 38** does not refer back to **Art. 7.18**, and we thus have an ambiguous situation, where valid publication requires on the one hand an illustration showing the essential characters, on the other hand requiring the type to be (one of) the illustration(s) accompanying the description. If the single illustration of the holotype does not show "essential characteristics", the publication of the name presumably would not be valid. But **Art. 7.18** is also ambiguously phrased: does it require a designated specimen (whose figure accompanies or is cited in the description), or does it require a specimen (whose figure accompanies or) is cited in the description? At present, it is not clear whether the Code accepts as validly published, the name of a species provided it has a designated type specimen and illustrations showing the

Designation or not mentioned

a specimen whose ---

5

the obvious!
7.18

essential characters (whether or not the figure of the that type specimen has been identified, as long as it is included in the figures), or whether it requires that the type specimen be clearly identified as such. This quandary is exemplified by the genus Spinaepollis Krutzsch 1961, with type species Pollenites spinosus Potonié 1934. Potonié had not designated a holotype from among his three illustrated specimens; Jansonius & Hills 1976 (card 2645) had designated Potonié's pl. 5, fig. 18 as lectotype. However, Krutzsch 1961 had stated that the original slide containing Potonié's specimens needed to be restored, and that after restoration the three original types could no longer be identified as such; for that reason, he designated another (or possibly one of the same?) specimen(s) on that slide as neotype (slide 189, coordinates 2.9 x 122.5), and provided new photographs of specimens assigned to the species, without specifying whether or not the neotype was included among them. Is the generic name Spinaepollis now validly published as of 1961 (single neotype by Krutzsch), or as of 1976 (designation of lectotype from among published figures), or still invalid, because we now happen to know that the lectotype cannot longer be identified on the slide, and the neotype may, or may not, have been illustrated? For sake of nomenclatural stability, I adopt the first possibility, and consider the name validly published as of 1961.

Photocopy
types were
Schweizer

nothing is said
about that

Expanding a bit further on the curation requirement of **Art. 37.5**: who determines which institution is sufficiently official to qualify as a permanent receptacle for type specimens, so that the names based on those types will be accepted as validly published? How indirect may a reference to an institution/herbarium be before we should consider that Art. 37.5 has not been satisfied? If no mention is made in the text as to where the types are stored, is it enough to assume that sample numbers like "FGS 333" indicate that they are stored by the Finland Geological Survey, if the authors of the paper happen to be employed by the Finland Geological Survey? Even if there appears little possibility of coincidence, I think that such is not sufficient. In many regions paleobotanists or palynologists have been employed by coal mines or oil companies with research institutes; publications have gone out, and continue to be published, in which storage at their institute is advertised. Yet, under the present economy, many of these institutes are folding, and it is not always clear where the types have gone. Does that retro-actively invalidate the publication of a name earlier accepted as validly published? And, is storage "in the authors private collection" really less trustworthy if measures have been put in place that his types will be sent to a larger institution at the end of his working career? I have found no evidence that this type of question was considered when Art. 37.5 was proudly adopted by the Plenary Session.

again
illustrations
would solve
this!

PROPOSALS:

6 - and is deleted in
Art. 7.18: First sentence (cited above) to read "The type of the name of a taxon of fossil plants of the rank of species or below is the specimen whose figure accompanies, or the designated holotype/lectotype cited in, the valid publication of the name (see Art. 38)."

Art. 37.1: Change "1958" to "1988" (or a later date). *no!*

Art. 37.3: Demote last sentence of Article to status of Recommendation. *This would foul up many discussions in respect to change the species names!*

Art. 37.5: Demote Article back to Recommendation 37A.2.

References: *Best 1977?*

Ertegl
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Godwin 1974
Erkmen, U. & Sarjeant, W.A.S. 1980. Dinoflagellate cysts, acritarchs and tasmanitids from the uppermost Callovian of England and Scotland: with a reconsideration of the "Xanthidium pilosum" problem. Geobios, no. 13, p. 45-99, pl. 1-8

Orin 1965
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~~Schultz 1985~~ ?

~~Hutzsch 1961~~

~~Mullerbach 1992~~

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in Jackson. I sometimes forget because it
seems so obvious. I have never had a
paid assistant since I left Shell Oil in
1962. I couldn't help but think of that
on the platform in Jackson — the others,
such as Frank Vascher, have relatives of
co-workers.

I agree with you re Puci — in my generic
list done for Index Nov. Gen. I accepted
a few — I believe the same ones as you
(I hope).

Whoops — your "p. 5." is where the fun begins!
You are right — 7.18 and 38 must be
cross-referenced. I believe a proposal to
that end would be good. However, the paleo-
members who sit on the drafting committee
could suggest it anyway. I've always thought
it obvious that a ^{new} species is not illustrated, if the
caption doesn't say what figure is of the
new taxon. However, I guess you are seeing a
case where the new taxon is illustrated but with
> 1 picture, and the type is not designated.
I guess per 7.18 there is a problem, but
wait, doesn't 38 require "... an illustration..."
If there are a slew of ems, or even 2, I claim
38 is satisfied. Now, you got a problem
with 7.18 — it seems to me

— must be exactly where... e.g., even "Penn. State University" is not good enough #.

with 7.1. I guess and no choice given between them? That is not a figure! 7.18 should be illustrated, and 38 amended to show that one figure is needed, or multiple pictures of one specimen.

mail is
delivered faster
here than at
the Museum - and
also on Sat. &
holidays



not
"6"

Traverse
Beethovenstr. 36, Nr. 25
6000 Frankfurt a. M. 1
↑
Deutschland

↑
the "7" is
needed

25. I. 92

Dear Jan.

Your letter in the envelope with no stamps
and no cancellation arrived at my
office yesterday - mystifying!

In answer to question 1: 1st class mail
from PSU is being forwarded, but
it takes a while. There also is, of
course, the danger that the Sec. will
~~misinterpret~~ her instructions!

2. I'm working on my chapter, and should
be done before too terribly long. But it is
hellish time to be doing it - the whole
question is like the former Communist world -
absolutely in flux! Scharschmidt, ^{has} discussed
it during most lunches. That, of course,
is very destructive of interest in our Committee.

3. see Van der Spammer. I am writing
at home (we are now "apartment" and
bath - we share a "kitchen" - nothing works -
with an Indian lady, who is very nice but
makes weird noise and clogs the sink).

So - I can't check this page with
the rest of the publication - and Schaefer's
may not have it anyway.

As you know, many of VdH's taxa are not
officially published for various reasons,
as you note in the cards. I guess you are
now asking "are all of his names -
at least in this one publication not validly
published because the author "does not
accept them in his own publication" - or whatever
the meaning of that rule is."

As I read this, it does not constitute
VdH "not accepting the name," per the ICBN
provision. ^{VdH's} Nevertheless we give our
spore-system here only as a proposal
--- "VdH speaks only of his
"system," which he says is provisional.
This does not, in my opinion, affect
the names as such, if they meet the rest
of the requirements.

Re ^{NCV}ICU - I am 99.99% confused
about what the significance of those lists
is. But they may be the wave of the
future?! Greuter has written Chalva & me,
attacking polyploid-polyploid for accepting
way too many (I believe he speaks of 60%
+ 0 way) names for the NCV list - etc. - etc.

Best,
Al

When this one page + envelope weigh 5.0g. cost 1.90, net = \$1.25 - !
\$1.07 (1.65 net) - So the net going out

PALYNOLOGY AND STRATIGRAPHY

Editors: J. Jansonius
D.C. McGregor

A1 + Betty
Dear Chapter coordinator,

92 01 12

We are in a new year; the year marking the silver jubilee of the AASP, the occasion that was in part the inspiration for our book. It is now obvious that we will not finish the project in 1992. However, we have made significant progress. Statistics can be used in various ways, and are a slippery measure to quantify our progress. Does our book have 23 chapters, or do we count the A and B parts of several chapters as 'subchapters', equivalent to full chapters?

Five (sub)chapters now have passed first reviews, and are nearly in their final form. Several more are very near completion, as is evident from correspondence with their authors. The finished contributions are first-class, and we are sorry that our resources do not allow to send each of you a xerox copy of these MSs; they would inspire you to finish your own contribution -- not only as soon as your enthusiasm can manage, but also to the same high standard.

This letter is to keep you informed on what is going on, but also to impress upon you that **you have an obligation to fulfil**. Colin and I have been patient so far, because we do not want to rush anyone into delivering a substandard product. Now, we have not only to consider our own obligation to the project, but as well the obligation to those contributors that have written their MSs: these are very up-to-date, but will not stay up-to-date for ever. I urge you to now give your first priority to finishing the MS for your chapter. Do work late, or reschedule some other business: **we want to see your work in the next few months -- that means January-February-March!**

We would ask each coordinator who has contributing authors to, if they are not already informed, immediately write/phone them for an assessment of their scheduling; this may be by a separate message, or by sending a copy of this circular letter. On the other hand, we have had some correspondence with contributors who had not heard back from their coordinators. We do want to keep to the present command structure, where the chapter coordinators are responsible for the deadlines of their contributors.

Moreover, I want to ask you to **send me, by return mail, your time table** for finishing the chapter you have agreed to coordinate/write. If there are obstacles that create delays, I would like to be informed immediately, so we can discuss matters by telephone and/or fax, and find solutions or alternatives. We want to end any procrastination. We have spoken with some authors in the past months, and agreed on an extension till later into the year. This could be done without delaying the date of the project's finish, provided you other authors **honour your commitment**.

Two of our contributors have received signal honours: Dr. Andrew Knoll was elected to the National Academy of Science; Dr. Alfred Traverse (who now is, for a 6 month sabbatical, at the Senckenberg Institute, Frankfurt a. M.) was in India where he received the biennial international medal at the Birbal Sahni Institute of Palaeobotany. Congratulations!

In conclusion, we want to wish you a happy and productive 1992.

*I sent some stuff to Penn State:
do they forward your mail,
or just cache it?*

[Signature]
Jan Jansonius
Esso Plaza West, room 1555
237 - 4 Ave SW
Calgary, Canada T2P 0H6

PTO

P.S.: In preparing the MS, please observe that we would like two spaces after each full stop.

In author citations, we want no comma between author and year. For new combinations, we want only the year for the last author: *Rossipollis reticulatus* (Ross) Krutzsch 1970; if the author validating the new combination also effected a significant (i.e., more than ephemeral) emendation of the taxon involved, the citation would look like: *Sagenotetradites bonapartensis* (Playf.) emend. Satterthwait & Playford 1986 (no comma!). Emendations not proposed at the time of a transfer should not be indicated in the list of taxa at the end of the chapter, unless the emendation is of fundamental significance, in which case the emending ("emend") author and year is separated from the validating author and year by a comma.

There are no uniform ways to handle abbreviations; we now want to standardize usage in our book by ending all abbreviations with a period (i.e., ca., ed., eds., emend., et al., ibid., viz., e.g., l.c.; etc. -- however: Ma = Million years); if "etc." ends a sentence, we will not use a double period.

In citation of Russian or Chinese literature, give the title in translation, but transliterate the author name and bibliographic reference. Transliterate using the Library of Congress convention [inverted R="ia" (not "ja" or "ya"); I-O="iu"; soft sign - resembling "b" - rendered by an "i"; short "ee" sound resembling "bl"="y"]. Add "(in Russian)" to the end of the reference.

Names of Chinese authors should be given in full, e.g. ZHANG SHANZHEN; not as ZHANG S.

In the References, papers cited in the text as "in press" should be listed as, for example, BROWN, A. In press: A discussion of.....; not as BROWN, A. 1992: A discussion of.....In press.

Only the first word in a title of a publication should be capitalized (except, of course, words that normally require capitalization, such as German nouns, formal stratigraphic units, etc.).

In composite text-figures, different parts of the figure should preferably be numbered by capital letters: 1A, 1B, etc.

Unpublished data from the author's files can be referred to by "(unpublished)"; unpublished information from someone else by "(Smith, unpublished)", "(Smith, in press)", etc.

The first time a taxon is mentioned in the text, cite author and year; *Skiagia ciliosa* (Volkova) Downie 1982; *Asterocapsoides* Yin & Li 1978; *Chuarina circularis* Walcott 1899, emend. Vidal & Ford 1985. In subsequent use in the text, and use in diagrams/charts, omit the author citation.

Once again we would like to state since we think it to be a very important point, that a palynological nomenclature system will result only if all students construct generic names in the same manner, and use the morphological terms in exactly the same sense.

Since it is not our aim to describe all the genera known up till now, but rather to indicate the theoretical possibilities, we will give formal descriptions for only the more common genera and subgenera. Other students who use our sytem, and who propose another genus or subgenus not described here with its type-species, may take the corresponding theoretically possible name and genus mentioned in this article, and describe it as a new genus with a type-species.

Undoubtedly there will be cases in which new generic names must be created since they are not covered in the theoretical possi**bi**lities mentioned here. In such cases the names will have to be formed in the same way, on the base of the main morphological characters of the corresponding new genus.

Without doubt there are still many problems, in the case of Paleozoic spores, involving priority, which only can be solved by an International Congress, since many of these spore-genera are well established and valid. But one may ask if it should not be preferable to unify all the names according to one and the same artificial system. Nevertheless we give our spore-system here only as a proposal, not describing the genera on the base of type-species, and leaving acceptance or rejection of this system to specialists in this field.

One item is left to be treated in this introduction, As the system is purely artificial based on morphological characters alone, both fossil and recent pollengrains and spores belong to the same artificial genera. In this respect it is generally of little importance to give the recent pollengrains a specific name,

A1, does this mean that we should not accept the generic names in Van der Hammen's publication? (e.g. *Polyadites*, *Tetradites*, etc.). Yet, he does have these as "nov. gen.", with a "nov. fsp." in each genus (which becomes the type sp.). Or, are they now ICU, and therefore ok.?

Two means of resolving the confusion are in our opinion:

- 1) Having all pollen students form the names of their new genera in the same way.
- 2) Having all pollen students use the morphological terms, employed in the construction of generic names and in the description of genera, in exactly the same sense.

Therefore we proposed to use morphological terms and definitions according to IVERSEN & TROELS-SMITH (1950), as correspondence with many European and American colleagues showed that this system of morphological names, being very logical and with well defined terms, is accepted as one of the best by many pollen-students. Consequently we also proposed to use a part of these morphological terms for the construction of the generic names.

We received, after the above mentioned publication (VAN DER HAMMEN, 1954 b), many letters from colleagues expressing enthusiasm about the system proposed. Several of them also gave valuable detailed advice, and we would like to express our gratitude to them all at this time.

After realizing that many other workers agreed with our system, we resolved to publish it in English in a more definite form, describing the new genera with their genotypes. Although we have used this system in a former publication (VAN DER HAMMEN, 1954 a) in order to classify some 150 species from the Maestrichtian and Paleocene of Colombia, we did not formally describe the genera, not wishing to create a new system without knowing the opinion of other students.

In the following pages we will first describe the way in which our generic names are formed, and then establish the most important theoretically possible generic names.

Boletín Geológico IV (2-3)

Dr. A. Traverse
Palynology - 435 Deike Bldg./ Penn State University
University Park, PA 16802, USA

92 01 02

Dear Al, I am presently near winding up the typing of the next Supplement of the Genera File; I am incorporating the many corrections that you had sent me earlier, among another list that I had worked up myself. Some of your corrections are a matter of choice and preference, but others are most definitely finding faults of omission or commission. I am afraid that I have been 'commissioning' the greater part of those.

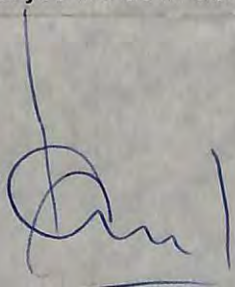
However, there are some things that are almost intractable. For instance, you asked whether it should be "Inapertisporites pseudoreticulatis" or "I. pseudoreticulatus". Well, Rouse originally wrote --is; yet, it is obvious that he meant his epithet to be a qualifying adjective, in the Latin language. In that case, the ending should have been --us. In the ICBN, Art. 23.5 requires that an adjectival epithet should agree grammatically with the generic name. In adjectives with the ending --us, I can make that ending agree grammatically (in case of a transfer to a genus with a name of neuter gender) as --um. However, if I maintain the original ending --is, I cannot accept that as a Latin adjective, and change it to --e in the same hypothetical case, simply because that would not be correct (grammatical) Latin. So, should I accept the writing --is as an epithet from whatever source (as per Art. 23.2) and leave it unchanged when transferred to a neuter gender? Undoubtedly, another man with a bit of knowledge of Latin will think that the only correct action is to change it to --e. Then, should we all swallow that, or should we correct that back to --is (as an "indeclinable epithet")? I should very much like to have your judgement on that, before I undo my earlier decision to correct the orthography of --reticulatis to --reticulatus. Yes, I am aware that I cannot find explicit consent for such a correction in the Code; but neither can I find an example where such an atrocity is maintained as originally produced.

I have now about 110 new cards typed up. In an earlier letter you implied that you might be willing to read through them before they were printed, just to help make sure that I did not make as much of a fool of myself again as I did last time. If that offer still stands, I'll make you a xerox of the lot ASAP, and mail them out. On the other hand, I can see that you may have more important things to do, in which case I would not want to impose on you. However, you are also still involved in the Utrecht scheme of listing generic names; if, for the benefit of that work, you would like to have a 'preprint' xerox of my holdings, let me know, and you shall have them.

How is the chapter coming along? I do not want to press you beyond reasonable limits, but I would very much like to have a continuous appraisal of all collaborators in the book about when they think they may submit the first draft. I know that you will be in Germany later, and wonder how all things will fit together.

With best wishes for the New Year,

sincerely



Jan Jansonius

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

Dear Al,

91 12 23

Thank you for your letter. Lots of news in a few lines. Firstly: congratulations on the honor received in India; I think you have well earned the recognition, and I am happy for you and Betty (who must be mentioned, I think, in all you have accomplished: there is physically too much production to manage it all by yourself alone).

I am steadily working along on the next supplement of the Genera File. Included are my first drafts on Puri's 1963 paper. I thought you might want to see what I did with it. My first inclination was to wish that it was not an effective publication. I wrote off to Ibadan in the hope to have them reject it, but I never did get an answer. However, we have in the past accepted the early work by Couper, from New Zealand, and van der Hammen, from Colombia, which also had mimeographed text parts. We also have, in the past, accepted the publications by R. & W. Remy, in *Argumenta botanica*, which had glossy prints in a pocket as their illustrations. By extension, I cannot see how we can not accept the University of Ibadan Botanical Studies as effective publication. It still makes it not a better piece of work. Thankfully, it is so bad, that many names can be rejected as invalidly published.

I did not treat the first few names on sporangia. You will have to evaluate those for your list of NCU. [I just received the latest Taxon, with the new proposals to give that list nomenclatural protection. I have seen such pious and specious reasoning only in politics and religion, and had never expected that bigots would deem botanical nomenclature worthy of imposing a dictatorship. It is incredible what some people will do to exercise power.]

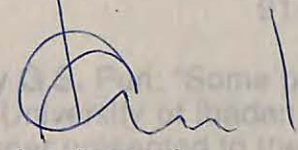
I was happy to learn that you have begun work on your chapter. We now are receiving significant bits and chapters, and the floodgates will open early in the new year. Too bad it could not have been spread a bit more evenly, but I am happy that the project is coming together!

When are you leaving for Germany?

I think that's all for now. I am not really a touch typist yet, but now can work with all 10 fingers, and find that typing is faster than writing. **You** just try to "type with Mavis" a bit, and see how you like it.

Best wishes for the New Year (I trust that you had a good X-mas. We sang a couple of other Messiah's, and the piece starts to grow on me. Now it's the Carmina Burana in April. Is Betty still with her choir?) and best regards to the two of you,

Dear Sir/Madam,


Jan Jansonius

P.S.: I just had a phone call from Rob Fensome, regarding the application of Art. 7.18. This article has been in the Code since 1961; I don't know who put it in, and why it has not been better cross-referenced with Art. 38. There are many instances in the palynological literature where there is a designated, curated, holotype, but where in the plate description the illustration of the holotype is not identified. In the Genera File I have accepted those as validly published. Fensome et al., in the Acritarch Index, have not accepted them. Are they correct? (I am afraid so.) Should we do something about this? Or should we just push the Editorial Committee of the Code to make better cross-references, so that it is clearer to all workers what the requirements of the Code for fossil plants actually are? Will you give me your opinion on this, please?.

Now that we are on the topic of nomenclature: I had submitted a note to Elsevier, which was printed in the latest issue; unfortunately, they left out a couple of lines, and turned my "insufficient" into "sufficient", thus going contrary to my intention. If you see it, you might as well know that I have asked for a new printing, of the whole thing as I wrote it. What is your opinion on the requirements for an explicit statement of where the types are stored: is "GSF preparation no. 1088" sufficient, if the authors have an address at the Geological Survey of Finland, but if there is no further statement in the text that the types are stored in the collections of the Survey? If yes, would "SSF prep.no. 1088" have been not acceptable? I think that you must accept literally what the Code states, or else there is no point to abide by the Code at all.

How much time do I still have before I can submit a proposal to change the Code?

JJ

Chairman/Curator/Librarian
Dept. of Geology
University of Ibadan
Ibadan, Nigeria

Dear Sir/Madam,

91 03 21

Recently I saw a copy of the publication by G.S. Puri: "Some plant-micro-fossils from the Cretaceous and Paleocene of Nigeria"; University of Ibadan Botanical Studies No. 10, p.1-142, with 10 photo plates (Paper presented to the 1963 Central African Scientific and Medical Congress, Lusaka, N. Rhodesia, August, 1963).

In this paper are a number of new names for species and genera of fossil spores and pollen. In order to evaluate whether or not these names were proposed in the required manner, I need some further information about this publication. It was not printed, but reproduced by mimeograph, which generally indicates that it was not published in a strict sense (although there are some exceptions to this). There is no printed cover, which makes it difficult to judge whether the copy I saw is an offprint, a preprint, or a regular issue of the U. of I. Bot. Studies.

Could you please tell me what is the usual method of distribution for the series "University of Ibadan Botanical Studies"? Is it available for sale, or on request, or on exchange? Can I still order a copy of No. 10? How many copies of No. 10 have been produced and were distributed? Could you provide me with the names of one (or a few) institutions or libraries in Canada or the United States where this series would be available? The paper was read at a 1963 conference, but was it also published in 1963, or possibly at a later date?; this is important information for determining the priority date of the new names in it that may be validly published. Dr. Puri did indicate that his types were on certain numbered slides; are these slides still preserved and available for study at the University of Ibadan?

Of the many names that are proposed in this paper, many were not proposed in a valid manner, because there was no designation of a type species, or designation of a unique holotype. For that reason, it would solve potential problems if the publication itself would turn out to be a casual report, that was sent to a small number of colleagues for their information, but that was not an "effective" publication in the sense of the Botanical Code for Nomenclature.

My interest in this matter is based on my involvement with the publication "Genera File of Fossil Spores", which is a compilation of all new names proposed for fossil spores, with an update every two years. I should like to evaluate the status of Puri's report before the next Supplement, and I will be much obliged if you could provide me with answers to my list of questions.

Yours sincerely,

Jan Jansonius
Esso Plaza West, room 1415
237 - 4 Avenue SW
Calgary, Canada T2P 0H6

VALSARITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 14

Type species: Valsarites senonianus Puri, *ibid.*, pl. 1, fig. 15

Diagnosis: "A large ascospore."

Description of type species: "This is an ascospore of some Ascomycetes, measuring 21 x 12 μ in the middle of the broader segment. It is divided into two halves by an equatorial wall that is continuous. The lower half is narrower, about 10 μ or so. There seems to be some sort of irregular and faint reticulation. The wall of the spore is not thickened."

Resembles ascospores of Endothia, Didymosphaeria, Othia and Valsaria; the size is closest to those of Valsaria insitiva.

Monotypic.

Senonian; Nigeria.

Remarks: The illustration of the holotype consists of a rather crude line drawing, mounted on a plate with many others, which then was photographed and printed, slightly out of focus, as a 3.5 x 5" (9 x 12 cm) glossy print.

PSILAMONOCOLPITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 106

Type species: Psilamonocolpites spheroides Puri, *ibid.*, pl. 10, fig. 293

Diagnosis: "Psilate, monocolpate grains are included under this."

Description of type species: "This medium-sized spheroidal grain measures ca 40 x 33 μ . Exine is between 3-8 μ in thickness. The furrow is thick-rimmed. The grain is psilate or it may be tegillate.

This has affinities with the Palmae and is named as Psilamonocolpites spheroides."

Senonian; Nigeria.

Remarks: The generic name is a senior synonym of Psilamonocolpites van der Hammen & Garcia de Mutis 1965, and Psilamonocolpites Mathur 1966.

The illustration of the holotype consists of a rather crude line drawing, mounted on a plate with many others, which then was photo-graphed and printed, slightly out of focus, as a 3.5 x 5" (9 x 12 cm) glossy print.

PSILAPERIPORITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 91

Type species: Psilaperiporites spheroides Puri, *ibid.*, pl. 9, fig. 258

Diagnosis (combined description): "This is a large spheroidal sporomorph, measuring ca 80 x 65 μ . Exine is 1-5 μ in thickness, psilate, or with fine scabrae. Fine rounded pores, 2-3 μ in diameter, are seen."

Shows Cyperaceous affinities. Pollen grains with 4-5 pores are found in Cladium complanatum and with 4 in C. articulatum (Cranwell, 1953, p. 44) of the Cyperaceae, but the size of these grains (30-60 μ) is smaller, and their exine is faintly granular to foveolate, often with large gemmae associated with the exits.

Monotypic.

Senonian; Nigeria.

Remarks: The illustration of the holotype consists of a rather crude line drawing, mounted on a plate with many others, which then was photographed and printed, slightly out of focus, as a 3.5 x 5" (9 x 12 cm) glossy print.

PSILATETRADITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 51

Type species: Psilatetradites kuylij Puri, *ibid.*, pl. 5, fig. 147

Diagnosis: "Pollen tetrads, exine without any visible reticulation or sculpturing, etc."

Description of type species: "This is a pollen tetrad, measuring ca 40 μ in maximum diameter. Individual grain is spherical, approximately 22-25 μ in diameter. Exine in grains is ca 1 μ in thickness and the grains are smooth, without any visible sculpturing."

Erica cinerea and E. tetrax are similar in size; Loiseleuria procumbens pollen are similar to this fossil form (fide Erdtman et al., 1961, pl. 24, fig. 3-4). Kuyli, Muller & Waterbolk figured similar tetrads from the Upper Cretaceous of Nigeria as Ericales.

Senonian; Nigeria.

Remarks: The illustration of the holotype consists of a rather crude line drawing, mounted on a plate with many others, which then was photographed and printed, slightly out of focus, as a 3.5 x 5" (9 x 12 cm) glossy print.

RETIPERIPORITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 91

Type species: Retiperiporites multiporoides Puri, *ibid.*, pl. 9, fig. 259

Diagnosis: "Under this reticulate sporomorphs with many pores are included"

Monotypic.

Description of type species: "This is a medium sized barrel-shaped sporomorph, with the proximal part flat and the distal part rounded. It measures ca 62 x 43 μ . Exine is 1 μ , becoming to 5 μ thick at the proximal part, in one corner. The intine is finely and closely reticulate, which may also appear as ends of fine spinules. There are several pores 2-4 μ in diameter scattered in the sporomorph. Affinities of this are unknown."

Senonian; Nigeria.

Remarks: The illustration of the holotype consists of a rather crude line drawing, mounted on a plate with many others, which then was photographed and printed, slightly out of focus, as a 3.5 x 5" (9 x 12 cm) glossy print.

RUGUSPOROMORPHITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 9

Type species: Rugusporomorphites nigeriensis Puri, *ibid.*, pl. 1, fig. 3

Diagnosis: "Sporomorph with stratification of irregularly arranged sinuous or convolute ridges or grooves."

Description of type species: "This is a fairly large, ovoid, not particularly well preserved sporomorph measuring about 135 x 95 μ . The exine has a well marked, though somewhat obscure, stratification. Meshes are large, double-walled, varying between 7-20 μ in length and breadth. The specimen is crushed at several places, but there are no indications of a slit."

Reminiscent of spores of Targioniaceae.

Monotypic.

Senonian; Nigeria.

Remarks: The illustration of the holotype consists of a rather crude line drawing, mounted on a plate with many others, which then was photographed and printed, slightly out of focus, as a 3.5 x 5" (9 x 12 cm) glossy print.



STRIATOSPORITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 44

Type species: Striatosporites akwara Puri, *ibid.*, pl. 5, fig. 122

Diagnosis: "Sporomorph striate, striae dichotomizing and seen to converge at both ends."

Ephedra is smaller, and has fewer striations.

Description of type species: "A large, peculiarly striate - inaperturate sporomorph is spindle-shaped, and probably ruptured at both ends. It measures ca 100 x ca 55 μ . The striae are dichotomizing in the form of ridges. These are separated from the main body of the sporomorph."

No similar fossil or living sporomorph is known to the author.

'Akwara' is an Ibo word for the fibrous covering of a peeled coconut skin, and is a very appropriate descriptive term for this form.

Senonian; Nigeria.

Remarks: We accept the name as validly and effectively published by Puri; however, as a junior homonym of Striatosporites Bhardwaj 1954, it is still an illegitimate name.

ALANGIACIDITES Puri 1963

no pic

University Ibadan Botanical Studies No. 10, p. 97

Type species: Alangiacidites psilatus Puri, *ibid.*, pl. 10, fig. 270,271 (no holotype designated)

Diagnosis: none given

Remarks: Two species were described, one of which was indicated as type species; however, for the latter no type specimen was designated. No generic diagnosis was provided. Consequently, the names of this genus, and of the two species, have not been validly published.

no. p1x

BREYNIOPSISIDITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 103, pl. 10, fig. 286

Type species: none designated (no named species assigned to this genus)

Diagnosis (combined description): "This is a small 6 colporoidate grain measuring ca 27 x 28 μ . Exine is slightly thicker than the intine and is reticulate. Meshes are close and regular."

Remarks: Although there is a single type specimen described, the generic name has not been validly published, because its type is not the type of a name of a species (Art. 10.1, ICBN).

no pix

FOVEOTRILETES Puri 1963

University Ibadan Botanical Studies No. 10, p. 33

Type species: Foveotriletes reticulatus Puri, *ibid.*, pl. 3, fig. 62-64 (No holotype designated)

Diagnosis: none given

The generic name is not validly published by Puri (and hence not a junior homonym of Foveotriletes Pierce 1961) because there is no single holotype for the genus and species (as required by Art.10.1 and Art. 37.1, ICBN), and there is no generic diagnosis (the genus is not monotypic, as Puri named another species on p. 34).

no pix

GEOGLOSSUMITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 17, pl. 1, fig. 19

Type species: none designated (no named species were assigned to this new genus; hence, no nomenclatural type exists, and the generic name is a nomen nudum)

GRANADICOLPITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 107

Type species: Granadicolpites nigeriensis Puri, *ibid.*, pl. 10, fig. 298-300 (no holotype designated)

Diagnosis: "Under this are included dicolpate grains in which the sculpturing element is not clearly discernible."

Description of type species: "These are spindle-shaped pollen measuring ca 66 x 26 μ , ca 60 x 30 μ , and ca 70 x 35 μ . Exine is 1-2 μ in thickness. The colpi are on the side of the grain, which tapers into pointed ends. The grains are psilate to faintly and obscuredly reticulate."

Monotypic.

Remarks: Since no single holotype was designated for the type species and the genus, their names have not been validly published.

GRANUSPOROMORPHITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 11

Type species: Granusporomorphites sporogonioides Puri, *ibid.*, pl. 1, fig. 9,10
(no holotype designated).

Diagnosis: "Under this are included those sporomorphs in which the shape of tsculpturing is not clear, following Pierce, 1961, p. 20."

Monotypic.

Description of type species: "Two small sporangium-like structures figured here are ovate in shape with a tapering base, that may appear to be foot of a capsule (?). Both the specimens are nearly of the same size, measuring about 90 x 54 μ . Specimen in fig. 9 is a bit smaller. Foot like structure is between 12 μ x 11 μ in fig. 9 and is smaller in fig. 10. The sporomorphs have a thin outer wall, with reticulation which is indistinct and irregular."

Remarks: Two specimens on two slides were illustrated and cited as 'type', but neither one was selected as holotype. Thus, according to Art.10.1 and Art. 37.1 of the ICBN, the names of the species, and genus, have not been validly published.

GYROSTEMONACIDITES Puri 1963

no pix

on disk University Ibadan Botanical Studies No. 10, p. 96 , pl. 9, fig. 266

Type species: none designated (no named species assigned to this genus)

Diagnosis (combined description): "This is a small tricolpoid, spheroidal grain, measuring ca 27 x 22 μ . Exine is about 6 μ in thickness and is unique in being into thre parts at the colpus. There is a faint reticulation, which is not uniform."

Remarks: Although there is a single type specimen described, the generic name has not been validly published, because its type is not the type of a name of a species (Art. 10.1, ICBN).

HYDROPHYLLACIDITES Puri 1963

no pic

added on disk

University Ibadan Botanical Studies No. 10, p. 96 , pl. 9, fig. 267

Type species: none designated (no named species assigned to this genus)

Diagnosis (combined description): "This is a small tricolporoidate grain, oblate, spheroidal in shape. It measures ca 25 x 20 μ . Exine is ca 1-1.5 μ in thickness and the grain is somewhat psilate or with obscure pattern."

Remarks: Although there is a single type specimen described, the generic name has not been validly published, because its type is not the type of a name of a species (Art. 10.1, ICBN).

no pix

PSILAMONOLETE Puri 1963

University Ibadan Botanical Studies No. 10, p. 41

Type species: Psilamonolete reniformis Puri, *ibid.* pl. 4, fig. 116, 117 (No holotype designated)

Diagnosis (combined description): "These are two bean-shaped spores, without any conspicuous ornamentation. They vary in size between ca 42 x 33 μ to ca 80 x 53 μ . The exine is ca 2-5 μ thick. It is smooth and unsculptured."

Remarks: The generic name was not validly published for lack of a single type specimen for the genus and species (as required by Art. 37.1 of the ICBN). Hence, the name is not a candidate for junior homonymy with Psilamonoletes van der Hammen 1956. Puri did cite van der Hammen in his bibliography, but apparently was not aware of the existence of the latter name. His spelling was a careless mistake for an intended 'Psilamonoletes', as evidenced by the same error in quotation of Retimonoletes as 'Retimonolete'.

10.1 and +
^

Corrected on disk

has pix

corrected on disk

RETINAPERTURITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 75

Type species: Retiinaperturites megaspheroides Puri, *ibid.*, pl. 8, fig. 205-207
(no holotype designated)

Diagnosis: "Under this are included inaperturate sporomorphs with a conspicuous reticulation."

Description of type species: "These are large spheroidal sporomorphs measuring ca 104 x 92 μ ; 90 x 70 μ and 78 x 75 μ . Exine is not continuous with intine, and measures ca 2-10 μ in thickness. These are marked into a fine reticulation which is ca 1-1.5 μ in diameter."

Senonian; Nigeria.

Remarks: Since no holotype was designated for the type species and genus, their names have not been validly published.

RETIMONOPORITES Puri 1963

no pix

University Ibadan Botanical Studies No. 10, p. 89

Type species: none designated

Diagnosis: "Under this monoporate , reticulate pollen are included."

Remarks: Two species were described by Puri; as neither was designated as nomenclatural type, the names of the genus and species have not been validly published.

no pix.

RETIPOLYADITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 55

Type species: Retipolyadites cooksoniae Puri, *ibid.*, pl. 5, fig. 153,154 (no holotype designated)

Diagnosis: "Pollen grains united in a polyad. Number of grains 24 or more. The grains have a reticulation."

Description of type species: "These are two polyads of about 16 [sic!] cells each. They are nearly circular. Size in fig. 153 is ca 42 μ in diameter and fig. 154 is a little smaller polyad with somewhat irregular shape. This is ca 40 x 33 in size. Exine is ca 1.5-2 μ in thickness. The individual cells are irregular in shape and do not to have any furrows. There is a close mesh reticulation in the polyads.

Polyads with 16 cells occur in the family Mimosoideae."

Senonian; Nigeria

Remarks: Since no holotype was designated for the type species and the genus, their names have not been validly published.

no pix

TAMARICACIDITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 95

Type species: Tamaricacidites tricolpatus Puri, *ibid.*, pl. 9, fig. 264, 265 (no holotype designated)

Diagnosis (combined description): "These are two small tricolpate grains, measuring ca 24 x 25 μ . Exine and intine are not distinctly differentiated and grains are faintly baculate or reticulate."

Remarks: Because no single holotype was designated for the species and genus, their names have not been validly published.

XYLOGRAMMITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 16, pl. 1, fig. 17

Type species: none designated (no named species were assigned to this new genus; hence, no nomenclatural type exists, and the generic name is a nomen nudum; Art. 10.1, Art. 37.1 of the ICBN)

RETISPOROMORPHITES Puri 1963

no pix

University Ibadan Botanical Studies No. 10, p. 11

Type species: Retisporomorphites reticulatus Puri, l.c., p. 11, pl. 1, fig. 5-8 (no holotype designated)

Diagnosis: "For sporomorph with thick walled reticulation."

Description of type species: "Three sporangium-like bodies are medium-sized, measuring, 110 x 45 μ ; 92 x 48 μ and 75 x 40 μ , respectively. These are somewhat like half moon in shape. Fig. 8 measures nearly 130 x 35 μ and is strikingly spiral-shaped. All these specimens are distinctly reticulate. The reticulations may be thin-walled (fig. 7) or with slight thickenings (fig. 8). In fig. 5 the reticulations have quite thick walls in some parts of the specimen. The reticulations are dark in color."

Senonian - Paleocene; Nigeria.

Remarks: Since no single holotype was designated for the genus and the type species, their names have not been validly published (Art. 10.1, Art. 37.1; ICBN).

Corrected on disk

no p+x

RETITRICOLPORITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 111, pl. 10, fig. 3, 2

Type species: none designated (no named species designated as nomenclatural type)

Diagnosis: none given

Remarks: Puri named two species under this generic name, and described one unnamed species, which was designated as "type for genus and species". The generic name cannot be accepted as valid by monotypy, lacks a diagnosis, and lacks a type for the generic name which is the type of the name of a species (Art. 10.1, ICBN). Accordingly, the names for this genus and these species have not been validly published.

no p^x

RETIPOLYADITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 55

Type species: Retipolyadites cooksoniae Puri, *ibid.*, pl. 5, fig. 153,154 (no holotype designated)

Diagnosis: "Pollen grains united in a polyad. Number of grains 24 or more. The grains have a reticulation."

Description of type species: "These are two polyads of about 16 [sic!] cells each. They are nearly circular. Size in fig. 153 is ca 42 μ in diameter and fig. 154 is a little smaller polyad with somewhat irregular shape. This is ca 40 x 33 in size. Exine is ca 1.5-2 μ in thickness. The individual cells are irregular in shape and do not to have any furrows. There is a close mesh reticulation in the polyads.

Polyads with 16 cells occur in the family Mimosoideae."

Senonian; Nigeria

Remarks: Since no holotype was designated for the type species and the genus, their names have not been validly published.

no p. x

rechecked on disk

/
RETINAPERTURITES Puri 1963
^

University Ibadan Botanical Studies No. 10, p. 75

Type species: Retinaperturites megaspheroides Puri, *ibid.*, pl. 8, fig. 205-207
(no holotype designated)

Diagnosis: "Under this are included inaperturate sporomorphs with a conspicuous reticulation."

Description of type species: "These are large spheroidal sporomorphs measuring ca 104 x 92 μ ; 90 x 70 μ and 78 x 75 μ . Exine is not continuous with intine, and measures ca 2-10 μ in thickness. These are marked into a fine reticulation which is ca 1-1.5 μ in diameter."

Senonian; Nigeria.

Remarks: Since no holotype was designated for the type species and genus, their names have not been validly published.

HYDROPHYLLACIDITES Puri 1963

no pi'x

added on disk University Ibadan Botanical Studies No. 10, p. 96 , pl. 9, fig. 267

Type species: none designated (no named species assigned to this genus)

Diagnosis (combined description): "This is a small tricolporoidate grain, oblate, spheroidal in shape. It measures ca 25 x 20 μ . Exine is ca 1-1.5 μ in thickness and the grain is somewhat psilate or with obscure pattern."

Remarks: Although there is a single type specimen described, the generic name has not been validly published, because its type is not the type of a name of a species (Art. 10.1, ICBN).

GRANUSPOROMORPHITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 11

Type species: Granusporomorphites sporogonioides Puri, *ibid.*, pl. 1, fig. 9,10
(no holotype designated).

Diagnosis: "Under this are included those sporomorphs in which the shape of sculpture is not clear, following Pierce, 1961, p. 20."

Monotypic.

Description of type species: "Two small sporangium-like structures figured here are ovate in shape with a tapering base, that may appear to be foot of a capsule (?). Both the specimens are nearly of the same size, measuring about $90 \times 54 \mu$. Specimen in fig. 9 is a bit smaller. Foot like structure is between $12 \mu \times 11 \mu$ in fig. 9 and is smaller in fig. 10. The sporomorphs have a thin outer wall, with reticulation which is indistinct and irregular."

Remarks: Two specimens on two slides were illustrated and cited as 'type', but neither one was selected as holotype. Thus, according to Art.10.1 and Art. 37.1 of the ICBN, the names of the species, and genus, have not been validly published.

no pix

GEOGLOSSUMITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 17, pl. 1, fig. 19

Type species: none designated (no named species were assigned to this new genus; hence, no nomenclatural type exists, and the generic name is a nomen nudum)

no pax

BREYNIOPSISIDITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 103, pl. 10, fig. 286

Type species: none designated (no named species assigned to this genus)

Diagnosis (combined description): "This is a small 6 colporoidate grain measuring ca 27 x 28 μ . Exine is slightly thicker than the intine and is reticulate. Meshes are close and regular."

Remarks: Although there is a single type specimen described, the generic name has not been validly published, because its type is not the type of a name of a species (Art. 10.1, ICBN).

STRIATOSPORITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 44

Type species: Striatosporites akwara Puri, *ibid.*, pl. 5, fig. 122

Diagnosis: "Sporomorph striate, striae dichotomizing and seen to converge at both ends."

Ephedra is smaller, and has fewer striations.

Description of type species: "A large, peculiarly striate - inaperturate sporomorph is spindle-shaped, and probably ruptured at both ends. It measures ca 100 x ca 55 μ . The striae are dichotomizing in the form of ridges. These are separated from the main body of the sporomorph."

No similar fossil or living sporomorph is known to the author.

'Akwara' is an Ibo word for the fibrous covering of a peeled coconut skin, and is a very appropriate descriptive term for this form.

Senonian; Nigeria.

Remarks: We accept the name as validly and effectively published by Puri; however, as a junior homonym of Striatosporites Bhardwaj 1954, it is still an illegitimate name.

RETIPERIPORITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 91

Type species: Retiperiporites multiporoides Puri, *ibid.*, pl. 9, fig. 259

Diagnosis: "Under this reticulate sporomorphs with many pores are included"

Monotypic.

Description of type species: "This is a medium sized barrel-shaped sporomorph, with the proximal part flat and the distal part rounded. It measures ca 62 x 43 μ . Exine is 1 μ , becoming to 5 μ thick at the proximal part, in one corner. The intine is finely and closely reticulate, which may also appear as ends of fine spinules. There are several pores 2-4 μ in diameter scattered in the sporomorph. Affinities of this are unknown."

Senonian; Nigeria.

Remarks: The illustration of the holotype consists of a rather crude line drawing, mounted on a plate with many others, which then was photographed and printed, slightly out of focus, as a 3.5 x 5" (9 x 12 cm) glossy print.

PSILAPERIPORITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 91

Type species: Psilaperiporites spheroides Puri, *ibid.*, pl. 9, fig. 258

Diagnosis (combined description): "This is a large spheroidal sporomorph, measuring ca 80 x 65 μ . Exine is 1-5 μ in thickness, psilate, or with fine scabrae. Fine rounded pores, 2-3 μ in diameter, are seen."

Shows Cyperaceous affinities. Pollen grains with 4-5 pores are found in Cladium complanatum and with 4 in C. articulatum (Cranwell, 1953, p. 44) of the Cyperaceae, but the size of these grains (30-60 μ) is smaller, and their exine is faintly granular to foveolate, often with large gemmae associated with the exits.

Monotypic.

Senonian; Nigeria.

Remarks: The illustration of the holotype consists of a rather crude line drawing, mounted on a plate with many others, which then was photographed and printed, slightly out of focus, as a 3.5 x 5" (9 x 12 cm) glossy print.



CALDESITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 16

Type species: Caldesites nigerianus Puri, *ibid.*, pl. 1, fig. 18

Diagnosis (combined description): "These are two large ascospores of more or less the same size, lying partly one on the other. The larger measures 42 x nearly 20 μ (in the broadest part). The smaller spore is only a little smaller in dimensions. Both of these seem to be broken at the basal portion. They are thin-walled and are divided into more or less equal halves by the equatorial wall."

Corrected
on disk

Resembles⁴ ascospores of Caldesia sabina and Asterina melastomatis, but the latter is much smaller than the fossil spores.

Monotypic.

Senonian?; Nigeria.

RETISPOROMORPHITES Puri 1963

no pix

University Ibadan Botanical Studies No. 10, p. 11

Type species: Retisporomorphites reticulatus Puri, l.c., p. 11, pl. 1, fig. 5-8 (no holotype designated)

Diagnosis: "For sporomorph with thick walled reticulation."

Description of type species: "Three sporangium-like bodies are medium-sized, measuring, 110 x 45 μ ; 92 x 48 μ and 75 x 40 μ , respectively. These are somewhat like half moon in shape. Fig. 8 measures nearly 130 x 35 μ and is strikingly spiral-shaped. All these specimens are distinctly reticulate. The reticulations may be thin-walled (fig. 7) or with slight thickenings (fig. 8). In fig. 5 the reticulations have quite thick walls in some parts of the specimen. The reticulations are dark in color."

Senonian - Paleocene; Nigeria.

Remarks: Since no single holotype was designated for the genus and the type species, their names have not been validly published (Art. 10.1, Art. 37.1; ICBN).

Corrected on disk

no pix

RETITRICOLPORITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 111, pl. 10, fig. 312

Type species: none designated (no named species designated as nomenclatural type)

Diagnosis: none given

Remarks: Puri named two species under this generic name, and described one unnamed species, which was designated as "type for genus and species". The generic name cannot be accepted as valid by monotypy, lacks a diagnosis, and lacks a type for the generic name which is the type of the name of a species (Art. 10.1, ICBN). Accordingly, the names for this genus and these species have not been validly published.

TAMARICACIDITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 95

Type species: Tamaricacidites tricolpatus Puri, *ibid.*, pl. 9, fig. 264, 265 (no holotype designated)

Diagnosis (combined description): "These are two small tricolpate grains, measuring ca 24 x 25 μ . Exine and intine are not distinctly differentiated and grains are faintly baculate or reticulate."

Remarks: Because no single holotype was designated for the species and genus, their names have not been validly published.

XYLOGRAMMITES Puri 1963

University Ibadan Botanical Studies No. 10, p. 16, pl. 1, fig. 17

Type species: none designated (no named species were assigned to this new genus; hence, no nomenclatural type exists, and the generic name is a nomen nudum; Art. 10.1, Art. 37.1 of the ICBN)

no gr

THE PENNSYLVANIA STATE UNIVERSITY
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3 December, 1991

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

Dear Jan:

Just in from India--gave lectures at Birbal Sahni Inst. (reminded me--did I ever get the Puri publication back?--I still have the slip posted), and got their biennial international medal. There were a slug of lectures, and you were often quoted.

Re the nomenclature chapter--I'm working away, and now that the Sahni Centenary is behind us, it is front and center. I should finish by end of January or so.

Re "the chair"--that was a stupid decision. PSU's proposal was infinity better: 2 professorships (including mine made permanent), 8-10 rooms (including the 6 I now have), all made permanent, 2 permanent graduate assistantships, one permanent undergrad scholarship, total package worth \$3 million. LSU sent in a guy waving a check worth only 15% of PSU's offer--but it didn't really matter--the vote was rigged from the beginning, and nobody really ever read our great proposal.

All the best.

Yours very truly,

Alfred Traverse

AT/et

THE PENNSYLVANIA STATE UNIVERSITY
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1 July, 1991

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

Dear Jan:

You did way more for me than I intended for you to do--but it's a very useful compendium! I thought from rumors I had heard that you were now in a position to query the file on a computer, and it could just search for "fungal." The reason I wanted this is that I have 2-3 papers in the works on fungal spores, and want to be sure I've "covered the waterfront." Elsik sent me a list he has in his book chapter, but warns that many of the names on this list are illegitimate (actually, he says "invalid").

Thanks very much. Have a nice summer.

- Yours very truly,

Alfred Traverse

AT/et

DR. A. TRAVERSE
Deike Bldg 435
Penn State University

Dear Al,

91 06 24

I just finished working my way through your extensive commentary on the latest Supplement of the Genera File. I am mortified by the demonstration of my (our) poor quality control. It is easy to look for extenuating circumstances, but in the final term I feel that I am responsible; and in this effort I definitely seem to have blown it.

I appreciate your spending this much time on checking and rechecking, although I wonder if the subject matter warranted **this** much scrutiny. Obviously, I should have spent more time on it. One of my remedies is that I now have my own computer and laser printer, and can do my own typing; I will no longer be low man on the totem pole, in an environment where I have no real say in priorities.

At times your tone seemed to show some irritation which I noticed with regret, as I always have enjoyed our friendly relationship.

I will read once again through your list and respond as appropriate.

For each genus ('generic name'; the title of our publication is "Genera File of..."; -- should that have been "File of Generic Names of...") I wrote **one** entry. For ease of reference, I gave them a number. In an alphabetic file, such numbers have little value as page numbers. Later, if there is more information, I will reissue that entry, give it a new (higher) reference/card number and a later date, but will not call it page 2, or card 2; that designation, from the beginning, has been reserved for information that was too voluminous to be held on one card: then I would continue on 'CARD 2'. Again, if there is further material, I will reissue card 2, with its new number, but still as 'CARD 2', until that card is full in turn, and then I will start a 'CARD 3'. Your proposal would lead to cards that eventually would have to be read out of sequential order. In retrospect, it would not have hurt to have given more cross references, and your suggestion in that regard is valuable. I may well do that in the future (better late than never), just as I adopted your earlier suggestion to provide a register of these reference numbers in numerical order. However, that is another task that needs close monitoring and checking, and potentially would introduce more errors of omission than I would now care to consider. At least it must be clear to everybody that that is one thing where no one needs to look for what I did with it. You ask for the author name to be given as well on these cards #2; it wouldn't hurt, but I don't know either that it would make much practical difference (except in cases of homonyms). I always tried to be economical with text to be typed, because more bulk translated into more time, more proofreading, and more chance for introducing errors.

Kyrtome is the word as used in Kremp (Encyclopedia); I will try to remember, and be consistent from hereon in. I wonder if I tried to use a similar reasoning that makes some people prefer 'catalog' over 'catalogue'. It is one of those words where Webster has no easy solution. As for '**brochi**': that is

used by Kremp (Encyclopedia) , and should be as acceptable as 'kyrtomes'. Similarly, according to Kremp (p. 79, sub a., line 7), Erdtman used *laesura* for a single ray.

I have no explanation for the mess with 'auriculae' on 4577 with 192 AURICULIDITES I must have been asleep.

I shall not comment on a number of things that were obviously wrong and will need later correction. I am grateful for you pointing them out to me.

On 4580 under 2117 POLYVESTIBULOPOLLENITES, line 10 up, we should read **'junior** for 'senior'.

I am at a loss to explain the butchering of generic names (Simozonosporites; Dacrirotundina; etc.), as well as names of authors (Zhang Lujin; Gao). This should not have happened, and reflects my frantic state at the thought that if I wouldn't push the thing through now, it might never come to happen. This also will explain the gaffe regarding MESOTONGCHUAN-SPORITES versus QISHUIHESPORA. I had thought that the names Tongshuansporites and Tongshuanisporites were functionally the same, one with a correct connective vowel, one with a connective vowel missing by happenstance rather than by design. On top of that, the two spores come from the same area, the same age, and have very similar appearance -- without being identical. I explained my findings to the Chinese authors involved, who concurred with my notion that it was a case of involuntary similarity of name. I had suggested that I preface the younger of the two names with the prefix Meso-, and that seemed to be accepted. By now, our correspondence was through Telex (via the Geological Survey, because Esso no longer have Telex facilities, but in Nanjing they do not yet have Fax transmission). Then, as everything was typed and ready to go, I received another Telex with the request to change Mesotongchuansporites to Qishuihespora. I forwarded that message to the University typist, and changed the Alphabetic Listing in my office, because I was doing that one myself. Then, when everything was ready for stamping on the numbers with my hand stamp, it turned out that Mesotongchuansporites had **not** been changed. I lacked the heart to redo the whole thing once again. You did not yet notice (as I just did) that this also has thrown out the numbering of all names on card 4585 between 4704 and 4742. So much for good intentions.

As for **transliteration** versus translation of bibliographic references: my Esso librarian assures me that they prefer to have transliteration, because that makes it easier to retrieve. I know that I have not always been consistent in convention used in transliteration, but I now try to stick to the Library of Congress table.

In 4592 AGANOSACCITES I think that Zhang meant 'incipient' when he used 'primeval'.

Monggol and Mongol are both used by the Chinese. I should have stuck with the Mandarin form with 'gg'.

As for 4623 PHANEROSISPORITES: the new entry is correct on both scores; de Lima had sent me a copy of his thesis, which looked more 'effectively' published than Puri 1963. But when he published the generic name again, with a new holotype yet, I understood that I had been had.

As for ELSIKISPORONITES: Elsevier did not change the volume number in their page headers when they went from vol. 62 to vol. 63. So, when I copied the data from the printed text of the first page of the article or reprint, it showed '62'. When I had to recheck that in the case of HYPOXYLONITES and HYPOXYLONSPORITES to determine which had priority, I corrected 62 to 63 on 4685, but overlooked this one.

Under 4679 HAMMENIA, the proposed synonym is junior to the (name of the) type species; the remark occurs under the heading of the type species. The term 'colpoidate' is in the generic diagnosis, I did not want to interfere with it.

Regarding INAPERTISPORITES ex Rouse, the specific epithet of the type species is indeed *pseudoreticulatis*, and I presume from the Code that that must stay, no matter that it is obviously not a name that is composed 'arbitrarily' but rather ignorantly; it is awkward to treat such piglatin words as Latin, and easy to misspell them. However, that is what I did. The same goes for Gedinnian and Cyrillic, which I wrote incorrectly. At least these we can correct!

As for 4726 PLACOPOLLIS, Cornet validly published the MS name 'Placopollis' from Koob's thesis, and gave him credit by naming the type species after him. Koob did not have input into this publication, as far as I can see.

Card 4763 SETOSISPORITES differs from 3764 in that the later one spells out the emendation by Piérart in 1978, which the earlier version said did not exist.

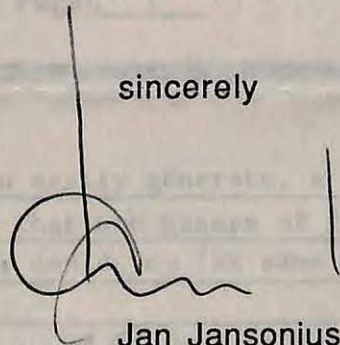
Why you would think that 4808 XILINIPOLLIS and 4186 XILINISPORITES are apparent homonyms is beyond me; by that time you probably were as distracted as I was when I tried to force the thing into the printers.

Once more: I appreciate what you did for correcting a piece of hasty work. I feel truly embarrassed by the number of sloppy errors, and shall be sharper next time around.

I trust that you received my letter with the listing of the fungal names by now. If not, let me know.

Best regards,

sincerely


Jan Jansonius

PENNSTATE



Dr. Al Traverse
THE PENNSYLVANIA STATE UNIVERSITY
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The Pennsylvania State University
Department of Geosciences
503 Deike Building
(814) 863-7823 [FAX]

Date: 14 June, 1991

FAX TRANSMISSION

Marked here if urgent reply is requested.

Transmission to:

Dr. Jan Jansonius
Esso Resources Canada Ltd.
2000, 4th Ave. SW
Calgary, Alberta, Canada T2P 0N6

Name: Dr. Jan Jansonius

Company/Institution: Esso Resources Canada Ltd.

Department: Esso Plaza - Room 1415

Fax Number: 403-237-2907

Phone Number: 403-237-2438

Transmission from:

Name: Alfred Traverse

Phone Number: 814-863-2419

Total Number of Pages (Including Cover Page): 1

Message:

Do you have, or could you easily generate, a list of all the genera in the J&H system that are genera of fossil fungal spores and spore bodies? If so, could you fax same to me?

Thanks, AL

*If any portion of this transmission is unclear, please call _____.

Dr Al Traverse

Chairman/Curator/Library
Dept. of Geology
University of Ibadan
Ibadan, Nigeria

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Dear Sir/Madam,

31 03 91

4 June, 1991

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

Dear Jan:

Got your fax of 30 May with answer to the query re your getting the comments package.

Now I am wondering about the Puri "book" (article?) sent in March per your request. Did you get it? What did you think about it? Interesting question re validity of publication. ?

Best.

Yours very truly,

Alfred Traverse

Al,
Yes, I did
receive it;

See p.2 of this fax: I think
it is not effectively published,
but got no reply yet.

AT/et

Thanks,

Calgary, Canada T2P 0H6

Chairman/Curator/Librarian**Dept. of Geology
University of Ibadan
Ibadan, Nigeria**

Dear Sir/Madam,

91 03 21

Recently I saw a copy of the publication by G.S. Puri: "Some plant-micro-fossils from the Cretaceous and Paleocene of Nigeria"; University of Ibadan Botanical Studies No. 10, p.1-142, with 10 photo plates (Paper presented to the 1963 Central African Scientific and Medical Congress, Lusaka, N. Rhodesia, August, 1963).

In this paper are a number of new names for species and genera of fossil spores and pollen. In order to evaluate whether or not these names were proposed in the required manner, I need some further information about this publication. It was not printed, but reproduced by mimeograph, which generally indicates that it was not published in a strict sense (although there are some exceptions to this). There is no printed cover, which makes it difficult to judge whether the copy I saw is an offprint, a preprint, or a regular issue of the U. of I. Bot. Studies.

Could you please tell me what is the usual method of distribution for the series "University of Ibadan Botanical Studies"? Is it available for sale, or on request, or on exchange? Can I still order a copy of No. 10? How many copies of No. 10 have been produced and were distributed? Could you provide me with the names of one (or a few) institutions or libraries in Canada or the United States where this series would be available? The paper was read at a 1963 conference, but was it also published in 1963, or possibly at a later date?; this is important information for determining the priority date of the new names in it that may be validly published. Dr. Puri did indicate that his types were on certain numbered slides; are these slides still preserved and available for study at the University of Ibadan?

Of the many names that are proposed in this paper, many were not proposed in a valid manner, because there was no designation of a type species, or designation of a unique holotype. For that reason, it would solve potential problems if the publication itself would turn out to be a casual report, that was sent to a small number of colleagues for their information, but that was not an "effective" publication in the sense of the Botanical Code for Nomenclature.

My interest in this matter is based on my involvement with the publication "Genera File of Fossil Spores", which is a compilation of all new names proposed for fossil spores, with an update every two years. I should like to evaluate the status of Puri's report before the next Supplement, and I will be much obliged if you could provide me with answers to my list of questions.

Yours sincerely,

Jan Jansonius
Esso Plaza West, room 1415
237 - 4 Avenue SW
Calgary, Canada T2P 0H6

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7 May, 1991

To: Dr. A. Traverse

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


Dear Jan:

A while back I completed a careful perusal of the most recent set of J&H "cards" (= slips). I couldn't get my comments typed, so I made a xerox copy for you, hoping you could read my scrawl.

Please let me know after a little while if you ever got these sheets. I am not sure if I sent them, ~~or not~~. If I didn't, I'll make a new copy and send it, as it represented many hours of work, and I surely wanted you to get it!

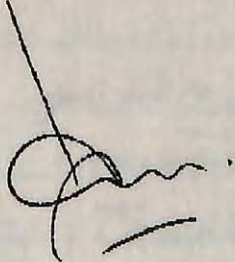
Best.

Yours very truly,


 Alfred Traverse

91 05 30

AT/et

Received
 However, I can start evaluating / digesting them.
 with thanks!
 it will be a few weeks before
 +.t.


File as correspondence

29-11-91

Janssonis

Jean Jan

I am incorporating the latest supplement. Yvonne! Questions or comments, to come requiring explanation urgently.:

4577 - 192 - Auriculiditer. You've lost me. l. 20 has no "auricula" - it has "auriculae"

l. 30 ditto

l. 32 ditto

But l. 5 has "aurical"

l. 7 has "auricula"

l. 8 " "

l. 11 " "

l. 14 " "

Can you illuminate?

4577 - 3825 - Coronaeponites, addition l. 4.

I say it's "kyrtome" - can you support

"Kyrtom"? (also see ^{your card} 4796, col. 2, 3 l. up)

4578 - 737. Whoops! you misspelled the generic name in the correction! ("small" error?)

4579 - "3710". The only Tecadinaipollenites card I have is 3705, and the 3.4 bit occurs 6 lines up. Am I missing a 3710 card? If so, may I have a view?

4580 - 2117. My 2117 already says "senior".

Explanation?

4580 - 2147. No matter how you count it,

~~4580~~ it's not line 4.

4581 - 2221 you meant 0,75 ~~2~~ for the first one. Small matter. (~~you need me~~ in legs.)

4581 - 2600 "Simozonosporites" : my 2600 in Simozonosporites. what goes on? "Small error"?

4581 - 4170 "Nov Qishuikespora (g.v.)" - trouble is, I find no such card - so can't "g.v." it. How I lost it? If so, could you please provide a xerox?

→ 4602 "Rhaebia" isn't enough - should say late Triassic, esp. as Rhaebia isn't always recognized any more.

4612 l. 3 from bottom; microtuberculate

4614 l. 6, 10 isn't it kyrtomes?

4628 ditto

4633 ditto for ER.

4660, 5l. up I don't have a source near at hand in which to check, but isn't it chytridiomycetes?

4671 last line Estonia is the correct spelling according to best authorities, though Esthonia is sometimes used.

4688, last line laevigata (cf. 4704, 3l. up)

4695 I believe "Aalcaian" is not enough.

For Karnian you (usually?) say "late Triassic" in addition. I feel less

I believe you should always give period, not just stage.

4807 last line: "Transbaikalia"
or "Transbaikal" (source:
Merriam-Webster Geographical
Dictionary)

general matter (figure me if I've brought it
up before): on a card such as 4314
(*Pechorasperites*) where you (or somebody) has
translated and transliterated, nowhere is
should say in the reference: "(in Russian)" (or whatever)

4592. l. 5 up: what do you suppose a "primieval saccus" is?
This is just a matter of
interest.

4595. l. 10 - up: Libya

4569. last l. i jr.

4605 what do you mean, "card 2"? There are
already 2, viz. 210 & 3300. Also, card should include
author name.

4586 l. 3 alongal (cf. l. 4)

4596 the generic name should include the author name (as
usual)

4584 reference to card 4596 is wrong - should be
Annulispora.

4607 l. 1, can't check it here, but isn't it
Palaeontological? (cf. card 4682!)

(including card 2)

General (to avoid repeats). I believe each card should
include the author, ^{name} and the generic name

4618 "card 2" - There's already a "card 2" (= 355);
Is this "card 3"?

4621. Why aren't cards like this labeled "Card 2"¹?
(there are a number of other examples in this volume - e.g.,
4667 4623)

4623 OK, what is it, "de Lima 1989" or
"De Lima 1978"? (I don't refer
to the orthography only, but the
situation)

4626 Why not "card 2"? (cf. 508)
(cf. 550)

4629 why not "card 2"?; l. 1: p:

4633 l. 5 up. kytomes (you spell it that
way elsewhere, for one thing)

4634 why not card 2? (cf. 642) (and always give
cross refs.!)
4636 why not card 2? (cf. ~~463~~ 4401)

4638 wouldn't it be a good idea to say what no.
card it is "card 2" ^(690?) top? Also heading
^(must!) should include a citation.

4642 even here, I believe a ref. to card 791
would be in order

4644 should refer to 813; last two lines; delete
make ... is (to read "make D. an obligate ...")

~~4647 should refer to 829~~

4651 why not "card 2"? Why no cross-
ref. at top to card 866?

4665 why not "card 2"? etc.

4657 " etc.

4660 This one reminds of a very hot letter I
got from one of some 25 yrs. ago, protesting
inclusion of items other than sporophylls & CRSP.

4661 Why not "card 2"? Why not
cross-ref. to 952?

4667 Why not "card 2"? Needs at least
cross ref. to 1036.

4671 This gets some comment ~~at~~ as 4660,

4674 "Severus" zinnii! — no date for authorship.
Also here, as often, $\frac{1}{4}$ are "laesura"
as = a radius or radial arm of a
laesura.

4678 Why not "card 2"? No cross ref.
to card 1212¹²¹³ This would be a very
important addition in the dozens of
such cases, and in the future.

4679. This one is just not adequately
explained. l. 17: "The genus [= the
generic name:] is proposed to replace
the illegitimate Stephanocolpate."
This requires at least a reference to
card 2678. Then, 4 l. up,
you say "Word corrected --- to be a
junior synonym" of what? Why?
So what? Is "--- colpoidate" =
"colpate"? If so, why not use
colpate?

~~4680. In the one you give a ref. to Acta
Palaeobot. Polynol. Series No. 1, p. 75.~~

4686 Looks like a mistake here: card

4658 lists Kumar 1990, same journal,
vol. 62, p. 18. It's possible that Kumar 1990,
v. 63, p. 18, is also from the same paper but its appearance.

4687 Why not "card 3" or at least a cross-ref. to 1305 + 1306? This seems to me essential in all such cases!
2. Is it "pseudoreticulatus" per 4687, or "pseudoreticulatus" per 1305 + 1306?

4688 This one bugs me - who is author of genus? I guess it must be I. Pflüg & Thomson. Why "card 2"? It's card 3, if you wear that generic name (p. cards 1305 & 3985). If all that's right, 1305 + 3985 should have been referenced.

4690 Why not "card 2"? - cf. 1353. At least a cross ref. necessary.

4691 Why not "card 2"? - cf. 3561. At least a cross-ref. necessary.

4692 ~~In a recent supplement you said that Thorens finds little errors. This matter cannot have just a night's sleep! The one-letter difference you cite between the two genera is bigger than ^{that} between, e.g., Laevigatosporites and laevigat. sporites! Also, see the footnote to ~~64.3~~ 64.3: The General Committee usually gets a shot at such things - it's one of the ~~its~~ electors. When there's a whole additional syllable (even if one letter), sometimes such names are maintained. You have a good point, but I believe the G.C. should have noted.~~

Have you opened a can of worms?

4706 last line: bedingian

4717 Why not "card 2"? Needs at least cross ref. to 1880.

4719 3ll. up: adjacent

4700 This is a convenient spot to express the caveat that I hate transliteration of Cyrillic. Why not use Cyrillic? There is no good, universal transliteration method: Valt's (Engl.-USSR) vs. Walz (USSR) ; etc., etc.

4706 last line Bedinnian (and it should add Devonian, in my opinion, but this is just a future reference)

4709 As noted above, I prefer it when you identify period for Carnian,

4764 I realize it isn't your doing, but an effort should be made to discourage use of "microspore" for "small spore" (as a ^{really} ~~does~~ know these are microspores, I suppose. Indeed, it's unlikely they're probably isopores.

2 lines:

4772 1 Vitrastructure (ch.)

~~4778~~ last 2 lines:

4801 last 2 lines: This is the sort of thing that sooner or later we will discuss about transliteration, Kholova prefers, and is very vehement about it: "Chlonova." But transliteration has to follow rules, damn it, not personal tastes. Furthermore, you misspelled Cyrillic

4726. What's this "Koo in Cornet"?

4727. As the title is translated as "Lullaby" (in Russian) it is not shared in French by "Lullaby" - you often do this.

4728. Why not "Card 3"? At least could be seen not to 2038 & 2039. It seems to me you should have the "Lullaby" card.

4730. Why not "Card 2"? At least needs cross ref. to 2078.

4736. Why not "Card 3"? At least need cross ref. to 2187 & 3575 - (The ref. to V. + H. is less paragraph - almost ~~incomplete - needs ref. presumably to card 3575~~ - probably go beyond "1979" to read: "Card 3575".

4737. Why not "Card 2"? Needs at least cross ref. to 2205.

4739. Why not "Card 2"? Needs at least cross ref. to 2227.

4740. Why not "Card 3"? Needs at least cross ref. to 2238 & 2239. Just here: "consequently, this genus becomes a synonym of *Stachys* and the name is accepted."

4742. Why not "Card 2"? Needs cross-ref. to 3901.

4744, 4 lines up: Taeniæ (at least, needs
a "sic")

4747 Why not "card 2"? Needs ~~cross-~~
ref. to 2352, ~~as stated~~

4751 Why not "card 2"? Need cross-
ref. to 3605.

4757 What do you really think of "brachi"?
1. Is it correct? (Sporos is Greek for
worse - is the plural brachi?) 2. Is it
diminutive?

4759. Why not "card 2"? Needs cross ref.
to 2539; l. 3 up; sic is not
an abbreviation but a Latin word
meaning "so, then." It needs no period.
(Perhaps you need a comma?)

last line:
4760 It could be argued that "Proterozoic"
before Vendian would have been helpful
- I drew a blank for a while, and AGE
Olsony didn't help.

4761 Why not "card 3"?
"para. 3" you reference card 2554,
but what about card 4161? It
should have been referenced too!

4763 Why not "card 3"? However, I don't
really see how this differs from
card 3764, except that the spre-drawing
is "correctly" oriented. Please explain. ←

4764 Is it the author's name "Gao" as
elsewhere? (Is this a "small error"?)

4766-67 Instead of "cards 1-2" there should
be "card 4-5" and a cross-ref. to 3600 or 4533
(what was really card 3, not card 2, as labelled).

4774. "hoc loco" - why not English,
"here published," as I believe you've done
in the past?
- 4775, l. 2 is it Palaeontology? - cf.
card 4635.
- 4776 why not "card 2"? Needs cross-ref.
to 2649.
4777. why not "card 2"? Needs cross ref.
to 2660.
- 4778 "I.S.G. " Bull." doesn't seem sufficient
to me. I suppose I can find it
eventually, but I believe a better abbreviation
would help.
- 4779 why not "card 2"? Needs cross ref. to 3769.
l. 8: Colombia. (Fring, you got
it right in "card 1."!)
- 4780 Should be "card 2." Needs cross ref.
to 2685.
- 4783 Should be "card 2," with cross
ref. to 2738.
- 4791 As stated elsewhere, this sort of thing
~~reminds me~~ seems hardly appropriate.
- 4793 I guess this is "card 3" (should
cross-ref. 2969 + 2970)
- 4794 Should be "card 2" w. cross-ref.
to 2998.
- 4796 Should be "card 2" w. cross-ref. to
3788 (or "card 3" w. cross-ref. also
to 4047)
- 4798 Compare w. 4692 - why don't
you note this is a homonym?!

4800. Top line i surely it's Zhang Lujin!
≡ (Do you consider this a "small error"?)
of Sino Sacciter etc.

4803 should be card 2 w. ~~the~~ cross-
ref. to 3793.

4808 This one is another that makes
me wonder about your files — did
you notice 4186? This is apparently
a homonym and not "card 2," but surely
you should comment! Is this another "small
error"?

[old one: 2352, l. 4 up. annulus — did
I already catch that?]

Best. Hang in there.

Al

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25 February, 1991

Dr. Jan Jansonius
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Dear Jan:

In a recent note you repeat that I said (correctly) that "anulus" is just wrong in English. Then you go on to quote me that "spelling is determined by usage", as if that contradicted my dictum about "anulus." It doesn't. Usage is what determined that annulus is right. Anulus gets "sp. -1 pt." on exams.

That Compositosphaeridium case is a bad one. In my very fallible and changeable opinion, the type of that genus remains the type of C. costatum, as apparently designated by Dodekova in 1974 (I am using only your data and have checked nothing). That can't be changed. The name of the species to which that specimen was referred should have been polonicum, apparently. That can be changed. But the specimen of costatum, now referred to polonicum, is the generic type. The species name in this case doesn't suffice because it had to be changed. Wow!

Am wading through the new supplement of cards. Thanks for acknowledgment! What accounts for the "new look"?--how was it accomplished? Much more readable.

Best regards.

Yours very truly,

Alfred Traverse

P.S. I have been awarded a Fulbright for study at Senckenberg Museum in Frankfurt next year.

AT/et

Feb. 91

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 (1954), as the "lectotype designated by Mildenhall(1972)."

In 1983, 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? (This is the position taken by Jansonius & Hills 1990)

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? (No; if the genus has become invalid, so go any species that had been assigned to it.)

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 ?

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19 February, 1991

Dr. Jan Jansonius
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Dear Jan:

I hate getting letters I sent out back--it's almost an insult.

Your recent^{ly} received envelope postmarked 11 Feb. came in less than a week!--about 5 days--a recent record for Canadian mail.

You're right, as far as I can see, re the mess about genera that were ok without specific holotypes 1958-83, but are now invalidated. You would do a great service by undertaking with a formal proposal and plenty of examples to get that changed at Tokyo.

Enjoyed copy of your letter to Moyes, but I am surprised to learn you are a member of a "Subcommittee for Palaeobotanical Nomenclature." You are a member of "IAPT.CFP (= Committee for Fossil Plants)." We shouldn't be creating synonyms out there, if that's what happened.

Best.

Yours very truly,

Alfred Traverse

AT/et

Sanjeant, presumably.

(from Vancouver)

Dear Bill,

91.01.30

I hate to bother you with trivia, but your treatment (in Harker et al 1990, p. 103-104) of Alterbidinium rcticorne (not: — ^{Palaeontographica 3219} is) leaves me scratching my head.

Firstly, rcticornis was not validly published in 1967, because Vozzhennikova did not designate a holotype from among the two specimens illustrated. Thus, the species epithet rcticornis in this genus is valid only as ex Storer & Evitt 1978, p. 93, by their designation of a lectotype. Further, as per ICBN Art. 37.1, it now is required that a name of a genus is not validly published as of 1958 if there is no holotype (see Art. 10.1: this is the holotype specimen of the first/original type species). Hence Alterbia appears, by implication, validated only in 1978 by Storer + Evitt. (~~I am not now looking what the implications of this are for species proposed in the genus between 1976 and 1978.~~) Of course, that name turned out to be ~~an~~ illegitimate ~~protonomen~~, and was replaced by Alterbidinium, proposed as a "nom. nov." for Alterbia, not a new genus with A. acutulum as type.

Thus, Alterbidinium inherited the baggage of errors bobbing in its wake. Thus, depending on taxonomic judgement, the name of its type species would be given as acutulum or rcticorne. However, Alterbidinium is a new name for Alterbia, which is a new name for Albertia Vozzh. 1967. The latter contained two species, curvicornis and rcticornis, the latter (in spite of being indicated as type species) invalid because no holotype was designated. That, by default, leaves Albertia as monotypic in 1967, and its type species automatically is A. curvicornis.

The holotype of the latter thus is the type of the genus Albertia, and the new names variously applied to that taxon.

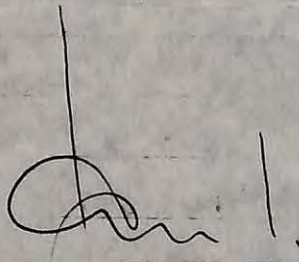
In retrospect, then, the validation of recticornis by Stover & Evitt was immaterial to the status of the genus as a taxonomic unit, and later name changes were just that.

However, in the light of these considerations it is unfortunate that, in your overwrought lists of synonyms and/or citation of the name, you evaded the question of relationship between recticorne and curvicorne within Alterbidinium; of these, curvicorne is the senior name.

The photo (Pl. 6, fig. 1) of "O. operculata" to me does not look like an operculum, but rather to be a hypocyst (and prob. assignable to O. cf. Singhii).

I find it confusing that you label text fig. 19 and Pl. 3, fig. 15 both "dorsal view" (even if you modify the latter with "low focus"); they are, respectively, dorsal and ventral aspect (I would have liked a statement that your photos are not mirrored images).

Best regards,



JAN JANSONIUS

cc: Judi Lentin
Graham Williams } for Index
Al Traverse } for nomenclature

Dear Al,

91 01 30

Thank you much for yours de dato 90 12 30. I found "diagnosis" on 3888 FUSUS POLLENITES; duly noted for next Supplement.

- 3886 FILLAEOPSISIDITES is one of those artistic efforts that strike different beholders in different ways. I chose to accept it because:
- the basionym of the type species is given on p. 361, in the particular (but incorrect!) way introduced by the doyen of paleopalynology, R. Potonié. There's no mistaking the author's intention.
 - although she does not cite verse + chapter on the earlier illustration + page number of the holotype, she goes an unusual step beyond that by providing 6 new focal levels of that holotype newly photographed. (However, her author citation on p. 362 is incorrect, and contradictory to that given on p. 361). Although she did not follow the format expected by Art. 33.2, she did provide all the facts necessary for an exclusive identification of the type.
 - Although we need the Code to set out proper procedure, at some point we need to apply common sense to temper bureaucratic rote. (Much as you say "anulus" is just plain wrong in English", but on the next line state "Spelling is determined by usage".

A propos: "annulus" may not be usually used in English, but seems ² correct as a Latin term (cf. margo, colpus, ambitus, diameter); at some point it is difficult to know whether a word is still Latin, or now English, and they don't carry a passport ("annulus" though is clearly English).

Thank you for the xeroxed pages from Bolkhovitinia, and the info on Puri; I'll order a xerox copy of it.

I do not like the new ICBN a bit. The zealots, in trying to spell out each of their preferences, are only digging more holes for the unwary to stumble into (and that includes themselves).

Case in point: Compositosphaeridium Dodekova 1974

Type species C. costatum (Dawey + Williams '66) Dod. '74
Hystrichosphaer. costatum Dawey + Will. 1966
Monotypic when published in 1974

Everybody now is in agreement that Hystrichosphaeridium polonicum Gorka 1965 is a senior synonym of H. costatum Daw + Will. '66. Accordingly, Lentin + Williams now

list Compositosphaeridium polonicum (Sotka) Etken + Sarg. as ³

type species of Compositosphaeridium.

However, now we look at Art. 10.1, which stipulates that the type of a genus is a specimen, i.e. the holotype "of a species" (craft! they don't say the holotype of the type species). Also, for purposes of citation, "the species name alone suffices". Now, how do we give the (identify) the type of Compositosphaeridium?

The correct name for the type species is C. polonicum. Its sole citation implies that the type of C. polonicum is the type of Compositosphaeridium. But Art. 7.2 requires that the nomenclatural type is that element to which a name is permanently attached. So, the type of Compositosphaeridium = the type of H. costatum, while its "type species" has a different type = type of H. polonicum. If this reasoning was correct, how can "the species name alone suffice"?

In early March I was asked to lecture to the U of C students, and I thought I'd talk nomenclature, and include this example. However, I'd like to have your opinion before then.

So much for now,
best regards + wishes for you both,

Ann!

PS 1 : See Lentin + Williams for further modification on the type by Erkmen + Sarjeant (*C. bulgaricum*).

PS 2 : How is the book on Organic Matter coming along? (I wonder if Dave Baten, who prepares chapter 19, will benefit from it in sufficient time).

PS 3 : This letter was written on 100% scrap paper (hope you don't mind).