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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.

xxxx
865-2342

14 October, 1977

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

Dear Jan:

Thanks for your answer to mine of 19 September. I have always disliked getting answers to my letters written on the original letter. Erdtman used to do that to one. However, your answer is so complete and to the point that one can hardly be annoyed!

I find the sheets with the small corrections very useable, and I think that one actually gains from the experience of transferring information and playing with the set of cards. It certainly wouldn't be worthwhile to replace them with new cards.

I am sorry that you're not going to be attending the 10th anniversary AASP meeting, not because you will lose anything by staying away but because I will miss the opportunity to see you. Best wishes to you and the rest of your family.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
COLLEGE OF EARTH AND MINERAL SCIENCES
UNIVERSITY PARK, PENNSYLVANIA 16802

Department of Geosciences

Area Code 814
865-2342

19 September, 1977

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

Dear Jan:

Just got the corrections and additions to the famous cards and have the following query:

What about card 898? Mine is defective and is printed only from about the middle of the card on (to the right). Among other things, the name of the genus is missing. Are you going to provide replacement cards in such cases?

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Dear Al, you'll excuse this way of answering your question. In the first Supplement should be card 3331 Echiperiporites, which replaces #898 (the print-shop loused that one up); please check if you received it.
Do you find the sheets with small corrections usable (they obviate a costly re-edition of the card) or would you rather spend more money and have replacement cards for most of them?

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we are having lousy wet weather, and everybody is way behind on outside chores, incl. the farmers and me. So far I have no plans yet to go to the Xth AASP meeting; do you think it is going to be worthwhile? Looking at the program, I think that may be Thursday + Friday AM should be worth attending. I'll consider selling it to management. Of course, in Dec. we already will be given a trip to Houston, and thus we already are scheduled to get our kicks.

Best regards for now,

Jan.

PS: (are there still un-equal opportunity universities in the US+A?)

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xxxxx
865-2342

28 July, 1977

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. S.E.
Calgary, Alberta, Canada T2B 2G3

Dear Jan:

Thanks for yours of 12 July. It would be my opinion the Chlonova's "Vesicula" is indeed among the names to be rejected under the provisions of Article 20. This is an interesting point philosophically because it will illustrate what I have been maintaining with Srivastava and others in correspondence for some time--that what really determines whether a thing will be regarded as within or without the provisions of the Code is more often than not a question of general acceptance. If you publish a note in which you propose a new name for Chlonova's "Vesicula" I personally would follow it. Others might disagree, and there is certainly no court of appeal as things now stand. It's much like the situation with Corollina. I think it is the correct name, whereas others feel differently. Nothing in the Code really will help. I stand on the Code and so do others who choose to use Classopollis. Which of us are correct? That is an unanswerable question. Time will tell which of the two opinions prevails. The same goes for "Vesicula". I think it would be worth proposing a new name. You're quite right that there should be a back reference to Article 20 in the various places where stipulations for valid publication are set out. This is an item which should be directed to the Editorial Committee. However, based on my now pretty considerable experience with how things go, the way most certain to get it referred to the Editorial Committee with some authority is to propose that such a reference be included. I think it would look silly to make such a proposal as a thing by itself, but if there were a considerable list of proposals for alteration, as I understand you have in mind, why not make this one of the list? That way it will come up. Someone will jump to his feet and say, "That should be referred to the Editorial Committee", but it will

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then be on their agenda with authority they can't ignore.

It's fine for us to be a reference for Johannes. By the way, we were on vacation ourselves just recently in Toronto, and our daughter, Celia, went through a glass enclosure, seriously lacerating her nose. With plastic surgery, it is now coming along quite well, and she will probably have no scars at all, but at the time it was scary and certainly caused us to cancel further vacation plans! Her stitches were removed when we got home and the marks are fading rapidly.

Best wishes to you.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

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EXPLORATION RESEARCH DIVISION
EXPLORATION DEPARTMENT

July 12, 1977

Dr. A. Traverse
529 Deike Building
University Park
Pennsylvania 16802
U. S. A.

Dear Al,

Thank you for your recent letter, and your reference to Lellinger; in due time I certainly will contact him. Possibly soon, with regard to a new genus by Khlonova "Vesicula": if vesicula is a current technical term used in morphology, the name is contrary to the Code (Art. 20). (Under Art. 32 the conditions of valid publication are set out under reference to Art. 16-17, 17-19, 21, 23, 24, 33-45, artfully dodging the stipulations of Art. 20. In the examples of the latter it says that words like Radix, Spina etc. cannot now be validly published as generic names. Really then, Art. 20 should be referred to in Art. 32?) I should like to have your opinion if vesicula is such a term? I almost would think it is.

Is the aside I bracketed above a matter for the Editorial Committee or must I propose, that a reference to Art. 20 be included in Art. 32?

Regarding your list of generic names: it is a pity in a way to let the thing slide. On the other hand, there may be more important goals in life.

Stan showed me the program of your Pentecostal service. I am not disturbed about your ministering; if a man has strong mothering instincts why not become a Father! I am happy that your arm has healed well: health is indeed a treasure.

Have a nice summer! (I again gave your name as a USA friend during the time that Johannes is in Meadowmount).

Best regards,

Jan Jansonius

JJ/ap

Hunt Institute for Botanical Documentation

21 June, 1977

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

Dear Jan:

Thanks for yours of June 8. My hand and wrist are now almost 100%. I do have other orthopedic distresses, but fortunately nothing really major--just annoying.

The problem with your requests with Stafleu may well have been that they have been too minute. He has responded quite well to my request for opinions when I have saved up a number of examples of rather difficult problems.

Regarding the Wickander and Loeblich paper, which I have not seen--you seem to be right about Putescortum, and I would go ahead and make that correction if I were you and if opportunity presents itself. If your classical sources convince you that the specific epithet you next mention is handled in a grammatically incorrect way, you have the right to correct it. As I understand it, however, an author is at liberty to decide how he wants to use the epithets which he employs. In other words, I would interpret that W. & L. have a right to do whatever they like with "lophos", as long as they are consistent and make quite clear what they are doing. Is there not also a possibility that they found some obscure Latin reference in which there was an adjectival form for -lophos? If they admit at the outset that it was a noun, there are of course rules for treating such nouns, and these are explained in the Code. Chances are they just "blew it" and you have a perfect right to make the appropriate correction, just as changing from alba to albus or whatever. As a good rule of thumb, one should almost never change the

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spelling of a generic name--your connecting vowel is practically the only exception. On the other hand, changes in specific and infraspecific epithets are actually encouraged, specifically when the form of the epithet is not grammatically correct or represents an accidental misspelling. (On the other hand, a deliberate misspelling such as pensylvanica with one n per Linnaeus, must be followed.)

Let's forget about the generic list for awhile. I'm on to some other things and can't give any attention to it at the moment anyway.

Regarding Article 33 I would say by all means get up a proposal. I would be delighted to act as the wall off which you bounce it in preparation and to make it a joint proposal if I agree with the ideas as finally drafted. However I must state again that I do not want to give the Editorial Committee anything like a free hand in revising the Code. I would like to eliminate or nearly eliminate their power to make changes inasmuch as I have seen some bad things happen from their activities. If you find things in the Code which aren't right, and you have found plenty, get up a list of proposals to take care of them and keep at it year after year. Eventually they will all get incorporated in the Code or disposed of in some other way that you will have to admit has been subject to careful scrutiny by a number of people. One problem which I am sure you have already recognised is that the Code over the years has been drawn up and modified by groups of people who can't imagine that there are folks making up names who are not classically educated. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

P.S. A man who can be of great assistance to you in any problem of botanical Latin is Dr. David B. Lellinger, Botany Dept., U.S. National Museum, Smithsonian Inst., Washington, DC 20242. Dave is on the Committee for Fossil Plants of the IAPT and is very knowledgeable in the botanical Latin area.

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EXPLORATION RESEARCH DIVISION
EXPLORATION DEPARTMENT

June 8, 1977

Dr. A. Traverse
529 Deike Building
University Park
Pennsylvania 16802
U. S. A.

Dear Al,

First: I hope that your arm is recovering well; I must say that your physical infirmity and increasing occupation with the spiritual realm has me worried a bit. As I said: I hope sincerely that you make a good recovery.

In the recent past I sent you copies of several letters on nomenclatural minutiae dashed off to Stafleu. He must detest my guts by now, for his answers are fewer and farther between, but the message gets through. Yet, I tend to worry about such things.

Now I see a new paper by Wicander and Loeblich (Palaeontographica 160 B, 4-6). With somewhat malicious satisfaction I note a wrong connecting vowel (p. 148): Puteoscortum should be corrected to Putescortum (Art. 73G). It is obvious that Loeblich is no great classical scholar, in spite of the flourish with which he serves up extravagant names. What does one do (p. 136) with C. bounolopha?; here, the noun lophos is treated as if an adjective, reflecting the generic gender. That is not correct - but then the ICBN allows a name to be made in any arbitrary manner. Does that mean that we can forget about linguistic and grammatical custom and rules? No, says the Code to that. So, should this last name be restored to: C. bounolophos, or: C. bounolophus? I don't dare ask Stafleu such questions any more. Even if it is dereliction of responsibility, if he considers such things too unimportant. Do you know if there is a classicist among the Editorial Board of the ICBN? If there is not, I think one should be adopted, and his services be made available and advertised.

The same goes for C. antera (anteris = support); and there are several others in this paper.

.....2

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I admit you were correct in requiring that each change of the Code be legalized by a vote (especially where I have found reason to be at odds with earlier unauthorized modifications).

Regarding your list of generic names: such an alphabetic running register may have a useful purpose. If nothing else, it should in part obviate our exhortation to customers of the Genera File to buy 2 sets, with one to stay in alphabetic order. A disadvantage of such a running list is, that it is outdated soon, and not easily brought up to date. (One solution is possibly to print on one side only, leaving blank space for notes and updates). I sent Len Hills a copy of your letter, and we are intrigued by your suggestion that we might "be interested in it as an adjunct to the File". So far, I was under the impression that you prepared that list under the auspices of the IOPT for the Smithsonian, and that it was to be integrated there, or published separately.

Are you now suggesting that it might become a part of the Genera File? If so, we are interested in the possibilities, and should like to see a sample page; as well, we'd like to know a bit more about your criteria (although I am mostly aware of those), and the present status of your listing. Has it been typed up yet in a formal format, or are you still collecting new names? (Our first update should be out this month; it has 144 cards, many new entries; also, I have already a half dozen new m.s. cards for next year's supplement). Let us know more explicitly what you were thinking about; we will treat all that information fairly and confidentially if you so desire.

Your remarks on our financing: Len Hills managed to get support from the U of C, and together we haggle hard with a small Chinese printer (who mostly prints business cards and menus). Still, the results are worth the extra effort, and hopefully we will get into the black this year.

Finally your letter on Art. 33. Indeed, the second paragraph contains this "clearly indicated", which is just as ambiguous as the "definitely indicated" of the first paragraph. I was going to write a longish proposal on this, documented with examples. Do you want to come in on that? I'd like to make the Editorial Committee delete these ambiguous phrasings, and replace them with a concrete clear statement (even if it takes a few more words). I also think that any technical term used in the Code in a particular sense, ought to be defined in it as well.

An example of the latter is "inadmissable" as in Art. 23. Galea galea Maier 1959 is a tautonym, which "may not" be made. A reference to Art. 23 in Art. 32 shows that this "may not" equates with "invalidly published". But Art. 23 should likewise refer to Art. 32. The index of the ICBN gives a tautonym as "inadmissable" when referring to Art. 23; it were better to say "invalid". Again, in Art. 55, tautonymy is called "an obstacle"; here again a cross reference to Arts. 23 and 32 were elucidating. I was unable for a while to determine, from its own wording, whether Art. 23 made a tautonym "invalid", or "illegitimate", in which case the original date would still prevail for purposes of priority.

.....3

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A propos: Khlonova just proposed a new genus Vesicula; do you know if vesicula is a technical term in botanical morphology? For if so, the name is not validly proposed.

So far for now. Best wishes for you and "/et" (more than ever your right arm!), and get well soon!

Jan Jansonius

JJ/ap

Hunt Institute for Botanical Documentation

9 May, 1977

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

Dear Jan:

Regarding the much discussed matter of Article 33: There is another curious matter about the article about which you have not yet made reference. In the second paragraph (not counting the small-type examples) it states that a new combination is not validly published unless the basionym is "clearly indicated". Yet I have submitted several examples to Stafleu for opinions in which he was willing to accept as 'clear indication" a rather back-handed, implied"indication". Clear indication, in my opinion, really means that the basionym itself should be stated someplace, but I have several opinions from Stafleu in which he was willing to accept something a good deal less definite than that. In other words, the "powers that be" for some time have been interpreting the first sentence of article 33 strictly and the second sentence very liberally and loosely. In my opinion, it would be better to interpret both sentences strictly and not to accept any indication short of explicit statement of the new combination and the basionym combination where required. That is about the only kind of way of handling it which seems to me to be absolutely uncontroversial. I would like to see the proposal for article 33 to include both sentences and explain very specifically how they are to be interpreted. What do you think?

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

28 April, 1977

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

Dear Jan:

Thanks for your recent undated letter. The part about the "Dieke" problem helped to cheer me up greatly. I am recovering from surgery to my right arm--needed for some time and suddenly critical as I would otherwise have begun to lose the use of my hand. I am at the moment very handicapped by having to do everything with my left hand.

I don't agree with you at all regarding the necessity of being "so formal" regarding changes in the Code. I have been at this business a bit longer than you and remember having several knock-down, drag-out fights a couple of decades ago with Stafleu's predecessor regarding the cavalier attitude of the editorial committee in making changes which were at least marginal to the intention of the Congress. I would rather that the editorial committee be empowered to make practically no changes even in wording and require <u>everything</u> to be the product of a formal proposal. If this had been rigorously done in the past, many slips betwixt the cup and the lip could have been avoided. I am going to write you more fully in a different letter about Article 33--I know that that is in another paper bag of stuff I'm supposed to work on.

Regarding corrections to the generic file--I have indeed collected a list of mistakes but I am absolutely in no position to do anything about it. In fact, I am dictating this letter at home with nothing but my memory to serve me. I do remember at least one, if not two, places where Michael Millay's name comes out as "Michael" instead of "Millay" in a generic reference, just as one example of the sort of thing I caught. However, in general, it is a super job and you should have no great concerns. I still have the intention of getting up my own generic list. For one thing, I had it so nearly completed before your generic file came out, that I find it frustrating to let it go without completion. Besides, as a simple list printed <u>ad seriatum</u> it may have some advantage. Possibly you would even be interested in it as an adjunct

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to the file. You so far have had nothing to say about that.

Regarding the charge for your cards of $6.00, that seems eminently reasonable to me--I think you could have got $10 without any problems. In fact, the financing of the cards is a marvel to me and makes me wonder if you and Len have not taken it out of your Christmas money.

Best wishes to your Betty and your very interesting children and my other friends in Calgary.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

IV. 77

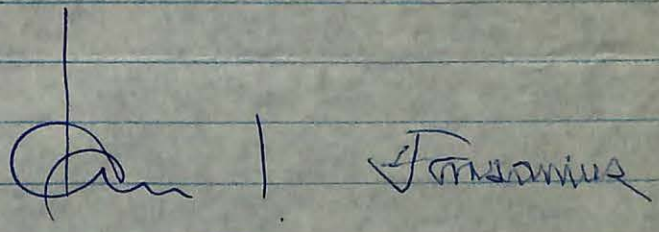
Dear Al,

Received your $ 6.= in perfect order.

Betty's advice to do zilch (o) about the Dicke problem is sensible. This is one occasion where, by doing nothing, the problem will disappear: in the mails!
However, come time to reprint them, you can tell them to do it write that time!

I didn't get far with Stafleu regarding an editorial change in Art. 33. Did you receive copies of my letters on that? I now shall write a formal proposal; a pity that things always have to be so formal, though.

Best regards to Betty + yourself,



PS Later this month we will bring the fist update of the Genera file to the printers. If you collected any glaring mistakes, I should gladly hear of them right now.
We now have ± 120 cards, and will have to charge about $6.=; do you think people will pay that?

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30 March, 1977

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

Dear Jan:

Thanks for the re-reminder regarding the six dollar check. I hope I manage to get it into the envelope this time. Of course (ahem!) if my assistant (Betty) were more on the ball.....

Your spotting of the inversion of the spelling of Deike on our envelopes was mortifying to me. I can't imagine how sommany hundreds of those things could have been used without anybody noticing it before. Most distressing. I really appreciate your sharp eye. Of course, along side of some of my other problems, it makes very little difference one way or the other.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
ENCL. CHECK FOR $6.00 !

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EXPLORATION RESEARCH DIVISION
EXPLORATION DEPARTMENT

March 16, 1977

Dr. A. Traverse
529 Deike Building
University Park
Pennsylvania 16802
U. S. A.

Dear Al,

Further to the earlier copies I sent you, of my question to Stafleu regarding Gocht's run-in with Art. 33, I now enclose Stafleu's answer, plus my reaction to it. For me and several people of my acquaintance the wording of Art. 33 is very unsatisfactory. Even if it goes back to the 1952 Code (at least), I don't think it should be enshrined. Now is a good time to push for a change, or would you not agree? If you do, possibly you could work on Voss; you may send him copies of my letters in any case. I also sent a set to Jim Schopf.

With best regards,

Jan Jansonius

JJ/ap

Hunt Institute for Botanical Documentation

INTERNATIONAL ASSOCIATION FOR PLANT TAXONOMY

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UTRECHT, 9 March 19 77
Telephone 030 - 53 18 30

Dr. Jan Jansonius
Imperial Oil Limited
339-50 Avenue South East
Calgary
Alberta T2G 2B3
Canada

Dear Dr. Jansonius,

I can really be brief about the paper by Gocht. There is no doubt really that Gocht did not make the combinations. His practice is actually precisely what the Code wants to avoid.

If you can come up with a less "ambiguous" version I shall be happy, but many English speaking colleagues have taken great care to make this present text unambiguous. I think one cannot win in this game: look for instance at the Eulophus example. In my opinion this could not be clearer!

I was not enthusiastic at the time about this requirement because, as you say, in cases like Gocht and Bentham the intent is obvious. Even so we abide of course by the very clear intent of the Congress which is that the combination must be spelled out.

Yours disconsolately,

F.A. Stafleu.

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EXPLORATION RESEARCH DIVISION

EXPLORATION DEPARTMENT

March 16, 1977

Dr. F.A. Stafleu
Intern. Bureau Plant Taxonomy
Tweede Transitorium - Uithof
UTRECHT
Netherlands

Dear Dr. Stafleu,

Yesterday, after I dashed off my letter on tautonyms, I received your answer on Gocht's still-born combinations.

I discussed the case again with Staplin and Pocock, who are born to the English language. They are positive that Gocht fulfilled exactly the requirements of Art. 33 as it is now worded.

Yet, the examples - to which you refer - "definitely indicate" that Gocht's transfer proposal was not adequate. The problem is that the actual phrasing of the first sentence of Art. 33 is lacking, if its interpretation depends on critical perusal of the following examples.

I understand that you don't want to change the traditional interpretation of Art. 33, because that would open a whole can of worms. I think that a modifying clause might improve things; for your contemplation I submit the following alternatives:

- A combination is not validly published unless the author definitely indicates - by his own example/usage or special/other typographical means - that the epithet or epithets concerned are to be used in that particular combination.

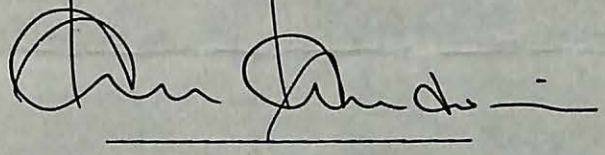
- A combination is not validly published unless the author uses the epithet or epithets concerned in that particular combination or definitely indicates such usage by special/other typographic means.

.....2

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I think these phrasings are a clarification of the intent of Art. 33, without altering things or introducing new twists. I should appreciate your submitting these versions to the other members of the Editorial Committee. In my opinion, this is a straight editorial matter. If you think otherwise I will write an official proposal in Taxon, but that would mean that the present unsatisfactory format will be with us for many more years.

Sincerely yours,



Jan Jansonius

JJ/ap

cc. A. Traverse
J. Schopf
H. Gocht

Hunt Institute for Botanical Documentation

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EXPLORATION RESEARCH DIVISION

EXPLORATION DEPARTMENT

March 15, 1977

file Jarosewich

Dr. F. Stafleu
Syst. Botanie
Tweede Transitorium/Uithof
UTRECHT
Netherlands

Dear Dr. Stafleu,

Please stop the presses for the new ICBN, until the following problem is attended to.

Art. 23 states that a name of a species "may not exactly repeat the generic name". In Art. 55 a tautonym resulting from a transfer is called "an obstacle". The Index refers to Art. 23 under "tautonym (inadmissable)".

In the real case of Galea galea Maier 1959 (a dinoflagellate), is the name validly proposed? Is it illegitimate? A later transfer to Areoligera by Davey et al. 1969 makes it presumably OK. However, it would help if Art. 23 would instruct what to do in such cases; the date of validity would change or not change when an alternate replacement name is provided? Who is the author of that name? After transfer, does the "inadmissable" name become viable again? The Code does not give a definition of "inadmissable". This is indeed a no-man's-land that must be charted.

While checking up an inconsistency caught by eye: Art. 23 instructs to merely hyphenate two-worded epithets and not to reject such a two-worded "species name". Yet, Art. 20 requires that a generic name must be one word, or hyphenated if two words are used; it refuses Uva ursi Mill 1754, but accepts Uva-ursi Duhamel 1755. Just for satisfying my curiosity, do you know why the Code is more lenient in the case of epithets?

.....2

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Maier 1959 also created "Areoligera biramum; derivatio nominis biramum = zweiästig". Of course, two-branched should be "biramosa". Since the German translation of the epithet is adjectival, should the Latin epithet reflect the gender of Areoligera, or should the orthography of this name not be corrected?

Yours sincerely,

Jan Jansonius

Reference: D. Maier, 1959, N. Jahrb. Geol. Paläontol., Abh., 107 (278-340)

JJ/ap
cc. A. Traverse

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEIKE BUILDING
UNIVERSITY PARK, PENNSYLVANIA 16802

College of Earth and Mineral Sciences
Department of Geosciences
Palynological Laboratories

Area Code 814
865-6543
865-2342

15 March, 1977

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

Dear Jan:

I just received a notice of non-payment of CIMP dues. I thought I had paid, but in case I hadn't, I enclose my check for six bucks per the dues notice.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
?encl: check for $6 for CIMP Hah! Try again. You really become a "verstrooide professor".

P.S. I still feel bad that we didn't get the next international palynological conference for Calgary, but since they have moved the next one up to 1980, perhaps we can shoot for Calgary in 1984 or whenever! Maybe you could pass along my feelings on the matter to Len Hill. Have done so

Hunt Institute for Botanical Documentation

15 March, 1977

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EXPLORATION RESEARCH DIVISION
EXPLORATION DEPARTMENT

February 21, 1977

Dr. A. Traverse
529 Deike Building
University Park
Pennsylvania 16802
U. S. A.

Dear Al,

Just as I was mailing a copy of a letter to Stafleu, yours of 15 February came in; I would like to react to it immediately, and will take things in your order (which is not my order of priorities).

Kassian! is what comes to mind regarding your unfortunately frustrated travel plans. Stanley was amazed at how cheap everything was over there; you probably could have managed on very little.

According to Stan, Hughes was very crafty and relied on ignorance of procedure plus incomplete mastery of the English language in order to declare pushed through,without further debate, a number of measures that were not really related to whatever was accepted by the meeting. I hope it was not a pyrrhic victory.

The organisation in Lucknow was o.k., but Stan was appalled at the poor facilities at the Birbal Institute (no higher lenses than 25x on their microscopes!) and the politicking and back-biting which seems to be their foremost activity.

I really like your suggestion to go over the Code carefully and remove the many small irritants that now abound in it. Is there any purpose in resubmitting your list to the Editorial Committee now that the Code is to be completely reset? You are right about a "valid genus", it is mental shorthand for "validly published", and (unlike in the past) I now am careful to differentiate between a "valid name", a "correct name" and "legitimate name". Is it just slangy shorthand to use "valid genus" and thus be objectionable to Emily Post, or would I confuse somebody's mind with that usage? It seems so verbose to repeat "validly published generic name" every time.

.....2

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A species name is a combination of a generic name and a specific epithet; a generic name by itself is not a combination. And indeed, the Index is almost a joke, and should be drastically improved. It should not be very difficult to do either.

This week we will visit our printer for the first set of supplementary genera cards. By the end of next month I shall submit my typescript to him. Therefore, if you have any corrections written down, please send them over in whatever form, so we can include them now.

As for the blanks etc.: did you read the first page of the set (numbers 572, 1069, 1341, 1528 are blank because not utilised; numbers 1070-1078 are not in alphabetic order). If you have further blanks or misprints, please let us know so we can claim credit at the printers. So your last item is my first priority!

I'm happy for the better weather and longer hours of sunlight that now keep you warm. Here it is like an English winter, even little frost at night; but we sure could use some precipitation.

Thank you for your letter,

Best regards etc.,

in haste, and not very carefully phrased, please "verexcuseer"

Jan Jansonius

JJ/ap

Hunt Institute for Botanical Documentation

15 February, 1977

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. SE.
Calgary, Alberta, Canada T2G 2B3

Dear Jan:

It has certainly taken me too long to get around to answering yours of 13 January. I have been in a state of shock about my failure to be funded to go to Lucknow, which still seems incomprehensible to me. After it was too late and apparently some complaints had been registered, the National Science Foundation came through with a travel grant--on 21 December! I would have had to leave in about four days. By that time I had, of course, long since abandoned plans to attend. Then I was elected president anyway, after apparently a very bad scene with Bharadwaj and the Soviets. Maybe it was just as well I wasn't there, but I have been having a difficult time sorting it all out in my mind.

I am very disappointed that the Calgary suggestion was not pushed through to completion, and I know very well that the only reason is that I was not at the meeting. Had I been in Lucknow, I am certain I could have got favorable consideration because it is certainly North America's "turn". I am not sure that the matter is finally settled without the possibility of reconsideration. I will be in a good position to look into that! For that reason, I believe that a formal invitation extended to the International Commission for Palynology, of which I am now president, would be politic. I am not sure I can overturn the council's decision to have the meeting in Cambridge--that may prove to be working against insurmountable odds, but I certainly intend to try.

Regarding the Catalog: I do regard my resignation as co-editor (that's the highest I was ever allowed to attain by Spackman despite the fact that I did practically all of the final editing and made all intermediate decisions and carried all of the responsibility) as final. Spackman never allowed me any control or say about either budget or personnel for the Catalog and how

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I managed to put up with this anomalous situation for eleven years is beyond me. You simply can't have the responsibility for something and no authority to take important steps to correct situations. I feel as if a great weight had been lifted from my shoulders. I agree that it is somewhat tragic because there is no question that the person in the United States best qualified to do this job is yours truly, just as the person best qualified to do it in Canada would be you. I am int

I am interested in your comments on the Voss correspondence. I agree with you there are many places in the Code which require editorial revision. In fact, I have a long list of such changes which I have noted in the back of various editions of the Code, and have submitted snipets of the stuff to the editorial committee from time to time with very little results. I really think what we should do, that is you and I, is to have a private meeting sometime between now and the next congress and submit a whole raft of these things as formal proposals to amend. Then one or both of us must be on the floor of the nomenclatural sessions to prevent the amendments from "being referred to the editorial committee". That is a very convenient device used to prevent change. You are right that it would be good to give an example of a validly published genus (not "valid genus"--you should stop using that expression) with a not validly published type species. In the same paragraph you talk about "combination or name"--I would understand combination to <u>be</u> a name. I do believe that all such instances of inconsistent usage should be cleaned up. I have occasionally had the notion that the best way to do this would be to put the whole thing in a computer the way they have done with the Bible. This would catch all inconsistencies and invite their correction! I agree that the use of the word "available" is unfortunate because it is also used in other nomenclatural connections.

You do not mention problems with the Index, which I have found exceedingly irritating. The Index should be more complete, and various inconsistencies should be eliminated. On the "nomen nudum" it is apparent that we each understand what the other is saying and we have simply got down to admitting that we have a disagreement of opinion. I think that this should be taken care of by one of us submitting a formal amendment on the subject to the next congress, which will automatically provide for both debate before the congress and debate at the congress. That way, we will find out what the majority opinion is. I really do prefer my broader interpretation, but I am weakening slightly.

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One of these days when I get around to it I am going to send you a request for replacement cards for my set, as I have quite a few blanks. I also will send you a list of corrections I have noted.

We are having a hard time keeping warm, as you most likely can imagine--we live in an electrically heated house, with supplementary wood-burning devices, and I have been cutting firewood every Saturday to prevent bankruptcy. However, in the words of the Vietnam war, I now see the light at the end of the tunnel, which means that the days are getting longer. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

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EXPLORATION RESEARCH DIVISION
EXPLORATION DEPARTMENT

Jan. 13, 1977

Dr. A. Traverse
529 Deike Bldg.
University Park, Penn. 16802
U. S. A.

Dear Al,

On my desk are several letters from you, which I was very happy to receive. I want to answer them briefly and then return to a load of service work that needs attention.

Regarding Calgary's invitation of the next ICP: Len gave instructions and relevant information and addresses to the Dean's office, who were going to issue the official invitation. Apparently they never realized the small time frame we were working in; by now it is too late to stimulate them. The same day that I received your call urging a telegram to India I had just started to do that very same thing. I did cable Stan Pocock, because he knew some of the background.

I was unhappy to learn about your resignation as editor of the CFSP; is that irrevocable, or just one more battle: I always enjoyed your conversation on various points that came up in this context.

Thank you for the copies of the Voss letters. I had no notion that the Code was going to be re-set this time. I might have given a few more suggestions on where the Code has been unclear to me. For instance, an example of a valid genus with invalid type species should be helpful. Also, some people now start using a suffix -iensis; I think -ensis is only correct, but do not know why; a reference to that might be helpful. Also, an example of the correct neuter ending -ense would be useful; I have not been able to convince some people (a.o. Davey & Verdier, vide Rev. Pal. Pal. 22(4):310). One inconsistency in the Code pertains to the use of the term "combination"; sometimes (Art. 33) the Code says "combination or name"; other times just "name" where also "combination or name" is meant (Art. 34,35,36). Myself, I could do with an example after the first paragraph of Art. 7, where it says "... or as a synonym".

In Art. 11, line 9: the word "available" appears superfluous; is it a hangover from the Zoological Code? I would advise to delete the word.

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In Art. 32, paragraph 1, sub (3): the exceptions here listed are not the only ones, as in Art. 41 exception is also made for Linaeus ed. 1 & 2; this should be included sub (3) in Art. 32.

Finally, the nomen nudum, this shivering waif. You had earlier sent me a copy of the page from the IAPT, and I should have mentioned it. Still, it is not part of the Code. You give the opinion that the Glossary gives two definitions, separated by a semi-colon. I think it more likely that the part after the semi-colon is adjunctive? (sorry, my syntactic vocabulary in English falls short), i.e. modifying what goes before. Thus, since a nomen nudum lacks a description, it is not a name in the sense of the Code; it is not validly published because it lacks a description. Anyway, my point is that such terms, used in the Code, should be defined there also. Otherwise you get an Alice-in-Wonderland situation where a word can mean anything I want to make it to.

Your comment on my Latin exegesis and applying that to Gymnasium was very amusing. You made a solid point, there!

Thank you again for your letters, and best personal regards,

Jan Jansonius

JJ/ap

Hunt Institute for Botanical Documentation

Gaylord
Fidelity Onion Skin
100% RAG BOND

5 January, 1977

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. SE
Calgary, Alta., Canada T2G 2B3

Dear Jan:

Just got a copy of yours to JMS, of 15-XII-76. Do you know of the IAPT Glossary of Botanical Nomenclature? You don't mention it? It is not binding, but has some value. Check "definition" of nomen nudum there--unfortunately, it is two different definitions separated by a semi-colon!

The reason I prefer the definition of nomen nudum, as "not validly published" as pragmatic. Your present definition is slippery, but the Code says what is not validly published. I see no reason to make the definition as complicated as your present approach would make it. However, I recall from previous correspondence you used to hold that nomen nudum meant without description (diagnosis). That is unequivocal, even though I didn't agree with it. Why did you change? I agree that "nomen nudum" is not a neccessary term if it just means "a not validly published name"--it is shorter, tho'!

I can't see that your Latin exegesis is pertinent. Try that approach on "gymnasium". The question is, what have nomenclaturalists meant by n.n. all these years?

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: J. M. Schopf

Hunt Institute for Botanical Documentation

31 December, 1976

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. SE
Calgary, Alta., Canada T2G 2B3

Dear Jan:

Thanks for the copy of the letter you sent me which you had directed to Stafleu. I could have saved you some trouble if you had thought to ask me because we had been through exactly the same case several years ago, and Stafleu had told us in no uncertain terms that a generic name can be validly published even though the type species indicated was itself not validly published. All that the code requires is that the type be demonstrable, not that it's name be validly published.

As you know by now, I did not make it to Lucknow. Therefore, I very much hope that somebody saw to it that the invitation from the Calgary group of palynologists for the 5th International Conference was presented to the required authority at Lucknow.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Vansonius

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22 Dec. 1976

Dear Jan:

Thanks for yours of 2 Dec. — I still have not received the materials from Lon Hills — perhaps he sent them directly to Jackson, which would have been fortunate because I gave up hope of being funded for the trip on 12 December and withdrew, cancelled subscription to the meeting, reservations, etc. (Since then a ticket did come (on the 20th) — but no per diem, etc.; too little and too late.)

I have written Blondeau indicating that an invitation from y'all — Hills & U. C. specifically — is in the works and urging its acceptance. Will forward whatever Lon sends as soon as it comes in. Wish I could be there & politic.

What did you mean by "keeping the lip zipped"? — about what?

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IPC can be at anytime, by the way — Utrecht was in Sept. Voronezh was in Aug. Leuven in Jan. Aug. would be your choice, I suppose, because of UC facilities.

Re quid pro quo — she refers that to you. I have resigned as Editor of CRSP (long story). The g.p.q. was his idea, and I should have dropped it, but at the time I was still trying to be agreeable.

Hang in there. Happy holidays.

Yours cordially,

Al T.

DR. ALFRED TRAVERSE
529 Deike Building
Pennsylvania State University
University Park, Pa. 16802

Hunt Institute for Botanical Documentation

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EXPLORATION RESEARCH DIVISION

EXPLORATION DEPARTMENT

December 15, 1976

Dr. J.M. Schopf
U.S. Coal Geology Lab
Orton Hall/University
155 S. Oval Drive
Columbus, Ohio 43210
U. S. A.

Dear Jim,

Today I received a copy of Al Traverse's letter (10/XII/76) to you, in which Al expresses his fear that my understanding of the term nomen nudum might prevail. (Am I ever glad I just asked you what you think it is!).

However, this now spurs me to spend some necessary time, and answer your earlier request for examples. Lacking as I am in traditional understanding in plant naming, I always have tried to find solutions for my problems in the Code. In the Index of the Code, the term nomen nudum occurs; in the Code it is only mentioned in Rec. 50B, but nowhere does it state what is meant by it. Since the Code is basically a legal document, many English words have restricted meaning in the Code, and are therefore properly defined. Terms such as "valid publication", "illegitimate name", "junior synonym" can be readily understood, but in the nitty-gritty of exact exegesis are found to have precisely measured content. For that reason I deplore that the Code introduces a technical term "nomen nudum" without stating what it is exactly. I therefore must go back to my Latin, which gives for nomen nudum: a bare name, an empty name, a deprived name, a name only.

A. A name only, a bare name, an empty name. This is the meaning that appeals most to me: just a name that is put out or used, nothing more. For that concept there are many examples: names invented for hypothetical morphological entities (that usually are found in nature, alright) but not introduced and validly published for a found entity.

One example I consider to be the case of Naumova 1939 (in Repts. of the 1937 Geol. Congr., Moscow), who proposed an early and quite logical system of names for many morphological entities of spores and pollen. Here, each name was briefly and summarily characterized by an adjective or similar qualification, which I consider to be an explication of the entity, but not - from the whole context of the paper, which is preliminary in character - a formal diagnosis. If these summary characteristics

Hunt Institute for Botanical Documentation

by later authors have been quoted in lieu of diagnosis, I have accepted such "diagnosis-by-referral" as adequate in the sense of the Code, if the diagnosis was adduced in the context of systematics, and the further requirements of the Code were met. So: I think Naumova's names are just that in 1939, even if some of them may have been in use in m.s. reports.

Pant - in imitation of Naumova, presumably - in 1954 published a whole system of logically derived names for spores and pollen; these were "just names", not yet "names for something concrete.

Another example are several Russian authors, who use manuscript names in plate descriptions and stratigraphic discussions, sometimes with a notation "gen. et sp. n.", but without any attempt to formally propose (= "validly publish") such names.

B. A deprived name. This is different in meaning: here is a name, but it has been done out of something. This then is a name, formally proposed as such, but not sufficiently so; it may lack in description, or illustration, or designation of a nomenclatural type, or whatever. It is in other words: invalidly published. I see no merit in introducing into the Code a formal term "nomen nudum", if it covers the same thing as "invalid", which is itself defined in the Code as everything which is not valid (Art. 32-45).

In the sense given under A, a nomen nudum is not validly published, hence invalid; but an invalid name is not necessarily a nomen nudum in that sense, and sometimes can be validated by satisfying only some minor formal requirement of the Code.

Examples of the sense given under B are numerous: any genus which is validated "ex" some author. For spores and pollen you can pull several out of the Genera File. I just cannot bring my feeling for language to accept a name deficient in only a (minor) formality, as "nomen nudum". To me, that is a nomen invalidum", or "a powerless name". However, I will accept any definition that may appear in the Code.

I really think that every technical term used in the Code should also be defined in it; that goes for homonym, paratype, legitimate name as well as nomen nudum, nomen ambiguum, type species, etc. On further reflection, I think that the conditions given in Art. 34 sub (2), (3), (4), and possibly (5) are technically those of nomina nuda. Although (2) is a "provisional name" it would often also be "nude"; of course, not all "nude" names conversely are provisional".

Examples of the concept sub A (essentially grabbed from my Genera File):

Chomotriletes Naumova 1939, XVII Geologorum Conventus. Rept. of Sess. v. 1, p. 355. Under group Triletes (Azonotriletes), given as subgroup "Chomotriletes Naum. - with concentric projections." Validated in 1953 as genus for alete algal cysts with concentric ribbing.

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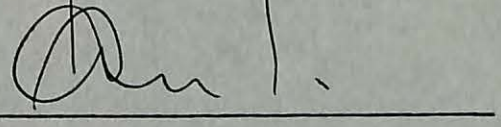
Chomosporites Pant 1954, Botan. Rev., v. 20, p. 50. Used in a listing of possible names useful for application in systematic palynology: "sub-group 13. Chomosporites (includes micro- and isospores of Chomotriletes Naum.) - with concentric ridges".

Cingulosphaeridium Lopukhin in Neishtadt (editor) 1966, "The importance of palynological analysis....", Akad. Nauk SSSR, Inst. Geograf.-Geol. Inst., Nauka, Moscow. Used in a stratigraphic discussion of the succession of Cambrian palynological assemblages; no species named or illustrated.

Cornaceoipollenites Potonié 1951 Palaeontographica, Abt. B, Bd. 91, p. 151. Used only in plate description (pl. 21, fig. 145) as Cornaceoipoll. parmularius R. Pot. 1934"; this new combination is not validly proposed, and there is no further diagnosis or description of the genus. [In 1960 (Synopsis III, p. 93, 94), Potonié validated the genus, which immediately became an obligate junior synonym of Tricolpopollenites Thomson & Pflug 1953, which has the same type species. Potonié recognized that in 1966.]

Wishing you a pleasant X-mas season and all things well for the new year,

With best regards,



Jan Jansonius

JJ/ap

cc. A. Traverse
E.G. Voss

Hunt Institute for Botanical Documentation

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EXPLORATION RESEARCH DIVISION

EXPLORATION DEPARTMENT

December 6, 1976

Dear All, just to keep you informed.

This brings to my mind that maybe you should circulate a once or twice yearly newsletter among the members of the paleobot. comm., so everyone is informed of this type of thing?

Best regards,

Dr. F. L. Stafleu
Systematische Botanie/University
Tweede Transitorium, Uithof
Utrecht, Netherlands

Dear Dr. Stafleu,

I hope my letter of 3 October on Kisselovia - which was inadvertently directed to the Tweede Uithof - did reach you. In the mean time, I have another puzzle that is related to the earlier problem. The case lies as follows.

Rossignol, July 1962 (Pollen Spores, v. 4, p. 134, pl. 2, figs. 4,5,6), proposed the new genus Concentricystes, without separate diagnosis, but in the next line proposed the new sole species Concentricystes rubinus, the description of which will be acceptable as combined description per the new edition of the ICBN. Rossignol did not indicate which of her three illustrated specimens represented the holotype. Therefore, the species C. rubinus was not validly published. Christopher 1976 (Micropaleontology, v.22, p. 145) argues that, the type species not being valid, the genus Concentricystes is itself not validly proposed, and thus adopts the name Pseudoschizaea, which was proposed by Thiergart & Frantz in September 1962 (Palaeobotanist, v. 10, p. 84) by a descriptio generico-specifica.

Christopher 1976 makes at least one obvious nomenclatural error elsewhere in his text*, and shows questionable systematic judgement in trying to separate his concept of Pseudoschizea from Chomotriletes. But that is beside the point.

I should much like to have your opinion regarding the question: can a genus be validly published per descriptionem generico-specificam if no holotype is designated for the type species. This basically, is a variant of my earlier question: Is it possible for a genus to be validly published (after 1958), if not all requirements for valid publication of its type species have been met at the same time?

It is almost embarrassing to see how many nomenclatural boners have been put in print by palynologists, that the ICBN did not anticipate. However, we'll have to try to straighten them out as they come.

With best regards,

Yours sincerely,

Jan Jansonius

JJ/ap
cc. J.M. Schopf
A. Traverse

*/ designating one of his own specimens as "holotype" (= neotype), rather than selecting a lectotype from Rossignol, in validating her species rubinus.

Hunt Institute for Botanical Documentation

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EXPLORATION RESEARCH DIVISION
EXPLORATION DEPARTMENT

October 3rd, 1976

Dr. F.L. Stafleu
Dept. Botany /University
Tweede Uithof
Utrecht, Netherlands

Dear Dr. Stafleu;

I would like your opinion on the following nomenclatural puzzle.

Vozzhenikova 1960 illustrated a fossil dinoflagellate Kiselevia ornata (pl. 3, fig. 2); name and epithet are new, but not described and thus not validly published. Kiselevia major is similarly illustrated.

Vozzhenikova 1961 (Dokl. Ada. Nauk SSSR, v. 139, p. 1462) gives the name as Kisseljovia, merely as incidental mention in her discussion.

Vozzhenikova 1963 (in Yu. A. Orlov, Osnovy Paleontologii, v. 14, Chapter on Algae, p. 183) re-illustrates the same figures, under the names Kisselovia ornata and K. major. (Note the new orthography of these names). In the text a generic description is given, in which the variation in size is given under reference to the illustration of both species, while K. ornata is given as type species. However, in this text neither of these species is described.

One could possibly argue that the description is a descriptio generico-specifica, inasmuch as it follows after both the generic name as well as that of the designated type species. But its translation makes clear - as does the reference in it to the illustrations of both species - that the description is of the genus, encompassing more than just the type species.

Vozzhenikova 1965 (fide Judy Lentin, who read the Russian text to me over the telephone) gives the generic name as Kisselevia.

Vozzhenikova 1967 (Fossil peridinids from Jurassic, Cretaceous and Paleogene deposits of the USSR; Nauka, Moscow; Engl. transl. Nation. Lending Libr., Boston Spa) formally describes many genera and species, both new and old. She gives the genus Kisselevia Vozzh. 1963, with the type species K. ornata (holotype a new photograph, pl. XLIV, fig. 6) and also describes K. major (with as holotype illustration that of 1960, pl. 3, fig. 1); both species are assigned to Vozzh. 1960, which is patently incorrect nomenclaturally.

Hunt Institute for Botanical Documentation

The genus is named in honour of I.A. Kiselev (as I transliterate from the Russian), which in a footnote in the 1967 paper is given as Kisselev.

So far for the facts pertinent to our problem, which is twofold:

1. What is the correct orthography, and oldest date of valid publication.
2. Does Art. 37 (ICBN 1972) say exactly what it means ("..taxon..valid only when..nomenclatural type indicated"), or does it intend to say: "a taxon...is valid only when its validly published type is indicated".

Vozzhenikova's problems with finding the most judicious transposition of Kiselev's name into latin phonetics are obvious, and may in part be the doing of her various editors. Lentin & Williams accepted as earliest validly published form Kisselevia Vozzh. 1967. They rejected Kisselovia Vozzh. 1963, arguing that there was no holotype designated for the type species K. ornata. This argument does not sound convincing; only one illustration of K. ornata was given in 1963; the species, however, was not then validly described.

This last aspect is of broader interest: in the palynological literature there exist several examples of new genera, for which older species were designated as type species, without these species simultaneously being validly published as new combinations under the new generic name. I have taken such genera to be valid, inasmuch as a type species was designated. Extending this benign interpretation, I cannot but accept Kisselovia as validly published in 1963, although its type species (designated in 1963) was validly published only in 1967 as Kisselevia ornata. The different orthography of the name can be considered a variant, although linguistic arguments can be adduced for either one, most antecedent variations showing an "e" rather than an "o".

This problem was submitted to me for an opinion by L.E. Stover; so far I have collected the necessary effectively published sources that bear on the question. Copies of these are enclosed, as far as available. My reasons for accepting Kisselovia as earliest validly proposed name are explained above. I would like to have your opinion on this conclusion, as well as on my "benign" interpretation of Art. 37. If you disagree with this interpretation, do you consider that I should organize evidence and examples, and make a formal proposal to either explicitly leave Art. 37 as is, or to substitute the words "a validly published" for the word "the" in the second line?

With best regards,

Yours sincerely,

Jan Jansonius

JJ/dh Encl:

Hunt Institute for Botanical Documentation

INTERNATIONAL ASSOCIATION FOR PLANT TAXONOMY

Office:
International Bureau for Plant Taxonomy
and Nomenclature
Tweede Transitorium, Uithof
Utrecht
(Netherlands)

Bankers: Messrs Vlaer & Kol, Utrecht

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Telephone 030 - 53 18 30

Dr. J. Jansonius
Imperial Oil Ltd.
339-50 Ave. Se.
Calgary, Alberta
CANADA T2G 2B3

just came in as
I signed other one

Dear Dr. Jansonius,

A type cannot be validly published, only a name can. Art. 37 (your question 2 on p. 2 of your letter of 3 October 1976 which I received only five days ago) says exactly what it means.

In other words a type species indication of a genus may be to an invalidly published name, e.g. to a pre-starting point name of a species (see e.g. Code p. 243, under the rejected homonym Martensia).

The correct orthography of your name is Kisselovia, dating from 1963.

Miss Zijlstra who worked on this case for ING at our office had come to precisely these conclusions before your letter arrived. She says that may be for future publications of generic names it might be advisable to require typification of generic names by means of species for which a validly published name is available.

With all good wishes,

yours sincerely,

F.A. Stafleu.

C: G. Zijlstra

Hunt Institute for Botanical Documentation

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PHONE 259-0671 AREA CODE 403

EXPLORATION RESEARCH DIVISION
EXPLORATION DEPARTMENT

December 2, 1976

Dr. A. Traverse
Deike Bldg./Palynology
University Park
Pennsylvania 16802
U. S. A.

Dear Al,

Cleaning a paper-jam off my desk, I came across yours of 31 March '76, where you suggest a "quid-pro-quo". I discussed this with Len Hills, and we are willing to provide the CFSP with a free copy of the Genera File, if the CFSP will print an extra page extolling the advantages of joining the two systems, to be included with your next shipment of materials or your next billing.

If you will enter into such an exchange of services, please write Len Hills for latest information on prices and postal tariffs: you might as well include that.

Best regards,

Yours truly,

Jan Jansonius

JJ/ap

Hunt Institute for Botanical Documentation

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PHONE 259-0671 AREA CODE 403

EXPLORATION RESEARCH DIVISION

EXPLORATION DEPARTMENT

December 2, 1976

Dr. A. Traverse
Deike Bldg./University
University Park, Penn. 16802
U. S. A.

Dear Al,

In brief answer to your letter of 29 October 1976, your suggestion that Calgary offer to host the next, Vth, ICP meeting has been nudged from indifferent rock onto fertile soil. Len Hills is the prime mover, supported by the facilities of the U. of C., and he will write to you with a formal invitation. We count on you to work on the AASP executives to make them willing to join forces, and for one time abandon the October date.

Have a good trip, and enjoy yourself in India (but keep your lip zipped!).

Best regards,

Jan Jansonius

JJ/ap

Hunt Institute for Botanical Documentation

USGS COAL GEOLOGY LABORATORY
P & S BRANCH

Orton Hall, 155 South Oval Drive, Columbus, Ohio 43210

November 18, 1976

Dr. J. Jansonius
c/o Imperial Oil Ltd.
339 - 50th Ave. SE
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

I need examples of what you consider nomina nuda, some of which may be incorporated in the new edition of the International Botanical Code.

You had asked for a definition to be included under Rec. 50B. The Editorial Committee did not think they should tamper with the present wording but were agreeable to including examples. The present definition given in the annotated glossary of botanical nomenclature, McVaugh and others, Regnum Veg. v. 56, p. 1-27, 1968, may be used, if desired. The point was made that not all illegitimate names are nomina nuda -- only those that lack descriptive material.

I would like to include some illustrations from palynological literature, and I am also asking Al Traverse to suggest a few. The list cannot go beyond two or three examples but these should be chosen to illustrate or "typify" particular kinds of situations.

What we need are names with abbreviated references in the regular style of the Code. I would also like to have the full reference in case anyone asks me. I suggested that it would be desirable to have full references included in a list at the back of the Code, but I don't think anyone else sympathized with this suggestion. I had to leave the Committee meeting early before they dealt with Hybrids and Appendixes, so I don't know what they did officially with my suggestion.

This letter is coming to you a little bit late because of the interference of the GSA meeting and delay in getting back to it since. If you can return the material before Christmas, I think that may be in time to include in the third draft of the Code revision.

With best regards,
cc: Al Traverse, Ed Voss

James M. Schopf
Geologist, W.A.E.

Hunt Institute for Botanical Documentation

29 October, 1976

Dr. Jan Janzonius
Imperial Oil Ltd.
339 50th Ave. SE
Calgary, Alta.,Canada T2G 2B3

Dear Jan:

What did you think of my suggestion for V IPC to meet in Calgary? If you agree it's a great idea, could the "Calgary Group" come up with a formal invitation? I could convey it to the governing board of ICP at the forthcoming conference in India (as co-sponsored by AASP?? --I would need to get an opinion on that from the President, I suppose. I don't think it's necessary).

Looking forward to hearing from you and with best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEIKE BUILDING
UNIVERSITY PARK, PENNSYLVANIA 16802

College of Earth and Mineral Sciences
Department of Geosciences
Palynological Laboratories

Area Code 814
865-6543
865-2342

August 9, 1976

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. SE.
Calgary, Alberta, Canada T2G-2B3

Dear Jan:

I have been a little behind in my correspondence (we are "took up" with building a new house and other distractions) -- yours is undated, and I hope has not been around very long!

I remember Johannes very well -- a fine young man -- and am delighted you would trust me as his pro-tem "guardian" -- we would, of course, do anything he needed and in the meantime, pray it won't be necessary! Because of our move, they might have had trouble reaching us if the problem arose on Sat. or Sun.! (New permanent home address: R.D. 2, Box 298, Huntindon, Pa. 16652.)

Glad to hear progress rept. on "file" -- for my part I have been spending dozens of hours on the generic list for Smithsonian, and your preliminary cards have been very useful -- 95% of time I agree with your decisions -- possibly you'd be interested in my reasons for the other 5% -- we should get together on this. I wonder if I should publish my list as a vol. of Catalog, with CFSP refs.? -- could you see any value in this, or would it be redundant? Could it be Traverse & Jansonius? We could chat in Halifax (I do plan to be there).

All the best.

Yours very truly,

Al

Alfred Traverse
Professor of Palynology

P.S. I am glad you're not mad at me -- I meant no harm by relaying to you suggestions of others here re coöp. between JF and CFSP.

Hunt Institute for Botanical Documentation

7-VIII-76

Dear Al,

Just a brief note touching on Something entirely new!

Our son Johannes presently attends the Meadowmount School of Music in RFD 2, Westport, NY 12993, near Elisabethtown in other words.

We will leave shortly for Holland, where we will spend 4 weeks. I have given your name to the school as a friend in the USA who may be contacted in case of emergency. I don't anticipate any problems, though, and I trust you will not mind this imposition. If any disasters might befall him, I trust you would not mind to be of help.

20 July - 17 Aug.

Hunt Institute for Botanical Documentation

Here life is sweet enough: no drought, no floods, the summer a bit coolish, but with fine fresh vegetables in the garden (now to be left for friends and the slugs to eat). I am building a small greenhouse, the roof is just glassed in.

With regards to the File: all the printing is done, and a team of industrious Chinese girls is nearly finished with the job of collating. Len Hills is postponing his vacation so as to expediate the shipping of the first sets as soon as they are ready.

That's all for now, there are a host of small jobs yet to be done. Best regards to Betty, Tate, and others I know, not least unto yourself! See you in Halifax?

John.

Hunt Institute for Botanical Documentation

XXXXX

April 28, 1976

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. S.E.
Calgary, Alberta
Canada T2G-2B3

Dear Jan:

In connection with preparing the pesky generic list for the Smithsonian Institution (what a waste of time!--with your cards coming out in the so near future--but they had to have the information immediately), I found innumerable minor errors in the cards, most of which you have presumably caught in proof-reading. I did not send information to the Smithsonian on any generic names that I did not see in the original myself, but I did use yours ~~cards~~ to check my information when I was in doubt. By the way, I don't seem to have a cards for *Deltoidospora* Miner 1935, *Gnetumites* Zaklinskaya 1957, or *Zamities* Zaklinskaya 1957. I just noted this morning that the reference to the paper by Taylor and Millay is misreferenced under *Vesicaspora*. I have a note to myself to tell you that the the author of *Biornatispora* is Lele & Streel, not Streel. I also don't agree with you on *Octacolpites* Vimal 1952--in my opinion ~~is~~tis not a nomen nudum because it was monotypic when published and is therefore OK. This is true also of other Vimal taxa, unless there is something I have been missing. I should also perhaps tell you that after long and hard reflection I have come to the conclusion that the names published in Naumova 1939 were in many instances validly published. The argument that they have no species is not at all relevant. The argument they are not labeled as genera is somewhat more troublesome. I am still wrestling with *Pollenites* but so far have been unable to convince myself that it was a validly published generic name, for lack of any morphological description ("pollen" is not morphological in content). More of the slips with my notes on them may surface as I clean up, and I'll pass the information on for what it's worth. I realize that my list for the Smithsonian is terribly incomplete--something just over 2,000 generic names, but it's better than nothing, which is what they would have otherwise had. I do think it includes most of the really significant generic names, but that doesn't help too much. What they wanted was completeness, but they forgot about the spores and pollen until it was too late for anyone to have enough time to do it. (They were not interested in any names but those validly published, so I am not as short as it might appear.)

Do

Hunt Institute for Botanical Documentation

Do you have in your library originals of xerox copies of all the publisations in which the names referenced in your cards were first published? If so, how difficult would it be to get xerox copies of certain ones of them, or pertinent pages of them? Could we arrange to pay for such to be done in Calgary?

Best wishes to you. I do wish we could get together either here or at Calgary to discuss some of the peskier problems that face those of us in palaeopalynological taxonomy. Best regards also to your family and to my other friends in your outfit.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

Hunt Institute for Botanical Documentation

Dear [redacted]: here's the requested cards. I discontinue sending further updates because in ± 3 months we hope to have the whole file published.

Thank you for Amoco – Amico comment. I rephrased the typescript somewhat. The gist is: Fritz was no amicus of Amoco!

In haste, best regards,

Van Sonsen.

Hunt Institute for Botanical Documentation

xxxxxx

March 31, 1976

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue, S.E.
Calgary, Alberta, CANADA
T2G 2B3

Dear Jan:

Thanks for yours of 8-III-76. In the future, perhaps you'd like to keep xeroxes of your editorial corrections, additions, etc., to the CFSP proof copies, so there can be dialogue? Mailing them back is also possible, except we cut 'em up, mark and otherwise mutilate 'em.

Monotypic: Your statement seems perfectly logical. I was sure, however, I could prove you wrong--but the only definition I can find is in the Glossary (Regnum Vegetalile 56), and it seems to say we're both wrong--that "monotypic" is just not an appropriate term at the specific level (because infraspecific entities are not "principal ranks"). Of course, the Glossary has no standing, but as far as I can discover, the Code itself contains no definition and no use of monotypic outside the genus--so, where else to turn for authority? Except for the "principal rank" thing, it would seem that the glossary favors my interpretation.

Regarding your request for advertising of the cards--is there a *quid pro quo*?--I mean, did you have in mind a free set of cards for CFSP? Could there perhaps be some mutual back-scratching? We are in the process of getting CFSP out on microfiche--would be a good supplement to the cards--containing most of the critical species. Could we work together symbiotically? Looking forward to hearing from you.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb
cc: H. T. Ames

Hunt Institute for Botanical Documentation

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PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY
PRODUCTION DEPARTMENT

March 8, 1976

Prof. A. Traverse
Palynology
Deike Bldg.
University Park, Penn. 16802
U. S. A.

Dear Al,

I just received your epistle of 26 February. I forgot the detail of my remarks scribbled in the proof sheets to which you refer, and reading your letter was much like watching TV with the sound turned off. However, I understand that my efforts were of some use and I am happy to know; you didn't have to be so effusive!

A few points intrigue me and I would like to elaborate.

Monotypic. The type of a genus is a species, thus a genus with a sole species is monotypic (even if several subspecies described simultaneously). The type of a species is a specimen; thus if the original publication features only one specimen, it is monotypic (even if several subspecies simultaneously). "Type" is a nomenclatural term, distinct from "content". Since species are not typified by a subspecific taxon, I don't like your use of "monotypic" for only one subspecific taxon included in a species. In fact, it seems incorrect thinking, in the terms of the ICBN; however, since the world will turn anyway, I'll not belabour the point.

Ermitteln. I can't recall the sentence involved; I may have foolhardily depended on my fragmentary command of German. Checking Langenscheidt's now, I find as English equivalents: investigate, inquire; and (through that process): ascertain. Unfortunately, Dutch occasionally seems so close to German, and I have goofed more times than I care to remember.

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Mfd. I know not what you are referring to; if you really want to know, send me a xerox of p. 40:64.

Corrolaria. I am mystified as to why you don't have a copy of my m.s. cards, unless Al considered he had that one in the bag already, when he made xerox copies here. Anyway, I enclose a copy of it.

Size of type. I don't think Potonié used this in a formal convention. To me, however, in the Synopsis I through III the small size type indicates an "aside"; it is one way of communicating with the reader without making a "statement". However, I notice that from volume IV on, the small type is used (almost) exclusively to identify diagnoses of the various taxa. For the rest I don't remember what instance brought on this discussion.

In closing once again: my remarks in volume 40 were entirely for your consideration and reflection. I would not have been offended if you had ignored them all, for I disown all of Maljavkina's works as much as possible. Similarly, you don't have to justify your reactions. However, it was an interesting exercise, and from our discussions of certain points I always feel I learn.

For now best regards to Tate
and yourself,

Jan Jansonius

JJ/ap
Encl.

Al:
Would you be inclined to enclose a circular and order form for the Genera File with your next mailing or issue of the CFSP? We hope to go to press

Hunt Institute for Botanical Documentation

March 2, 1976

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue, S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

I seem bo be lacking a card for *Ericipites* Wodehouse 1933. Also, there is no card for *Dioonipites* Wodehouse 1933. The latter generic name was introduced by Wodehouse without providing any species. However, it was validly published. Do you know if anybody ever assigned species to it? The first one so assigned would presumably be the type. Thanks as usual.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

Hunt Institute for Botanical Documentation

February 27, 1976

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue, S.E.
Calgary, Alberta, CANADA
T2G 2B3

Dear Jan:

Thanks for your recent letter (undated). Very interesting indeed. You and we still have not reached a meeting of minds regarding the selection Malyavkina for coverage in the Catalog. It conveyed no judgement as to desirability of the lady's work. It, like Everest, is just there and must be covered (climbed?). I admit also that there was an element of extending "honor" to Malyavkina's work in order to please the Leningrad VNIGRI people. Based on my reception there last summer, I was wasting my time. That is too long of a story to cover here, but will be fun over a glass of something or other one of these years.

I greatly appreciated your evaluation of possible candidates for the editorial committee. There is no hurry about the matter. I am most interested in the nomination of McGregor, and perhaps that is the way we should go.

Thanks also for the sheets about *Amoeosporites* and *Amicosporites*. I have read and reread the business and still am not sure I completely understand what happened. It sounds really weird. Thanks again for your continuing help..Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

P.S. Your typist's problem with her knee arouses deep sympathy--you may remember that it is about a year right now that I had to have mine operated on. That particular part of the anatomy is a totally inadequate design.

Hunt Institute for Botanical Documentation

February 26, 1976

Dr. Jan Jansonius
Imperial Oil Ltd.
239 50th Ave, S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

It is usually my practice to answer the members of the editorial committee in detail, following revisions that result from the committee's sharp eyes. However, I have so many pages of comments on yours, that I think I will shorten it somewhat and hope that you will understand. Tate and I very much appreciate the obviously great amount of effort and time that you put into your editorial work an this volume. Some of the things that you found were also found by Chaloner. You two are certainly the most productive members of the committee. Some of the corrections that you suggested were a result of misunderstandings of our style, and the best thing I can do about them is to offer some expl planation here.

The "all-cap" business that we put at the bottom of a page which has to do with the type species or type description of a generic name varies considerably depending on circumstances--it is not supposed to be the same thing always. For example, if the type species is identifiable, and it is one of the species we treat, the bottom of the reference page will contain the information "type species of the generic name." In other cases, where there is no identifiable type species, we say "generic description," etc.

Where we say in our reference material "type horizon" this information has to do only with what applies to the type, if that is identifiable.

On p. 40:7, you picked us up on our use of the expression monotypic species. According to the Code, a monotypic genus is a genus with only one species, while a monotypic species is a species with only one infraspecific taxon.

On p. 40:9, we changed the wording at the bottom per your suggestion.

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On p. 40:15--and this same argument applies to other cases--*Spirellata* happens to be a sub-group with only <u>one</u> genus! Therefore <u>any</u> of the information applying to *Spirellata* also applies to its single genus *Spirellina*.

On p. 40:28, we have changed the basionym bit to make the subject more clear, but the basionym <u>is</u> "...*compacta*!" Your back-of-the-page comment confused me--you say *A. compacta* is the type species in the last line and deny it in the second line.

On p. 40:29, you misinterpreted us--the previous reference to which you refer is on 40:28 and had to do only with the subforms!

(I am going to be skipping a good many of the comments that you made from here on out to keep this letter under ten pages. However, I would like you to know we adopted nearly all of the corrections. You were particularly good at catching our superscript references, which were an quite a few cases incorrect!)

On p. 40:40, you are right that *Tabellina spinellata* f. *typica* should be the autonym. However, the heading to the description covers forma *minor* as well. We have changed the wording of the footnote per your suggestion.

On p. 40:44, I used the word "recognized" rather then "select" because the German is "ermitteln" which is, it seems to me, closer to "recognize" then to "select", even though the latter word would be better in English.

On p. 40:50, as I commented above, we used "type species of the generic name..." because we have that information available. Wherever possible we say "type species, etc." and the statement, whatever it is, <u>is always</u> at the bottom of the page.

On p. 40:51, this was an HTA translation that I didn't check. You are right, and we have changed the wording.

On p. 40:54, we were so glad that you picked up the misspelling of "warrant" and "recognized", but we do not agree with you on the use of the word "morphology" where we mean "form".

On p. 40:63, you were wrong about lectotypic designation. The generic lectotype <u>is</u> stated, but the specifid lectotype is <u>not</u>--it is only implied. The truth is, Potonié did not at that point understand typification!--He cited a <u>specimen</u> frequently for a generic type! You are right, however, about the confusing nature of footnote 2, and I have rewritten it to express the information in <u>tabular</u> fashion. After much thought and discussion with Ames, I decided that was the only way to go, and hope you will agree that the result is a great improvement. The stimulus for this we owe to you!

Hunt Institute for Botanical Documentation

On p. 40:64, I am sorry to report that we have no idea whatever what "Mdf." means.

On p. 40:66, you make the point that wherever we say such and such a generic name was validly published, we should add "in our opinion." It is my opinion that "in our opinion" it is quite clearly impiled in any such thing. On the same page you noted the misspelling in the fern subfamily, but it unfortunately turns out that even the "correct" spelling is wrong because the ending is reserved for tribes! Chaloner also picked this up and suggested a different spelling, also wrong, and we have despaired of doing anything better then putting it in quotation marks and keeping it the way Malyavkina had it.

On p. 40:78, we are so grateful that you caught the misspelling of the generic name. That would have been terrible! However, regarding the subgroup of Velata and its monotypy, the same comment is applicable here as in the case of *Spirellata* above.

On p. 40:130, you said for us to look at the card for *Corollaria*. This was impossible because I have no such card. Would you please send it? On p. 40:98, we rephrased footnote 3 per your suggestion.

On p. 40:106, we rewrote the whole 2nd half of footnote 1, which among other things misspelled "abridged"!

I am not commenting specifically on a couple dozen minor things that you picked up, but we are very grateful to have them because the result is to make the volume more nearly professional. I refer to such things as the word genera on 40:114.

On p. 40:131, and a number of other places you picked up, the Russian includes the information "possible name...", and I simply failed to pick that up on my first go through. Ames does not read Russian. Mine is better now then a year ago!

On p. 40:133, the question of alternate names comes up in connection with the generic name *Crassulina*. We are working under the assumption that the alternative name objection applies only to the species, and since genera were not required to have any species within them until 1958, the generic name would be O.K. We therefore do not accept the idea that Potonié validated *Crassulina* because it did not need to be validated.

On p. 40:136, you have raised a point which Schopf also raised, and we have introduced a new page in the Compiler's Preface to take care of it. The gist of this is that we do not intend to change the contents of Volume 40 with respect to the Leningrad Congress. It does not become applicable until its publication. For example, the 1969 Seattle meeting resulted in the 1972 Code!

On p. 40:150, you bring up a suggestion to reprint a picture from another volume. This is not our policy, and we give complete reference to the place at which it can be found.

Hunt Institute for Botanical Documentation

On p. 40:154, and some other places, you picked up a mis-translation, which I find very embarrassing. I just don't know how it happened, but in any case we now have it corrected. What the Russian says, of course, is "illustration of a not permanentày preserved object."

On p. 40:156, you raised an interesting point which we might discuss sometime--what standing does the size of type which Potonié uses in his synopsis volumes have? I really don't think that the size of type can be alleged to have any nomenclatural standing at all. Perhaps the author said that he was going to proceed that way, but I have been unable so far to find anything in the references, or whatever, that would substantiate that point.

On p. 40:162, you are quite right that the words you had circled are another version of the expression "possible name". Tate and I are of the opinion that the expression "possible name" is susceptible of interpretation as not meaning "alternate name" within the sense of the Code, but we recognize that others so interpret them.

On p. 40:163, you have a comment which requires a little explanation on our part. We do not change forma *typicas*tm the autonym at the varietal level unless there is a coördinate infraspecific taxon at the varietal level.

On p. 40:165, you have spared us a serious blooper, and I am intensively grateful--this is a case where we had f. *semistriatus* instead of f. *triassicus*.

On p. 40:220, the explanation of why we do not want to be in the position of validating names is that it has been the policy of the Catalog from the very beginning to avoid being a primary source. There is a chance that we have inadvertently validated certain names, but never intentionally. Jim Schopf feels that we should drop this policy and go ahead and validate some things when it seems to be pertinent, but we have so far been resisting the suggestion. We would like to think that we are not completely stiff necked, however, and if a good argument can be made we would consider changing the policy--not in Volume 40, however.

There are many other thánges that we have effectedpper your suggestions, but it is just impossible to list them all. Suffice it to say that we are very grateful and look forward to much similar coöperation from you in the future. Thank you so very much. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

Hunt Institute for Botanical Documentation

Feb. 76

Dear AT,

Our typist is home with a knocked knee (bumped by a car) so I'll answer in long-hand.

With Maljavkina: I think you did her too great an honor. When you sincerely try to follow her taxonomic reasoning, I think that she had no honest intention or not the faintest notion about what she was doing. It's usually preservation, or a different aspect that is described, but not overall structure or sculpture, or gross morphology. She was good in politics, for she impressed many people, including some of the best (thinking of you, now!). And her nomenclature was something else again.

Navmova is different, she was one of the original great innovators; if her work now seems lacking it is by chance of geographic location and historical disruption (WW II). I'll gladly help restore her legacy.

Candidates for our little group: I always trusted your judgement, especially since you invited me to join! But without kidding: not all that many people seem dedicated to nomenclature per se. I have been struck by the interest in these matters by the Playfords on several occasions; but who in that team is the conscientious one, I do not know (Colin McGregor

Hunt Institute for Botanical Documentation

might know). For that matter, Colin has a better than average interest in nomenclature himself.
Another man is Pirozynski; Kris now is paleomycologist in a position specially created for him in the Nat. Mus. of Nat. Sci., Paleontol. Div., 1767 Woodward Dr, Ottawa 4, Ont. K1A 0M8. He is knowledgable in fungal ~~nomenclature~~ more than palaeobotany, but has a great drive and enthusiasm.
Srivastava professes some interest in these matters, but I don't always see I to I with him, he seems too zealous.
If you're an equal opportunity employer, you might consider Carol Chmura; she struck me as competent.

Come to think of it, Colin McGregor might be a good man for this work.
~~A propos~~, I hope it was not I who doesn't respond in sufficient depth?

With best regards,

Ian Johnston

Hunt Institute for Botanical Documentation

February 9, 1976

Dr. Fan Jansonius
Imperial Oil Ltd.
339 50the Avenue, SW
Calgary, Alberta
T2G 2B3 Canada

Dear Jan:

Thanks for yours of 21 January. I appreciate the extra slips, and I did receive *Densosporites*, etc. I also have your corrected proofs from Volume 40 of CFSP, about which I think I have written previously. Tate and I are gradually working through your comments. We very much appreciate the detailed work that you did and only hope that you will not be angry with us for accepting only some of your comments! We were especially looking for errors of fact and interpretation, of which you have found a number. We are less inclined to accept corrections of our style, though sometimes we agree your version is better (question is--is it enough better to merit cutting and pasting?).

Now regarding the generic file, my guess is that you could sell 2 sets here--one to Penn State as an institution for $150 and the other to me personally for $50-60. There is a chance of other personal subscriptions. You will be astounded by the information about the active subscription for CFSP--it's a wonder that we can keep going. We have between 300 and 400. I think at the moment closer to 300 paid subscriptions. We do sell one (1) to the Soviet Union. It was obvious to me from my visit last summer that what they do is make multiple xeroxes and distribute them to their various labs. If very many people did that sort of thing we would be, for once and for all, out of business! It will happen to your cards too--probably including at PSU (I can't police the xerox machine all the time!) Best wishes to Stan and others.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

Hunt Institute for Botanical Documentation

January, 26, 1976

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue, S.E.
Calgary, Alberta, Canada
T2G 2B3

Dear Jan:

Just a quick note to let you know that we did get the MS materials for Volume 40 from you. By the way, in the future, it is our intention that you will return only the pages on which you have comments, in order to keep the cost of mailing down. Of course, it's perfectly all right to send the whole thing if you prefer. Maybe you don't want it cluttering up your shelf! Or even your wastepaper basket!

I agree with you wholeheartedly that the Malyavkina thing is a terrible mess, and both you and Chaloner have wondered out loud why we undertook it. The feeling I had was that, like it or not, the Malyavkina papers are in the literature, and if we have any intention to make the Catalog "complete", we simply must cover the dreaded things. Schopf, for one, agrees. I am now trying to convince Ames that we should afford the same luxury to Naumova (1937?-1938?-1938?). I haven't looked at your comments in detail; we will get to that soon, and you will hear from us. In themmeantime, thank you very much for your help. I knew that you would be a valuable member of the committee. Best wishes as always. (You don't really mean it about the blood pressure, do you?)

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb
cc: H.T. Ames

P.S. Whom else (preferably in N. America) would you nominate for the committee? I am allittle dissatisfied with one member of the present group--doesn't respond in sufficient depth.

Hunt Institute for Botanical Documentation

Dear Al,

in haste / 21 Jan 1976

Here's two more new genera; I trust you rec'd the missing slips which I xeroxed for you. I was happy with the comments: the more you question my procedure, the more we can make sure things are right. Did you also receive the proofed vol. 4o of the CFSP?

I'd like to ask you, and thru you any students/associates: If the genera file will cost $150.- for institutions (2 sets for 250.=), and $50.-60.- for individuals, what sales will we have in Penn State? We are deadlocked again in search for cash and I'd like to document my own sales projections. How many active subscriptions do you now have for the CFSP? (if that is not confidential). Do you sell any to USSR?

Best wishes + regards,

Hunt Institute for Botanical Documentation

January 15, 1976

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue, S.E.
Calgary, Alberta
T2G 2B3 Canada

Dear Jan:

Ames and I have just been discussing your treatment of *Ephedripites* and *Welwitschiapites*. We are unable to understand why you accept *Welwitchiapites* as validly published in Bolkhivotina, 1953, while *Ephedripites* you consider as validated by Potonié in 1958. It seems to us that what is sauce for the goose is sauce for the gander, and that *Welwitschiapites* probably should be regarded as validly published also in Potonié, 1958, if indeed somebody else (such as Bolkhovitina herself) did not validate the thing in the interim! Best wishes and looking forward to hearing from you.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

P.S. By the way, is there any reason why I should have no slip for *Densosporites?* In fact, I have no slips at all between *Denexinis* *Diaphaniaphanitesporites?*

Hunt Institute for Botanical Documentation

December 12, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue, S.E.
Calgary, Alberta, Canada
T2G 2B3

Dear Jan:

Thanks for yours of 14 November, which arrived by American mail somehow! I wonder if you'll ever get my response. Regarding *Ascodesmicites*--this would appear to be precisely the sort of case that I corresponded with Stafleu about earlier. He stated at that time that the generic name would be validly published regardless of the fact that the type species was not validly published. I am taking the liberty of sending a xerox copy of this letter to Stafleu posing the question, and will let you know how it comes out. I'm also raising a couple other issues at the same time! Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

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PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY

PRODUCTION DEPARTMENT

November 14, 1975

Dr. A. Traverse
Dept. of Geology, Palynology Laboratory
Pennsylvania State University
University Park, Penn.
U. S. A. 16802

Dear Al:

Re our earlier discussions, about a genus being valid even if its designated type species is not: here is a stinker!

Ascodesmisites Trivedi et al. 1973 (Geophytology, v.3, p. 129) is a fossil fungus, effectively and validly proposed by descriptio generico - specifica; however, inasmuch as no holotype was designated, the type species is not validly published. Does or does this not invalidate the generic name? I think in the new phrasing of Art. 42, Stafleu may want to consider this, possibly as an example?

Too bad we had no time to visit more in Houston.

Best regards,

Jan Jansonius

JJ/er

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PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY

PRODUCTION DEPARTMENT

October 10, 1975

Dr. A. Traverse
Dept. of Geology, Palynology Laboratory
Pennsylvania State University
University Park, Penn.
U. S. A. 16802

Dear Al:

You are a scholar, so please tell me which is gramatically (as required by the ICBN) correct:

Rhombodinium draco (Gocht) Vozzhenikova 1967 subsp. freienwaldensis

or

Rhombodinium draco (Gocht) Vozzhenikova 1967 subsp. freienwaldense

which was originally proposed as:

Rhombodinium draco (Gocht) Vozzhenikova 1967 forma freienwaldensis

(examples from J.K. Lentin & G.L. Williams 1973, GSC Pap. 73-42, p. 120)
In other words: does the adjectival epithet have to agree with the noun "forma", or with the noun "Rhombodinium"? One would think the latter, as that seems to come natural when the forma is elevated to rank of subspecies (or possibly even to that of species).

I think the Code could profitably have this example as Example in Art. 23, or eventually Art. 73, especially because many authors do not realize that adjectives derived from a place name (____ ensis) also have to be made neuter (____ ense) if the generic name is of neuter gender. (I now notice, under Art. 26, a Lycopodium inundatum L. var. inundatum, which answers my first question. However, I also notice Ceratium cf. fusus forma incerta Deflandre 1936, and he was no fool?)

However, I notice another puzzle in this publication:
"Wetzeliella (Rhombodinium) glabra (Cookson) var. granulata Wilson 1967"
In cases like this, does the specific epithet reflect the gender of the generic name, or of the subgeneric name? (=Rhombodinium glabrum (Cookson) Vozzh.).

In this vein, I notice a name "Spiniferites reginaldi"; this makes me wonder about the latinization of Reginald: should it be Reginaldus or Reginaldius? I would say that for given names the first case would be correct (Paulus, Janus, Arnoldus, etc.), in which case the correct genitive should be Reginaldi, not Reginaldii. However, how do we know if Reginald is a given name or a surname?

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With regard to Art. 37: If a new genus is diagnosed, and several species assigned, among which a type species for which no holotype is indicated among the figured specimens, is the genus still validly proposed (but the type species not)? In other words, should we read Art. 37 like:

". . . is valid only when a valid nomenclatural type is indicated . . .",

or do we not reverse Art. 43 (where a species is not validly published if not assigned to a valid genus) and accept a genus still as validly published even if its type species was not? And even if this type species is the only one assigned?

Best regards,

Jan Jansonius

JJ/er

14 Oct.

With a mail-strike threatening for later this week – in haste:

Just received your report on Leningrad; Sorry I could not be there.

I hope you will transmit those motions in this leter worth the effort to Stafleu for his perusal.

I will probably not attend ~~AAAS~~/Houston.

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October 24, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue, S.E.
Calgary, Alberta, Canada
T2G 2B3

Dear Jan:

Thanks for yours of 10 October which set a new record by taking 10 days to get here. The "you are a scholar" set my teeth on edge. You really know how to get your knife in, don't you?! Still, there is that possibility that you really mean it....

Let's start with your queries from the top. Infraspecific epithets agree in gender with the generic name. This is made quite clear in the first paragraph of Art. 24. That, unfortunately, does not really answer your question because I must know the gender of *Rhombodinium*. I presume it is neuter. Referring to my trusty copy of "Botanical Latin," by W. T. Stearn, page 94, shows me that the correct adjectival ending, assuming that *Rhombodinim* is neuter, would be the alternative you offer ending in the letter "e".

Regarding the example of *Ceratium* which you bring forth, you will notice that there is a "cf." involved. When that gets in the picture, everything hits the fan because it means that the names following it are not being formally attributed to the generic name in question but are being compared. There is thus no reason to change their form from what they were in the publication from which the compared form was taken.

Next question: Art. 11 makes it perfectly clear that a subgeneric name must agree in gender with the generic name to which it is referred. I haven't looked into this exhaustively, but I presume this means that in the case which you set, *Wetzeliella* (*Rhombodinium*), the name should have been changed in form when made a subgeneric name under *Wetzeliella* (*Rhombodinia*). As an alternative, though I feel less sure of my ground

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here, it could be kept in the same form with the understanding it will be treated as feminine. I can think of a precedent for that procedure: the names of trees, such as Quercus, which are masculine in form, are nevertheless treated as feminine.

Next question: this one is treated in Art. 73, Recommendation 73 C (b). The correct latinization of Reginald when in occurs as a specific name should be reginaldii. In the Catalog we have the practice of "correcting" all of the names of that sort which use only a single "i". It doesn't really matter whether Reginald is given name or surname. (You wanted to name one alfredii, perhaps?)

Next question--with reference to Art. 37. Ames and I have run this one past Stafleu, and I know what he says, but it is admittedly a "sticky wicket". Stafleu's position is that a taxon is validly published if the type was clearly stated and is determinable, even if the type itself was not validly published. This comes up, for example, in cases where a new combination is the type species of a newly published generic name, if the new combination is not validly published. By extension from that he said I would feel that a genus is validly published if the type species is clearly stated, even if itself not validly published. The situation is, of course, irregular, and somebody should add the additional matter required to complete the valid publication of the type species. However, when this is done, it would seem to me that correct procedure would be to cite the generic name as published by the original author, even if the type species then had to be cited as validated by someone else.

I am getting a little tired of referring things to Stafleu because I think he has "blown" several recent ones I sent him. I think I am going to find a different mastermind. The truth is that we should try to get the wording in the Code straightened up to the point where it is not necessary to have an extralegal counsellor to help one with these things.

Sorry that you won't be in Houston, I would have enjoyed seeing you. Betty is going to accompany me, and it looks like a good and profitable trip. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

cc: H. T. Ames

Hunt Institute for Botanical Documentation

October 10, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue, S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

Thanks for yours of 11 Sept.--came while I was in North Dakota with a grad student. Thanks also for the replacement *Calamospora* page.

O.K. on the *Toratisporites*, etc. question and thanks for the nice compliment. When I finally got around to it, I did work hard for a while on the problem. Re Tate's contribution--he did make a couple of good suggestions, but it seems to me that the real labor was invested by yours truly. I think that mention in the acknowledgements is in keeping with the facts--indeed, until the last red pencil and past-up job I did, it would have been all I deserved too.

Re your proposals' fate--you will have heard about your success on Art. 42 in my official report by now. Your proposal (#118) on Art. 3 was defeated, but we eliminated organ-genera entirely! Your proposal (#42) on Artical 73 was defeated, but Nicolson's proposal (#175) which accomplishes the same business was passed! So, you really batted 1.000!

Regarding Stafleu and Art. 33, he now has me thoroughly confused. I recently sent him some examples of non-explicit reference to the basionym, and he said they were o.k. Now we don't know where we're at. However, the wording of the article is clear enough to me---if it meant that a figure and/or page number was sufficient, why would it also require that!?

Enclosed, zerox copy of letter from Bharadwaj, which corroborates your finding!

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

Enclosure: Zerox letter

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PRODUCTION RESEARCH AND
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PRODUCTION DEPARTMENT

September 11, 1975

Dr. A. Traverse
529 Deike Building
University Park, Penn.
U. S. A. 16802

Dear Al:

Thank you for your letters; you tantalize me with suggesting that some of my proposals (which ones?) were accepted in Russia.

Enclosed is an entry for Calamospora; it seems strangely simple, but apparently nobody ever critically reviewed the genus.

Regarding the unfortunate confusion around Classopollis: as long as you know about it, you can take steps to make corrections. I have fallen in enough traps not to be critical.

Congratulations on the fine job on Toratisporites; nearly all your red remarks hit the bull's eye. Some of them I wished I had thought of myself. However, I beg to differ with regard to the Art. 42 argument (adduced by Playford & Dettmann): it is ambiguous because Art. 42 deals with new monotypic genera based on a new species, and Corrugatisporites, based on S. solidus, does not fall in that class. I think we don't have to spell out our reasons, and best leave that bit as it was.

I'll gladly accept Tate in the acknowledgements; if you think his contribution warrants it, we could have him head off the list of author names? Any suggestions for a suitable journal?

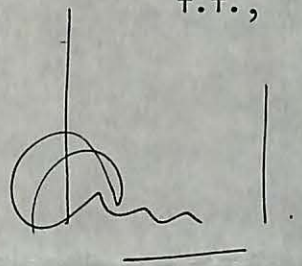
I cannot take leave without imploring you to get on to Stafleu and make him take the word "indicate" out of the Code: if it means "give", or "state", or "spell out", one of those unequivocal terms should be used! My dictionary seems to suggest ("indicate") that "indicate" means "pointing out with more or less exactness". Thus, who needs the basionym in full, if the figure number is given? I could argue this forever, without ever the certainty that sometime you must see things my way. That is because of the ambiguity introduced by the very word "indicate". Is it because at one time there was an argument as to how exacting we should be, and the word "indicate" was chosen as a compromis? But then, we cannot later change the meaning of that word exclusively to be "spell out". I have the feeling that I am being had.

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Thank you, finally, for the xerox copy of Bhardwaj's paper. Some new genera in it, I notice. However: what is the date,* and the precise bibliographic reference,* for the new taxa? I would much appreciate to find out as soon as possible.

Best regards to all, and happy to know you're safely back from Russia,

t.t.,



Jan Jansonius

JJ/er

Enclosure.

*1974, Birbal Sahni Inst. Palaeobot., Spec. Publ. No. 4, p. 7-52 (courtesy Gerhard Kremp).

Hunt Institute for Botanical Documentation

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College of Earth and Mineral Sciences
Department of Geosciences
Palynological Laboratories

Area Code 814
865-6543
865-2342

19 August, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. S. E.
Calgary, Alberta
Canada

Dear Jan:

Have (finally?) finished my work on the nomenclatural note. In my present secretary-less state, the easiest way to handle this seemed to be to cut the paper up and make a paste-up, which is enclosed. The suggested corrections are in red, my comments thereon in black india ink. It seems to me, that with the corrections suggested, the paper is in pretty good shape. However, there remains a sizable possibility that you won't like what I did. The big thing, though, is that I am convinced that this sort of note definitely needs to be published.

You will note that I suggest adding Tate's name to the list of acknowledgments. He made several useful suggestions, including the one that Toratisporites be styled "nomen novum".

Looking forward to hearing from you and with best wishes, I am

Yours very truly,

Alfred Traverse

Alfred Traverse
Professor of Palynology

cc: H. T. Ames

P. S. Zeiss has just told me that he proposes to do only a paper (and hence an M. S.) on the dinos from Arctic and switch over for the Ph. D.. to Spackman, where the pastures are greener. The materials from Stan are not well preserved, and I have been having a hell of a time finding $$ for Harvey. Lack of frequent communication from Calgary hasn't helped.

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Toralisporites n. gen. and its relationship to
Corrugatisporites and *Lygodioisporites*

by J. Jansonius*, S. A. J. Pocock* & A. Traverse**

Abstract - *Lygodicisporites* Potonié 1951 ex Delcourt & Sprumont 1955 is an obligate junior synonym of *Corrugatisporites* Thomson & Pflug 1953, because both genera have the same type species. *Corrugatisporites* is maintained, with the type species *C. solidus* (Potonié) Thomson & Pflug. *Toralisporites* is proposed as a new genus for the repudiated concept of *Corrugatisporites* sensu Potonié & Kremp 1955, and Potonié 1956.

Comment:

Thomson & Pflug did not make the combination C. solidus, because they failed to state the basionym — giving the bibliographic citation was not sufficient because Art. 33 demands the the basionym be "indicated" and a bibliographic reference provided. Obviously, then, "indicated" must mean "expressly stated".

Therefore C. solidus (Potonié) nov. comb. is probably new in this paper, although in the correct place I am also suggesting a "conditional" baptism statement about that.

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Introduction

In spore/pollen systematics - as in other fields of botany - the Rules of the International Code of Botanical Nomenclature must be strictly applied, especially in cases where an original error in procedure was corrected without regard to the Code. A case in point is the name *Corrugatisporites*, the history of which is traced below; this case is more complicated than most, because changes in the Code have invalidated some practices that were tolerated by earlier Codes then in force.

Potonié and Kremp (1955, p. 96) state that

'*Corrugatisporites*' was a manuscript name of Potonié & Kremp (Potonié & Kremp, 1955, p. 96), and probably in common conversational usage among German palynologists, some of whom did not know the origin of the name. *Corrugatisporites* first occurs in print in Thomson & Pflug, 1953 (p. 55), who ascribed the genus to Ibrahim, 1933. Although

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** Penn. State University, University Park, Pennsylvania 16802, U.S.A.

We thank Dr. F. L. Staplin and Dr. W. A. M. Jenkins for their critical reading of the manuscript. Permission from Imperial Oil Ltd. to publish this contribution is acknowledged with appreciation.

Comment: First changes were to improve (in my opinion) the language.'

Comma after Ibrahim is because the comma should only be omitted when the author's name is part of a name citation.

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"Corrugatisporites solidus Potonié

this was mistaken, Thomson & Pflug did provide a diagnosis and assigned the species ~~*Corrugatisporites solidus*~~ (Potonié) Thomson & Pflug and two new subspecies to the genus. Correct citation of authorship is *Corru-*

for the generic name

(Unfortunately, the new combination was not validly published because Thomson and Pflug did not explicitly state the basionym, Sporites solidus Potonié 1934, as required by 1972 ICBN Art. 33. However, this does not affect the status of the generic name, as the type is obvious, even though its new combination was not validly published.)

gatisporites Thomson & Pflug 1953 (non Ibrahim 1933). Before 1958,

. . . . .

Not validated by some author, published by Ibrahim, 1933.

Comment:

The use of "non" in the Code is intended for quite a different situation (check Rec. 50C)

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designation of a type species was not required for a genus to be validly published, providing all other requirements had been met. ~~Thomson 6~~

nor was it necessary for a genus to include any species. Thomson 6

Pflug did not designate a type species; however, it being the only species assigned to the genus, *C. solidus* must be accepted as the type species (1972 Code, Guide for the determination of types, sub 1, referring to Article 10).

Comment:

The original version was in error in citation of *C. solidus* and made the determination of type too complicated — if there is only one species involved, it is the type, period.

In 1951, Potonié (p. 144) used the new combinations "*Lygodioisporites solidus* (Potonié) and "*L. adriennis* (Potonié & Gelletich), illustrating the two species with new photographs of the holotypes. The genus *Lygodioisporites* was not explicitly proposed or diagnosed, but Article PB 3 (1952 Code), did not require a diagnosis for genera of fossil plants published before 1953.) In 1955, Potonié & Kremp (p. 96)

Comments: I suggest citing the names in quotes, just as given by R. Pot. They were not validly published because the generic name was not validly published.

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" (R.P.T.) "

indicated that *Corrugatisporites solidus* (Potonié) Thomson & Pflug should be considered as the type species of *Lygodioisporites* Potonié 1951. Under the retroactive requirements of the latest edition of the Code, this procedure makes *Lygodioisporites* a junior synonym of *Corrugatisporites*. Later in 1955, Delcourt & Sprumont (p. 33) provided a

Hence, *Lygodioisporites* was not validly published & and therefore designation of a type for it was not effective.

..

Comment: I am against much talk about previous versions of Code and latest version — it unnecessarily confusing. And all taxonomists know that the Code is always retroactive unless (as is often done) it is made expressly otherwise.

diagnosis for *Lygodioisporites* and, also, designated *Lygodioisporites solidus* (Potonié) Delcourt & Sprumont as its type species; under the present Code, they are considered as the validating authors, but

Lygodioisporites is a junior nomenclatural synonym of *Corrugatisporites*.

Comment: ATA would prefer "illegitimate [redacted] synonym" in cases such as this, but I feel "junior" is helpful (it is right that it is redundant!) and that "junior nomenclatural" means more to the *hoi polloi* than does "illegitimate".

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In 1955, Potonié & Kremp (l.c.) argued that, with *Corrugatisporites solidus* removed from the genus and assigned to *Lygodiosporites* as type species, the genus *Corrugatisporites* was virtually redundant, ~~"empty"~~, except that Weyland and Greifeld (1953:42)

In 1953, however, Weyland & Greifeld (p. 42) had ~~already~~ assigned two more species to *Corrugatisporites*, of which they ~~also~~ cited Ibrahim as author. Therefore, in order to give *Corrugatisporites* a 'new' content, Potonié & Kremp (1955, l.c.) designated one of these, *Corrugatisporites torulus* Weyland & Greifeld, as ~~-~~neotype for *Corrugatisporites*; they also gave (ibid.) an informal differential diagnosis (the genus "is close to the Paleozoic genus *Carptotriletes* Naumova, but differs clearly, a.o. in the more regular ~~distribution~~ of the cristae. The cristae of *Lygodiosporites* are quite different."). This "new sense" of *Corrugatisporites* was formalized by a new diagnosis in Potonié 1956 (Synopsis I, p. 41).

Comments:

Check Pot. & Kr. — I think you'd agree that they say it would be empty except for the Weyl. & Gr. spp.

Next changes are for "tighter" wording

"Neotype" is probably not a happy choice here — check Code's definition; admittedly difficult — why not sidestep?

a.o. is not a regular Engl. abbreviation — I think "v.a." is not renderable to an " in common usage.

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The two genera *Corrugatisporites* and *Lygodioisporites* have been widely used in the sense defined in Potonié's Synopsis. In 1965, Playford & Dettmann (p. 140) concluded that *Lygodioisporites* is a junior nomenclatural synonym of *Corrugatisporites* Thomson & Pflug s. str. (non sensu Potonié & Kremp, 1955); they cited Article 42 of the Code which is, however, somewhat ambiguous in this case. ~~Playford & Dettmann consider Corru-~~

because *Corrugatisporites* had no validly published species in Thomson & Pflug, 1953, and was not therefore strictly speaking, monotypic. Playford & Dettmann consider Corr-

Comments: 1. See previous explanation on "junior nomenclatural..."

2. This seems to me to be the real reason Art 42 is "ambiguous in this case" and should be spelled out.

gatisporites sensu Potonié & Kremp to be a synonym of *Duplexisporites* Deák emend. Playford & Dettmann. [9] Strict application of the present (1972) edition of the Code, which differs significantly from earlier editions with the special provisions for fossil plants, dictates that *Lygodioisporites* is an obligate junior synonym of *Corrugatisporites*. The latter is to be understood in the sense of the circumscription of *Lygodioisporites* given by Delcourt & Sprumont 1955, and Potonié 1956. [9] The concept of Potonié & Kremp 1955, and Potonié 1956, later named *Corrugatisporites* and typified by *C. toratus*, is of a spore requiring a new name (*Toratisporites*); we consider it undesirable to merge

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[illegible] with that of *Duplexisporites*, which appears to be a subcircular spore with concentric ridges on the distal face, and an equatorial cingulum. *Toratisporites*, by contrast, is triangular with ridges (cristae) formed by the confluence of verrucae and aligned parallel to the sides of the triangular amb. We therefore propose the new generic name *Toratisporites* for the concept in recent literature generally referred to as *Corrugatisporites*, as typified by *C. toratus*. The name *Corrugatisporites* is maintained, but its circumscription is changed to agree more closely with the morphology of its type species, *C. solidus*.

Systematics

Toratisporites ~~n. gen.~~ nomen novum

Type species: *Toratisporites toratus* (Weyl. & Greif.) n. comb.

Basionym: *Corrugatisporites toratus* Weyland & Greifeld 1953, p.42, pl. 11, fig. 57

Comments: 1. If interpreted as a new genus, it could be argued that *T.* is in trouble because the type is type for and the generic name — just a technicality, but "nomen novum" is less debatable.

2. Suggest "clear and direct statement" of what new name replaces.

3. Suggest basionym be so labelled.

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Diagnosis: Trilete microspores; amb triangular with rounded corners to rounded triangular; sides more or less sinuous; proximal face smooth, trilete rays usually extending to equator, distinct, accompanied by more or less distinct labra; distal face and equator furnished with sculpture of heavy, usually discontinuous ridges that are uneven along their crest and composed of more or less completely confluent verrucae; ridges usually paralleling one or all sides of the triangular amb, occasionally anastomosing.
In *Duplexisporites* the cingulum is not formed by the fusion of verrucae, and the distal ornamentation consists of concentric rings.

Comment: I'd rather call this the "Descriptor", but won't fight about it.

Toratisporites toratus (Weyl. & Greif.) n. comb.

Corrugatisporites toratus Weyland & Greifeld, 1953, p.42, pl. 11, fig. 56 - 59 (lectotype fig. 57, selected by Potonié & Kremp, 1955 p.96).

Description (after Weyland & Greifeld):

40 - 55 microns; outline rounded - triangular to subcircular, sides convex, wavy to warty. The faces similarly covered with broad, warty ridges and isolated warty elements. Dehiscence ridges extend almost to the equator.

L. Senonian; Quedlinburg, Germany.

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[C]atisporites anagrammensis (Kara-Murza) n. comb.

Captotriletes anagrammensis Kara-Murza, in Bolkhovitina 1956, p. 57, pl. 6, fig. 88.

Cicatricosisporites dunrobinensis Couper, 1958, p. 137, pl. 17, figs. 13-15.

Corrugatisporites anagrammensis (Kara-Murza) Pocock, 1970, p. 60, pl. 11, figs. 11-13.

Description:

Exine two layered. Commissures through nexine simple straight slits extending its full radius. On sexine they are flanked by raised lips extending about 2.5 microns on each side at the proximal pole and thinning slightly towards the equator. Amb triangular; sides convex and equatorial angles fairly sharp. Nexine thin (about 0.5 micron) and tightly appressed to sexine but with occasional rounded air spaces developed between the two layers. Proximal sexine smooth. Distal sexine ornamented with oblique, round-topped ribs, 3-7 microns wide and about 2 microns high. Up to six ribs are developed parallel to one equatorial face, enclosed by one or two ribs developed parallel to the other two faces. The outer rib may form a continuous band of equatorial thickening. Distal face strongly convex. Proximal face somewhat flattened. Color amber. Equatorial diameter 26(38)51 microns. Length of commissures 15-20 microns.

Corrugatisporites Thomson & Pflug 1953 (non Ibrahim) s. str. *

Synonym: *Lygodioisporites* Potonié 1951 ex Delcourt & Sprumont 1955 nov. comb.

Type species: *Corrugatisporites solidus* (Potonié) Thomson & Pflug, ibid. * *
(by monotypy).

basionym: *Sporites solidus* Potonié 1934, Arb. Inst. Palaeobot., Bd. 4, p. 42, pl. 1, fig. 35.

* Comments:

* The 'non ... sm.' bit is inappropriate. I have previously explained Ibrahim. 's. str.' suggests a s. lato — what would it be? — I would avoid issue.

* * previously explained — maybe somebody else prev. published the name, but it is definitely not "C. solidus (Pot.) Thom. & Pflug."

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Diagnosis: Trilete microspores; amb triangular with rounded corners, outline more or less uneven to warty; proximal exine usually smooth; trilete rays long, usually extending to equator, with more or less distinct labra; exine, especially distally, ornamented with irregularly distributed verrucae, grana or tubercles, in part fused into short ridges, that are not aligned parallel to the equator and the Y mark (as in *Toratisporites*), but may be disposed more or less radially or irregularly. Warts more discrete than in *Camptotriletes*.

Corrugatisporites solidus (Potonié) Thomson & Pflug, 1953 [unless previously validly published]

Comment: See previous comments. It would, of course, be nice to know if somebody else has formally published this!

basionym: *Sporites solidus* Potonié, 1934; p. 42, pl. 1, fig. 35

Description (after Potonié):

"In polar view triangular with strongly rounded corners; outline rather strongly undulate (arches as wide as 3 microns) to verrucate; verrucae with rounded tops, irregularly shaped in top view; the character of this irregular delineation indicates that the now verrucate exine surface is derived from a reticulate pattern. Proximal side rather smooth, but on either side of the trilete rays a row of small verrucae can be discerned." Holotype 36 microns.

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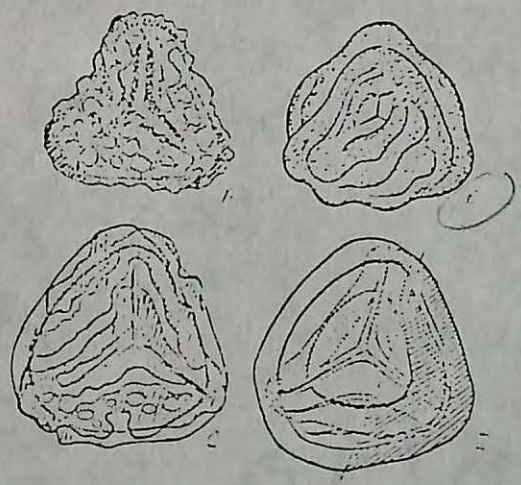
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WEYLAND, H. & GREIFELD, G., 1953, Ueber strukturbietende Blätter und pflanzliche Mikrofossilien aus den untersenonen Tonen der Gegend von Quedlinburg. Palaeontographica, Abt. B, Bd. 95, p. 30-52, pl. 6-13.



Textfigure 1. Line drawings after the holotypes of the following species:

A*- Corrugatisporites solidus ; B- Toratisporites anagrammensis ; C*- Toratisporites toratus ; D*. Jupiterisporites generalis. (* marks type species of those genera)

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Toratisporites n. gen. and its relationship to *Corrugatisporites* and *Lygodioisporites*

by J. Jansonius*, S. A. J. Pocock* & A. Traverse**

Abstract - *Lygodioisporites* Potonié 1951 ex Delcourt & Sprumont 1955 is an obligate junior synonym of *Corrugatisporites* Thomson & Pflug 1953, because both genera have the same type species. *Corrugatisporites* is maintained, with the type species *C. solidus* (Potonié) Thomson & Pflug. *Toratisporites* is proposed as a new genus for the repudiated concept of *Corrugatisporites* sensu Potonié & Kremp 1955, and Potonié 1956.

Introduction

In spore/pollen systematics - as in other fields of botany - the Rules of the International Code of Botanical Nomenclature must be strictly applied, especially in cases where an original error in procedure was corrected without regard to the Code. A case in point is the name *Corrugatisporites*, the history of which is traced below; this case is more complicated than most, because changes in the Code have invalidated some practices that were tolerated by earlier Codes then in force.

'*Corrugatisporites*' was a manuscript name of Potonié & Kremp (Potonié & Kremp, 1955, p. 96), and probably in common conversational usage among German palynologists, some of whom did not know the origin of the name. *Corrugatisporites* first occurs in print in Thomson & Pflug, 1953 (p. 55), who ascribed the genus to Ibrahim 1933. Although

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We thank Dr. F. L. Staplin and Dr. W. A. M. Jenkins for their critical reading of the manuscript. Permission from Imperial Oil Ltd. to publish this contribution is acknowledged with appreciation.

this was mistaken, Thomson & Pflug did provide a diagnosis and assigned the species *Corrugatisporites solidus* (Potonié) Thomson & Pflug and two new subspecies to the genus. Correct citation of authorship is *Corrugatisporites* Thomson & Pflug 1953 (non Ibrahim 1933). Before 1958, designation of a type species was not required for a genus to be validly published, providing all other requirements had been met. Thomson & Pflug did not designate a type species; however, being the only species assigned to the genus, *C. solidus* must be accepted as the type species (1972 Code, Guide for the determination of types, sub 1, referring to Article 10).

In 1951, Potonié (p. 144) used the new combinations "*Lygodioisporites solidus* (Potonié)" and "*L. adriennis* (Potonié & Gelletich)", illustrating the two species with new photographs of the holotypes. The genus *Lygodioisporites* was not explicitly proposed or diagnosed, but Article PB 3 [1952 Code] did not require a diagnosis for genera of fossil plants published before 1953. In 1955, Potonié & Kremp (p. 96) indicated that "*Corrugatisporites solidus* (Potonié) Thomson & Pflug should be considered as the type species of *Lygodioisporites* Potonié 1951. Under the retroactive requirements of the latest edition of the Code, this procedure makes *Lygodioisporites* a junior synonym of *Corrugatisporites*. Later in 1955, Delcourt & Sprumont (p. 33) provided a diagnosis for *Lygodioisporites* and, also, designated *Lygodioisporites solidus* (Potonié) Delcourt & Sprumont as its type species; under the present Code, they are considered as the validating authors.

In 1955, Potonié & Kremp (l.c.) argued that, with *Corrugatisporites solidus* removed from the genus and assigned to *Lygodioisporites* as type species, the genus *Corrugatisporites* was virtually redundant.

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In 1953, however, Weyland & Greifeld (p. 42) had already assigned two more species to *Corrugatisporites*, of which they also cite Ibrahim as author. Therefore, in order to give *Corrugatisporites* a 'new' content, Potonié & Kremp (1955, l.c.) designated one of these, *Corrugatisporites toratus* Weyland & Greifeld, as neotype for *Corrugatisporites*; they also gave (ibid.) an informal differential diagnosis (the genus "is close to the Paleozoic genus *Camptotriletes* Naumova, but differs clearly, a.o. in the more regular distribution of the cristae. The cristae of *Lygodioisporites* are quite different."). This "new" sense of *Corrugatisporites* was formalized by a new diagnosis in Potonié 1956 (Synopsis I, p. 41).

The two genera *Corrugatisporites* and *Lygodioisporites* have been widely used in the sense defined in Potonié's Synopsis. In 1965, Playford & Dettmann (p. 140) concluded that *Lygodioisporites* is a junior synonym of *Corrugatisporites* Thomson & Pflug s. str. (non sensu Potonié & Kremp, 1955); they cited Article 42 of the Code which is, however, somewhat ambiguous in this case. Playford & Dettmann consider *Corrugatisporites* sensu Potonié & Kremp to be a synonym of *Duplexisporites* Deák emend. Playford & Dettmann. ¶ Strict application of the present (1972) edition of the Code, which differs significantly from earlier editions with the special provisions for fossil plants, dictates that *Lygodioisporites* is an obligate junior synonym of *Corrugatisporites*. The latter is to be understood in the sense of the circumscription of *Lygodioisporites* given by Delcourt & Sprumont 1955, and Potonié 1956. The concept of Potonié & Kremp 1955, and Potonié 1956, later named *Corrugatisporites* and typified by *C. toratus*, is of a spore requiring a new name (*Toratisporites*); we consider it undesirable to merge

that concept with that of *Duplexisporites*, which appears to be a subcircular spore with concentric ridges on the distal face, and an equatorial cingulum. *Toratisporites*, by contrast, is triangular with ridges (cristae) formed by the confluence of verrucae and aligned parallel to the sides of the triangular amb. We therefore propose the new generic name *Toratisporites* for the concept in recent literature generally referred to as *Corrugatisporites*, as typified by *C. toratus*. The name *Corrugatisporites* is maintained, but its circumscription is changed to agree more closely with the morphology of its type species, *C. solidus*.

Systematics

Toratisporites n. gen.

Type species: *Toratisporites toratus* (Weyl. & Greif.) n. comb.

Basionym: *Corrugatisporites toratus* Weyland & Greifeld 1953, p.42, pl. 11, fig. 57

Diagnosis: Trilete microspores; amb triangular with rounded corners to rounded triangular; sides more or less sinuous; proximal face smooth, trilete rays usually extending to equator, distinct, accompanied by more or less distinct labra; distal face and equator furnished with sculpture of heavy, usually discontinuous ridges that are uneven along their crest and composed of more or less completely confluent verrucae; ridges usually paralleling one or all sides of the triangular amb, occasionally anastomosing.

In *Duplexisporites* the cingulum is not formed by the fusion of verrucae, and the distal ornamentation consists of concentric rings.

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Description (after Weyland & Greifeld):

40 - 55 microns; outline rounded - triangular to subcircular, sides convex, wavy to warty. The faces similarly covered with broad, warty ridges and isolated warty elements. Dehiscence ridges extend almost to the equator.

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Exine two layered. Commissures through nexine simple straight slits extending its full radius. On sexine they are flanked by raised lips extending about 2.5 microns on each side at the proximal pole and thinning slightly towards the equator. Amb triangular; sides convex and equatorial angles fairly sharp. Nexine thin (about 0.5 micron) and tightly appressed to sexine but with occasional rounded air spaces developed between the two layers. Proximal sexine smooth. Distal sexine ornamented with oblique, round-topped ribs, 3-7 microns wide and about 2 microns high. Up to six ribs are developed parallel to one equatorial face, enclosed by one or two ribs developed parallel to the other two faces. The outer rib may form a continuous band of equatorial thickening. Distal face strongly convex. Proximal face somewhat flattened. Color amber. Equatorial diameter 26(38)51 microns. Length of commissures 15-20 microns.

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Corrugatisporites Thomson & Pflug 1953 (non Ibrahim) s. str.

Synonym: *Lygodioisporites* Potonié 1951 ex Delcourt & Sprumont 1955

Type species: *Corrugatisporites solidus* (Potonié) Thomson & Pflug, ibid. (by monotypy).

Sporites solidus Potonié 1934, Arb. Inst. Palaeobot., Bd. 4, p. 42, pl. 1, fig. 35.

Diagnosis: Trilete microspores; amb triangular with rounded corners, outline more or less uneven to warty; proximal exine usually smooth; trilete rays long, usually extending to equator, with more or less distinct labra; exine, especially distally, ornamented with irregularly distributed verrucae, grana or tubercles, in part fused into short ridges, that are not aligned parallel to the equator and the Y mark (as in *Toratisporites*), but may be disposed more or less radially or irregularly. Warts more discrete than in *Camptotriletes*.

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Sporites solidus Potonié, 1934; p. 42, pl. 1, fig. 35

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trilete rays a row of small verrucae can be discerned."

Holotype 36 microns.

Acknowledgment

H. T. Ames

References

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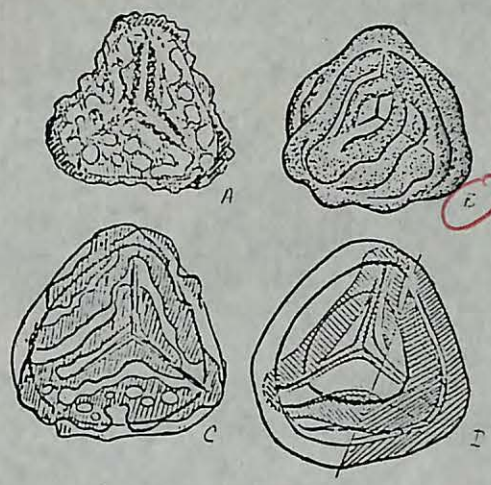
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13 August, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Ave. SE
Calgary, Alberta T2G 2B3

Dear Jan:

Re yours of 28 July: you are quite right about the points you bring up, and there are unfortunately still other errors in the Hartford Basin paper.

What I regarded as the best solution of the Classopollis-Corollina-etc. problem was outlined in the abstract you will find in the back of your current Geoscience and Man (p. 159). I still think that is a good statement, as does Tate.

It was my intention that Bruce would follow the principle outlined in the abstract--suppress Circulina. I did not follow through on my intention, however, and in preparing the final MS Bruce decided that the things Malyavkina called C. simplex were probably separated endexinal bodies of Corollina, and he decided to use that name* for them. This is something I should have caught but didn't (nor did the reviewers, darn it!). It is also true that Circulina funifera is the correct type for Circulina (see CFSP Vol. 38), thus compounding the error. However, the main problem is that the endexinal bodies should not have been named at all but should have been designated as "possible endexinal bodies" and let it go at that.

You are also (naturally, naturally!) right about the form of citation for C. meyeriana. Also, the information for the photos from Zauer should have made it more clear that we were doing the designating. There are also quite a number of other errors of citation and whatnot--you were kind not to mention some of them.

Rick Pierce has indicated over the phone that I can have a little space in the next G & M for a correction statement. In the errata statement I probably will not deal with the comparatively minor mistakes, but at least the Circulina blooper must be dealt with!

It is a pity that these problems flaw this report because Cornet really has done a good job of field and laboratory work, discovering and describing palynofloras that were previously unknown.

* C. simplex

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I should also add that Tate was not involved at all in the Cornet-Traverse contribution.

Yours very truly,

Alfred Traverse
Professor of Palynology

P. S. Re the Corrugatisporites project: I was sure that I wrote you a letter about this in the brief period in June when I was back from the Black Sea and hadn't yet left for Leningrad. Tate also thinks I wrote such a letter. Neither of us seems to have a copy, however. I was then, as now, without a secretary, and God knows what happened. If you didn't get such a letter, my apologies. The gist of it was that, in view of my prolonged absence from the country, you might wish to take Tate in my place as collaborator. Now, I gather from your letter, you are still waiting for me--I'm embarrassed but will get to work on it.

cc. H. T. Ames
B. Cornet
S. Srivastava

Enclosure: copy of letter to Srivastava

Hunt Institute for Botanical Documentation

DR. ALFRED TRAVERSE
529 Deike Building
Pennsylvania State University
University Park, Pa. 16802

1 August, 1975

Dr. Jan Jansonius
Etc.

Dear Jan:

Bharadwaj sent me a reprint of the enclosed paper with the request that I xerox it and send you a copy. An unusual request and one that I hope doesn't become epidemic!

Just back from Congress. Very sorry you couldn't be there. I helped get your proposals pushed through (the ones I favored, that is!) On the form-organ-genus business I spearheaded a successful drive to get the whole organ-genus business taken the hell out. It has been a pain from the beginning. Besides, its a taxonomic matter that doesn't belong in the Code.

Gotta run. Best wishes to all.

Yours very truly,

Alfred Traverse

Hunt Institute for Botanical Documentation

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PRODUCTION DEPARTMENT

July 28, 1975

gave copies to Ames & Cornet

Dr. A. Traverse
Mr. Tate Ames
Mr. W. Cornet
Deike Bldg./University
University Park, Penn.
U. S. A. 16802

Dear Friends:

Just received Geoscience & Man, v.II, with Cornet & Traverse.

Why do you give Circulina simplex as type species of that genus, where already a neotype (C. meyeriana) and a lectotype (C. funifera) had been designated? In the CFSP you consider the last one as the correct type. Yet, here you state that C. funifera really belongs with Corollina, which makes the two genera synonymous!

A propos: C. meyeriana is not ("Mal.) Klaus", but "Klaus n.sp." As for Zauer's 'neotypes': he never struck me as very perceptive; his taxonomy is as muddled as Maljavkina's, but that does not give one any more confidence in trying to make the latter meaningful?

Please let me know as soon as possible whether I have overlooked something, or whether you erred in these instances.

In haste, with best regards,

Jan Jansonius

JJ/er

Tate,
I'm having new glossy prints made of all the figures of Triassic 1962 paper, for your use in the CFSP.

All I ask for is your comments re Corrugatisporites.

Hunt Institute for Botanical Documentation

May 6, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

Thanks for the most recent batch (according to what you say, practically the last!) of the famous Jansonius cards. We continue to make occasional use of them, and it is only frustrating to me that I can't find the time to digest more of them thoroughly. As you know, I have been trying to get up a generic list for the Smithsonian project. It will seem to have been a useless task when your cards are out, but when they first wanted this list your cards were still just a project underway, and I thought I could be of some assistance to them. Believe it or not, I have not used any of your cards working up my list because I was hoping that the last step I would take would be to compare what I found with what you found and that this would doubtless produce some interesting results. In fact, it already has but there is no time to pull it together. I just have occasionally noted that my slips of paper with names have some interdsting contraditions vis-a-vis yours. However, I am only at the 1800 level and am only working on the names and types so you can see that I just don't belong in the same "ball park" with you! In any case, I have greatly appreciated your kindness in providing us the cards because quite apart from the Smithsonian list we have made considerable use of them in connection with the Catalog work, which reminds me thatyyou were supposed to have received Volume 40 for review long ago, but I have not succeeded in pushing it through Dr. Spackman's office, and now I am leaving for what amounts to nearly three months. Don't give up!

Best wishes and thanks for the bon voyage. I should have some good stories to tell my classes at the very least!

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi

Hunt Institute for Botanical Documentation

DR. ALFRED TRAVERSE
529 [illegible]ike Building
Penn[illegible]vania State University
University Park, Pa. 16802

5-V-75

In haste, wishing All Gods speed and Behorden Vaart!

We went to see the Orient Express some time ago and I can just imagine what Istanbul will look, and smell, like.

Thank you, both, for correspondence and various comments. I have modified my Horstisparites somewhat, and hope that the Compromis will be accepted.

If you come across any other points where my interpretation of the facts in the light of the ICBN, seems shaky — please let me know.

After this envelope-full, you will receive not more than about two dozen entries (I'm waiting for inter library loans), and that will be the end. I then have a total of 3300 - 3500 names carded. Hopefully, publication after that will be a matter of months only.

I'll keep this short. Best regards,

Jan.

Hunt Institute for Botanical Documentation

April 10, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

Thanks for the additional card material which came with yours of 21 March. I am trying to keep up to date on all this stuff but have been falling pretty badly behind lately. First of all, regarding Pollenites--our reservations did not stem from the bleakness of Potonié's description in Pot. & Gell. Rather, we were seriously raising the issue that it is not at all clear that Potonié meant to describe a genus. This seems to me to be quite a possible interpretation of the language used, of which I stubbornly believe my translation is as good or better than yours. It may well turn out that the dispute is insoluble and will have to be left to the court of time.

I am glad to hear your opinion about the acceptability of glued-in glossy prints. I think you are probably right, and I hope I can remember at Leningrad to discuss that with Stafleu if I ever get him cornered.

I was amused by your comment on Ol' George H. While I was in the hospital I had the privilege (?) of doing the official comment on his latest manuscript for Geoscience & Man. This is about the third Hart manuscript I have had a go at, and I sincerely hope it is the last. What a thankless task! I wrote something on the order of 20 handwritten manuscript pages of comments for which I will get not even so much an acknowledgment, much less co-authorship. Thanks for the copy of Pant, 1954.

The Naumova stuff we of course had. In fact, I had recently had the photographer make copies of the illustrations with his super-duper equipment so as to be ready for potential republication in CFSP. The dispute about Hymenozonotriletes is simply as to whether it was validly published in Naumova or not. If it was, what Mehta did was superfluous.

Hunt Institute for Botanical Documentation

Now regarding Anagrammites--what puzzled me is that I thought I understood your previous letter to indicate that a note had been published and was concerned that the note might have listed me as co-author. The reason that would concern me is that I feel that I had an inadequate opportunity to revise the course of affairs. You will perhaps recall that in the summer of 1973 after my return from Calgary, Tate and I spent some time discussing the whole business and he convincdd me that at least it was not as clear cut a situation as we had thought when we all worked on it up in your lab. After that point, I don't recall having gotten anything more from you on it though my letters to me of 15 August, 1973, make it pretty clear that I was worried.

Betty tells me that she has heard of the Kemelman book but, of course, I have no time for casual reading as Stan can assure you from having seen me in action. I barely have time to open my mail—but your letters are an exception!--I always manage to work them to the top of the pile.

Latest news on me is that I am going to be an on-board palynologist on the Glomar Challenger on their forthcoming Black Sea cruise. I leave from Istanbul, Turkey on the 19th of May, returning in mid-June. I am assured that my knee will be able to take it by that time.

Very best wishes to all my friends in your vininity and to yourself, of course!

Yours very truly,

Alffed Traverse
Professor of Palynology

AT/vsi

Hunt Institute for Botanical Documentation

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865-2342

March 27, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

Al claims to be far behind in both his professional and avocational duties following knee surgery which cost him twelve days in the hospital, a few more at home, and still limits his mobility. At your suggestion I have looked into the Colisporites problem and have come to the following conclusions. These views have been reviewed with Al and he appears to be in agreement with the following opinions.

Certainly, Horst's dissertation has no status under the 1972 ICBN. It does not even constitute effective publication under the rules. Thus reference to it should have no formal nomenclatural significance or value except, in this case, to indicate the origin of the epithet "bulbosus" to be in Horst's, 1943, dissertation. Because it was not even effectively published, it (per the Code) should not be treated as formal synonymy per se. Apparently, it should be reserved for separate treatment elsewhere in your text. However, it does provide evidence as to what the original authors of the name had in mind as the type of this generic name. We encountered a similar case in which the name of the designated type species was only effectively published, but the genus included a second validly published combination which was probably also legitimate (± Potonie & Kremp, 1955). Upon request to Fran Stafleu we were informed that the type designated was clearly indicated and the "taxon" would serve this function regardless of its nomenclatural state when validated. The new species with the legitimate name could not serve this nomenclatural function. It is true that the Code states that a nomenclatural type need only be indicated. There is no reservation as to nomenclatural status although one would normally expect one to select a legitimate name as type, but one is not always correct in his views as to whether the name of a taxon has been validly published or not. The present situation is a case in point. Potonie & Kremp obviously believed that distribution of the photographic copies of Host's dissertation constituted valid publication or they would not have designated the type as a new combination. Furthermore, if the Horst dissertation had been effectively published, their "synonymy" would have provided a direct reference (per Article 32) to validate the species name. As the Horst text was not effectively published it does not appear to be capable of serving as a nomenclatural reference to meet this need.

Hunt Institute for Botanical Documentation

My view concerning the status of Colisporites varies markedly from what you have indicated in your manuscript. In no way could I consider Potonie & Kremp, 1954 (p. 138) to be provisional publication of this name. Their intent to formally present Colisporites as the name of a new genus is absolutely clear. Their treatment also constituted valid publication of the name which should be indicated to be Colisporites Potonie & Kremp, 1954 (p. 138 Geol. Ib.). That the genus may or may not contain the validly published name of an included species does not affect the validity of the generic name so long as there is a generic description and the name is published prior to 1958. Several years ago Al was enlightened by some friends at the Smithsonian in Washington, D.C., that tens of generic names of extant plants have been recognized as validly published although they did not contain a species. Their protolog contained only a generic description. Fran Stafleu subsequently verified this view in a personal communication to Al.

Colisporites as published in Geol. Ib. Bd. 69 was either published without a contained species or it may be interpreted as possibly having been monotypic. As it was published after 1953, Article 42 requires that a generic description be provided if the concept of a descriptio generico-specifica is to be applied. This requirement seems to have been met. The proposed combination C. bulbosus is not a new combination because the epithet appeared only in Horst's dissertation. The crux of the problem lies in whether one would recognize the illustration identified as "Colisporites n.g." in Potonie & Kremp, 1954, p. 120, pl. 6, fig. 23, as derived from a specimen of the only "species" recognized as possibly belonging to the genus! Certainly it is more diagnostic than many drawings that have been accepted as types. (Also, Horst states in 1955 (p. 164) that species is based on single specimen. Although van der Hammen proposed generic names and provided illustrations of forms he had not seen I do not believe Potonie & Kremp capable of such a practice.) Therefore, I feel that the figure cited could serve also as the type of the species and that both the generic name and combination could be recognized as validly published. The type of Colisporites could therefore be referred to as C. bulbosus Horst 1943 ex Potonie & Kremp 1954.

If you do not accept the above view regarding the species I then believe you must cite the type as Colisporites bulbosus Potonie & Kremp 1955 ex Horst 1955. Your identification "Colisporites bulbosus (Horst) ex Potonie & Kremp in Horst 1955" implies that Potonie & Kremp were the authors of the "protolog" provided in Horst 1955 which I do not believe to be the case! It is attributable solely to Horst.

Finally, I believe your question of whether we allow them any choice as to the type of Colisporites (C. bulbosus or C. algae) has no meaning because the Code clearly indicates that an author's choice of type can be superceded only if it consists of mixed assemblage or is lost. (In the 1954 case there was only one possible type and holotype and in the case of Potonie & Kremp 1955, the types were clearly indicated not to be C. algae.) I do not see that subsequent authors can change this selection under present rules.

Hunt Institute for Botanical Documentation

Here's hoping the warm weather of spring may soon reach Calgary.

Cordially,

H. Tate Ames
Associate Editor, CFSP

HTA/vsi

Hunt Institute for Botanical Documentation

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PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY
PRODUCTION DEPARTMENT

March 21, 1975

Dr. A. Traverse
Deike Bldg.
University Park, Penn.
U. S. A. 16802

Dear Al:

I don't know that I can trust this world: I write you about a procrustean bed, and you answer that you're in hospital for some orthopedic surgery? I didn't mean for you to take things that literally!

I'll quickly answer some points. You'll be the last person I should lecture on the virtues of charity; it has a recognized place in the Code, when the Preamble instructs that "in the absence of a relevant Rule or where the consequences of Rules are doubtful, established custom is followed." I do not take this to give us license to consider Pollenites valid becuase so many people have thought it to be so. Clearly, the Code requires a "diagnosis or description"; however, established custom allows us to charitibly accept Potoniés statement (in Pot. & Gell.) as describing the limits of the generic concept 'Pollenites'. Just as many 'valid' diagnosis are not tiresomely exhaustive and and for instance delete the information that a fossil spore is described, but jump in straight away with : "trilete"; etc.; from usage in that and other papers, it is quite clear what was involved with Pollenites.

Just as Imgrund's thesis was not valid, so was that of Horst not effectively published - circumstance prevented him from going the proper road. It was only the fact that Imgrund, Horst, Klaus (and a few others?) where Potonié's Ph.D. students that caused Potonié to stretch the rules for these cases. Carbon copies of numerous other thesis have never been considered valid. Similarly, photographed copies of a type script are not effectively printed. Klaus himself - now that he has his own judgement - considers his thesis as not validly published. Horst later - very sensibly - updated his thesis into the 1954 Palaeontographica publication. The proof of this pudding for me is the unavailability, even when trying hard, of a copy of this thesis.

Sometimes it worries me to see how Stafleu's harried and hurried snap interpretations are treated as gospel. Obviously he has been well grounded in matters of nomenclature, but his opinion is just that. I would certainly accept glued in photographs - glued into a printed text, that is - as valid/effective publication. Most people cut standards and costs, and go to collatype or half-tone, or even line drawings. I don't think the Code necessarily should rule on every eventuality; anyway, I see no problem here.

Hunt Institute for Botanical Documentation

With regard to Hart: he has consistently shown such lack of interest in proper nomenclatural procedure (or even sound taxonomic practice, for that matter) that his notions about the earlier Russian literature fit right in. (A propos: in those days of Stalin much of the Russian mail apparently was destroyed by the US Customs?).

I am enclosing a copy of Pant 1954.

Regarding Naumova's genera: I just cannot see that the names are valid as per 1939. If the genera become valid per first author actually using them, than Mehta unwittingly earned that honor for Hymenozonotriletes. And look again, that really is what Naumova seemed to have in mind (xerox enclosed); Mehta stayed well within the limits of Naumova's circumscription; and the 'Guide for the determination of types' (ICBN, p.75) is clear enough: "If he included only one element, that one must always be accepted as the holotype."

About Cookson: charitibly I accept all those that were diagnosed and are binomial, even if introduced as "sporetype"; I do not accept those that are introduced in the trinomial coenotype system of Erdtman.

I think there was no misunderstanding on Pacit, so don't worry.

The "we" on Anagrammites was you, Stanley Pocock and I, in an attempt to straighten out the confusion around Corrugatisporites and Lygodioisporites - remember? Unfortunately, Anagrammites was a genus already, mothered by Bolkhovitina 1956.

The latest book by Kemelman (Tuesday the Rabbi saw red) now is out in paperback. It should be excellent reading while laid up on an orthopedic mattress. Are you familiar with that series? This one is about as good as the Rabbi was on Friday and Saturday.

Best wishes,

Jan Jansonius

JJ/er

Enclosure.

Hunt Institute for Botanical Documentation

March 66, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

Re Colisporites: This came while I was hospitalized recently for two weeks (knee surgery). I am still only about 1/2--and very far behind, so I discussed issue with Tate and asked him to write you. His analysis agreed with mine--that C. was o.k. as of 1954 as monotypic with -combined description. The description provided is clearly referred primarily to the genus, as witness the caption of the illustratinn.

"The hurriedemjI go, the behinder I get" (or something).

Greetings,

Alfred Traverse
Professor of Palynology

AT/vsi

Hunt Institute for Botanical Documentation

March 10, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

I am in the hospital for some orthopedic surgery and have time for a tad of correspondence. I believe I'll write you multiple letters--too many subjects for one!

First--thanks for the generic sheets--I continue to make great use of them, as do students and Ames.

Thanks also for scolecodont paper from B. & M. That will help quite a bit with the slight presentation of scolecodonts in my course. Most of what I am currently presenting is from your seminar here in PA a couple of years ago.

I agree with you on Proposal 43. I suspect it will have heavy sledding in Leningrad--so many systematists are also classicists. Perhaps it would have been better to restrict the deletion to generic names? However, I still think Srivastava is wrong anyway--e.g., in Wodehousea.

Tell Stan thanks for the W. A. P3 report--read with interest! I was a little astonished to find on p. 12, l. 7, that microplankton are not (by implication) "palynomorphs"! Hmmm.

Best regards.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi

Hunt Institute for Botanical Documentation

March 10, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

This one will respond to various matters brought up in yours to Ames of 16 Jan. and to me of the same date (you too are a productive correspondent!).

First, regarding Pollenites and the letter to HTA: I still don't think the Pot. bit was satisfactory as a description or diagnosis, no matter whose translation of the German is accepted. But I do think "merely" is a better translation of schlechthin than "plainly"--in fact, "plainly" is misleading because in current English usage it means "obviously" or "clearly". I agree that "vorläufig" has a time connotation--so does "preliminary". There could, in fact, hardly be a more exact translation of vorläufig than "preliminary"! But all that is a tempest in a teapot--not greatly influencing the issue. I think what Potonié presented is not sufficient to make it clear that a form-genus for fossil, dispersed pollen was intended. For the time being let's (as they say in Pennsylvania Dutch-English) "leave it go". I loved your prerustean bed analogy.

Re Imgrund's thesis: we agree it was not effectively published. Re Klaus' & Horst's theses: probably you're right that they were not effectively published, but I am troubled by the possibility of opening a Pandora's box--Hart, I hear, is convinced that, inter alia, Malyavkina, 1949, was not effectively published because, in those days, the Stalinists did not believe in sending their goodies abroad. As far as the mode of duplication is concerned, I don't see why photographic copies aren't o.k., and I have been troubled about Stafleu's decision on a question of mine that illustrations in a monograph I turned up do not count as illustrations per the Code because they are photographic prints pasted into the book. Another place where the Code needs to be more explicit!

Hunt Institute for Botanical Documentation

Re Naumova: you should not compare with Erdtman, 1947, because he included a categorial disclaimer. van der Hammen, 1954-1956, is still another (important!) case--a million problems--some of his names seem o.k. Could you provide a xerox of Pant, 1954? I would greatly appreciate it.

I guess I need say little more re Hymenozonotriletes--I'll stand on my 7 June letter. But I would argue that Mehta's species could not "determine the content"--the description and picture in Naumova did that. Mehta was not at liberty to assign just anything to H.--hence, my argument of misassignment. "Equisetosporites" is irrelevant, of course--Pseudotsuga is also not a "false hemlock".

Re Malyavkina: I agree that, for the moment, we are "talked out" on this one! But both Tate and I feel that you are wrong in dismissing Cookson's names out of hand--we think some of them are (admittedly marginally) o.k.

Re Paclt proposals. I just didn't put it clearly (too hurried). What I meant is that no case for urgency has been established, and conservation should only be used for such cases. Why carry Cyclostigma around in the appendix to the Code? Who cares if it is renamed Tricylostigma or Motorcylostigma? If it's a homonym, rename it!

Who is the "we" of the Anagrammites note in Micropaleontology?

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi

P.S. Don't worry about CFSP work. The next set of proofs will be for Vol. 40--and I can't see you getting it before April 1 anyway.

Hunt Institute for Botanical Documentation

November 26, 1974

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

This is with regard to the--as it turns out--sad case of the "Horst thesis". I got the negatives from Tate Ames, who said they were Horst's, took them to our still photographer and had a print made for you. I "conned" the biology department into paying for the job. Alas, when I went to pick up the prints, only then did I discover that it was a print of the thesis of R. Imgrund, per the enclosed xerox of the title page. Tate has now combed his files and says that this is the only microfilmed thesis he has. Sorry.

Question: Do you want this print? Or do you have Imgrund already? Let me know if you want it, and we will mail it to you immmadiately .

Best wishes.

Yours very truly,

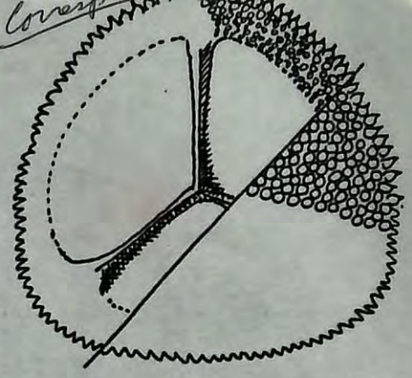
Alfred Traverse
Professor of Palynology

AT/vsi
Enclosure: xerox of title page

Hunt Institute for Botanical Documentation

COLISPORITES Potonié & Kremp 1954 ex 1955
1954 Geol. Jahrb., Bd. 69, p. 138
1955 Palaeontographica, Abt. B, Bd (98), p. 93

Dear Al:



Type species (see also Remarks):

Colisporites bulbosus (Horst) ex Potonié & Kremp in Horst 1955 Palaeontographica, Abt. B, Bd. 99, p. 164, pl. 17, fig. 4,5

Colisporites bulbosus (Horst) Potonié & Kremp 1954, l.c.

Triletes (Apiculati) bulbosus Horst 1943, Dissertation, p. 48, fig. 4,5 (not effectively published)

Diagnosis (1954): Trilete megaspores, amb. convexly triangular with more or less broadly rounded angles; rays Y mark usually extending nearly to equator, curvaturae fairly to very close to equator; surface of exine all over ornamented, even often on the lips (tecta) of the Y mark, with more or less flat elements, (strongly) reduced in the contact areas, very pronounced subequatorially and between equator and curvaturae, often evolved into strong coni to pear-shaped spinae that cause a dentate extrema lineamenta; distally elements fairly large, closely crowded, often polygonal at their bases.

In Tuberculatisporites the coni are more scattered.

Description of type species: based on single grain, 900 μ, rounded triangular; exine has granulose structure, 30-40 μ thick; trilete rays 2/3 radius, with labra of 30-70 μ width; contact area distinct, without sculpture; rest of spore spinose to tuberculose, coni 20-40 μ long, blunt, 25-35 μ at base, bases virtually touching each other, in top view resembling bulblets. Westphalian A; Silesia.

Remarks: The genus, when proposed in 1954, essentially was a provisional name, since the type species had not effectively been published. In 1955, Potonié & Kremp assign a new species, C. olgae, and again refer to C. bulbosus as the type species; the latter was effectively published shortly after by Horst (who was a student of Potonié). Potonié had considered that Horst's thesis had been effectively published by distribution of a number of photographed copies to major German libraries; Horst's 1955 publication is based on the unpublished thesis.
Under the circumstances — and since no contest with other taxa exists — it seems pedantic to consider that the genus is valid as of Potonié & Kremp 1955 (Bd. 98), with obligate type species C. olgae, which was the single validly published species included in the genus; Potonié + Kremp also, see below

If you'll remember Hymenozonotriletes, where I (firmly still) consider that H. triangularis must be the obligate type species: the ICBN, in the Guide for type determination states that if only one element was included in the description of a taxon, "that one must always be accepted as the holotype". However, in the case of Potonié & Kremp, the original author's of Colisporites definitely express at the time of the original publication of the name of the taxon (1954, 1955) their choice of C. bulbosus as the type. Do we allow them any choice, or say that in Bd. 98 of Palaeontographica they had only C. olgae to choose from as the single valid species assigned to the genus, and indeed ipse facto validating the genus?

These things are getting to me. How nice to make a bold and noble decision and for all times say: thus it shall be. But we are bound by the Code and hobbled by the errors in judgment by our predecessors. Any lasting decision must be right by the Rules to which we subject ourselves. To decide in this matter is weighing the imponderabilia that the Code never foresaw. I don't know that the genus has been used again, and thus we even lack an "established custom to follow", although "custom" undoubtedly would have followed Potonié.

Please let me, soon, have your and TATE's ideas on this particular problem.

Best regards,

Hunt Institute for Botanical Documentation

I-75 file w. Tauschius letter

Pollenites R. Potonié 1931.

Pollen schlechthin werden vorläufig zu der künstlichen Gattung *Pollenites* gestellt.

Pollenites cf. dolium solum R. Potonié.

Taf. 2. Fig. 20.

R. Potonié, Zeitschr. Braunkohle 1931, Heft 16, Taf. 2, Fig. 22.
O — △ ?, (), ⌒, gr, 3, 3 ⌒, 36 μ
Umrißlinie fast glatt, an den Polen etwas rauh.
Weitere Vorkommen: Grube Babina bei Muskau, Miocän.
? Grube Cecilie im Geiseltal, Eocän.
Präparat: 28 d; Z. 8. 6, 80. 9 (Fig. 20).

Zusammenfassung.

Es werden 7 Arten Pteridophytensporen und eine Pollenart abgebildet und beschrieben. Sie stammen aus der oberpaläocänen Hartbraunkohle von Dorog in Ungarn. 4 der beschriebenen Arten sind neu. 6 der Arten sind bisher nur in eocänen, eine vielleicht auch in älteren Schichten, 2 auch in miocänen Kohlen gefunden worden. Dies ist vielleicht ein statistisches Ergebnis, daß den Weg weist, auf dem sich Sporen und Pollen für die Stratigraphie auswerten lassen.

Literatur.

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Hunt Institute for Botanical Documentation

File: Traverse

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PRODUCTION RESEARCH AND
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PRODUCTION DEPARTMENT

January 16, 1975

What say we now, Tate, Chief, apples? Oh,

H. T. Ames
Deike Bldg./Earth Sciences
University Park
Pennsylvania 16802 U.S.A.

Dear Tate:

Thank you for your letter and good wishes, and with pleasure do I return the favour: may you and all yours prosper in 1975.

Regarding Pollenites: the translation letterally goes thus: "Pollen plainly for the present time are assigned to the artificial genus Pollenites". From the word order it is clear that 'plainly' qualifies the word 'pollen', in the meaning 'without differentiation'; it does not (as in Traverse's version) qualify 'for the present time'; the 'vorläufig' conceivably could mean 'preliminary', but in the context it has more the meaning of time ('for now') than of tentativeness ('preliminary'). I consider that the statement is intended as a definition of the name 'Pollenites', and acceptable as the description or diagnosis required by the Code.

Since the Code did not require a type species until 1958, the genus can be valid without one. Because of the widely conceived diagnosis ('Pollen plainly', or 'All pollen') it is uncalled for to assign a type species that would immediately restrict the genus to much narrower concepts ('only angiospermous pollen'). Since P. dolium solum was (invalidly) introduced earlier by Potonié, and since the specimen in Potonié & Gelletich is only compared with this species but not identified with it, I see no grounds for construing that specimen as the obligatory type for Pollenites. The genus is valid through diagnosis, not through descriptio genero-specifica.

Of course, the application of the Code to the early workers is of necessity like fitting a Procrustus bed, for those pioneers were rank amateurs in the field and quite ignorant of sound practice and proper procedure. Some recognized their limitations, and refrained from formal nomenclature (Raistrick, Dulhunty, Virkki, etc.), and a few informed themselves and produced fine work that stands without subsequent problems (Bartlett, Ibrahim). Potonié and his group started from scratch and only much later tried to salvage their heritage by coming to an accomodation with the Code. That effort failed, in part because all the special PB articles could not be maintained. Later, Potonié still was not at ease with the Code and tried to adapt Erdtman's ideas. Maybe it is time to either redescribe the old types where they still exist, or get rid of most of the older names as nomina dubia, because from description and illustration we still do not know what they stand for?

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I trust you will discuss this matter again with Al, so I will not deal with it in a letter I owe him.

With best wishes,

Jan Jansonius

JJ/er

Hunt Institute for Botanical Documentation

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PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY
PRODUCTION DEPARTMENT

January 16, 1975

Dr. A. Traverse
Deike Bldg./Earth Sciences
University Park, Penn.
U.S.A. 16802

Dear Al:

Thank you for the bombardment of letters recently received on a variety of subjects. I just answered Ames on Pollenites, and hope to deal with all the other matters here.

1. Effective publication

I wrote to Horst, and enclose a copy of his answer, which is touchingly representative of an earlier generation. Art. 29 says that microfilm of a typescript is not effective publication. By extension we all assume that the xerographed theses (which have gone thru a microfilm stage) are not effectively published. But is the photographic print of Horst's 1943 thesis 'indelible autograph', or is a typed text never 'autograph'? (it is handwritten, if not in long hand). Horst claims adequate distribution, but I tried to obtain his thesis from Krefeld and drew a blank. I have sympathy for his circumstances and trust he acted in bonafide, but still feel that we should lump his case with the others, and consider his thesis not effectively published. I will try some other German institution for a copy.

The thesis by Imgrund, of course, was only deposited in five copies at the Aachen T.H., of which one top-copy and the other carbon copies. So here we don't even have an attempt at distribution, and in his later publication Imgrund explicitly shares that point of view; he thought that Potonié was incorrect in thinking it effectively published in 1952.

I also received a copy of some pages of Klaus's 1950 thesis (re Tetrapidites). Potonié got a copy of that and again considered the new names validly published. Klaus says that he "personally is not so sure about it, because it was never printed". I agree with Klaus, and consider his thesis not effectively published.

2. Naumova 1939

I disagree with Stafleu who dismissed the paper, because the taxa were called 'subgroups'; I think that is just a case of semantics, due to poor translation. Some translaters still will translate a Russian 'genus' into an English 'subgroup'. However, I consider her lists of names [conform Art. 34, (2)] to be "names merely proposed in anticipation of the future acceptance of the group concerned (so-called provisional names)". In the same category are

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similar publications by Erdtman 1947, Pant 1954, and Van der Hammen 1954-1956. Still, the names were effectively published, and later authors could refer to those 'diagnoses'.

In the case of Hymenozonotriletes, it now can be argued, that Mehta validated the genus by his acceptance of the provisional name, using Naumova's earlier 'diagnosis'. A type species at that time was not required (although assigned by good workers, such as Ibrahim), and you can argue that Potonié's neotype should be followed. On the other hand, a 'lectotype' (chosen from the 'original' material on which the genus was validated) would have priority over a 'neotype'.

But, much as it is solid custom (and the Preamble gives established custom much weight) in palynology to consider a single specimen the automatic holotype, so a genus based on a single species must consider that species as its nominifer, as its obligate type, even if the genus is circumscribed by a separate diagnosis and not defined by a descriptio generico-specifica. In the latter case it is considered that the single species is the type, through the use of the word 'monotypic' genus ('the type is alone, single'). But as Art. 42 recognizes the case of a 'monotypic new genus based on a new species', it implies that the code also recognizes the existence of 'monotypic new genera based on an earlier species'. This is in analogy with Art. 26, par. 2: . . . "automatically establishes . . ."

In those older genera, where the genus is based on more than one species, a choice of a lectotype may be required if no type was designated by the author. In case of monotypy, I consider that no choice or designation is required, because the type was alone, singly, present already. So I will stick to my latest treatment of Hymenozonotriletes - what say you to that? (Your argument of 'misassignment' carries little weight, inasmuch as it is the first species, the type, and thus determines the content; and what did they know in 1943 about what was a spore and what a pollen: it was trilete, wasn't it? And how about Equisetosporites for ephedralean pollen?)

3. Maljavkina

Once again - for the last time - let me state that I regard Maljavkina as a misfit, in that her thinking on systematics and nomenclature were outside the main stream of Linnean classification. If you want to try and fit it in, it obviously will not fit the mold: parts will go by the board, elsewhere you will have lacunae. Genera and subgenera such as Dipterina, Aliferina, were part of her 'artificial system', and she freely transfered species back and forth to such 'natural' genera and subgenera as Podocarpus, Archaeopodocarpus etc. etc.

On top of that, her illustrations and descriptions usually are inadequate for the job, and you don't know how to positively identify your material in her terms. (Maybe access to her type collection would help, but not the published data). Thus, I have been looking at all legitimate technicallities that in fairness allow me not to accept as many of her names as I can manage. Much as the names of Cookson (based on Erdtman's 'coenotype system') are only valid ex Couper.

Maljavkina knew what she had in mind for her names (or did she, always?), and on the basis of that knowledge devised keys that will guide you to a genus or a species, but she did not give a formal diagnosis. That is enough for me! (Am I unreasonable in this?)

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You translated p.24 (thank you) and marked a passage about 'general characteristics': the characters of the genus are not repeated in specific descriptions, but that still gives me no true diagnosis.

Really, the 'Maljavkina system' is as unique as the 'coenotype' system, even if M. uses 'genera'.

The table 13-1 (p. 114), gives 'theses' and 'antitheses'; these must be kept separate of course. Thus '6-theses' pertains to Siliculina, '6-0 antitheses' to Acuminella. Both these share the data of 4-0 antitheses, and again 1-0.

When you used "0-4", this '4' is not part of key 13-1, but refers to the fourth subgroup in table 13-2. On p.38-v, you should have said: "The description for Acuminella thus is at least the paragraph "6. . .0". The '6' leads the reader back to "4-0" at the top of p.115", etc. Please note that Maljavkina in her index (1949) does not refer to the page of your descriptions in the keys, but to the pages heading the species content, as the page of the new genera.

4. Nomen nudum

Since a clear definition is wanting in the Code, there is little merit in further arguments: you go by "because it is invalid"; I would like to go a bit farther, and say "invalid because not attached to a nominifer", or something like that. I will come back to this in the future. The fact that 'nomen nudum' is often not used in the same sense as 'name invalid' points to a hidden treasure under the calm surface.

5. With regard to the nomina conservanda of Pacit:

I have read through his data again, and the arguments presented by Lellinger and Chaloner - and yourself. Pacit has researched his stuff properly, and indeed all conservation of names is against renaming in the future, which is unavoidable if the nomen conservandum is not conservatum. Lellinger is wrong, when stating that an illegitimate name does not exist: it must be valid, before it can become illegitimate! In conclusion: I now want to change my mind, and : although Pacit sounds reasonable, it maybe more practical to deny his request. It should not cause real hardship. Should I initial and alter the ballot, or will you change it into a 'no' for me?

6. Finally, your proposal for me to join the editorial board of the CFSP:

I think I shall be happy in that illustrious company, and accept your kind invitation. However, I shall decline to do any work for you until April, because the genera card project now has usurped all my free hours, and I want to get that load off my back.

A propos:

In the newsreport (Micropaleontology) for Canada we inserted an item regarding our new genus 'Anagrammites'; too bad that that name was used already by Bolkhovitina. Soon I shall have to make a decision in this matter, and dig up our old data. Do you have any new ideas?

With best regards,

Jan Jansonius

JJ/er (Enclosure.)

Hunt Institute for Botanical Documentation

Dr. Ing. Ulrich Horst
3 Hannover-Buchholz
Wolfenpfad 10

Hannover, den 20.12.1974

Herrn

Dr. Jan J a n s o n i u s
c/o Imperial Oil Limited
339 - 50 Avenue South East
C a l a g a r y, Alberta T2G 2B3

Sehr geehrter Herr Doktor Jansonius !

Ihr Brief vom 4. Dezember wurde mir vom Niedersächsischen Landesamt für Bodenforschung zugeleitet, da ich seit dem 1.7.1973 dort nicht mehr tätig bin, weil ich mit dem 65. Lebensjahr die Altersgrenze erreicht hatte.

Ich freue mich, von Ihnen nach längerer Zeit zu hören. Ihre beiden Fragen will ich gern beantworten :

1) Meine Dissertaion von 1943 gilt als veröffentlicht. Sie wurde zuerst in Schreibmaschinenschrift für die Prüfung eingereicht und dann, nach der Genehmigung, infolge der Kriegsverhältnisse nicht gedruckt, sondern fotokopiert, und zwar in der vorgeschriebenen Anzahl, d.h. alle größeren wissenschaftlichen Bibliotheken bekamen diese unverwischbaren Ausfertigungen. Sie gelten nach internationalem Brauch als Druckwerk.

2) Diese Dissertaion können Sie durch das Geologische Landesamt in Krefeld erhalten, sicherlich auch von der Staatsbibliothek in Berlin, der Deutschen Bücherei in Leipzig und anderen großen Bibliotheken. Mein Doktorvater, Prof. R. POTONIÉ, der jetzt leider auch in hohem Alter verstorben ist, wußte darüber genau Bescheid. Vielleicht kann Ihnen Frau Dr. Hildegard GREBE in Krefeld behilflich sein. Für mich ist es jetzt etwas schwierig, weil ich nun Privatmann bin.

Ich wünsche Ihren Arbeiten viel Erfolg ! Wenn ich Ihnen irgendwie behilflich sein kann, will ich es gern tun.

Mit den besten Wünschen zum Weihnachtsfest und zum neuen Jahr !

Anlage

Ihr Horst

Hunt Institute for Botanical Documentation

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Department of Geosciences
Palynological Laboratories

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January 8, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

Holiday Greetings, but I must say you raised a devilish spector when I encountered your entry for Pollenites Potonié 1931 ex Potonié & Gelletich 1933 in Traverse's photocopy of your manuscript. It was examined with much interest because valid publication of Pollenites during the early thirties will have a tremendous impact upon status of generic names employed in Wolff, Thiergart, Raatz, Thomson & Pflug, etc. Al and I have been operating for sometime now under the impression that Pollenites had not been validly published and possibly still may not be validly published in the sense of a taxon of generic rank. Thus, I could not look upon your views as a blessing during this Yuletide season.

Seriously, however, I do wonder if the phrase included under the caption Pollenites in Potonié & Gelletich 1933, 525 is truly recognizable or will be recognized as a descript-ion or diagnosis per either your translation of the German or Traverse's. He has a slightly different version which reads: "Pollen grains are merely preliminarily placed in the artificial genus, Pollenites". I would contend that neither your translation nor his qualify as a diagnosis or description of a taxonomic category on the grounds that it includes elements of extant plants. As such it is a substitute morphological term for pollen grains; not an artificial generic name as claimed. This weakness is bound to be the subject of extended debate which never may be reconciled by some. I can only feel it will greatly hinder rather than promote stabilization of "Cretaceous-Cenozoic" generic nomenclature, but the latter is not a legitimate argument for its suppression.

I have often predicted to Al that Pollenites would be validated via monotypy (probably in some obscure South American or Indian journal in the fifties to early sixties). This almost appears to be a case in point;

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perhaps a stronger argument than being dependant upon that non-restrictive phrase as a generic definition.

Such confusion exists within and between Potonié, 1931, and Potonié & Gelletich, 1933, that the taxon designated "P. cf. dolium solum", in the latter publication could be recognized as either a new species or infraspecific name at any subsequent authors descretion (see Art. 35, 1972 ICBN, 2nd paragraph).

Please note the following oddities from Potonié 1931 (Mitt. 1) & Potonié & Gelletich 1933:

1. P & G, 1933, listed synonymy cites pl. 2, fig. 22 in Mitt. 1 which actually refers to "P. dolium megaventriosum nf" (fig. 22; see enclosed photocopy).

2. P., 1931, Mitt. 1, pl. 2 captions illustrate and describe the following: "P. dolium solum n. sp." (fig. 21), "P. dolium clasum n. sp." (fig. 35), "P. cf. dolium solum n. sp." (fig. 36), and "P. cf. dolium n. sp." (fig. 39). There is no P. dolium n. sp., per se, in Mitt. 1 or an earlier publication!

3. Ca 1959 I believe I recall G.O.W. Kremp saying that the illustration in Potonié, 1931, (Mitt. 1) were mixed up. Boy was that an understatement.

I believe it possible, although perhaps not desirable, to recognize P. dolium-solum Potonié 1931 ex Potonié & Gelletich 1933 as a new species, per Art. 12, 1972 ICBN. "A name of a taxon has no status under this Code unless it is validly published (see Art. 32-45)." If this view is appropriate than Pollenites would be validated via monotypy and the description of P. dolium solum could serve as a descriptio generico-specifica per Art. 42. Furthermore, designation of a type species is also clearly implied by the genus being monotypic . (This would also be true if one accepted the "generic diagnosis" as validating the generic name. You apparently did not recognize it as published in your manuscript as you state it was "not designated". Technically you may be correct, but in fact, I would feel that it is clearly indicated!)

If one doesn't like the hyphenated specific epithet one could probably drop the "solum" as superfluous based upon the assumption that these authors did not recognize that Pollenites and epithets assigned to it had not been validly published previously. Of course this implies that they were not describing a species, but an infraspecific taxon. However, Art. , 1972 ICBN reads that nomenclature ~~of~~ a species is the sum of its subordinate taxa.

At this time I can but enter in to the Christmas spirit rendering one chorus of "God Rest You Merry Palynologist--let nothing you dismay, etc., etc., etc."

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You, too, will have to modify your manuscript about generic potentially recognizable in Raatz, Thiergart, etc. such as Liquidambarpollenites etc. if you accept Pollenites Potonié 1931 ex Potonié & Gelletich 1933.

You you & yours for prosperous &
Happy New Year,

H.T. Ames
Associate Editor, CFSP

HTA/vsi
cc A. Traverse

Enclosure

Hunt Institute for Botanical Documentation

January 7, 1975

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

I bet you think I forgot all about the "Hymenozonotriletes question". I haven't! The paper you sent about it is now dog-eared and much marked from thought given to it and discussions of the matter between Ames and me.

You have done a fine and interesting piece of research regarding Mehta's contribution. However, I am having trouble convincing myself that you are right. The difficulty is that in my opinion Hymenozonotriletes was validly published by Naumova in 1939. It is true that the "description" is pretty bare, although the drawing provided, I suppose, must be considered also as part of the generic protolog. The fact that no species was referred to the genus at the time of Naumova's publication in 1939 is not fatal to the valid publication of the generic name, as I believe we have previously discussed. That leaves us with the question of the standing of Mehta's contribution. The "spore" illustrated by Mehta certainly resembles Naumova's 1939 illustration as much or more than the conventional Hymenozonotriletes species. The only problem is that Ishchenko and Naumova herself, presumably using the original material, were in no doubt that the genus was for spores with a membranous zona. It's a bit sticky, but at the moment I guess I would have to consider Mehta's spore as simply misassigned. For one thing, if it is a pollen grain it is clearly referred to a genus which was for zonotrilete spores from the beginning.

I guess the critical question is the status of Naumova 1939. We are currently contemplating "covering" the paper in CFSP.

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Which reminds me--how would you like to be on the editorial board of CFSP? You could help us greatly. The rewards are small--you are listed, and you get a free copy of each volume. The duties are to read each volume in proof for mistakes and other errors of fact and interpretation. At the moment Chaloner is doing us the most good with his searching comments. We could use yours too. How about it?

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi

Hunt Institute for Botanical Documentation

December 30, 1974

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

This is a response to your undated letter, hand-written and enclosed with your typed letter of 4 December. I have a number of things to write you about, but I feel that it is best not to mix too many of them in one letter. I will state though that I am now very puzzled about the Imgrund-Horst business. Ames insists that the only microfilm he got from Grebe was that of Imgrund's thesis. Maybe it is Hilde's memory which is faulty?

Now regarding the Malyavkina business--there are quite a few comments need to be made so that we are sure we understand each other:

1. You apparently have not picked up previously that the policy of the Catalog of Fossil Spores & Pollen as to what will be covered is the criterion of effective publication, not valid publication. We have covered all sorts of things that are not validly published, so we are always careful to make a stipulation that it is not our intention to validate something in case it should not already be validly published. Jim Schopf has sometimes said that we should go ahead and validate where necessary, and there are at least two or three examples of instances where we have accidentally done so.

You seem to be saying that a generic description cannot be a generic description unless it is categorically so labelled. I find no support for such a position in the Code. If it is possible to piece together from Malyavkina's book what bits of information provide the generic description, it seems to me not to matter eseentially that they are provided in such an irregular fashion.

In view of your comments about page 24 in Malyavkina, I thought you might be interested in the enclosed translation of the page in question, by me. I don't know that it helps too much althoggh it does make it very clear, including the use of the word description in a couple of places, that that's what Malyavkina thought she was doing. (Incidentally, we were delighted to get your information about Thomson & Pflug's statement regarding holotypes. We had missed this and hope that our having missed it has not created any problems.)

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Tate and I have gone over your comments on our use of the information from Malyavkina and do not hink that you are right. In the example which you picked for Acuminella, the material labelled "6" applies to Siliculina. It is quite clear that the material we chose is in fact what applies to Acuminella on the pages in question. Of course, one could aupply still further material by working back in the key and eventually end up with "spore" or "pollen".

We don't know where you got the notion that we have said that Malyavkina is valid in toto. We don't believe that and didn't say it. Parts were validly published and parts weren't, and we comment on that in such places as our Nomenclatural Comments. As I have said above, our criterion for the covering of taxa in the Catalog is effective publication. By the way, it is not very pertinent that few Russian authors adopted Malyavkina's names. This is in large part due to internal Soviet scientific politics, according to Samoilovich (who should know).

I am trying with some success to be good natured about your little lecture on symbolic logic. I suppose it is probable from four years of philosophyjin seminary that I probably have had as much exposure to it as you! I agree that the Glossary does not have the force of an Article in the Code, and it is most unfortunate that the Code itself apparently contains no definition of nomen nudum. However, I still feel that the wording of the definition in the Glossary supports my point of view, for whatever the Glossary may be worth! The key word is the word "because". The definition states that a nomen nudum is not a name because not validly published. In other words, the quality of not being validly published is what causes a nomen nudum to be a nomen nudum. Nothing is said about anything else being an effective cause of nakedness for a name. It is therefore legitimate on the basis of the logic of the definition to conclude that lack of valid publication is the one effective cause of nomenclatural nudity. Now, it may well be that this definition is in error, and I should note that Tate Ames agrees with your interpretation, but the wording of the definition in the Glossary certainly doesn't support your case.

I have shown your copy of the letter from Grebe to Tate Ames, but he still maintains that as far as he knows he does not have the negative of the Horst thesis. Keeping good track of things is not one of Tate's virtues, as I believe even he would admit!

I have other things to discuss with you, but I think it is too confusing to include them also in this letter, and I will handle them separately.

Best wishes of the season to all of you.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi
Enclosure: translation of Malyavkina page

Hunt Institute for Botanical Documentation

Dec. 74

Varsonius

Dear Al,

While the typist is working on my other note, I will address myself in longhand to the question of Maljavkina. I am in turmoil over this: on the one hand you and Tate (in CFSP, v. 38) leave open the possibility that her 1949 paper contains many invalid genera — by saying that if they are invalid you have no intention to validate them. On the other hand you treat them as valid — why otherwise go to the bother of cataloguing the species if they are not valid because not assigned to a valid genus?

As for diagnoses, you assert that the last two entries in the keyed dichotomous tables constitute the diagnosis. As you say: 'all these parcels of information comprise the generic description'. I consider that a proper generic description ought to contain all this information; but no such description was provided. As I do not read Russian sentences, I am at a disadvantage. But I wonder about p. 24 (or 25?) in Maljavkina 1949, under the heading "ПРАВИЛА ПОЛЬЗОВАНИЯ": does she give any clues here as to what she considers a diagnosis or description? (Like Thomson & Pfug, who state in their introduction that the first illustration always

good!

Hunt Institute for Botanical Documentation

is the holotype). I would half go along with you, if not those tables are a mix-muddle that sometimes lead to a genus, other times to a species or even a variety. Any distillation of a generic diagnosis out of such basic material is a creative act (unless Maljavkina gave instructions as to how to go about it.

A propos, it seems to me, that you misunderstand the working of these keys. The first entry has number ①, with two alternatives; the second, alternative for this and every set of choices is marked "0".

When you say: "0.... 4" is the last entry for Acuminella, the "4" identifies numerically the subgroup Ovalifera, which contains the single genus Acuminella. The "0" does not lead you back at all. However, that entry above, which referred you to the set of choices "6" (which is the "0", or alternative choice, under "4"), contains the additive bit of information that pertains to Acuminella. Maljavkina ordered her subgroups numerically, and gives this number in her keys.

6 applies to Schistona!

So much for this mess: maybe p. 24 (25?) contains a clue as to what to do; but I just cannot buy your case that Maljavkina (1949) is valid in toto (pro parte, the monotypic genera must be accepted). It is telling as to their utility, that few Russian authors have adopted her names.

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Finally, regarding your comments on the Glossary (Regnum Vegetabile, v. 56), of which you sent a xeroxed sample page. As it is not a part of the Code, it carries little authority. It really should be an integral part of the Code. Again, specifically now: it gives that "Nomen nudum, a 'naked name' is not a name in the sense of the Code because not validly published".

But it does not follow that each "not validly published name" can be called a 'Nomen nudum' (Like: table = furniture
furniture ≠ table).

The glossary is helpful, but in this instance (and others) could stand a bit more detail, and especially: examples. Parables still make the best teaching!

In haste I now take leave! Best regards to all.

Hunt Institute for Botanical Documentation

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PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY

PRODUCTION DEPARTMENT

December 4, 1974

Dr. A. Traverse
Deike Bldg.
University Park, Penn.
U. S. A. 16802

Dear Al:

Thank you for the copy of Imgrund; I have no interest in the full thesis, inasmuch it consisted of a type script and carbon copies, and hence no effective publication. I'm happy though with this one page, which confirms that.

I shall write Horst, who is retired and well and living in Germany. If I get a copy of his thesis, I will let you share; you deserve something for the expenses you put the Biology Dept. to.

Have you people made up your mind regarding my exegesis of Hymenozonotriletes?

Best regards,

Jan Jansonius

JJ/er

Hunt Institute for Botanical Documentation

October 29, 1974

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Street S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

Just a note because it suddenly popped into my mind regarding the "official" glossary of terms put out by the IAPT. This was published by Regnum Vegetabile, Vol. 56, 1968. I carry mine in a pocket in the back of my copy of the Code, which you can do if you cut it down a bit on a paper cutter. Unfortunately, the definitions are not all as good as they might be, and a good example is what they do with nomen nudum. The page in question I enclose in xerox copy. The part of the definition which follows the semicolon is really all that they need and should not have got into the question of description and diagnosis because a nude name is nude for any reason whatsoever that precludes it from being validly published.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi
Enclosure: page from Taxon glossary

Hunt Institute for Botanical Documentation

October 25, 1974

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

It was certainly fun to be with you and Betty at the time of the recent convention, and we covered a lot of ground in our conversations. However, there were other things that needed discussion which we failed to bring up. For example, there was the matter of the "negative"of Horst's 1943 thesis". It appears that Tate does have such a thing. He had left the office by the time your letter of 8 October arrived, and I was unable to find the negative among his things. We will have it sent to you in the near future but ~~our~~ are hanging on to it until our photographer can get free to make us copies. I suppose that means that there will be something like a three-week delay because the photographer is absolutely swamped with material for the Spackman-lead field trip for the Miami meeting.

No time to comment in detail at the moment, but thank you for the comments on the Potonié proposal.

We just received the official approval of the Soviet Union for your nomination to the Committee!

Best wishes and thanks again for your hospitality.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi

Hunt Institute for Botanical Documentation

September 5, 1974

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

Thank you for the recent additional batch of the famous Jansonius cards. I have them all mounted on separate sheets of paper and have inserted in three-hole notebooks. We are making very good use of the information and truly appreciate being allowed access to the cards before their publication.

Thank you very much. Hope to see you in Calgary.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi

Hunt Institute for Botanical Documentation

May 7, 1974

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

Thank you for the recent further additional pages for the "Jansonius card file". These will be of use to me in my continuing effort to provide the generic list which the Smithsonian wants. I have so far sent them 1250 annotated names with numbers.

I wish Stan could find some time to let me know his latest feeling regarding a visit to Penn State to sit in on Harvey's comprehansive examination and give us the benefit of advice for Harvey's Ph.D. thesis. I really would prefer it if Stan could come twice, once somewhat before the comprehensive exam so that we could set up Harvey's program and have it going, and again for the exam. However, I am realistic enough to know that it may be impossible to justify such an ambitious travel program. In any case, I do need to know what to do so we can get the exam set up and make other plans. Naturally I would like it if Stan could also give a seminar while he is here along the lines that you did for us on scolecodonts a couple of years ago. That reminds me--the State College Choral Society had its annual concert the other night (Bach B-Minor Mass), and it naturally reminded me of your visit here when ye attended the concert together. This was their 25th anniversary performance, and they imported several expensive soloists. The concert is alleged to have been a huge success. With my knowledge of music, all I can attest is that they worked very hard and there was a large crowd!

Best wishes as always.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi

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PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY

PRODUCTION DEPARTMENT

April 24, 1974

Dr. A. Traverse
529 Deike Bldg.
University Park, Penn.
U. S. A. 16802

Dear Al:

In haste, I want to reply briefly to your last letters. We wish you luck and success in your quest for further sustenance for the CFSP.

I decided to commit myself to cooperation with Len Hills because he has experience in dealing with the local community of printers, and succeeded in setting up a scheme to finance the deal. Though fully aware of the expertise in nomenclature that you (and Tate) could offer, I considered that it might be more important to make some physical progress: I'd like to get this project off my back as soon as possible. Your earlier suggestion of an "official number" for each card is still valid. However, until the file is completed and ready for reproduction, I want to hold off. It would be nice (and easy to check) if the original numbering coincides with the alphabetic order. The idea of a "CFSP ref." is good, and if time permits we may try to insert that.

Don't feel apologetic re "Hamipollenites"; I just wanted to inform you as yet of what had happened.

Best regards, also for Tate.

Jan Jansonius

JJ/er

Hunt Institute for Botanical Documentation

March 15, 1974

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

Thanks for yours of 14 II. It seems to be the season for bad news from Calgary--Stan can fill you in on his contributions thereto (which will all sort out, we are sure). I am especially sorry to hear that we will not be directly involved in the generic card deal. I suppose you must have got a good deal from C.S.P.G. I do believe that we could have helped assure a good (better?) product.

Tate and I have discussed your objections to Volume 37, CPSP. Let me say at the outset that I am sorry we did not send proofs of the pages to you before the fact. I have started doing that in the case of still-extant authors, as you know from receiving such pages for Volume 38 (Corollina, etc.). (Unfortunately, there will be no more communications from "R. Pot." He died January 26. I was very sorry to hear this.) We did not have a copy of the LRW-JJ letter, of course. We are not sure we understand the nuances of your comments. When we were preparing the material we could only compare the orthography you used in print with that Wilson published. Your letter, which is very interesting and much appreciated for our archives, "thickens" the plot. Wilson was the source of both Hamiapollenites and Hamipollenites spellings! I'm sorry that we imply that you made it up. As it turned out to be superfluous anyway, I guess that no great harm is done.

We are, on the other hand, convicted by your comments regarding the statement that somehow slipped into our coverage that you had included both non-saccate and saccate forms in the genus. We simply are unable to figure out how that crept in--perhaps by way of a third author? We no longer have any memory and can only apologize at this point.

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Regarding the placement of the generic diagnosis, I'm sure you realize that the Catalog is a specific catalog in the first instance, and we put the generic description with the type species if our coverage includes the type species. Otherwise, the generic information is put with the species that includes the other most detailed information. Admittedly, it is a bit difficult to decide which species we should pick for this "honor", and we even are afraid sometimes that it might involve accidental selection of a lectotypic species for a generic name. You will note, that we are always careful (or at least intend to be) to disclaim any intention of such accidental typification).

Hoping you will forgive us for the boo-boo regarding Hamiapollenites and that our other explanations are understandable, I am with best wishes

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi
cc H.G. Ames

P.S. One aspect of the generic cards business we should perhaps still discuss is re my work for Smithsonian on which you helped me--they are having me assign a number to each generic name--could that be worked into cards? Also--how about a "CFSP-ref." (e.g.: CFSP 32:165)?

Hunt Institute for Botanical Documentation

February 20, 1974

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

This is further to our previous talk about a "sustaining subscription". We decided not to "bill" you for this money for 1973 but to wait until calendar 1974, and as you directed us in your letter of 12 June, 1973, we have now submitted the invoice you mentioned. Unfortunately, we did not get many other favorable responses, but I think that the fact that you were willing to help us will be a strong bargaining point with some of the other companies.

Thank you again for your help in this and so many other ways.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi

Hunt Institute for Botanical Documentation

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PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY
WESTERN REGION PRODUCING DEPARTMENT

February 14, 1974

Dr. A. Traverse
Deike Bldg.
University Park
Pa. 16802 U.S.A.

Dear Al:

A quick note between rush jobs.

Enclosed are a number of xerox copies for new genera, or corrections/ updates for older ones. New is Crassitudisporites Hiltmann 1967, which I think covers our embryonic "Anagrammites". One more issue we have to contend with, but this one may solve our whole problem! re Hamiapollenites

Received today vol. 37 of the CFPS. I take exception to two statements on p.179-180. I did not elect my own orthography but used that given to me by Wilson in a letter. Because LRW is such a poor correspondent, I was all set to use my own data to validate the concept, but renamed it "Discussion" instead of "Diagnosis", when just before the proofs LRW wrote me that he would be first.

Further you state that my description would include non-saccate forms, but that is not so: I only said that the sacci were proportionally small, and characteristically "terminal" (i.e. equatorial, no overlap). I consider Distriatites different: in structuring of saccus wall, appearance of c.b.

Another point: I do not quite understand why my 1962 generic diagnosis (as well as that by Wilson) for Vittatina occur under the heading "Vittatina lata". At best it is poorly placed, and - without the original heading - hard-to-understand information.

Thank you for mentioning Platysaccus; no entry either in my xerox file. I will make a new card shortly. Work has now started in earnest on the genera file, which we hope to publish this year, under auspices of the Can. Soc. Petr. Geol.

Best regards for Tate and yourself,

Jan Jansonius

Enclosures:
JJ/er

Hunt Institute for Botanical Documentation

STATE OF OKLAHOMA
OKLAHOMA GEOLOGICAL SURVEY
UNIVERSITY OF OKLAHOMA
PHONE JEFFERSON 6-0900. EXTENSION 2081 OR 2082
NORMAN, OKLAHOMA

August 3, 1961

Dr. J. Jansonius
Exploration Research Department
Calgary, Alberta
Canada

Dear Dr. Jansonius:

The copy of your company report on the "Palynology of the Permian and Triassic sediments, etc." has just arrived and I appreciate having it. You have done a fine piece of work on the floras and the publication will be very useful in furthering these studies. I am sorry I have been unable to work closer with you and it is unfortunate that we are duplicating Hamipollenites (your Hamaspora). The plates have been published more than a year and I suspect the remainder of Circular 49 will appear in September. You have had the same trouble as I have had with Vittatina and Luber's 1940 paper. We have not had any satisfaction from Luber or others in Russia about that paper, so like you, I have recognized V. subsaccata as the genotype. One of our Survey geologists is on his way to Russia and will attempt to secure more information about Luber's reported 1940 paper.

The little fossil that you have honored me with by name could be related to the desmid genus Staurastrum. For some time I have entertained the idea that this and several other desmid genera have a long geological history, and even may have had some marine species. Last year I found a number of Staurastrum fossils in the aquatic sediments of the Two Creeks forest bed (Pleistocene) that cannot be distinguished from one of our Sylvan shale (Ordovician) Veryhachium[species]. A note about these observations is to appear in the Oklahoma Geology Notes soon.

I hope that we shall have the opportunity of meeting in the near future.

Sincerely yours,

L. R. Wilson

L. R. Wilson,
Professor of Geology

only answer to 2 requests (Jan, Febr. 1961) for information

LRW./fw

Hunt Institute for Botanical Documentation

January 17, 1974

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue S.E.
Calgary, Alberta
Canada T2G 2B3

Dear Jan:

In working with the materials I have obtained from you last June and since, I find no card for Platysaccus. Is this because there wasn't one, or because I missed it? In any case, I would appreciate your latest ideas on the subject. You note that I have carefully refrained from giving the generic name an author citation. That is part of the problem, of course!

I am very discouraged at times about how little time there is available to me to forge ahead on my work on a generic list, but I am grabbing an hour here and there, and you will be hearing from me some more about it one of these days. I hope all goes well with you. I still frequently think about the delicious hours I had in Calgary early last summer!

Best wishes to you all.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

November 5, 1973

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada T2G-2B3

Dear Jan:

Thanks for yours of 26 October. I was very sorry that neither you nor Stan could make it to Anaheim. I thought it was a pretty good meeting, and besides I wanted a chance to chat with one or both of you. I guess I will probably be hearing separately from Stan regarding Harvey Zeiss' progress. I especially wonder whether he can't promote a trip here to meet with Zeiss and to give a talk to the palynologiaal graduate students as you did a while back. You might mention this to him. All I need is some indication of when would be the best time, and I will then see that an official invitation is issued. I do think that we must plan for Zeiss to attend the AASP meeting in Calgary next October and stay a few extra days for discussions.

Now about the famous "Corrugatisporites-?". I was fairly certain that Stafleu would render the decision he did because we have had a number of other similar cases, and the opinion in Utrecht seems to be that a hard line must be taken on this issue in order to keep the lids on several dozen pandoras' boxes. Your letter seems to imply that you do not understand, but I am sure that you do that botany recognizes no "commissions" in nomenclatural matters. We have the Code, and taxonomic opinion, and nothing more. There is, therefore, nobody to whom to direct a question regarding "formal decision". Actually, the closest thing there is is the Committee on Nomenclature of Fossil Plants, of which yours truly happens to be the reigning secretary, but I have no authority to make any bidding decision, except in matters such as the conservation of generic names and a few other such things, and even in those areas I am able to exercise only some influence, but the Committee as a whole makes the decision. However, it has been my experience now after seven years of work on the Catalog of Fossil Spores and Pollen that if the guy in

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Utrecht, namely Stafleu these days, makes a ruling, that is ordinarily tantamount to a "formal decision". I would suggest that it would be useful for you to write Stafleu expressing your opinion. I am pretty sure that the answer you get will be that we have to maintain the letter of the law, and the letter of the law says that a new combination requires a "full and direct reference". Isn't life complicated?

I dontt recall Stan writing me about the PEI mementos, but I will greatly appreciate them when they come and of course want to reimburse Stan for whatever they cost.

I have had a long and interesting letter from Samoilovich regarding the question of typification of Corollina, and at the moment I would say that it seems evident that "topotype" material for the genus is available, even though Malyavkina's slides no longer seem to exist. At the moment I am still pretty much convinced that Corollina should be the generic name for the whole ball of wax. I wish you could have heard my paper on the subject in Anaheim so that you could have argued with me.

Regards to all.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

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PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY
WESTERN REGION PRODUCING DEPARTMENT

October 26, 1973

Dr. A. Traverse
529 Deike Building
UNIVERSITY PARK, Pennsylvania 16802!
U. S. A.

Dear Al:

John Sulek transmitted your concern re our note on "Corrugatisporites". The cause for the hiatus in our correspondence was: 1/ lack of time, demands for service work before deadlines, as well as some loose ends that had to be dispatched. 2/ Also, since Stafleu's "snap decision", I feel we are in an impasse. I do not agree at all with him, since there is no ambiguity, only the lack of form or style which is not explicitly required in the Code. I therefore would apply, to whatever commission that handles such ticklish matters, for a formal decision. Do you know to whom I should address myself?

I trust Stan wrote you that the PEI mementos are due in shortly, and then will be forwarded?

Best regards,

Jan Jansonius

JJ/bam

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEIKE BUILDING
UNIVERSITY PARK, PENNSYLVANIA 16802

College of Earth and Mineral Sciences
Department of Geosciences
Palynological Laboratories

Area Code 814
865-6543
865-2342

August 29, 1973

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada T2G 2B3

Dear Jan:

Thanks for yours of 22 August which, like mine, took five days to arrive, despite air mail. Your comment about using a single envelope is certainly common sense, but I hasten to defend my action by explaining that it is a product of the way things work when one uses Dictaphone as much as I do. The letters were not dictated on the same day, or even within a couple of days of each other, but they were all typed the same day. Usually I go ahead and stuff all three letters in one envelope, but if things are somewhat hurried it isn't worth the trouble.

There really is no necessity to "conserve" Pollenites, if we think it's desirable to have it around. All that has to be done is to provide a description and designate a lectotype. Right? As you say, conservation of palynological genera will probably not happen in our lifetime. You just can't make a good case for conserving generic names which are used by so few people. I remember that good old Bill Sarjeant practically came unglued at the seams when I recommended to the committee on fossil plant names that his proposal to conserve Hystrichosphaera be turned down, but it was really the only thing to do within the "spirit of the Code". I have written Stafleu asking for the information about how many palynologists subscribe to Taxon. I agree with you that that would be most interesting. When the question of the valid publication of Corrugatisporites solidus came up, I flashed off a hand-written letter immediately to Stafleu to see whether he would agree with me that this appears on the surface of it to be a fatal blow to our original conclusions. As you can see from the enclosure, he scribbled an answer across the bottom of my letter, and returned it to me. Whether it makes sense or not,

* one of problems with dictation —
you have to remember to say "paragraph"

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and I have considerable reservation about that, it does seem clear that Art. 33 requires both a proper bibliographic citation, and a citation of the complete basionym, in this case "Sporites solidus R. Pot. 1934". (or at least "Sporites solidus")

I will look forward to hearing from you both whether you "give up" on the question of valid publication of C. solidus and what you would propose to do now, if you agree.

Regardless of the kind comments you made about the Corollina pages, I still think that some softening is necessary. Frank is of course right that in the Soviet Union they do not much use the name Corollina, but that is a matter of no importance, as I am sure you realize. Samoilovich, in fact, wrote me a letter not long ago complaining about the nonuse of the name Corollina, and I suspect that one of the things that goes on is the Moscow-Leningrad competition. Corollina is "Leningrad name".

I do agree with you fully that it would be most desirable to see Malyavkina's types, and I am going to write Samoilovich immediately requesting information about their existence. Frankly, on the basis of other things I have heard, I doubt that there are such things around. Nevertheless, the concept for a combined genus for Corollina, Circulina, and Classopollis is to be found in Malyavkina's 1949 contribution. Corollina and Circulina were definitely validly published (typification was not required, you will remember), and I think that we are at perfect liberty to use the name and designate a lectotype if that be necessary. Actually, in view of the broad interpretation I would give the genus, I would think that Malyavkina's illustration could stand.

We--Tate and I--do not find ourselves in agreement with you about the general validity of Malyavkina's publication. We find nothing in the Code that requires a generic description to appear on any one page, and although we admit that her procedure was eccentric, it was hardly any more so than that of Thompson & Pflug, or some of various other authors' tabular presentations. You seem to feel otherwise, and we would consider it very illuminating if you could explain with quotation of chapter and verse from the ICBN why you do not agree that a description is a description, even if it appears broken up on several different pages, providing that it is clearly labelled. In fact, of all the things in the letter, this is the most critical, since it would affect the publication of our current volumes of the Catalog (38-40).

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I think that the fact that we tend to come down on opposite sides of the fence is most useful and means that our nomenclatural collaboration will be productive.

Best wishes to all.

Yours very truly,

AT

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Copy : Jones

Hunt Institute for Botanical Documentation

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PRODUCTION RESEARCH AND
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WESTERN REGION PRODUCING DEPARTMENT

August 22, 1973

Dr. A. Traverse
Deike Building
University
UNIVERSITY PARK, Pennsylvania 16802
U. S. A.

Dear Al:

Thank you for your 3 letters of the 15th, which arrived here 5 days later. You might as well have used a single envelope!

My proposal (suggestion, rather) that *Pollenites* be conserved would save the trouble of tidying up all the cases where species became valid only (much?) later. Also, it would be a test case: it probably would not be approved at the Botanical Congress, and that would make it very clear. Even when accepted, the more or less obligate selection of a lectotype would immediately restrict the genus drastically; I do not think waste basket taxa are much in style nowadays.

Do you have any notion how many palynologists are subscribing to Taxon? It would be interesting to know. However, it may be unfair to hide our paper there. The point you brought up (or rather: Tate) regarding Art. 33 is "very interestink". One of the things is that I do not know what is meant in the Code by "clear indication". Obviously, Thomson & Pflug could hardly have been more clear in indicating what specimen they referred to (the "full and direct reference"). If with "clear indication" is meant "explicit citation", then those words should have been used. However, in paragraph 2 (Examples) it appears that the "clear indication" in Linnaeus' *Species Plantarum* is not a fully spelled out new combination, but the epithet separate in the margin opposite the new generic name. In view of these things, I consider that Thomson & Pflug could not have been less ambiguous in their reference to the basionym except for a full citation of the old combination. I think *Corrugatisporites solidus* (Pot.) is valid as per Thomson & Pflug 1953. I will ask Stan for his opinion, when he comes back from vacation.

Regarding the CFSP pages on *Corollina* etc. I did not think that Tate had been unduly sharp - no need to worry or to change anything. And my attitude regarding Maljavkina's work remains determined by two things: I cannot read (and hence evaluate) the Russian text, and I think the illustrations (of the holotypes?) are awful and do not allow meaningful comparison in most cases. Frank found an argument strengthening

.../2

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this attitude: even in Russia the genera *Corollina, Circulina* are not used, whereas *Classopollis* is. You may be right in forcing the issue into the open, but more meaningful would be a set of photomicrographs of the various (and multitudinous!) holotypes accompanying your re-organized (not original!) descriptions.

Why do you not write to Klaus, and ask what he meant by "Hilfstypus"? You are in the same quandary: you want to emend the diagnosis of *Corollina* so it includes *Classopollis* etc., but on what will you base the taxon. Have you seen the type, or a good illustration of it? If you "make do" (= sich behelfen mit) with other specimens, or your accumulated experience, those will be your Hilfstypen.

Maljavkina probably intended to publish all her taxa; they were effectively published, but I do not know about the requirements of valid publication. If taxa are not accompanied by a description, they must be accompanied by a reference to a previous effectively published description of that taxon (and in that rank). If the necessary (i.e. adequate) description in Maljavkina's case must be collated from an analysis in the form of dichotomous determination keys, I would think that it gave me a technical excuse to invalidate those names, and I would be happy to suppress them. Those that are monotypic (still quite a few) often must be accepted (if the species was described adequately).

We will wait for Stafleu's comment re *C. solidus*.

Best regards, also to Tate,

Sincerely,

Jan Jansonius

JJ/bam

Hunt Institute for Botanical Documentation

August 15, 1973

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada T2G 2B3

Dear Jan:

On the flap of the envelope in which you sent the copy of the Corrugatisporites paper you commented on the Pollenites question. I can find no evidence that I ever responded to that, and I think I'll handle it with a separate letter--separate, that is, from the one which will convey the Corrugatisporites paper back to you with my comments, which should be any day now!

As it happens, Ames and I have been preparing material for supplementary volumes to the Catalog, and the first supplementary volume will include comments on Pollenites, Sporites, Sporonites, Sporangites, among others. My conclusion was that Pollenites certainly was not validly published--or did you ever find a generic description? This causes some problems regarding species which were therefore also not validly published. Some of them have become type species of this and that, and that will require tidying up. I do not see any point in validating Pollenites at this late date, unless I suppose there is a place for a genus that could be used in the wastepaper basket way that Schopf contemplated for Sporites (which, as I see it, is a later synonym of Sporangites!).

That takes care of the envelope flap--now for the main subject of the envelope, which will require a separate letter. Best wishes to everybody.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

August 15, 1973

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada T2G 2B3

Dear Jan:

Thanks for yours of 9 August with the upgraded version of the note on "Anagrammites". I am not avoiding you on this subject, but am still meditating about it. You will not have to wait much longer, however, as I am beginning to emerge from the accumulated correspondence and so on that awaited my return from Nova Scotia the other day after nearly three weeks absence, and I should be able to get this thing off to you quickly. Regarding the technical matters, I can tell you right away that Taxon is really the place for this sort of publication from a systematic point of view, but I share your fear that it is not read by many palynologists (who could blame them?). Therefore, I would fully support the Pollen et Spores suggestion. I see no reason to acknowledge the "Smithsonian", since they wanted me to go up for quite a different reason, and they are going to get their money's worth whenever I can get around to finishing the generic work for them. However, I am delighted that you did mention it in the letter because it reminds me that they still owe me some money!

I intend in this letter to finish off the question of the CFSP pages. I sent them to you hoping that you might be willing to comment in some detail about the particular points which were made, and especially to alert you that some rather sharp sentences regarding one of your publications on the matter was inserted by my friend Ames. It is my intention to soften up those references because I am not that sure that we are right!

You have raised some points in your letter which are very critical to us in the treatment we have given Malyavkina 1949, in our volume 38, and there is still time for you to respond. You are quite right,

Hunt Institute for Botanical Documentation

per your second paragraph, that we have collated descriptions (I steadfastly refuse to use the term "diagnosis" for such material) for Malyavkina's paper. I do see anything in the ICBN which compels an author to put his description all on one page, and it is quite obvious that Malyavkina intended the various parts of her "index" to comprise a description. I do not see how under the requirements of the current Code you can possibly get around the fact that Malyavkina did provide descriptions for the genera which she presented. I do not see how you can urge thatfact that blocks of the description appear on different pages (clearly labelled as to their nature) invalidates the provision of a description.

Now, for the second part of your reaction, presented in your third paragraph--in brief, my current feeling is (based on our study of the Hartford Basin material, so rich in "Circumpollis") both the "Classopollis" and the "Circulina" things should be put in a single genus. It is quite clear to me that the concept of that genus is obviously present in Malyavkina's Corollina and Circulina presentations in 1949. Given that fact, I see no reason why one should not elect Corollina as the name for the whole ball of wax, reduce everything else to synonymy with it, transfer other species to it, and provide an emended diagnosis for the genus.

We still think that Klaus's "Hilfstypus" could be interpreted as neotypes for the type species. Why not?

A translation of Malyavkina 1949 would be a fine idea. Perhaps I can ask Harvey about it in Anaheim.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEIKE BUILDING
UNIVERSITY PARK, PENNSYLVANIA 16802

College of Earth and Mineral Sciences
Department of Geosciences
Palynological Laboratories

Area Code 814
865-6543
865-2342

August 15, 1973

Dear Jan & Stan,

Have just got around to detailed examination of the M.S. I have many ± minor comments, which I will send along as soon as I hear from you about a major problem which Ames has called to my attention--I like to think I might have picked it up myself after getting all the literature together, etc.--but Tate spotted it first: it would appear, unfortunately, that *Corrugatisporites solidus* was not published validly as a n. comb. in T. & Pfl. 1953, per 1972 ICBN Art. 33, para. 4. I am not as sure as Tate on this and am also appealing to Stafleu for a ruling*, but it would appear that T. & Pfl. did not "clearly indicate" the basionym for "*solidus*", i.e., "*Sporites solidus* Pot 1934". If this conclusion is correct, we are in trouble, not so!? In that case, *S. solidus* was available to typify *Lygodiosporites*?! Let me hear as soon as possible on this clanger.

Best regards to all.

Al

Alfred Traverse

* see enclosure

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WESTERN REGION PRODUCING DEPARTMENT

August 9, 1973

Dr. A. Traverse
Deike Building
University
UNIVERSITY PARK, Pennsylvania 16802
U. S. A.

Dear Al:

Enclosed find a new and upgraded version of the note dealing with Anagrammites. I want to add diagrammatic sketches of the types, but no photographs. I would like your opinion as soon as possible; once a m.s. has gotten this far, I become impatient and want to finalize it and send it out. Taxon may be a suitable magazine, but Pollen et Spores would probably reach a wider audience? I wonder if you want to acknowledge the Smithsonian travel fund or something.

Yesterday I received your xeroxed pages of CFSP vol. 38, and I briefly discussed them with Stan. Part of my problem is that I do not read Russian, hence cannot readily evaluate Maljavkina's text. From appearances it seems, however, that you collated an "original" diagnosis for Circulina from disjunct passages of a determination key that was part of the original text. I consider that not a original diagnosis (which by the Code then in force was not required! Potonié still honors the old Codes).

However, the main objection that Stan and I have against (the use of) Maljavkina's names is that the illustration is too poor to serve as a standard with which other morphologies can be compared. This is not merely a nomenclatural matter. What is needed is for some Russian to rephotograph and redescribe the types of a number of these controversial Maljavkina names. Even if you decide that they are all valid, how are you going to apply them without wild gambles or extraneous projections (what the Germans call "hineininterpretieren"). This of course was Klaus's quandary. He really worked with Balme's material (which he called "Hilfstypen") as a crutch to enable him to go where Maljavkina gave no support. In a broad sense Corollina (or Circulina) may include all 3 genera here under discussion (Corollina, Circulina, Classopollis); but when one takes narrower concepts, I do not know what

.../2

Hunt Institute for Botanical Documentation

Maljavkina really circumscribed. This may be due to my inability to read Russian; but the illustrations certainly show no diagnostic features on a detailed level.

"Hilfstypus" is neither neotype nor cotype, in the sense of the Code; but poor Klaus needed something!

I am sorry that we still are "rather negative" on this matter -- but what can one do?

Harry Leffingwell has organized some Russian translations for a group of oil companies. Maybe he would consider translating Maljavkina, now that you treat it in the CFSP?

Best regards,

Jan Jansonius

JJ/bam

Encl.

Hunt Institute for Botanical Documentation

June 29, 1973

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada T2G 2B3

Dear Jan:

Since I dictated the last letter, this mornigg yours of 20 June has arrived. The package of genera cards runs from H through M. I think I said enough about that in the previous letter.

A note on Corrugatisporites, etc., seems much to the point, and if you would like me to be joint author that would be fine. I am not sure I made that much of a contribution, but I certainly enjoyed discussing the rather thorny problem. Unfortunately, I get the impression from our Catalog work that there are many similarly troublesome problems waiting to be discussed. Well, perhaps it is not so unfortunate after all. Maybe I can even promote some additional trips to Calgary or you to State College on this sort of thing! As I said in the other letter, I really did enjoy being with you all, and could hardly believe that the time was gone when it was. I do hope you will be willing to have me again later on in the generic project. By the way, I am making a further printing inquiry about the cards, hoping that we might get a figure closer to your liking. However, I don't want you to get the impression that I will be in any way hurt if you decide to have them printed elsewhere. It was only that I thought that perhaps we might be able to make some contribution to the project if theyywere under CFSP auspices.

I'm glad to hear that you liked the candle. I was really struck by that design and bought several of them at Chinook Ridge. I gave the others to relatives of mine in the Edmonton area. Best wishes from all of us here.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

June 28, 1973

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada T2G 2B3

Dear Jan:

Thank you so much for the kindnesses extended to me during my recent visit. We are getting the dictabelts typed off, and the generic list work is going forward as time permits. I look forward to hearing from you about how you feel regarding the question of letting me have further of the cards per our previous discussions. Whoops!--the mail girl just came in with the package containing the batch of cards which you sent. I am slightly appalled at the cost of the postage, but I suppose we will make it worthwhile to somebody some day! What I will do is return the cards with comments, and that will be a sign that the procedure is working.

Also, please extend my thanks to Betty and to your children for a delightful evening at your home before I left to come back. I do hope that you were successful in getting the candle from the motel room which I wanted you to have as a jesture of gratitude. Your family is simply marvelous, and I greatly admire your ability to maintain your home as an island of rationality in an irrational world.

Looking forward to many future contacts, I am

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

June 73

TO A.T.
on envelope-flap from Corrugatisporites paper.

PS I guess Potonié must have felt that Pollenites was a valid genus; it probably was, too, at the time, but not any more according to the present Code. Should we therefore preserve it? JJ

Jansonius

Hunt Institute for Botanical Documentation

IMPERIAL OIL LIMITED

339 - 50 AVENUE SOUTH EAST, CALGARY, ALBERTA T2G 2B3

PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY
WESTERN REGION PRODUCING DEPARTMENT

June 20, 1973

Dr. A. Traverse
Dept. of Geology, Palynology Laboratory
Pennsylvania State University
UNIVERSITY PARK, Pennsylvania 16802
U. S. A.

Dear Al:

By separate mail I am sending xerox copies of my m.s. genera cards. I trust you will be able to decipher them, but if you have any questions, you can return those to me for clarification. By the same token, if you have any remarks pertinent to the information on them, I would appreciate if you will point them out to me.

I am working on a short m.s. note on the question of Corrugatisporites etc., which we discussed; joint authorship Jansonius, Pocock & Traverse if you will agree: I will submit it to your scrutiny.

We all enjoyed your visit here, which was fruitful to us, as I trust it was for your. Best regards from us all,

Sincerely,

Jan Jansonius

JJ/bam

Al, we were nearly embarrassed by the beautiful candle we picked up for you: it was really too much. We hope we didn't do your Betty out of it – we could have mailed it for you. Thanks you again; my Bettie really goes for candles.

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339 - 50 AVENUE SOUTH EAST, CALGARY, ALBERTA T2G 2B3

PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY
WESTERN REGION PRODUCING DEPARTMENT

May 2, 1973

Dr. A. Traverse
Dept. of Geology
Palynology Laboratory
Pennsylvania State University
UNIVERSITY PARK, Pennsylvania 16802
U. S. A.

Dear Al:

I checked with Stan Pocock, and the date of your visit (June 10-15) is o.k. with him also.

I checked some hotels/motels nearby, and came up with the following (in order of reputation, but all acceptable).

Tradewinds (H), 6606 McLeod Trail; (252-2211). $13.00; near shopping, 20-25 minutes by foot.

403

Majestic Inn (H), 4510 McLeod Trail; new (243-5510). $14.00; 12 min.

Sherwood Inn (H), 5809 McLeod Trail; (255-7756). $13.00; 15 min., near shopping.

---X---

403-

Continental (M), 4637 McLeod Trail; (243-7791). $9.00; 10 min., has coffee shop; kitchenette.

Ranch (M), 5229 McLeod Trail; (255-4944). $10.00; 7 min., kitchenette; near small shopping centre.

For all these specify a room at the back, away from the traffic. Several smaller local firms have cheap rates on car rental, if you need that.

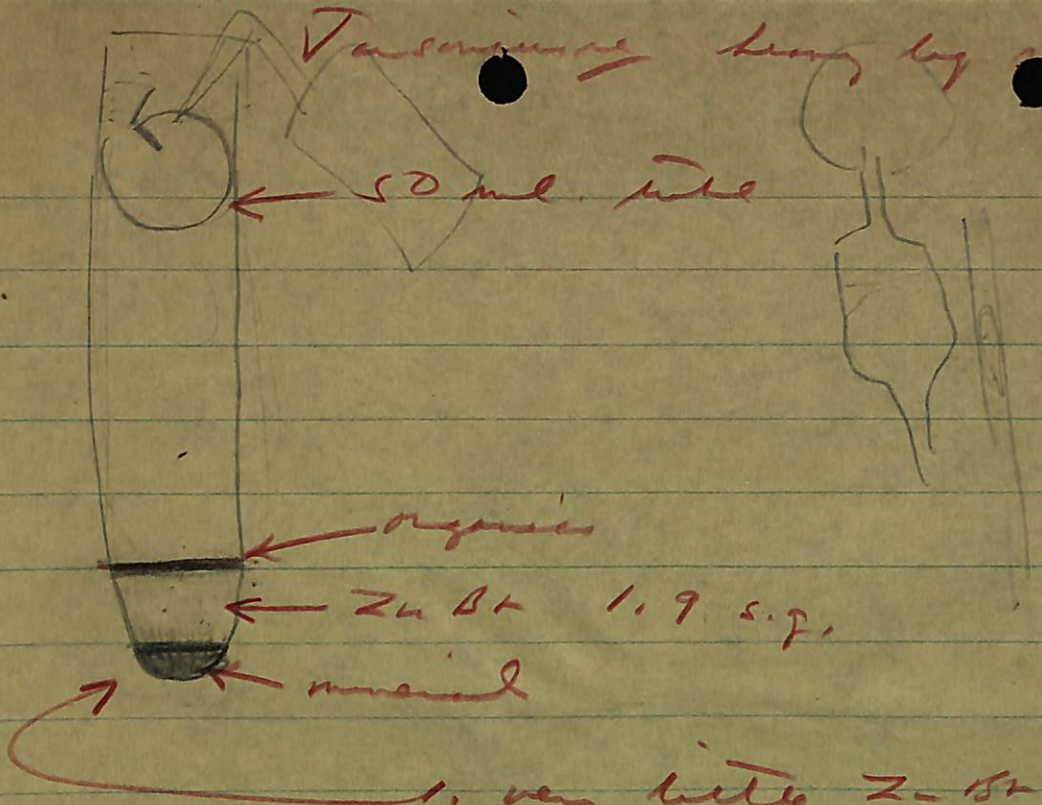
With best regards,

Sincerely,

Jan Jansonius

JJ/bam

Hunt Institute for Botanical Documentation



18-IV-72

file as correspondence.
Jansonius

Mezgebu
MCH

1. very little ZnBr2 should remain
2. spin 1/2 hr. at 800 rpm. (except to completely remove mucoid)
3. lubricate inside of tube w. spray bottle
4. invert tube slightly and quickly—
5. organics can be stored almost indefinitely

Hunt Institute for Botanical Documentation

November 16, 1972

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada

Dear Jan:

Thank you so much for letting me have pictures of scolecodonts and chitinozoans. As it turned out, I only used one of them, the smashing SEM picture of Xanthoprionssp.--partial apparatus. I am returning herewith the other pictures with my thanks. You are duly acknowledged in several places in the paper for this contribution!

Best wishes as always.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi
Enclosure: pictures

Hunt Institute for Botanical Documentation

November 8, 1972

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada

Dear Jan:

Believe it or not the nice pictures which you mentioned to me at Newport arrived in the mail today! I certainly am glad to have them, and I'm sure that at least one of two of them will find their way into the plate which I am preparing for the paper which I believe I mentioned in the original letter.

Thanks again.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

November 3, 1972

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada

Dear Jan:

It was good to have a chance to talk with you some more about the generic index, and I am already going ahead with inquiries about the financial side of the picture.

I have looked into the question of the card size per our discussion over breakfast last week. I discover as I had suspected that there are readily available on the market file cabinets and file boxes for 6x9 inch cards, and I think they would be a clear improvement over the 5x8 inch size which your samples are. For example, the sample card I have in front of me for Alisporites shows almost no margins, which is very undesirable. With the 6x9 inch card there would be the possibility of at least a half-inch margin and possibly the reduction required would not be so great either.

I am corresponding with the people at the Smithsonian who want me to work up the generic list to see if they don't have a little cash which would enable me to make a trip out to Calgary to compare what I come up with with what you already have, but even if they do have such money I suppose it would be months before I would be ready to make the excursion.

Looking forward to hearing from you regarding the suggested change in card size, I am

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

IMPERIAL OIL LIMITED
339 — 50 AVENUE SOUTH EAST, CALGARY 24, ALBERTA

PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY
WESTERN REGION PRODUCING DEPARTMENT

December 13, 1972

Dr. A. Traverse
Deike Building
University
UNIVERSITY PARK, Pennsylvania 16802
U. S. A.

Dear Al:

Thank you for yours of November 24, with a copy of your letter to Stafleu. I have no objection to cooperate with you - as far as I have time - on the Stafleu list of generic names. You should be able to pull most of them from your reference library. However, you are welcome to visit and check your list against my file. March 1973 will be alright, and we should be able to give you a desk to work on.

I have received several letters, all supporting publication of the genera cards. However, several indicate only that the lab or institution would buy one (or several) sets. A price near $90.00 will deter most individuals, but a price nearer $30.00 would seduce many. Thus I hope to find means to aim for the latter.

I am glad you received the photos of the scolecodonts; the spares came back to me.

Best wishes for a pleasant Christmas season and a happy New Year for Betty, you, and your family.

Sincerely,

Jan Jansonius

JJ/bam

PS I've looked at Maljavkina again, and I now think that you are right in considering the majority of names to be validly published. Do You know if there is a French or English translation around Somewhere?

Hunt Institute for Botanical Documentation

November 24, 1972

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue, S.E.
Calgary 24, Alberta
Canada

Dear Jan:

Dr. Stafleu recently sent me a copy of a letter he wrote to you acknowledging a sample set of generic cards. The enclosed letter to him also is probably mostly self-explanatory regarding the promise I probably foolheartily made to provide them with another kind of card, simply listing the genera, authors, and type species. I mentioned this to you in Newport. I do hope that you will feel that I am not trying to "pull a fast one" on you. It seemed to me that if I could undertake this little job you would be grateful to be free of the obligation so that you can go ahead with the much more important project on which you have embarked. However, it would seem foolish for me not to consult what you will already have prepared by, say, next March or so, and I have asked the Generic File people to provide me a little money for air fare to Calgary sometime in the spring so that I can work for a few days with your file and with you in order to get their cards in some kind of decent shape. I was thinking in terms of coming out during the recess between the winter and spring terms, which would be sometime in March. Would this be agreeable to you? If Imperial didn't like it, I could work in your basement or nearby Anglican church, or something.

I am going forward with inquiries about financing for the Generic Index cards which you are preparing, and really feel some confidence that it will work out.

Hunt Institute for Botanical Documentation

Do let me know if my idea regarding the trip in March would be acceptable. Best wishes as always.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi
Enclosure: letter
P.S. By the way, I should have emphasized that as far as I understand it there is no "credit line" involved in this project for Stafleu. It will be purely a labor of love as far as I understand.

Hunt Institute for Botanical Documentation

October 11, 1972

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada

Dear Jan:

I recently wrote a paper on the history of paleopalynology for which I have prepared a plate of representative "palynologic fossils". One aspect in which the plate is in my opinion deficient is regarding your special friends, chitinozoans and scolecodonts. Could you provide for me a few photomicrographs of each group? I would want glossy prints and a slip of paper for each indicating the name of the bug, stratigraphic position (very general), geographic location, and magnification. The magnification should preferably be such that the photos would be on the same order of size as those for the spores in the plate--I suppose five centimeters in diameter or less. I only need two or three good examples of each. Needless to say, the source will be duly acknowledged in the caption. Of course, the best solution to my problem would be if you happen to have a few of these things lying around spare because I really don't want to put you to any extra work, but I will very much appreciate your help in this matter. I would like to have them as soon as possible because I need to get the manuscript turned in. Several years ago Tony Jenkins gave me some beautiful SEM pictures of chitinozoans, but they are much too large. If he still has the negatives for those pictures, two or three of those had a more "normal" magnification would be highly welcome--or if he could loan me the negatives I could have them made (that goes for the scolecodonts pictures too). The negatives would be returned to you in a few days after receipt.

Hunt Institute for Botanical Documentation

Looking forward to seeing you in Rhode Island and hoping that you can help me in this business of preparing a proper plate for my historical paper, and with best wishes to Frank, Stan, Tony, and the others, I am

Yours very truly,

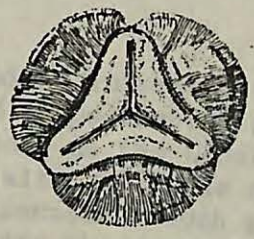
Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

CATALOG OF FOSSIL SPORES AND POLLEN

DEIKE BUILDING
THE PENNSYLVANIA STATE UNIVERSITY
UNIVERSITY PARK, PENNSYLVANIA 16802
U. S. A.



THE
INTERNATIONAL COMPENDIUM
OF
PALYNOLOGICAL SYSTEMATICS

August 18, 1972

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
CANADA

Dear Jan,

Al has forwarded a photo copy of your letter of 4 August to F. Staflue re. Art. 42 ICBN and *Caryae ?-pollenites*. Incidentally, I would like to see a copy of your manuscript to Taxon as I do not recall all of the points raised while you were here. Al is off on a + fortnight vacation so I'm not sure when you will receive his views in the matter.

Essentially, I agree with you that species assigned to *Pollenites* should not be recognized as valid until *Pollenites* can be established as a valid generic name and epithet are again attributed to the genus. Unfortunally, many authors have attributed numerous species to the genus and it might be possible to have *Pollenites* conserved--against what, I couldn't say! Secondly, I just can't rid myself of the idea that some joker may not have assigned a single new species to *Pollenites* in the period 1930-1952 in the "Lower Sublovian Paleophy_to_geological Bull." or some other journal of similar ilk.

In the case of *Caryae ?-pollenites*, I'm inclined to go along with Potonié that the name was not validly published in Potonié and Venitz, 1934--not on the grounds that the interrogation point suggested taxonomous doubt (see Art. 34, 1964 ICBN Note 1), but that such a symbol or abbreviation, implying toxonomic doubt, are conventionally employed in one of two standard positions. To do other wise, does not definitely indicate how the epithet or epithets of this particular combination are to be used which is a prerequisite of Art. 33, 1964 ICBN for valid publication.

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Facetiously, one might interpret the symbol "?" to be part of the name. It is hyphenated to *pollenites* and allied to *Carya* by the "e". Thus if we treated Potonié as the Linnius of Palynology we could transcribe his symbols also, with the following results: *Caryae interrogation-point-pollenites*. Such a name might not include any more alphabetic characters than some of the other orthographic pteratoids proposed in the past decade.

Seriously, like Jim Schopf when all else fails, I would step back ten yards and punt-contending that the generic name was clearly not published "in the spirit of the Code".

If the decision goes that the 1934 treatment in Pot & Venitz constitutes a descriptio generico-specifica please send me the orthography of the generic name thus validiated.

Should you find time, please take a look at Theirgart, 1949, re "Bennettitae-*Pollenites* complex" and tell me what to do. They have been presented in the CFSP 24-111 & 114, but the matter is due to come up again shortly. The following orthographies all appear as possible generic names: Bennettiteae-*Pollenites*
(?Bennettiteae-) *Pollenites*
(Bennettiteae) *Pollenites*
Pollenites

Aureal Cross was here at the time and would have gone with all being recognized as *Bennettiteae-pollenites*. I could have gone along except for the consistant capitalization of "P" in *Pollenites* and non-italicizing of "prefix" Bennettiteae in combination with the epithet *deformatus* and *lucifer*. Yet the "generic name" is used else where in a variety of ways. Really stretching a point, possibly one could again apply Art. 33. Fortuately, in CFSP we can always fall back upon that which was done in the original publication with or without commentary.

Good Luck & Best Wishes

H. Tate Ames

HTA/kai

Hunt Institute for Botanical Documentation

August 4, 1972

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.E
Calgary 24, Alberta
Canada

Dear Jan:

The slide which you sent arrived safely today, and your technique of packaging it seems to me to be great--worth publishing? I have been using Adams slide mailers, and experience about two-three percent breakage.

Good to hear that you're still working on the generic file. I do hope that you will keep the CFSP in mind as the eventual means of publication. All I have to have is concrete agreement between us, and I could start working on the question of financing. Perhaps we can discuss it in Rhode Island.

Best wishes as always.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

July 72

Dear Al,

Enclosed is the slide you wanted; I trust this method of shipping is effective, but let me know if it broke for I have more slides.
I'm busy with 1000 things (a.o. a move of office space, scolec. paper for Rhode Island ~~AASP~~ meet.) but keep up with the genera file. I'll make some dummies for hand-outs in October. In haste,

Jan J

Vansonius

Hunt Institute for Botanical Documentation

July 10, 1972

Mr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24
Alberta, Canada

Dear Jan,

Just noticed in the summary of your paper on Alisporites the statement that "a few slides...are available to interested workers". I guess, as one who is working on Mesozoic materials pretty regularly, and advising the work 66 others doing so, I would qualify as an "interested worker". In any case, I certainly would appreciate receiving one of the slides for incorporation in our study collections.

Best wishes as always.

Yours very truly

Alfred Traverse
Professor of Geology & Biology

AT/MJT

Hunt Institute for Botanical Documentation

June 22, 1972

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada

Dear Jan:

Yours of June 9 arrived here during my absence last week and has been quite interesting reading on my return! Let's take your questions and comments one at a time:

1. The price of $75 per set was something on the "guessy" side, but I suspect that it will turn out to be not far from what must be charged unless (which is not impossible) some subsidization could be found somewhere. Considering that even students must pay $25 for a copy of Tschudy & Scott, it doesn't really seem too extraordinarily expensive for what will be an exceedingly useful compilation.

2. I appear to have alarmed you unnecessarily regarding the question of "financing". I believe I have been telling you consistently that the Catalog of Fossil Spores and Pollen is a financially marginal operation. By this I mean that we seldom have a nickel more in our account than necessary to print the next volume, and it is not possible to have a volume of the Catalog published until there are enough dollars in the account to pay the printing bill for the next volume. Therefore, what I was trying to convey to you is that if we undertake to publish the Jansonius generic index, some way will have to be found to come up with the $25,000 to pay the printing bill before the contract is let. I did not mean to give you such a scare about that, however. I just meant to have you aware of the problems involved and that the money will have to be found somewhere. I was certainly not expecting you to come up with it! It will undoubtedly be the most difficult part of the whole project unless I get outrageously lucky in approaching various people for some help. What we will need presumably

Hunt Institute for Botanical Documentation

is some sort of an advance in funds for this purpose from somebody. If we should have subscriptions in hand for essentially the whole sum, I think that negotiating a loan will be relatively easy.

3. Where you start getting specific about how much to charge per set of cards and so on in your paragraph 3, I think you're giving us some very good ideas. I shall certainly take up the matter at the ASSP Executive Committee session in Rhode Island, but I doubt very much that the AASP will undertake to sponsor the project. It is only that they might be willing to place a sufficiently convincing stamp of approval on it that we would be assisted in raising the money which we need.

4. I am wondering whether you are now pretty definitely thinking in terms of finishing the project in installments?--I had thought from our previous consideration that you would really finish the whole thing as a one-shot deal.

5. In response to your last paragraph and to the P.S., I hasten to say that I am not at all discouraged about the proposition and think we should definitely go forth with the "joint project". From what you say, we will definitely ~~will~~ able to get our heads together in Rhode Island, and I intend to leave some extra time at that meeting specifically to do so with you and to politic on the project's behalf with some of my friends who will presumably be there.

Betty joins me in post-dated good wishes for your singing in the Requiem. It would have been fun for us to have heard it. Best wishes until we meet again. Don't hesitate to write some more if you have further ideas.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

IMPERIAL OIL LIMITED
339 — 50 AVENUE SOUTH EAST, CALGARY 24, ALBERTA

PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY
WESTERN REGION PRODUCING DEPARTMENT

June 9, 1972

Dr. A. Traverse
Department of Geology
Deike Building
UNIVERSITY PARK, Pennsylvania 16802
U. S. A.

Dear Al:

More than a week has passed since I received your letter of May 15, which crossed my letter. Your information regarding the pricing of the genera cards was welcome. It seems reasonable for a printer to arrive at the estimate given to you (about $50.00 per set of 4,500 cards), even if it doubles that quoted to me by our Imperial Oil reproduction department. But then, we did not include overhead and we would not have to make a profit on this job. Still, it bugs me to see the price climb to a level where it is out of easy reach for students and many individual palynologists (about $75.00 per set, including boxes).

The real venom of your letter, however, was in the tail, where you stated not to know how to finance this deal. I was surprised to learn of that, because you appeared very confident in earlier letters (March 19, 1971) and during my visit to Penn. State that financially the publication is a dependable venture, even if each issue loses some money. It was this very stability of financial basis of the CFSP which attracted me, because commercially it seems not an interesting venture to publish the genera file. In any case, I am not in a position where I can command up a sum of $25,000.00. Neither do I think that my company would advance such a sum for this purpose. It thus appears that our cooperation in the publication of the genera file is not a workable proposition after all.

There is the possibility to invite prepaid subscriptions, possibly through the offices of AASP and CFSP. Also, of course, the whole undertaking must be dealt with in steps, and thus both subscription and income and printers costs come piece-meal. A charge of $15.00 per set of thousand cards prepaid ($18.00 after appearance ?) should recur once a year for five years. Institutional subscribers probably can afford $20.00 and $25.00 respectively. The last issue does not need a special box, but can be filed into the first four boxes, and thus will be cheaper to produce. The same goes for future updatings, which should appear every (other?) year and should not require any losses, as each subscriber should be willing to protect the earlier investment. I expect

.../2

Hunt Institute for Botanical Documentation

every subscriber to the CFSP and a large majority of the members of AASP to want a set of genera cards. If the "Jansonius Index Generorum Sporarum Dispersarum" is published by the CFSP under auspices of the AASP, could AASP possibly canvas a number of larger oil companies for a commitment to support -- for five years only -- the present undertaking by a subsidy of $1000.00 (or even $500.00) per year? Such support, plus prepaid subscription, should see the project become a reality.

If you can see any merit in these considerations, I am willing to provide a brief write-up plus some mocked-up cards (with printing in upright position!) for a presentation to the AASP executive. Do you still have a meeting before Rhode Island? (I will be there to give a paper on scolecodonts).

On the other hand, if you consider my present thinking as unrealistic, let us not drag out our deliberations, but terminate any thoughts of seeing the genera file into print.

With best wishes,

Jan Jansonius

JJ/bam

P.S. Tonight I am singing in the Brahms' Requiem; I hope it is not over our joint project of the genera file that I'll "weep my tears".

Hunt Institute for Botanical Documentation

May 23, 1972

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada

Dear Jan:

Yours of 11 May was most welcome, but apparently crossed with my earlier letter to you regarding my research into the printing of the "Jansonius Index". I guess I had better wait for your comment on my letter before getting into that further. It might be worthwhile to check into other possible publication sources--I know of one through some friends in the Geography department here--it's in another state, but that would pose no real problem. I have a feeling, however, that the people at Commercial Printing here in State College are anxious for the business and were really sharpening their pencils--they might even go a little lower I suppose but I doubt that the final figure will be very much below what I quoted in the letter. That's just an awful lot of plates to be prepared.

I have glanced over your MS for Taxon. I suppose I had better not comment on it in detail at this time, but your point of view seems reasonable to me and includes things which Tate and I had discussed several times as being defective in the present reading of the Code. Perhaps I will write you again more thoroughly on the subject when I have had time to digest your letter more fully. In the meantime

In the meantime, best wishes as always.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

May 15, 1972

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada

Dear Jan:

Probably you have been wondering whether I would ever get around to writing you regarding further information on the "Jansonius generic index". I have spent most of this afternoon going the thing over with the people at the printing company who have been printing the Catalog for several issues now. I ran into one little roadblock which was that I could find nowhere in my folder of information from you the exact number of proposed cards, so I got an estimate from them on the basis of both 4,000 and 5,000 cards. The printing card for 5,000 cards would be approximately $25,000 and for 4,000 cards about $20,000. That was not a firm quotation but an informal estimate that will probably hold up pretty well. At 500 sets, which is what I used for a ball park estimate, that would give a printing cost of $50 for 5,000 cards per set and $40 for 4,000 cards per set. If more sets could be sold, the amount per set could be decreased, but I really think 500 sets is pretty near the maximum of reasonable expectation. If the number of sets to be sold were less than 500, the cost per set would, of course, rise rather sharply. As we mentioned the other day, we would want to package the sets in a substantial and attractive box of some sort, and there would be photographic and handling costs here in the Catalog establishment. I would therefore guess that pretty close ball park figure for the cost of the index to a subscriber would be $75. I emphasize that this is just a guess, but it is interesting to me that it ended up being in the same general zone of magnitude which you had in mind when we discussed the thing here.

One problem which I do not really understand at this point is where we would raise the $25,000 for the payment of the printing bill. This would have to be paid before the money from the subscribers

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is obtained, and at the moment I am not at all clear in my mind as to where I would raise that sort of money. Any suggestions? It would, of course, be in the nature of a loan, but from experience I would say this is rather difficult to arrange. Do let mekknow how you react to this and what further thoughts you have had on the subject. We all are still very interested in undertaking the project and are sure that the various problems can be ironed out one way or another.

Best wishes from your friends here at State College.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

IMPERIAL OIL LIMITED
339 — 50 AVENUE SOUTH EAST, CALGARY 24, ALBERTA

PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY
WESTERN REGION PRODUCING DEPARTMENT

May 11, 1972

Dr. A. Traverse
Department of Geology
Dyke Building
UNIVERSITY PARK, Pennsylvania 16802
U. S. A.

Dear Al:

Firstly, I wish to thank you for the very pleasant and hospitable reception you gave me during my visit to State College. I enjoyed my trip enormously because of it. I hope you will accept my apologies for not having written sooner, but things were hectic upon my return, both at home and in the office. Incidentally, I missed my connection in Pittsburgh, because Allegheny Air had oversold the flight to Toronto, and I was left behind with six others. I hope you found your Bask beret in your car; I noticed it in my vehicle as I drove by the parking lot.

We at Imperial have decided to await the results of the pricing job you are initiating in State College, regarding the off-set printing of the genera cards, before we pursue the idea any further of Imperial sponsoring an edition. Thus I hope that you will press that matter both with regard to time and price. I still feel strongly that a low price -- even at the cost of slightly more utilitarian production -- makes the work so much more available to the palynological profession, indeed would make it a working tool rather than an inaccessible reference source.

Enclosed is a Xerox copy of the m.s. sent to Taxon; as secretary of the Palaeobotanical Committee you would want to see it. Just today I got a reaction from Jim Schopf, who by and large agrees with my point of view as being reasonable, but did not have all references available to check them out. He also advised me to send you a copy.

Give my regards to your wife, and also to Tate and Bob Dunay; I hope to get some material for him in the mail soon.

Best regards,

Jan Jansonius

JJ/bam

Encl.

Hunt Institute for Botanical Documentation

April 24, 1972

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada

Dear Jan:

This is to express somewhat formally than I did when you left my pleasure in your being able to spend some time with us, thanks for all the good advice you gave us and especially for your interesting talk for the group which assembled at my home on Tuesday night.

I will be letting you know in the near future what I have found out regarding printing costs for the "Generic Index". We are enthusiastic about the possibility of publishing it at Penn State under the CFSP auspices.

Jane Dolsen says to tell you that she thinks that the plant in which you expressed an interest is Episcia according to my books that would be in the Gesneriaceae, as you guessed. I must say, however, that the species illustrated in Bailey's Cyclopedia of Horticulture does not resemble the one in Jane's office very closely.

Once again, thanks. Hope we will see you in Rhode Island.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

April 10, 1972

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary 24, Alberta
Canada

Dear Jan:

We are looking forward to your visit the week of the 16th of April. I do hope that you will keep in touch with me about your time of arrival. One item along that line which may be of interest is that I have tickets for you and me for a concert for the evening of the 16th. The concert starts at about 8:30, so naturally I am hoping that you could arrive by something like, say, 7:30. However, as I said over the phone whatever you decide to do will be all right with me, though I would appreciate knowing of your plans as soon as possible. If you are sure that you are going to be in town on the evening of the 16th, as I understand is the case, I should probably also make you a motel reservation.

I seem to recall from our previous discussion that you agreed to present a talk in my home for graduate students and others who may be interested, and I have gone ahead and scheduled that for the evening of Tuesday, 18 April. I have announced your title as having to do with a general survey of the scolecodonts picture, although I am wondering if it might not be possible to present something on chitinzoans too--probably that would make it too long. I have discovered in the past that about the most an evening group can absorb with any profit is something on the order of an hour. We then break up for beer and other refreshments and an informal talk.

Anyway, this is to let you know that we are looking forward with eagerness to your visit and to join you in hoping for good weather!

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

7 April 1972

Jean Bob — Yes. We will find what you need. [illegible]

Jan Jansma (Imperial Oil Ltd.)

is coming to see us in a couple of

weeks — will present a seminar

at my home (on scolecodonts and/or

chitinozoans) on 18 April, for graduate students.

Either Jan or I are covering his

lodging and cost of the seminar. Could

I submit a small expense account for

a dinner and/or a couple of lunches

for him in connection with entertaining him?

Al Traverse

Hunt Institute for Botanical Documentation

March 19, 1971

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue South East
Calgary, Alberta
Canada

Dear Jan:

Your interesting letter of 11 March just arrived (I never can believe the mail service around here!). I will do my best to react to your points and to answer questions as best I can at this point.

I am glad that you were not counting on making money on the project, because that would be quite out of the picture. On the other hand, you should not be greatly concerned that the Catalog is going to fail. It has been kept going one way or another now for well over a decade, and I see no reason why it should not keep going. I was merely trying to emphasize that it is strictly a hand-to-mouth operation, and we do not make enough money on it to cover the basic expenses (Penn State, in effect, subsidizes the Catalog)--let alone pay a fee to our authors.

As far as publication delay is concerned, ours is not really too much different from what you state for the "Fisk Memorial Papers". From the time we received your copy, providing it was in acceptable shape, until the volumes actually appeared, would be less than a year. Perhaps, much less than a year.

The Catalog is printed by off-set, and I believe that will be in the foreseeable future the best combination of quality and economy we can obtain. Providing that the original formats are in good shape, it really is very good. For your line drawings it should be sensational. The only trouble we have is with reproduction of half-tone photographs.

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I believe that if the Catalog were to undertake publication of your generic cards, the best way would be to maintain our loose-leaf presentation, with the illustrations included with the text, and I feel sure that we could design a standard format which you would find unobjectionable. Not only do many subscribers rearrange the loose-leaf pages within the volumes, as you suggest, but also some people like to get a set of the material on cards ("Option A"), so that they can be filed in drawers independently of volume designation. We have in these fourteen years acquired some feeling for what the market wants! As I say, the question of format I am sure could be resolved, providing that we came to an agreement on all of the other matters.

I have a firm plan to visit Calgary for the Triassic conference next summer. However, I am disappointed to discover that the dates of that conference (23-26 August, 1971) conflict with your vacation plans. (Incidentally, I am green with envy of anybody who can take a six week vacation, but that is aside!) I also rather more tentatively had hoped to attend the AIBS sessions in Edmonton in mid-June. Perhaps, if you respond favorably to this suggestion, I should firm up a plan to attend that meeting and include a day or so to confer with you on the generic index. I will be looking forward to hearing what you think about that. I don't recall the exact dates of the AIBS convention, but presumably somebody in your outfit will know. As I recall, it is not very far from Calgary to Edmonton as things go. It seems rather funny to be contemplating two trips to Alberta, when I have never been there in my life before! Thanks for your continued interest in this possible project.

Best wishes to you.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi
cc W. Spackman
H.T. Ames

Hunt Institute for Botanical Documentation

IMPERIAL OIL LIMITED
339 — 50 AVENUE SOUTH EAST, CALGARY 24, ALBERTA

PRODUCTION RESEARCH AND
TECHNICAL SERVICE LABORATORY
WESTERN REGION PRODUCING DEPARTMENT

March 11, 1971.

Dr. A. Traverse,
Department of Geology,
Pennsylvania State University,
Deike Building,
University Park, Penn. 16802,
U.S.A.

Dear Al,

Thank you for your letter of 29 January; I am sorry that an answer had to wait till now, but winter is a busy time for arctic exploration, and we find ourselves hard pressed.

But my genera cards is the present subject! I had no notion of financial reward, and the lack of such enticement from the side of the S. and P. Catalog is immaterial to me. However, your sombre words regarding the financial difficulties of the Catalog, whose existence seems uncertain from one volume to the next, makes me apprehensive that I might go down - horse and all - in midstream. As I already dread the amount of work involved in seeing a compilation of this nature and scope through print, the Damoclean sword of financial collapse hanging over the Catalog makes me a trepid lover loath to embrace. Joking aside, I learned about the possibility that a joint effort by Esso, Humble, and Imperial in subsidizing a publication in the "Fisk Memorial Papers" might be possible. This now is an attractive alternative avenue for publication, and I feel I must pursue this before committing myself one way or another. Thus the rest of this letter is gratuitous in that I am not binding myself to the Catalog - yet !

Your point is well taken that the entries cannot be printed as supplied by me: I started the project six years ago without thoughts of actually preparing a m.s.; only more recently have I upgraded my format and been more consistent in style and substance. Not knowing Russian myself, I have not questioned the Russian translations (although some short Maljavkina diagnoses I "translated" myself with a small dictionary); Colin McGregor and Imperial Oil have supplied most translations, but they may have to be checked. The other translations are

.... 2/

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all by myself (German, French, Spanish, Rumanian) and mostly done meticulously. The format of the entries at present is cumbersome, because of the quotation of the source; at least it has to be more uniform. The line drawings are nearly all by myself, except where the original author gave a line drawing (sometimes I "improved" upon those). The original ink drawings are on my original filing cards, which I will cut up and sacrifice to facilitate better reproduction of them. A number of them need additional work, which I prefer to do myself. A more pressing matter is, how to organize the entries into a larger work: strictly looseleaf and alphabetical (as the present Catalog), with the illustrations inserted with the text? Or a running text (alphabetical or arranged in a "turma" system) with the illustrations grouped morphologically on separate plates. The several options may make a substantial difference in the production costs, and in this matter I am rather a novice. A compromise edition might emulate Deflandre's latest volumes with cards (but I hope to merit a more soigné appearance and production). A looseleaf format as in the present Catalogs may make for an overly voluminous and cumbersome product, although it allows for individual taste in rearranging the entries.

The "Fisk Papers" recently had an in-press time of six months; such quick processing is very tempting: a person would know that the toil would not drag on forever. The Catalog in-press time would, at least in part, depend on availability of funds? How would you envisage a Catalog edition: off-set printing, through photo-negative, from typed masters? Or is some type setting machine with varied fonts available?

Well, I have asked even more questions than you gave me answers. I hope that you will maintain this present amiable correspondence, so the whole project will stay in motion without set-backs or lazy delays. If I don't spur myself on, it won't go anywhere. When will you be in Calgary? I plan a visit to Holland from 1 August - 15 September.

With best regards,

Sincerely,

Jan Jansonius

JJ:es

Hunt Institute for Botanical Documentation

January 29, 1971

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary, Alberta, Canada

Dear Jan:

It has been an unconscionably long time since we received your proposal regarding the generic index. You probably thought we have been ignoring it, but that is far from true. The subject has been much discussed by Professor Spackman, Tate Ames, and me. We are of the opinion that the idea has considerable merit and should be explored more fully.

I suppose we should burst a balloon or two at the very outset. The first one of these is to relieve you of any notion you may have had that the Catalog of Fossil Spores and Pollen is a paying proposition! Every volume loses money, and it is only a question whether we can scrape together enough money from somewhere to keep it going financially until the next volume is printed. In other words, if the Catalog were to undertake publishing your generic index, there would be some glory, but no cash, at the end of the rainbow!

We also have talked a great deal about the format in which we could publish the entries and such matters as what it would say on the spine of the volumes ("Jansonius, Traverse and Ames?"). The samples which you sent along are quite interesting, but I have some doubt as to whether we would publish each generic summary in exactly the way you would send it in. I rather suspect we would instead have to come to some standard style and edit the entries to conform. I also checked your Russian translation on one which you sent and noted that the translation is quite a bit less literal than I would be inclined to use. Another problem which occurs to us is whether you would want our illustration department to rework the line drawings or use them exactly as you submit them

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However, my purpose at the moment is simply to say that we are interested in your proposition, and would like to hear more about it, especially in relation to the matters which I have raised above but also in any other way you would see fit. I will be making a trip to Alberta some time during the coming summer--I'm not exactly sure when, and it would be very good if we could sit down for a face-to-face discussion of these problems. So let's hear from you about all this, and we'll take it from there!

Very best wishes to you.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/mdw

Hunt Institute for Botanical Documentation

October 27, 1970

Dr. Jan Jansonius
Imperial Oil Ltd.
339-50 Avenue S.E.
Calgary, Alberta
Canada

Dear Jan:

Your letter of mid-October was waiting on my desk when I returned from the Toronto meeting. That will explain why I did not discuss it with Tony Audretsch, in case you were wondering.

First of all, let me say that we will take care of your request for a new page 105-106, as soon as possible. I have an appointment next Monday with the printers to discuss the printing of the required extra pages. What a headache!

Regarding your proposal on generic diagnoses, I think it is an extremely interesting propoxition, and I have put it on the docket for discussion between Spackman, Ames and me, and you should be hearing something further about the deal before too long (warning-"too long" is very liberally interpreted around here).

Sorry I missed seeing you at Toronto. The meeting was a great success, beautifully organized, and well attended (110 registrants). Some of the previous hold-outs joined forces with us at the time of the meeting (examples-Ed Stanley, Dick Scott). As it now stands, your co-worker, Dr. Staplin, is the only significant non-member in North America. Reminds one of the mother who was watching the regiment drilling and said, "They're all out of step but John". I do wish he would go ahead and join us. Best wishes to you until we meet again.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

February 10, 1969

Dr. J. Jansonius
Imperial Oil Company
339-50th Avenue S.E.
Calgary 24, Alberta, Canada

Dear Jan:

Absolutely sans rancˇ! I wasn't trying to tell you off, or even trying to convert you to CFSP. Just wanted to be sure you had your facts straight. Volume 30 treats Samoilovitch's Western Siberia book, or, rather, part of it. The rest appears in Volume 31. Thirty is in press.

I guess the only real area of disagreement between us at the moment is the selection of papers. It really seems to me that taxonomic worth has to be considered, along with availability. But enough of all that.

Speaking of Stan's paper, which will also be in Vol. 31, reminds me to tell you that he is likely to be pretty annoyed at our treatment of his work. We have never worked a paper that contained so many errors, and I'm afraid our footnotes come off sounding a little critical. Tried to stick just to the facts.

Instrongly urged the program committee to try to get you to give a paper at the next AASP convention, which will be held here in October of this year. Hope they contacted you and that you will.

Best wishes as ever.

Yours very truly,

Alfred Traverse
Associate Professor of Geology

AT:kwc

Hunt Institute for Botanical Documentation

Dr. Alfred Traverse
Catalogue of fossil Spores and Pollen
Deike Bldg., Penn. State University
University Park, Penn. 16802, USA

28 Jan 69

Dear Al,

In haste this informal reply to your letter: As our reference library is in good shape, we rarely find it necessary to go to the Catalogue – this habit was thoroughly ingrained by its early lack of generic diagnoses. Thus it had escaped me that in the last issues this complaint has been met: please forgive my rashness.
I maintain my opinion about accessibility: in the last issues only papers have been extracted that are still in print, that can be ordered easily through any bookstore. We have several copies of the Schulz paper (of which I made a translation some years ago) and our library is linked in with others through the Inter-Library-Loan system. I will say that v. 29 was not too bad, and possibly an eye-opener for some subscribers. However, I know that one of your next volumes will contain Stan Pocock's paper. Little notice is taken of literature from countries as USSR, Turkey, India, Brazil; even if an individual orders a recent publication from these countries, he will be unsuccesful. The Catalogue here might well provide an extra service.
As the title in full reads "Catalogue of fossil Spores and pollen" I think that you should stay within this definition. Tasmanites etc. are linked in with the acritarchs and dinoflagellates, and I am sure that Eisenack will cover them eventually. But even if there is a little bit of use (= stratigraphic application) in having some extraneous matter in the Catalogue, I still think it is not keeping

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faith with the subscribers. Why not also give pictures of woody tissue or epithelial structures that may have significance? My point is that if you start to include other groups (that might fill some dozen Catalogue volumes by themselves) you get never done with even the spores and pollen. Especially with the raising costs: I for one would have preferred a lower price for v. 29 and have done without all the acritarchs (or algal cysts, whatever you prefer) that I now was forced to finance.

I realize that only so much can be done – it will still be many years before the Catalogue is ever up to date. However, I feel that only then will it become a useful instrument. I will for now maintain my decision to cancel my subscription after the next index volume: if I had unlimited resources I would not do so.

Thank you for your letter and I hope you will be sans rancune. With best regards,

Jan Jansonius

Hunt Institute for Botanical Documentation

January 16, 1969

Dr. Jan Jansonius
Imperial Oil Enterprises Ltd.
500 Sixth Avenue West
Calgary 1, Alberta, Canada

Dear Jan:

Thanks for yours of 9 January, 1969. We will of course honor your request for subscription cancellation. However, the statements you make are without foundation. In order to keep the record straight, and in urging you to investigate the facts before you make up your mind permanently, I submit the following comments on your points--

1. Generic diagnoses are given. There was a time, many volumes ago, when they were not, but that was long before I came here. I call your attention, in the most recent volume, to p. 29-136, 29-115, etc., etc.

2. I can't see how you came to the conclusion you have about accessibility. Just to take one example, the paper by Schulz in vol. 29 is, so far as I know, not accessible in any North American library. The selection of papers, of course, is a difficult matter, and we try to effect some balance, stratigraphically, geographically, and so forth. We don't want to catalog only "odd" papers.

3. Regarding acritarchs, you apparently have not read our statements on the matter (Compilers' Preface to volume 29)--we very categorically asserted that we would not cover any acritarch which is already being catalogued. To be specific, we are covering things like Tytthodiscus. Did you have information that Eisenack is going to cover things like that? If so, it is stretching "Dinoflagellates, Hystrichosphaerids, and related microfossils" close to the limit. It certainly is not our intention to catalog what others are cataloging.

It would be useful to us to have constructive comments from you as a (former?) subscriber about ways that we could improve our operation. It is too bad that we have to increase prices, but how can we hold the line

Hunt Institute for Botanical Documentation

when everything from printing to shipping-boxes is going up? Do favor us with some positive criticisms--ways we could do better.

Best wishes.

Yours very truly,

Alfred Traverse
Editor

AT:kwc

Hunt Institute for Botanical Documentation

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REGIONAL [redacted] MANAGER

W. B. DINGLE,
ASST. REGIONAL OPERATIONS MANAGER - PRODUCTION

J. C. UNDERHILL,
ASST. REGIONAL OPERATIONS MANAGER - EXPLORATION

January 9, 1969

Dr. Alfred Traverse
Catalogue of Fossil Spores and Pollen
Deike Building
Pennsylvania State University
UNIVERSITY PARK, Pennsylvania 16802
U. S. A.

Dear Dr. Traverse:

In reply to your circular letter of December 20, 1968, I want to confirm my subscription only for volume 30 and the index volume covering volumes 21-30. My subscription number is D-329; please have it cancelled starting with volume 31.

I consider that the usefulness of the Catalogue has not met early expectations and in the last few years has decreased in proportion to the increase of its price. My specific gripes are mostly that: 1/ no generic diagnoses are given, although these often contain information that is not repeated in the specific descriptions.

2/ although complete usefulness of the Catalogue depends on completeness of coverage, the recent trend to excerpt only papers that are easily accessible and not out-of-print in preference to more obscure and less accessible papers reduces the immediate benefits of a subscription.

3/ the decision to include acritarchs in the catalogue --thus duplicating the Catalogue of fossil acritarchs and dinoflagellates by Eisenack--is a further obstacle in reaching an early complete coverage of fossil spores and pollen and makes it even more doubtful that the Catalogue ever will catch up with the literature.

In summary, I have decided that for me the benefits of continuing subscription are not worth the charge.

With best personal regards,

Sincerely yours,

Jan Jansonius

JJ/bm

Hunt Institute for Botanical Documentation

October 7, 1968

Dr. Jan Jansonius
Imperial Oil Ltd.
339 50th Avenue Southeast
Calgary, Alberta
Canada

Dear Dr. Jansonius:

My inadequate filing system seems to say that I never acknowledged your nice letter of 27 March. You were probably right in that letter in hinting that our stationery-claim of being "the international compendium..." is a bit pretentious. Still--there isn't anybody else in the field, so guess it is accurate, since what we do does seem to fall within the requirements of the word compendium per the dictionaries we have.

By the way, if you are working on a compendium of spore genera, I do hope that you will be willing to share it with us as soon as possible. I am curious as to what you mean by "spore genera in the English language"--I thought that they were all in the Latin language.

Congratulations on your "Spore nomenclature....Lille Palynologists", Pollen et Spores 10 (1968) 177 pp. I haven't seen the paper by the Lille group, but what you say seems much to the point. Folks just never seem to realize that the purpose of names is to name, not to classify!

Thanks again for your letter and the further info re what you did with Gnetum. I still have trouble accepting the genus as a member of the Pinatae!

Best wishes.

Yours very truly,

Alfred Traverse
Associate Professor of Geology

AT:kwc

Hunt Institute for Botanical Documentation

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PRODUCTION RESEARCH AND
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WESTERN REGION PRODUCING DEPARTMENT

V. C. LARSON
MANAGER

March 27, 1968

Dr. Alfred Traverse,
Department of Geology,
Deike Building,
Pennsylvania State University,
UNIVERSITY PARK, Penn. 16802,
U.S.A.

Dear Dr. Traverse:

I, too, am happy that we are developing a more active and fruitful contact. This certainly has been helped along by your new position, in which you are less required to hide your light under a bushel.

What is the significance of the heading: The International Compendium of Palynological Systematics"? You certainly arouse my curiosity; especially as I have been working on a "compendium" of spore genera in the English language, without much expectation to complete this project within two years.

Our idea of assigning Gnetum to the Pinatae is conservative in so far as most systematicians leave the "Gnetales" (including Ephedra, Gnetum and Welwitschia) in the gymnosperms s.l. We separated Ephedra and Welwitschia, which are very different from Gnetum in many respects, as far as possible on our chart. Being without competence to evaluate the systematic position of the latter on basis of the vegetative characters, we concluded, that if it is a gymnospermous plant, its pollen morphology agrees most with those of the asaccate Pinatae. We have no other basic data to go by than those in the published literature; however, these do include electron microphotographs of Gnetum pollen (Gullvåg, B.M., 1966, Grana Palyn., v. 6, no. 3, p. 435-475, pl. 1-27).

Happy to have been of assistance already,

With our best regards,

Jan Jansonius

JJ:jp

Hunt Institute for Botanical Documentation

March 20, 1968

Dr. Jan Jansonius
Imperial Oil Ltd.
300 9th Avenue S.W.
Calgary, Alberta
Canada

Dear Dr. Jansonius:

Thanks very much for "Systematics of the Chitinozoa" and "Relationships among Gymnospermous Pollen". As far as I can tell, we have not been exchanging things much, and I am glad that the ice is broken.

You will be either happy or embarrassed to learn that I have made considerable use of both papers in my just completed annual course in general palynology here. The drawing on p. 346 of the Chitinozoa paper was especially useful.

Your idea of sticking Gnetum in with conifers shakes me up. I suppose you have looked at the leaves and "flowers" of Gnetum. Somehow I find it hard to buy the thing as related to conifers, despite the inaperturate pollen grain. Perhaps you allhhave some details about fine structure of the wall that I dnn't know about?

Thanks for remembering me.

Yours very truly,

Alfred Traverse
Associate Professor of Geology

As

AT:kwc

Hunt Institute for Botanical Documentation